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EXPERIMENTS IN THE ARMCHAIR: A HISTORY OF MICROECONOMETRICS AND PROGRAM EVALUATION AT PRINCETON

EXPERIMENTOS NO ESCRITÓRIO: UMA HISTÓRIA DA MICROECONOMETRIA E AVALIAÇÃO DE PROGRAMAS EM PRINCETON

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Tese apresentada ao Programa de Pós-Graduação em Economia do Departamento de Economia da Faculdade de Economia, Administração e Contabilidade da USP, como requisito parcial para obtenção do título de Doutor em Ciências.

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RESUMO

Esta tese tem a intenção de se somar aos esforços recentes de economistas e historiadores em contar a história da microeconometria. Isso acontece por meio de uma narrativa que conecta a ascensão da avaliação de programas no governo dos Estados Unidos e na universidade Princeton. Começando com o fato de que a maioria dos economistas não tem nenhum controle sobre o processo de geração dos dados utilizados em suas análises aplicadas, o argumento se desenrola para destacar o papel dos econometristas como observadores passivos. A análise se desenvolve destacando como esse conceito resume a responsabilidade da econometria por dados não experimentais. A tese narra como, desde a década de 1950, os economistas sabem que não podem intervir nos dados que analisam e devem estar preparados para lidar com qualquer problema inerente à coleta externa de dados. Usando métodos bibliométricos e fontes secundárias, a tese argumenta que a microeconometria emergiu da mudança de compreensão das observações passivas como simultaneidade para as observações passivas como variáveis omitidas. Este argumento holístico é contrastado com uma microhistória das instituições governamentais dos EUA para avaliação de programas e da Seção de Relações Industriais de Princeton. Assim, conta como, na década de 1970, a pobreza estava aumentando nos Estados Unidos e os economistas estavam mais preocupados com ela do que com o aspecto teórico e filosófico das observações passivas. Nas organizações do governo dos Estados Unidos, a avaliação do programa passou de um tema qualitativo nas ciências sociais para um problema quantitativo dentro da economia. A tese segue de perto esse desenvolvimento usando metodologias de processamento de linguagem natural, arquivos do governo dos Estados Unidos e histórias orais de membros do *Office of Economic Opportunity*. A partir dessa narrativa, a tese mostra como a avaliação de programa sai do governo e vai para o interior de um departamento de economia, mais especificamente a Seção de Relações Industriais de Princeton. Olhando de perto o departamento, a tese contrasta as soluções de dois jovens estudiosos de Princeton para o problema das variáveis omitidas, o modelo de seleção de James Heckman e o estimador de diferenças em diferenças de Orley Ashenfelter, para demonstrar que o confronto entre randomistas e modeladores estruturais é um artifício criado a partir de duas soluções para o mesmo problema. Bibliometria (utilizando a metodologia inovadora de redes relacionadas), evidências primárias e secundárias demonstram que as soluções não eram concorrentes nos primeiros dias de variáveis omitidas, e que a concorrência resultou de uma mudança de contextos dos principais autores. Por fim, ainda de um ponto de vista de dentro do departamento, a partir de nove entrevistas realizadas com atores da Seção de Relações Industriais das décadas de 1970 e 1980, a tese analisa como os experimentos naturais se desenvolveram gradativamente dentro do departamento, sem revoluções ou reviravoltas. A tese conclui mostrando como as tarefas corriqueiras da vida acadêmica de Ashenfelter e seus alunos transformaram a economia.

Palavras-chave: microeconometria; avaliação de programas; Princeton; experimentos naturais; guerra à pobreza; observações passivas.

ABSTRACT

This thesis joins the recent efforts of econometricians and historians to tell the history of microeconometrics. Because economists mostly do not have any control over the collection of the data they use in their applied analyses, econometrics became a tool of passive observations —a concept that epitomizes the accountability of econometrics for nonexperimental data. In the thesis I add to that literature by connecting the rise of program evaluation in the US government with Princeton University. To do so, I discuss how, since the 1950s, economists knew that they cannot intervene in the data they analyze and have to be prepared to deal with any inherent problem of such data. Using biobliometric methods and secondary sources, I argue that microeconometrics emerged from a change in the understanding of passive observations as simultaneity to passive observations as omitted variables. Although this change may seem purely internal to econometrics, I argue that a micro history of US governmental institutions for program evaluation and Princeton's Industrial Relations Section is necessary in the history of microeconometrics. I show how, in the 1970s, poverty was on the rise in the United States and economists and econometricians were more concerned with it than with the theoretical and philosophical aspect of passive observations. Within the government, program evaluation went from a qualitative topic in social sciences to a quantitative problem inside economics. In order to analyze this change, the thesis uses natural language processing methodologies, archives from the US government and oral histories of the members of the Office of Economic Opportunity. I then show how program evaluation traveled from the government to Princeton's Industrial Relations Section. Looking at the grassroots of the department, I contrast the solutions of two Princeton young scholars to the problem of omitted variables, James Heckman's selection model and Orley Ashenfelter's difference-in-differences estimator, to demonstrate that the clash between "randomistas" and structural modelers is an artifact created after those two solutions to the same problem. Evidence from bibliometrics (using the innovative methodology of related networks), primary and secondary sources demonstrates that the solutions were not concurrent in the early days of omitted variables, and that the present-day clash resulted from a change in the contexts of the two main authors. Finally, using nine interviews conducted with actors in the Industrial Relations Section of the 1970s and 1980s, the thesis analyzes how natural experiments developed gradually inside the department, without revolutions, turns or shifts. The thesis concludes showing how the ordinary business of the academic lives of Ashenfelter and his students ended up transforming economics at large.

Keywords: microeconometrics; program evaluation; Princeton; natural experiments; war on poverty; passive observations

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PREFACE AND ACKNOWLEDGMENTS

Four years ago, I had just started my PhD. at the University of São Paulo without a clear idea of what my thesis would be. I was interested in the philosophy of economics and the history of economic thought. Prof. Pedro Garcia Duarte, my advisor, was certainly more inclined to advising a thesis on history rather than philosophy.

I was full of ideas, but mostly on the intersection of philosophy and computation. Those were not exactly topics that Pedro could help me with. Then, I remember smartly proposing to do a history of philosophy of economics. I would still do philosophy and my advisor would be pleased that I was doing history as well. Pedro liked the idea and so I started the review of the literature. Wade Hands's *Reflection without Rules* was the first book I read. The history was already there. I could not imagine doing something beyond what Hands had done.

I had a problem – as most PhDs have. What could I do in my thesis? As a "smart guy" still wanting to study philosophy, I stick to my strategy. I would find a philosophical concept that interested economists and would do a history of that concept in economics. After pondering about this, causality appeared to me as the concept to study, and I am not even sure why or how. But it seemed as nice a concept as any other. Pedro liked this idea as well.

I started again the review of the literature. Causality was not thoroughly discussed in the history of economic thought. Kevin Hoover had worked on it, but it seemed that there were topics to be explored. From Kevin's papers, I went to the history of econometrics – who else rather than econometricians would be concerned with causality, right?

Well, historians of econometrics had a lot to say about macroeconometrics and time series, but not exactly about causation. Together with Pedro, I discussed the idea that maybe causation was not in the literature because microeconometrics was more concerned with it than macroeconometrics, and perhaps the history of economic thought had not focused on the history of microeconoemtrics. To test that, I downloaded the mains books and articles on the history of economic thought and searched for the term "microeconometrics". What happened was appalling: historians of economic thought had not mentioned the term not even once in the selected papers and books in contrast to several mentions to macroeconometrics. I decided that, instead of a history of causality, I would do a history of microeconometrics. At the time I called these topics "experiments outside the laboratory" because I could not differentiate microeconometric techniques from natural experiments and field experiments very well. My project's name then was "A History of Experiments Outside the Laboratory as a History of Econometrics" inspired by Marcel Bouman's book "Science Outside the Laboratory Measurement in Field Science and Economics".

What you are going to read in the next pages is the development of this project. It was a project of discovery as well as history. Numerous hard questions came along the way: What is microeconometrics? What is program evaluation? How have they started? What is causality? Are macroeocnometricians and microeconometricians concerned with it? Are these fields different geographically? How Europe does microeconometrics? Does other disciplines also have similar concerns? What are field experiments? What are natural experiments? So on and so forth.

For most of them I would never find a definitive answer and Pedro helped me to polish my thesis to what I would be able to respond. I defined a place, actors and topics more narrowly. Panhans and Singleton (2017) had indicated that Princeton and Orley Ashenfelter would be an interesting starting point. Heckman (2001, 2000) showed that the the dispute between "randomistas" and structural modelers had to be historically investigated. Levit and List (2008 and Angrist and Pischke (2010) put me on the track of experiments, quasi-experiments and instrumental variables.

The question that followed was: how Princeton participated in the development of quasi experiments that lead to the dispute between randomistas and structural modelerss? It was a Frankenstein, but one that I could work with.

As the thesis developed, "outside of the laboratory" appeared to be the wrong label to the topic I was researching. Field experiments play a secondary role in the thesis. The focus is on instrumental variables, natural experiments, and other econometric techniques. Thus, it is not a history of all experiments that happen outside the laboratory in economics, but only of those that happen in the armchair of offices. The end product that you have in front of you is thus now called: "Experiment in the Armchair".

I certainly have not reached this point alone. Several people have helped me along the way. First, I must thank Prof. Pedro for his willingness to advise such a stubborn student. It is very hard to change my mind, and Pedro, in the moments that he had to, was able to do it. Thanks to him, I can say proudly that I am a historian of economic thought. I was very skeptical about history in the beginning, and now I more than enjoy it.

Our history of economic thought discussion group was very helpful also. I could have not done this thesis without their concerns and ideas. I thank all the members that have contributed to the group: Lúcia Centurião, Matheus Assaf, Matthieu Renault, Bruno Damski,

Hugo Chu, Tiago Graciani, and Jéssica Nascimento. Lúcia has been my cohort colleague and has also been advised by Pedro, I must thank her for sharing the same anxieties or at least saying so the times I came to her to rumble about the PhD.

With Matheus I had the wonderful opportunity of sharing a home at Durham during our time as research fellows at the Center of Research on the History of Political Economy of Duke University. He and Michelle made our humble home, a happy home.

My time at Duke was essential for the development of my thesis. I would not be any close to the historian I am now without my time there. The place and community were amazing. Kevin Hoover accepted to be my sponsor, and I could not have had a better one. He read my papers with care, discussed about writing and the US history with me, and although we disagreed a lot in these discussions, he gave me rides and even invited me to a philosophy group. I learned a lot about how to be a researcher with him.

Paul Dudenhefer was also important during my time at Duke. Paul always made sure everything was ok with all of us research fellows and was nice to know someone was looking over us. Also, in his writing group and several conversations we had, he taught me that we should see ourselves as writers and that writing is a craft to be learned. This helped me to start developing my own writing style, that although does not pleases everyone, is mine and in constant development.

I must also name everyone else who spent time with me at the Center during my time there, each one of them was important in their own way. Jason Brent, Steve Medema, Roy Weintraub all made comments about my work, helped me in arranging the interviews with some of the actors in my narrative, and incentivized me to keep going. With Jennifer Jhun I had a lot of interesting conversations about the philosophy of economics. It was a great break on all the microeconometrics and history that helped me to maintain a critical perspective on what I was doing. The other research fellows were great both personally and academically. I had fun and learned a lot from them. I must then thank Nathalie Sigot, Daniel Nientiedt, Stefan Kolev, Melissa Vergara-Fernández, Anthony Rebours, Camila Orozco-Espinel, Soroush Marouzi, Pierre-Christian Fink, Jonathan Cogliano and Anna Noci.

People in São Paulo and at USP were also particularly important in this journey, in special my PhD cohort: Hector Luz, Lúcia Centurião, Karina Saas, Keyi Ussami, Lucas Rodrigues. We had some good times and interesting discussions that helped me a lot.

I've been to France twice during my PhD. and had the opportunity to participate in conferences there, the first in Nice and the second in Lille in 2019. In the first I had interesting discussions with Uskali Mäki and N. Emrah Aydinonat. In the second, the 2019

ESHET, I presented the third chapter of this thesis. I must thank Andrej Svorencick for commenting the paper in that opportunity, and Professor Harro Mass for his thoughtful comments as the chairperson of the session.

During those two weeks in France in 2019, I also had the opportunity of working at the University of Cergy Pontoise, in which Yann Giraud kindly received me. He discussed my research and helped to narrow my focus. Beatrice Cherrier also discussed my research with me in those weeks in Cergy.

During the development of the thesis, I had to present my work at least three times for committees. They were composed by Marcel Boumans, Naércio Menezes and Renata Narita. In all of them, I received important insights to put my research on the right track.

In 2018, I submitted the first chapter of this thesis to a competition for young scholars of RHETM. My paper was selected to be reviewed and the editors Carlos Eduardo Suprynìak and Scott Scheall asked Prof. Mary Morgan to read the paper. All three of them gave me feedback that helped in writing the chapter. I decided to withdraw from the competition, but the feedbacks were very important for the thesis.

Finally, I must also thank all the researcher who accepted to be interviewed by a young historian of economics: Orley Ashenfelter, David Card, Joshua Angrist, Michael Ransom, Gary Solon, Joseph Altonji and Daniel Hamermesh. All of them took the time to discuss about their histories and memories and made this thesis possible. Orley not only agreed to one interview, but three, and I must thank him a lot for that. Hamermesh, Solon and Card also offered me important feedback by email that ended up being relevant for the development of the thesis.

I am grateful also to The São Paulo Research Foundation (Fundação de Amparo à Pesquisa do Estado de São Paulo - FAPESP) for funding the research and believing in it since the beginning, funding both the research at USP (process number 2018/07105-6) as well as my visit to Duke (process number 2019/17650-4). I must also acknowledge a short period of funding from Conselho Nacional de Desenvolvimento Científico e Tecnológico (CNPq).

In a more personal side, I must thank my friends Emilio Bier and Arthur Marques. The first was always there for me during the whole period even though not being geographically in the same place. Also, he made me buy a PS4 that was essential to keep in touch with him and coping with the hardships of the PhD. This was more important than expected. The second, he heard my rumbling about my thesis and gave interesting insights from the perspective of someone from Cinema. I even read books about documentary making because of him. He also received me in Paris the two times I've been there and helped me find cheap beer there, something that is not easy to do if you are a tourist.

Of course, my family has always supported me. They do not actually understand what I do and the specific hardships that we deal during a PhD, but they did their best to help me. My father always received me with the best barbecue every time I went back to South Brazil. My mom took care of my mental health, something that only a mother could do. And my grandmother has always given me the love that she had, even in the hardest moments for her, and this has made me stronger to finish this PhD.

At last, I must thank my wife, Caroline Paim da Silva. She has probably not read more than this preface and was still the most important person for me to finish this whole project. While on the surface a PhD. might be about the thesis content, for me it was also about maturing. The most important lesson I learned during this PhD. was not about microeconometrics or history, it was about learning what I value the most: her and our family.

INTRODUCTION

The history of microeconometrics is mostly an untraveled territory because, in fact, the history of econometrics at large is also mostly an untraveled territory. The first systematized research on it happened in the late 1980s and early 1990s. Roy Epstein (1987), Mary Morgan (1990), and Duo Qin (1993) told the history of the Cowles Commission approach in the first book-length incursions in this history.

The term microeconometrics has not been cited not even once versus several citations to "macroeconometrics" in their books. This is not a surprise, given that the Cowles Comission approach had been hardly developed with the aid of macroeconomic datasets and problems. It is not thus a misconception of the authors. In fact, their works reached a success that led to further development of the history of econometrics, such as the History of Political Economy special issue of 2010 (see: Boumans and Dupont Kieffer 2010).

However, in following closely their footsteps, the history of econometrics as told by historians of economics has up to this point mostly developed around the Cowles Commission Approach. This situation contrasts with that of microeconometrics. In 2000, a History of Political Economy conference was organized titled "Toward a History of Applied Economics", stating the necessity of studying further the topic (Backhouse and Biddle 2000). Despite the efforts of the conference, not much has been said at that moment about the history of microeconometrics.

This situation has been changing recently. In 2017, Roger Backhouse and Beatrice Cherrier (2017b) organized a second conference on the history of applied economics "The Age of the Applied Economist: The Transformation of Economics since the 1970s". In constraining the analysis from the 1970s onwards, the conference ended up bringing new topics to the forefront of the history of econometrics such as microeconometrics, experiments, natural experiments and causality.

While these subjects are still under the scrutiny of the historian's community, one major episode has already been identified but has been scantly analyzed: the rise of experiments. Field experiments and quasi-experiments have been initially analyzed by the history of economic thought community (see Panhans and Singleton 2017, Biddle and Hamermesh 2017), but mostly by economists involved in these practices (see Levitt and List 2008, Angrist and Pischke 2010). This thesis comes to sum up to these initial efforts of econometricians and historians of telling the history of microeconometrics. The connection between these experimental methods and econometrics is still obscure and the objective of this thesis is, therefore, connecting these narratives of experimental methods with the history of microeconometrics and econometrics at large. This will happen through a history of the rise of program evaluation in the US government and Princeton.

Chapter 1, thus, starts with the fact that economists mostly do not have any control over the data generating process in their applied analyses. They are borrowers of data, that usually comes from institutions to their desks and computers. For Trygve Haavelmo (1944), Tjalling Koopmans, (1979) and Marcel Boumans (2010), this means that economists are *passive observers*. For them, this nomenclature summarizes the fact that economists cannot intervene in the data they analyze and have to be prepared to deal with any inherent problem in the external gathering of data.

In contemporary textbooks and econometrics classes, these inherent problems are classified as endogeneity bias. The most common are measurement errors, simultaneity, and omitted variables. However, these problems have not always been in the same category. Each one of them was tackled in different periods of time.

The first, in the 1930s, was measurement errors. In the 1950s simultaneity was dealt with. In the 1970s, omitted variable were the focus of econometricians. Dealing with each one of these problems developed econometrics in a different manner. Omitted variables, in the 1970s, are behind the rise of the field of microeconomertics and program evaluation.

In other words, Microeconometrics has emerged from the changing understanding of *passive observations as simultaneity* to *passive observations as omitted variables*. Thus, the widening of the passive observation problem led to the development of microeconometrics, as we shall discuss in chapter 1.

While looking from a remote point of view, the conceptual and technical transformations of passive observation explain the emergence of microeconometrics, when looking closer, this narrative misses historical relevant facts. Terms such as passive observation, omitted variable and simultaneity have not been in the spotlight in the actual historical scenes. In the grassroots of economics there was a more acute conundrum to be dealt with: poverty.

In the 1970s, poverty was on the rise in the United States. Since World War II, the American society had overlooked this problem due to the more pressing issues of international relations But in that decade the situation of poor people was getting uncontrollable and starting to be widely reported in the media.

Magazines such as *Life* and *The New Yorker* stamped poverty in their covers. The democrat presidents, John Kennedy and Lyndon Johnson, decided to act on the issue while also dealing with the Vietnam War. In 1964, Johnson signed the Civil Rights Act and the

Economic Opportunity Act. These were important achievements for American society, but which the consequences to social sciences were still to be uncovered.

Since 1946, the Council of Economic Advisers had become an important institution inside the government. However, up to the 1960s, it was mainly concerned with macroeconomic problems such as product, productivity, employment, and so forth. Poverty, crime, and health had never been in their radar before. This was about to change with the arrival of the War on Poverty, or more precisely, with its development.

Chapter 2 will be concerned with these developments. The chapter is about how program evaluation entered the realm of economics through research facilities of the US government. The narrative follows closely the development of program evaluation inside US government during the War on Poverty and is bolstered by oral histories of actors to show how program evaluation went from a qualitative topic in the social sciences to a quantitative problem inside economics.

During the 1970s, data produced by the government while fighting poverty started to reach the universities. In the economics departments, students were quick to notice the problem of omitted variables and had, thus, to find a way to circumvent it. From our current notion of microeconometrics, we know that at least two solutions must have arisen during this period. In the contemporary microeconomics literature, economists are divided into two communities, randomistas and structural modelers (see: Deaton and Cartwright 2017, Heckman and Vitacyl 2005, Heckman and Urzua 2009, Imbens 2009, Heckman 2010, Angrist and Pischke 2010, Ravallion 2009, 2018).

The question that emerges then is how from one single problem two opposing communities have developed. James Heckman (2000, 2001), for instance, presents a departmentalized history in which structural modelers and randomistas developed distinctly, without much contact with each other. Chapter 3 comes to question this point of view.

Using bibliometric methods, the chapter shows that current discussions were not as present as one might imagine and that structural modelers and randomistas were concerned with the same problems. Both were concerned with omitted variables and how to deal with them and I propose that this problem is the core of microeconometrics.

Moreover, it delves into the history of James Heckman and Orley Ashenfelter, who nowadays are known as precursors of structural modelers and randomistas respectively. From today's point of view, we know that they came to be known as the main solvers of the sample selection bias. By the end of the 1970s, they had implemented randomization to the labor data in two different manners: the sample selection model (Heckman 1979) and the difference in difference estimator (Ashenfelter 1978).

Chapter 3 shows that both had a close relationship and similar concerns in their early careers. Both are labor economists with a PhD. Degree from Princeton. They are almost contemporaneous, the former is the youngest born in 1944, and the latter is two years older, born in 1942. They met in Princeton and ended up publishing together during the first years of their careers (Ashenfelter and Heckman 1971,1972,1973,1974).

Heckman followed his path leading him to Chicago and Ashenfelter stayed in Princeton, becoming the director of the Industrial Relations Section (IRS) and the main name behind Princeton's labor economics. Before that, Ashenfelter joined the effort of the Government against poverty. Ashenfelter assumed the directorship of the Office of the Assistant Secretary for Policy, Evaluation, and Research (ASPER) in 1971. He accepted the offer, and his choice affected not only his career, but also the course of Princeton's Industrial Relations Section and of economics at large.

In fact, his history at Princeton contrast with the literature on the history of natural experiments. In in widely circulated papers words such as "revolution", "turn" and "shift" set the tone of the narratives (see Panhans and Singleton 2017, Biddle and Hamermesh 2017, Levitt and List 2008, Angrist and Pischke 2010, Backhouse and Cherrier 2010). This narrative imply that natural experiments developed outside economics and were suddenly imported to economics departments through revolutionary efforts.

This is not an accurate picture. In the same way that structural modelers developed their research agenda in universities focused on modeling agent behavior, in Princeton natural experiments developed naturally and gradually inside the industrial relations section directed by Ashenfelter. He had stayed only one year in Washington and then went back to Princeton. From then on, he followed the interests acquired in government and enlisted students interested in these same conundrums.

Chapter 4 shows how the ordinary business of the academic lives of Ashenfelter and his students, publishing in top-tier journals and pushing forward their discoveries, transformed economics. This is a history about how day-to-day can be as impactful as any revolution. It is, thus, a micro history of an economics department and seeks to contribute to understanding how knowledge is produced in the grassroots of the departments.

The chapter follows closely the accounts of several actors in the history of the IRS to tell this history. A total of nine interviews were conducted in 2020 with Orley Ashenfelter (APPENDIX 7, 8 and 9), David Card, Joshua Angrist (APPENDIX 6), Michael Ransom, Gary Solon (APPENDIX 10), Joseph Altonji and Daniel Hamermesh (APPENDIX 11) to understand how the IRS dealt with "natural experiments" and program evaluation. Inside Princeton's IRS, as told in the interviews, program evaluation saw no "shift", "revolution", nor "turn" as we shall see in the coming pages.

1. PASSIVE OBSERVATIONS IN THE DEVELOPMENT OF MICROECONOMETRICS

1.1. Long story short

Economists, for the most part, do not gather data themselves.[1]. They do not have any control over the collection of data or the data generating process. Econometricians are mostly consumers of data. They borrow data from institutions to their desks. According to Tjalling Koopmans, (1979), economists are the meteorologists of economic life, *passively observing* the turbulences and fluctuations of the economy.

To Marcel Boumans (2010), being passive observers rather than active gatherers of data came with a price for economists. Passive observations have and inherent problem. Without engagement in the data generating process, economists cannot intervene in the data to detect causal factors. They cannot isolate phenomena before their experiments and models. Nowadays, the passive observation problem is widely known as endogeneity bias, but to reach this encompassing notion of the problem a long road had to be traveled.

Long story short, the endogeneity bias as the contemporary manifestation of the passive observation problem evolved roughly through solving its different sources. Initially, during the 1930s, errors in variables (nowadays, measurement errors) were the focus of econometricians. In the sequence, the probability revolution of the 1940s solved the identification problem (simultaneity bias). From the 1950s to the 1970s, omitted variables, especially as occurring in panel data, began to be dealt with. In the 1970s, in industrial relations departments, self-selection and more advanced forms of omitted variables were tackled, giving rise to a new field of econometrics: microeconometrics.

Considering this overview, econometrics has gradually expanded its point of view of the error term and how to cope with it. As is going to be seen, this happened through a circuit created between econometricians and society, establishing passive observations as the responsibility of econometricians and allowing increasingly demanding tasks from the society. Microeconometrics has emerged during the passage from *passive observations as simultaneity* to *passive observations as omitted variables*, when econometricians started to deal with new disaggregated data. Thus, the widening of the passive observation problem led to the

[1] Contemporary exceptions are experimental economics and field experiments which will be discussed throughout the thesis.

development of microeconometrics. Microeconoemtricians became – roughly - accountable for omitted variables and macroeconometricians for simultaneity and time-series.

History of economic thought has up to this point condoned this transformation. Following the footsteps of Roy Epstein (1987) and Mary Morgan (1990), the history of econometrics has focused on the history of the Cowles Commission approach. It comes as no surprise that the initial efforts of the history of econometrics, thus, were closely related to the history of macroeconomics and mathematical economics. Renowned members of the CC were leading researchers in the field of dynamic economic models, economic fluctuations, business cycles and general equilibrium. The lack of focus on microeconometrics is interestingly illustrated by the Cowles' publication celebrating its 50th anniversary, which covered the role of the Foundation in Mathematical Economics, Macroeconomics, and Econometric Methodology (see Malinvaud 1988, Solow 1983, Debreu 1983).

Hence, in four of the classic book-length inquiries on Econometrics, Epstein (1987), Morgan (1990), Francisco Louçã (2007) and Duo Qin (1993), the term microeconometrics has not been cited not even once versus several citations to "macroeconometrics". Searches in databases that include history of economics' journals and papers depict a similar picture. Microeconometrics is considerably under-cited in historical papers.

This is not a problem per se, given that, as seen in the short story, simultaneity and macroeconometrics developed first. However, it might disseminate a false departmentalized history, in which microeconometrics and macroeconometrics developed as distinct and separated fields. Except for James Heckman (2000, 2001, 2010), this has been the case in widely circulated papers (see Panhans and Singleton 2017, Biddle and Hamermesh 2017, Levitt and List 2008, Angrist and Pischke 2010, Backhouse and Cherrier 2010), where words such as "revolution", "turn" and "shift" set the tone of the narratives. This thesis comes with an argument in favor of a smoother transition from macroeconometrics to microeconometrics and that claims that the development of econometrics happened gradually through the discoveries of different facets of passive observations.

1.2. Technical Prelude: Randomization

Before starting the historical narrative per se, it is important to clarify some technical details of randomization and simultaneity that will help in understanding the remaining chapters. Heckman (2000) advocates that simultaneity, and its solution are *exclusivity* of macroeconometrics. This means, in his point of view, that macroeconometrics developed as a field of its own, with its own methods and techniques aimed towards dealing with simultaneity – that for them was identical to the problem of identification. While this is a strong opinion that the following thesis will not hold back entirely, it is an interesting point to bolster the following arguments.

Technically, simultaneity is described as follows. In a system of equations, there are two or more variables that simultaneously cause each other. The most common example, and the one that tormented economists for decades is the case of equilibrium between supply and demand[2]. Greene's (2008) textbook example illustrates the problem. Suppose that the following equations define a market's equilibrium system:

$$y_i = \alpha_0 + \alpha_1 x_i + \alpha_2 k_i + \varepsilon_i \tag{1.1}$$

$$z_i = \beta_0 + \beta_1 x_i + \beta_2 w_i + u_i \tag{1.2}$$

$$z_i = y_i \tag{1.3}$$

The first equation depicts the quantity demanded of a good y_i, where x_i is the price of the good and k_i is the income of consumers. The second equation defines the supply of this same good z_i, with the input price w_i and the price of the good x_i ,once again, determining it. The last equation is the equilibrium condition. ε_i and u_i represent the error terms of each equation. In this example, the estimator is unreliable, given that the price is correlated with the error terms, as seen in the following equation[3]:

$$x_i = (\alpha_0 - \beta_0 + \alpha_2 k_i - \beta_2 w_i + \varepsilon_i - u_i)/(\beta_1 - \alpha_1) \tag{1.4}$$

Following Heckman's (2000) account, during the same period that macroeconometrics was becoming an independent field focused on solving the above issue, economists were

[2] In the 1910s, before the advent of solutions for simultaneity, Moore found a positive demand curve and was criticized by his colleagues (Morgan 1990 Ch. 5).
[3] It is important to remember that the hypothesis of exogeneity, or uncorrelation of the error term with the regressors, is essential for a consistent estimation of the OLS parameters. See Wooldridge (2002, ch. 4) for a discussion on the issue.

also discovering sampling and non-sampling errors in passive observations. These are errors commonly found in the active collection of data. In these data sets created through interviews of sampled populations, sampling errors occur when the sample does not represent the aimed population. Omitted reasons for the interviewees to participate or not participate (missings and truncation) that create the error are called selection-bias. Non-sampling errors, in addition, are the errors that occur in the interview process. Those are errors committed by the interviewers, respondents and the even researcher that built the questionnaire. Interviewers may record with mistakes of formulate the questions wrongly. Respondents may be unwilling or incapable of answering. Researchers may censor and truncate the possibility of answers when designing the answer pool.

Noticing sampling and non-sampling errors also in passive observations put them on the track of widening the problem of identification beyond simultaneity. Thus, differently from *the* identification problem of the 1940s which was exclusivity of macroeconomics - for Heckman - and was equaled to simultaneity, sampling and non-sampling problems were old issues in numerous areas, in special all areas which had control over the collection of data[4]. The novelty in econometrics was to acknowledge that those were also problems of passive observations. As follows, "unobservability" was a common issue, and thus, its main solution was simple and widespread: randomization. Randomization solves omitted variables in the following manner. Imagine that a program is being evaluated and $X_i'\alpha$ is a vector of control variables and βT_i is a dummy variable representing whether an individual participated in the program:

$$Y_i = X_i'\alpha + \beta T_i + \varepsilon_i \tag{1.5}$$

However, assume that the decision to participate in the program omits certain characteristics of the individuals. Thus, the process has a sampling error, where the individuals in the program are different from the population in an unobservable characteristic. Motivation is usually the most common example. As a result, $cov(T_i, \varepsilon_i) \neq 0$

[4] In a non-exhaustive list, as exposed by the historical literature, microeconometricians could be inspired by seven major areas: agriculture, statistics, health/medicine, education, psychology, social sciences, and economics. These areas have a direct connection with economics and could be easily accessed by economists. Agriculture, since the turn of the 20th century, was an essential part of economic theory (see: Fox 1989). Statistics was the main source of inspiration for econometrics (see: Morgan 1990, Louçã 2007). Health and education would become applied fields of economics during the 1950s (see: Teixeira 2000, Panhans 2018). Psychology defined the research on decision making and gave rise to the behavioral and experimental schools of economic thought (see: Svorencik 2015, 2016). Social scientists in the 1950s and 1960s would form interdisciplinary groups with economists in the public sector. Finally, economics was their own area and, thus, macroeconometrics was in the next room.

and the estimator is biased. In economics, this specific error of omitted variables is known as self-selection, given that the individuals that participated in the program self-selected themselves because of their unobservable characteristics.

To solve this problem, it is possible to use the following procedure[5]. First, assume the following conditional probabilities:

$$D_{11} = E[y_i(1)|T_i = 1]$$
$$D_{10} = E[y_i(0)|T_i = 1]$$
$$D_{01} = E[y_i(1)|T_i = 0]$$
$$D_{00} = E[y_i(0)|T_i = 0]$$

These are, respectively, the conditional probabilities of the: treated in case they have been treated; the treated in case they have not been treated; the non-treated in case they have been treated; and the non-treated in case they have not been treated. The first and last are readily accessible, while the middle ones cannot be observed. To exclude the effect of self-selection, we are interested in acquiring $D_{11} - D_{10}$. In other words, the interest is the counterfactual case where treated individual are compared with themselves in the case they have not been treated. Unfortunately, the only accessible probabilities are D_{11} and D_{00}, which allows only to address: $D_{11} - D_{00}$. Summing and subtracting D_{10} in this last equation allows to observe its biasedness:

$$D' = \{D_{11} - D_{10}\} - \{D_{10} - D_{00}\} \tag{1.6}$$

The first term of the equation is the counterfactual case of interest. The second term is the self-selection bias. This bias is easily removed with a simple assumption: randomized assignment of individuals in groups of treatment and control. This happens because in the presence of randomization $D_{10} = D_{00}$ and, thus, the second term becomes zero. Hence, in the case of randomized selection, it is possible to assume that the individuals that have not been treated are equal to the individuals treated in the case they have not been treated.

Although an encompassing solution for omitted variables, randomization is only applicable in special cases. The researcher has to have control over his data to properly randomize groups of treatment and control. Lab experiments represent the best scenario,

[5] Based on Menezes (2012)

where the researchers have the greatest control over the variables. The situation gets worst the lesser the control over the data generating process. Non-randomly generated passive observations are probably one of the worst-case scenarios.

Thus, from this point of view, to control the several different biases in econometrics, the only tool necessary was to randomize observations actively. However, active observation and control were exactly what lacked in the passive observations of numerous program evaluations of the 1970s. Data was already available and not randomized. It had only to be analyzed. Thus, economists had to search for ways to clean the data without controlling the observations. The ones who face this quest became microeconometricians.

1.3. The probabilistic revolution as a problem-orientation

"Statistical information is currently accumulating at an unprecedented rate. But no amount of statistical information, however complete and exact, can by itself explain economic phenomena. If we are not to get lost in the overwhelming, bewildering mass of statistical data that are now becoming available, we need the guidance and help of a powerful theoretical framework. Without this no significant interpretation and coordination of our observations will be possible." (Frisch 1933, p. 2)

According to Ragnar Frisch (1933) in the foundational volume of *Econometrica*, both economists and the society would be lost in a hodgepodge of uninterpretable data without econometrics. Hence, econometrics should be the tool that would unify economists and help society in understanding their data. However, it took a few years before econometricians acknowledged this common responsibility.

Until the 1940s, econometrics was institutionally and methodologically divided. The famous "measurement without theory" debate between Koopmans and Routledge Vining (see: Koopmans 1947, 1949, and Vining 1949) illustrates the situation. In 1947, when the debate broke out, whether econometrics was inductive or deductive was still under debate. The Cowles Commission, home of Koopmans, argued in favor of a deductive use of econometrics, while the NBER, defended by Vining, advocated a more holistic and inductive approach. Even though the Cowles Commission was perceived as victorious, inductive and deductive econometrics kept being used (Qin 2013).

The state of econometric textbooks of the period represents this unsettled nature of econometrics as well. The first econometric textbooks would come out only during the 1950s, with Jan Tinbergen (1951), Gerhard Tintner (1952) and Lawrence Klein (1953). Therefore, although econometrics was facing internal debates while advancing institutionally, there was not yet a widespread knowledge that deserved to constitute a systematization in a textbook. More importantly, there was not widespread knowledge of the role of econometrics neither for economists themselves nor for the society until that period. To enroll outsiders in their doings, econometricians should establish a role for their community. The "probabilistic revolution" realized this task.

The "revolution" was the theoretical process through which econometrics implemented probability theory in its practice and became as it is now presented in contemporary textbooks. While revolutionary narratives are normally not as explanatory as aspired, it cannot be contested the theoretical importance of the three main contributions by Trygve Haavelmo (1944), Koopmans (1950) and William Hood and Koopmans (1953) - alongside with the debate about "measurement without theory". More than consolidating the probability theory in econometrics, together they refocused the econometricians' efforts from errors-in-variables to errors-in-equations (see Morgan 1990), presented the current textbook exposition of identification, settled the superiority of structural models[6] over inductive reasonings, and theoretically proposed the use of maximum likelihood methods.

For the trio and the community, their approach had solved one of economics' everlasting conundrums. According to Boumans (2010), this conundrum was the problem of passive observation: "the problem of detecting and measuring the main causal factors of a certain phenomenon outside the laboratory, that is, without the possibility of intervention" (Boumans 2010, p. 107). This perception of accomplishment can be seen in Marschak's introduction to the Cowles Commission Report in the 1950s when he states that "a new milestone was reached in 1943" (Koopmans 1950, p. 4). For Jacob Marschak, the milestone allowed economists to overcome the natural "passivity" of economic observations:

"[…] an experimenter could replace the natural conditions by laboratory conditions. To study one of the several relations, the experimenter observes the random values taken by one variable when the other observables that determine it are made reasonably free of the influence of errors and shocks. The economist cannot thus control variables and isolate relations. His data are produced by the existing economic structure, as described by a system of simultaneous relations between these variables: the observables themselves, the errors and the shocks." (Marschack in Koopmans 1950, p. 3)

The perception of having reached a breakthrough came from the fact that, as noticed by Boumans (2010), for 1940s econometricians, *the problem of passive observation* was greatly connected to the problem of simultaneity. As Haavelmo (1944) highlights in his seminal paper:

[6] Structural models follow a deductive reasoning scheme: structural modeling. The reasoning starts with a theoretical model, the identification of the causal relationship, the specification of an econometric model and its estimation and test.

> "A most dangerous but often used procedure in this field is to "fit each equation separately" without regard to the fact that the variables involved are, usually, assumed to satisfy, simultaneously, a number of other stochastic relations. If that is done, it is afterwards almost sheer luck if we have not created inner inconsistency in the system as a whole, such as, for instance, the assumption that some of the variables in one equation remain constant in repeated samples, while-because of another equation in the system this is impossible." (Haavelmo 1944, p. x)

Haavelmo and his colleagues presented a solution for simultaneity using maximum likelihood techniques. Their solution is not the focus here[7], but what it represents in the literature is. The "revolution", as a single solution for simultaneity, is normally regarded as a methodological unifier. It is perceived frequently as a unification of econometricians around a common tool for solving simultaneity. This point of view is found in several sources. For instance, according to Qin (1993, 2013), Biddle and Hamermesh (2016), Heckman (2000, 2001), the revolution created an agreement around simultaneous equation models (SEM) and maximum likelihood.

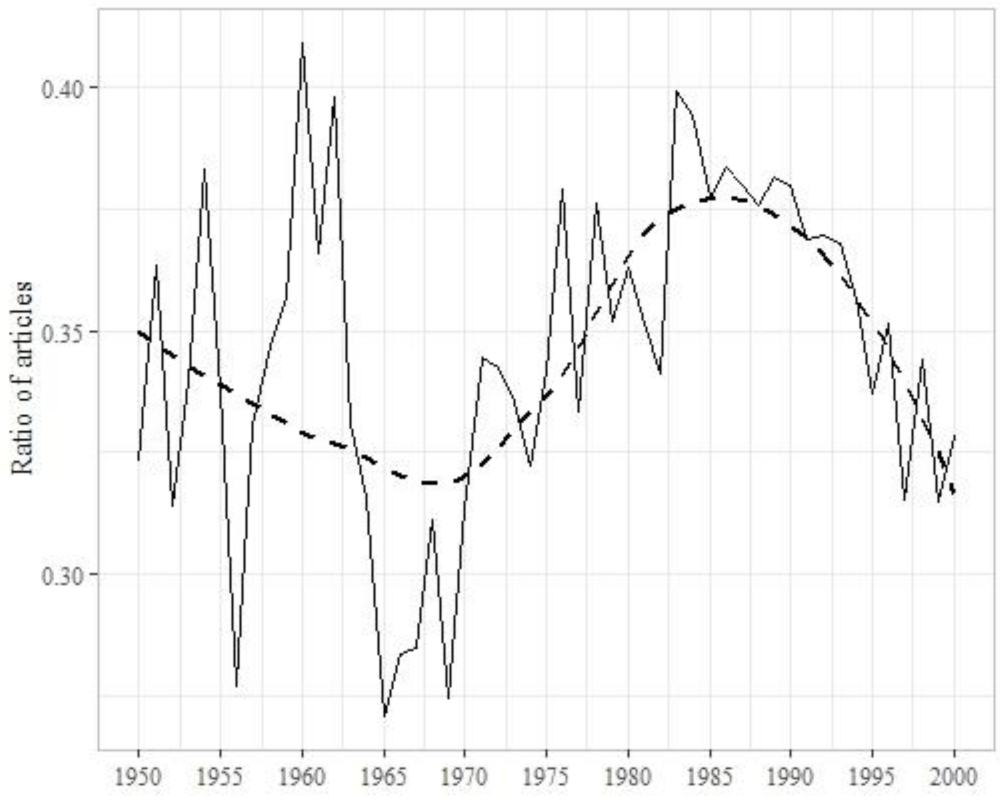
Roughly, the unification's narrative is presented as follows. Error-in-variables became minor errors, not worrisome to economists. Error-in-equation became the real problem and manifested itself as the simultaneity bias, called identification problems solely[8]. Hence, economists of the 1940s and 1950s thought that, in order to be capable of making causal claims and testing their theories properly, they should control *exclusively* the simultaneity bias, identifying the structural parameters of a system of equations. Simultaneous equation models and the method of identifiability through building a complete system counting exogenous and endogenous variables became the accepted solution.

However, this narrative may be contested. Early on, in the 1950s, econometricians criticized the trio's propositions and advocated in favor of different methodologies. On the technical side, for example, solving simultaneity would prove to be a great challenge in the lack of computers. Maximum likelihood and identification rapidly faced criticisms. Maximum-likelihood was technically hard to be implemented, and ordinary least squares with its alternatives gradually regained their status (see Epstein 1987, 1989). Thus, in the 1950s, the advantages of instrumental variables for dealing with autocorrelated errors and endogeneity started to be observed (see: Theil 1953, 1954). This can be observed in the following figure:

[7] For more about Haavelmo's solution see: Bjerkholt (2007) and Boumans (2010)

[8] For Heckman (2000), simultaneity is still understood by most economists as the identification problem. Identification, endogeneity, simultaneity, and so on, tend to be confusing nomenclature even nowadays.

Fig. 1.1 – Mentions to Simultaneous Equation Methods vs. Other Methods[9]



Source: JSTOR

In this search in economics articles stored in JSTOR, mentions to Maximum likelihood or Simultaneous Equation Models (SEM) - known methodologies for dealing with simultaneity - never reached more than 50% of the mentions to the cluster of words related to single equation methods. Moreover, In 1953, the year of the publication of Hood and Koopmans (1953), the cluster started a brief, but strong, fall, until 1956. It regained strength from 1956 to 1960. From then on it fell to its lowest point until the 1970s when some new macroeconometric practices started to arise and mentions to simultaneous equations regained momentum. The inconstant picture depicted in the figure demonstrates that SEM and Simultaneous Equation were far from dominating discussions and even faced a period of very few mentions when compared to other estimators.

Actually, the so-called revolution faced numerous challenges. The point being raised here is that there probably has not ever existed a consensus on the methodology presented in the papers of Haavelmo, Koopmans and Hood. For instance, further than technically

[9] Words for simultaneous methods searched: simultaneous equation*, SEM, maximum likelihood. Words for other methods: single equation*, instrumental, cross-section

burdensome, simultaneity was also soon criticized to not be readily solvable through the "counting" of endogenous and exogenous variables. Qin (2013), in this regard, summarizes the macroeconometric developments of the second half of the 20th century in three alternative approaches to the Cowles commission Approach: Bayesian econometrics, the London School of Economics approach, and the Vector Autoregressive approach. The first questioned the very probabilistic nature of econometric data and the other two proposed different forms of evaluating causality in econometrics.

These criticisms began during the 1950s. Bayesian econometrics has its roots in Marschak's (1954), and Dreze (1962) works, according to Qin (2013). The other approaches followed the developments of Wold's (1954, 1956) causal chain models, as well as Orcutt's (1952) and Sargan's (1958, 1959) ideas.

As follows, to depict the revolution as a methodological agreement may oversimplify the turmoiled nature of the econometric debates of the period. However, if there was not a methodological agreement, what is the common claim of the 1940s and 1950s that brought together all these different views? The claim was not a solution, but a problem: the problem of passive observations. This can be seen in Haavelmo's admission of the multifarious problems accompanying passive observations:

> "In the verbal description of his model, "in economic terms," the economist usually suggests, explicitly or implicitly, some type of experiments or controlled measurements designed to obtain the real variables for which he thinks that his model would hold. That is, has in mind some "true" variables that he would like to measure. The data he actually obtains are, first of all, nearly always blurred by some plain errors of measurements, that is, by certain extra "facts" which he did not intend to "explain" by his theory. Secondly, and that is still more important, the economist is usually a rather *passive observer* with respect to important economic phenomena; he usually does not control the actual collection of economic statistics. He is not in a position to enforce the prescriptions of his own designs of ideal experiments." (Haavelmo 1944, p. 6-7)

Haavelmo, Koopmans and Hood, already in their publications, perceived that the problem of passive observation is of a much broader nature than simultaneity itself. It encompasses simultaneity but is not defined by it. In the above citation, Haavelmo recognizes that without control over data, economists face measurement errors, for instance. Hence, although true that at the 1940s and 1950s simultaneity was their main concern, econometricians were aware that *the problem of passive observation* encompasses all biases that hamper causal claims, even though they could not at the time name all of them. The most commonly found on contemporary econometric textbooks are the endogeneity biases: simultaneity, measurement error, and omitted variables.

For the trio, Haavelmo-Koopmans-Hood, back in the 1940s, omitted variables were not yet a concern and measurement errors could be dispensable. The leftover was simultaneity.

Haavelmo's (1944) efforts, followed by Koopmans (1950) and Hood and Koopmans (1953), thus, presented a method for dealing with passive observations, and in doing so, they equaled passivity to simultaneity. Consequently, to unify around SEM, was to unify around the problem of passive observations. As follows, at the same time they claimed for SEM, they claimed that econometricians should be accountable for passive observations: the ones capable of controlling "the stream of experiments that Nature is steadily turning out from her own enormous laboratory, and which we merely watch as passive observers" (Haavelmo 1944, p. 14).

Interesting evidence bolstering this proposition is that other social scientists were accountable for social experiments with an active collection of data until the mid-60s, not economists. Program evaluation was at the time ascribed to social scientists or field specialists, such as specialists in education or criminology. For instance, in the 1930s, Francis Chapin advocated in favor of experimental methods and active collection of data in sociology:

"Experimental work in sociology means the possibility of passive description in terms of standardized units of a scale of measurement. *Experimental method is observation under conditions of control.* All factors save the one to be measured are held constant. Otherwise we would not know whether the effect was due to both factors in combination, or to that one which overbalanced the others. If no effect ensued we could not tell which factor was responsible or whether one neutralized the other." (Chapin 1931, p. 541, emphasis added).

During the 1930s, Chapin examined effects of programs of housing and extracurricular activities, for instance. Following the same line, Ernest Greenwood's (1945) research is another book-length exemplar of advocacy in favor of experimental research in sociology. According to Forstelund et al. (2007), randomization and experimental research can be found in social and educational intervention as soon as 1928. As a result, the analysis of actively collected data was already a known efficient way of evaluating programs in many fields during the second quarter of the 20th century, but it was not the responsibility of economists.

At the same time, it is possible to detect that economists were engaging more and more with passive observations. Macroeconometrics started to be a common practice in banks and international aiding agencies such as the World Bank and the United Nations (Alacevich 2017, Toye and Toye 2004). As a consequence, early in the 1950s, economists' discussions about passive observations achieved a new level in society. They gradually started to control both macroeconomic stability policies and the course of development and growth policies[10].

[10] The examples of the role of passive observation in economist's policy discussions are numerous. Among the most relevant are the rise of growth accounting, cost-benefit analysis and international trade theories (see Crafts 2009, Alacevich 2017, Toye and Toye 2003). It is also interesting to notice, in this regard, that

This spreading of economist's accountability for passive observations led, in no more than 35 years, Oskar Morgenstern to portray the "control of economic variables" as the sole empirical problem alongside another twelve theoretical ones in "Thirteen Critical Points in Contemporary Economic Theory: An Interpretation" (Morgenstern 1972). Wold (1969) - reflecting on the 33 years of the econometric society - in even more explicit recognition of the relationship between econometricians and passive observations, advocates that Econometrics was "Pioneering in Nonexperimental Model Building". From his point of view:

> "econometrics has played a pioneering part in posing these problems [scientific evolution from deterministic (exact, residual-free) models to stochastic models, and distinction between causal and noncausal forecasting model] and in establishing principles and methods of general scope for their treatment. Its influence is very broad, serving as a pioneer in the wide field of the social sciences, and in the still wider realm of nonexperimental model building." (Wold 1969, p. 372)

Thus, in a few decades after the probabilistic revolution, the problem of passive observations was already the established role of econometricians in the developing data world. The probabilistic revolution had oriented the data problems that econometricians should deal with. According to Koopmans (1979), passive observations were already a tradition of economics among other sciences in the 1970s: "like the meteorologist, the economist has *traditionally* been confined to drawing inferences from *passive observations*, records of data generated by the turbulence of the atmosphere or the fluctuations and trends of economic life."

Clearly, by the 1970s, society's accumulated passive observations had to be displaced to the economist armchair to be explained. Inspired by Bruno Latour (1982), from then on, economists tacitly claimed that: "If you want to understand passive observations, you have to listen to my econometric methods" (Latour p. 260).

1.4. From Simultaneity to Omitted Variables

In the 1950s and 1960s, economists had become accountable for "passive observations". This meant that where passive observations were available, econometrics methods would be there. Economists were overcoming resistances of passive observations and making new measuring instruments. Society was demanding predictions and advice. Hence, there was no

Krugman (1994) argues that the high theory of development economics disappeared due to its incapacity of accompanying the new quantitative model-driven economics.

unique method, but a unified perception of responsibility: econometricians should (at least) be able to deal with passive observations.

This perception led to numerous developments of macroeconometric institutions especially suited to cope with the simultaneous nature of passive observations. As an illustration, int the United States after World War II, the Council of Economic Advisors and the Joint Economic Committee were institutionalized.

On the other hand, microeconometric developments followed a more peculiar path as some of the society's demands came from unusual sources: non-simultaneous and not time-related passive observations. This was the case, in the 1960s, of economists being demanded to evaluate anti-poverty policies. The foremost exemplar of this new role was their calling to test the success of Johnson's War on Poverty.

Curiously, when asked to evaluate social programs, economists and econometricians were not accustomed to facing social problems (Fleury 2010, Forget 2010). Jean-Baptiste Fleury (2010), for instance, demonstrates how poverty was far from a concern in economic terms in the 1960s. The next chapter will delve into this situation more deeply. The lack of touch with social problems indicates that economists have been selected more for their capacity of dealing with the growing amount of passive observations rather than by their knowledge about social conundrums.

The fact is that, during the period, few social experiments had been judiciously conducted and, thus, the majority of the data was of a nonexperimental kind. If dealing with non-macroeconomic social problems was not the expertise of economists, dealing with nonexperimental data was. Therefore, alongside with developing macroeconometric institutions, econometricians had an stimulus for developing also microeconometric facilities aiming to solve non-simultaneous passive observation problems and cross-sectional conundrums.

In this regard, the newborn Office of Economic Opportunity is a prime example. It became a chief force of the War on Poverty, putting economists in a new position both theoretically and empirically. Fleury (2010) affirms that Johnson's calling "offered an important opportunity for economists to claim expertise over the 'social'" (Fleury 2010, p. 316).

Another interesting illustration is the case of the General Accountability Office (GAO) – intended "to be a critic of the financial activities of the Government "(Moore 1968, p. 32) -, which since the 1960s started to employ economists among its specialists. According to Moore (1968), the office had to cope with the "ever-increasing" conundrums related to the "proper gathering and utilization of data by management for decision-making purposes" (p. 32). The employment of engineers, *economists* and mathematicians was part of an effort of "promoting the use of more scientific and sophisticated tools of management within the Government." (p. 33). From then on, systematically, the institution proposed a wide array of techniques for the evaluation of microeconomic policies. During the 1960s, the programing-planning-budgeting system (PPBS) and discounting methods were among the main discussions, but already in the 1970s and 1980s, the office realized extensive evaluations of social experimentation (GAO 1981).

The point is that economists became aware of their relevance in public policy in a multitude of areas. Stigler's (1964) presidential address was a symptom of the changing role of economists in the state: economists perceived that they had to build a "body of knowledge" suited for "policy formulation". Hence, more than in macroeconomic stability and growth policies, from the 1960s forward, economists were embedded in the state's rhetoric of dismantling poverty (Forget 2011).

For Romain Huret (2010) this meant that economists had to disconnect the problem of poverty from that of inequality as an aggregated issue. Education became the main vehicle of this disaggregation. According to Pedro Teixeira (2000): "During the 1960s governments viewed education as a major instrument for improving and equalizing social opportunities. There was a strong belief that education could be a powerful force to promote social mobility" (Teixeira 2000, p. 264-265). On the specific case of the United States, Evelyn Forget (2011) highlights the centrality of these policies for facing poverty during the 1960s. "These programs [The Manpower Development and Training Act], […], became central to the War on Poverty." (Forget 2011, p. 205).

As a result, educational policies (manifested as Job Training Programs) were among the main concerns of applied economists in the 1960s and 1970s. These educational-training policies were an interesting change in the tasks demanded to economists. It meant organizing and evaluating ex-post and ex-ante the effects of policies for individuals. Results and analysis should be realized as soon as possible, which discarded the use of long time-series data - the previous main source of econometric information.

Economists should embrace cross-sectional data and the newly available panel data[11]. The 1960s, therefore, marked the engagement of economists with a new kind of passive observation: non-aggregated data. Individual passive observations on health, crime, and education started to flow towards economists' pen and papers.

[11] For more quantitative information on this see fig. 2.4

From the 1950s forward, the prevalence of data papers among the papers about crime, health and education increased significantly in economic papers. These new data formats, however, were not exclusively presented by the demands of the postwar society. Econometricians were already facing new problems of passive observation coming from their own inquiries. Noticeably, the 1960s and 1970s represented an increase in the dissemination of new data formats by economists themselves. The Panel Study of Income Dynamics (PSID), for instance, has started in 1968 at the University of Michigan, becoming a valuable source of information (McGonagle 2013). In this regard, Edwards (2011) highlights the efforts of George Kantona for offering new data for economists in Michigan. He headed the efforts of the Michigan Survey Research Center for developing data on intentions, expectations, and attitudes. During the 1970s the Penn World table also started to be recorded. In a great extent, the dissemination of these new data sets is correlated with the development of computation during the 1960s and 1970s (see Renfro 2003, 2011, and Backhouse and Cherrier 2016)

Anyhow, the first known uses of panels by economists are normally prior to the wide dissemination of panel data. The usually cited examples are Yair Mundlak (1961), Clifford Hildreth (1949, 1950), Irving Hoch's (1958) PhD. research, and especially Pietro Balestra and Marc Nerlove (1966) (see: Nerlove 2002). These were either theoretical efforts (such as Hildreth's) or individual efforts for acquiring data (as that of Balestra and Nerlove). It is interesting to notice that this first crusades in the realm of panel data had nothing to do with individuals. They were panels of states, cities, or countries[12].

Therefore, there was a concomitant movement of economists looking forward to dealing with passive observations and society presenting demands related to the problem. On one side, economists created their own panels, disaggregating their previous variables. On the other side, the society offered an increasing number of panels on individuals to be analyzed.

Regardless of their source, already in the first incursions, these *new* passive observations posed a different challenge for economists. While measurement errors and simultaneity were known issues of macroeconomic data sets, panel data evinced that they were not the only hiding conundrums of dealing with data provided by passively observing nature. Ariane Dupont-Kieffer and Alain Pirotte (2009) highlight that the debates around the error term and unobservable heterogeneity had already been present in econometrics' discussion since the 1930s and 1940s, but the lack of explicit modeling of the error was a hindrance for the

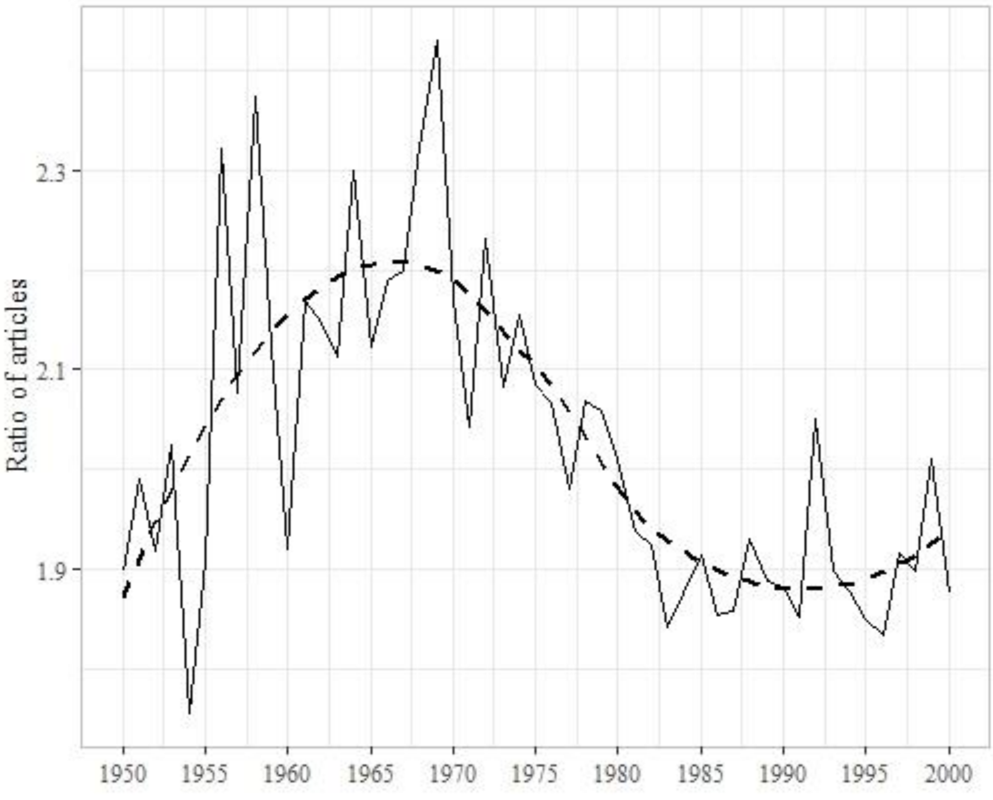
[12] Eventually, these developments would also propel the development of the literature on cross-country growth (see: Durlauf 2009).

development of the discussion. According to the authors, panel data renewed the interest of econometricians in the error term and thus brought a new issue to be faced. In the words of Hildreth (1950, p. 3):

"It may be believed that there are unobserved individual characteristics which cause individuals to act differently and which are persistent over time. There may be unobserved influences that affect individuals in pretty much the same way but change over time."

The lack of control over economic observations that manifested as simultaneous relations in macroeconomics was reinvented as individual heterogeneity in microeconomics - or "unobservables". As a result, from the 1950s to the 1970s microeconometric data problems started to gain relative importance when compared to simultaneity problems. This can be observed in the following figure:

Fig. 1.2 – Microeconometric Problems vs. Simultaneity[13]



Source: JSTOR

[13] Searched terms: (unobs* OR truncat* OR self-select* OR omitted variable* OR censor*) / simultan*; constraints: Economics discipline, only Journals

Only in the 1970s, with the macroeocnometric crisis and the Lucas critique, simultaneity reclaimed its importance in discussions. However, at this point, passive observations were already far more relevant than simultaneity - including problems of unobservables such as truncation, self-selection, and censoring. Unlike macroeconomic data sets, the process through which microeconomic data was gathered could not be accepted "by definition". Assuming random distributions of measurement errors and correct sampling where these were not real characteristics of the data sets revealed to be crucial errors in microeconometrics. Causality would not be identifiable, and results could not be generalized. Therefore, econometricians discovered, in this new problem, a distinct facet of passive observations: individual heterogeneity. This facet propelled microeconometricians to develop their own methods and field of research.

The remaining of this thesis is concerned with the specific development of a way of inserting randomization in economics: natural experiments. Throughout the chapters a micro history that starts with a book about poverty, goes to American organs and lands in Princeton will be disclosed. The concept of natural experiments will be demystified and inserted as a part of the history of econometrics. Revolutions and shifts will be left aside for understanding a smooth and continuous path towards credible methods in economics.

2. RAISED WITHIN GOVERNMENT WALLS: THE EARLY YEARS OF PROGRAM EVALUATION IN THE UNITED STATES

2.1. Rediscovering Poverty in a Social-Scientific Government

While the emergence of microeconometrics comes from the changing understanding from passive observations as simultaneity to passive observations as omitted variables, this narrative misses historical thickness. In the grassroots of economics and program evaluation, terms such as passive observation, omitted variable and simultaneity were not the main concern. Actually, all that economists and evaluators wanted to achieve in the early 1970s was understanding poverty, a rising problem in US society.

On Saturday, the 13 of January 1963, someone wanting to have a pleasant morning could reach the closest newsstand and find an excellent selection of magazines. Fresh in the newsstand was the recently arrived volume of the New Yorker, edition from that same day. Behind the artist Abe Birnbaum's cover depicting Ice Fishermen, the reader would be surprised. The largest-ever review of a book in the magazine was there to be read: Dwight MacDonald's "Our Invisible Poor" (MacDonald 1963). Reading the *New Yorker* was undoubtedly an honest way to spend that morning. Like this, Michael Harrington's (1962) *The Other America* pleased the morning of many Americans on that Saturday. These are just two examples of numerous others such cases that turned poverty a topic visible to the national eyes.

The tale from that time tells that among those Americans, President Kennedy was also enticed by MacDonald's lines and Harrington's book - as well as by Galbraith's (1958) book *The Affluent Society* and Homer Bigart's articles at the *New York Times*[14]. From his interest onwards, histories about the unveil of poverty abound[15]. The narratives are straightforward. The desire of overcoming poverty became a governmental concerm and would start receiving all the attention it needed to become a reality. Kennedy's assassination did not hamper the process; on the contrary. Lyndon Johnson, Kennedy's vice-president who rose to Presidency, would become the name behind America's unconditional War on Poverty.

Nearly one year after MacDonald's *Our Invisible Poor* and a month after Kennedy's death, in the same newsstand, Lyndon Johnson was stamped in the issue of the 31 of January 1964 of *Life* sitting on the front porch of the Fletcher's – a typical low-income family of the

[14] See Shriver Oral History for a personal account and O'Connor (2001) for a secondary review.
[15] see O'Connor 2001 for an extensive review and O'Connor 2020 for a recent article on the subject.

Appalachia destined to be the symbol of American poverty. "The Valley of Poverty" – heading of the main story of the magazine - reported: "In a lonely valley in eastern Kentucky, in the heart of the mountainous region called Appalachia, live an impoverished people whose plight has long been ignored by affluent America" (LIFE 1964).

Kennedy, Johnson, *The New Yorker*, and *Life* are symbols of what has been described as a more extensive transformation in America: a rediscovery of poverty. After almost twenty years after World War II worrying about developing the third world, the 1960s United States was finally looking to its own problems and (re)discovering that they were worse than expected. Fights against the Vietnam War and for Equal Rights inflamed movements on the streets of the country during that decade and the next.

The government reacted fast. In July of 1964, Johnson signed the Civil Rights Act. A month later, on the 20 of August, the Economic Opportunity Act was effective. Although the signatures were a crystal-clear change for equal rights, its effects on the social sciences were far from foreseeable[16]. Since the War, social sciences secured a vital role in American Government. The Council of Economic Advisers had been established in 1946, and discussions for a Council of Social Advisers[17] were not unusual. This indicates that economists, sociologists, and political scientists were gradually occupying more posts on governmental organizations.

Still, having a place is not the same as having a role. Up to the 1960s, the Council of Economic Advisers was mainly concerned with macroeconomic problems. They dealt with the few measurable social indicators of the time: product, productivity, employment, and so forth. In contrast, poverty, crime, and health were then intangible social problems. This was about to change with the arrival of the War on Poverty, or more precisely, with its development.

From 1960 to 1970, "poverty knowledge"[18] in the US government went from unknown and qualitative to fashionable and measurable. All that to, in the Nixon years, lose its fashion and witness the questioning of its measurability. It went from a social science problem to an economics problem and then to a multidisciplinary endeavor. Poverty knowledge woven for fifteen years inside government walls until disciplinary borders became clearer, and the disciplines were set free. The following story demonstrates that, among these

[16] About the development of Social Sciences in the post-war, see: Backhouse and Fontaine (2010)
[17] Proposed in Bill #S.5 of 1969, for instance. See: Sheldon and Freeman (1970)
[18] Alice O'Connor's (2001) introduced the term: "poverty knowledge".

disciplines, program evaluation in economics was born and promoted microeconometrics to a new status[19].

This chapter is a slowly paced narrative through the headquarters of US government that demonstrates that the connection between poverty, economics and program evaluation is far from obvious and occurred through numerous real conflicts of innumerous known and unknown agents. First poverty had to become a problem for economists, then government had to fail in fighting it. After that, evaluation methodologies had to develop inside different agencies to finally become a consolidated scientific field. Conceptual discussions were secondary in this history.

2.2. Growth and Poverty into the Johnson Administration

"Mostly unmeasurable" seems a fair adjective for social problems in the 1950s and 1960s. It would be unfair with bureaucrats and academics of the 1950s to say that poverty was unmeasurable (Huret 2018 p. 2). Someone had to supply Michael Harrington and those interested in writing about poverty with the data to back their arguments. This "lesser-known" bureaucrats and academics that formed the "invisible network" of poverty did it (Huret 2018, 2010). And so, Harrington's 1962 book is filled with numbers: "There are 8,000,000 or more people living in the most miserable of conditions"; "In New York City there are some 300,000 hard core Public Assistance cases". (Harrington 1962 p. 118, 142).

Still, these unknown figures were ill-equipped when compared to modern data and statistics. What amused Harrington's readers was (and still is) the palpable side of poverty. Trying to choose a single sentence from Harrington that highlights his way of describing poverty is hard; the book has uncountable examples. Adjectives follow numbers instilling feelings, just like his sentences above do with "most miserable" and "hard core". Poverty is not a problem of numbers in the book, it is a problem of human beings. Harrington's use of statistics is "impressionistic" as MacDonald noticed in 1964.

Data and life stories made poverty receive broad attention. For most Americans, it felt like suddenly poverty was everywhere: books, radio, magazines, television. The Vietnam War and racial inequality helped to show different sides of the problem. Imagine yourself a fan of boxing in the 1960s, excited with Muhammad Ali, experiencing amazing fights, but

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19 Section 2.2 to 2.4 rely heavily on Alice O'Connor's "Poverty Knowledge" (2001) and Alain Huret "The Expert's War on Poverty" (2018).

also fierce criticisms to the War and racial problems. It was hard to not be aware of the issues of the day.

These were the early 1960s. A liberal, John Kennedy, had risen to the White House in 1962. With him, a new Council of Economic Advisers was formed to walk through the third floor of the Executive Office Building in Washington. Walter Heller, "the present-minded professor who tempers earnestness with cordiality and intellect with a touch of ambitious worldliness", became the chair. Kermit Gordon and James Tobin, "the twin rocks of Gibraltar", completed the "pragmatist" trio. Their task was direct, although complex: they had to make the economy grow (TIME Mar 03, 1961).

During his campaign, Kennedy repeatedly used a slogan affirming that he would "get America moving again", and the economists did their best to achieve high growth rates for him. The 1960s in the US was known as the period of growthmanship, a negative term coined by Richard Nixon "to criticize Kennedy's single-mindedness in allowing growth to dominate the political debate" (Boianovsky and Hoover 2014, p. 199). The committee of Price Stability for Economic Growth of Dwight Eisenhower's preceding administration became the Committee for Economic Growth under Kennedy. As advocates of the "new economics", the trio's main strategy would be to promote tax cuts as a Keynesian stimulus for the desired growth rate[20].

From Kennedy's campaign to its initial governing years, new economics' policies were the Council's main commitment. Beyond the main trio, Robert Solow, Arthur Okun, and Kenneth Arrow joined the Council as advisers. During that period, Solow had just written his model on economic growth (Solow 1956), and Okun would soon discover the Okun's Law (Okun 1962). Those were economists with high credentials for a growth-centered liberal government - even though they advised only during Kennedy's debut year.

Growth, however, came with a dilemma for those economists: trickle-down effects were uncertain. Would growth hit the poorest? Harrington, Galbraith, and the media claimed that it would not. For them, the poor needed specific policies. The Council disagreed. Of course, growth was the best way to help the poor. In 1962, when the council looked for answers to the challenge, Walter Heller invited Robert Lampman to join the Council as an adviser on poverty issues.

Lampman had been Heller's colleague in Wisconsin. He was specialized in poverty and was also - probably not by chance - a believer in the power of growth for the poor (see

[20] See Tobin (1974) for a review of new economics. See Boianovsky and Hoover (2014) for a history of the development of growth as an academic subject at MIT during the 1950s and 1960s.

Huret 2018). In 1963, when the pressure for governmental responses to the problem of poverty was on its peak, Heller relied on Lampman to start CEA's official analysis of poverty that culminated in the 1964 Economic Report of the President.

Lampman must have had a hard time writing the report, because science and politics are not always easily combined. In 1963, poverty was no more than a "powerful campaign theme" that had to be integrated into his scientific predilection for growth (O'connor 2001, p. 152). At first sight, for him as for several economists in the 1960s, poverty was not an economics problem.

Poverty as knowledge in the early 1960s was unsubstantial (see Huret 2018, 2010). Until 1962, the word "poverty" appeared in less than 5% of sociology or economics papers indexed in JSTOR. The term did not appear in 97.5% of all paper indexed in the platform. In the early 1960s, then, the nowadays obvious relationship between poverty and economics was far from established. More specific methods in economics, such as econometrics, were even farther to be applied to poverty. Few were the economists who studied the issue, and Lampmann was one of them.

In hindsight, as is going to be seen, to quantify poverty has been an important step in the development of microeconometrics. But in the early 1960s the problem was not even part of economics. Without poverty being a quantifiable concept nor a problem of eocnomists, psychological traits of poor individuals and their communities' characteristics dominated the discussions.

Although there was only a small community researching poverty, the nuances of dealing with individuals and communities created two different branches of poverty knowledge. The first was the "culture of poverty", which claimed that the poor had a culture of their own that trapped them in the lower classes. Poverty was a trait of individuals for those researchers. The second, that is in many ways connected to the culture of poverty, focused not on poverty per se, but the issues in poor communities like crime, delinquency, and lack of study. Poverty related issues were a trait of communities for this second group.

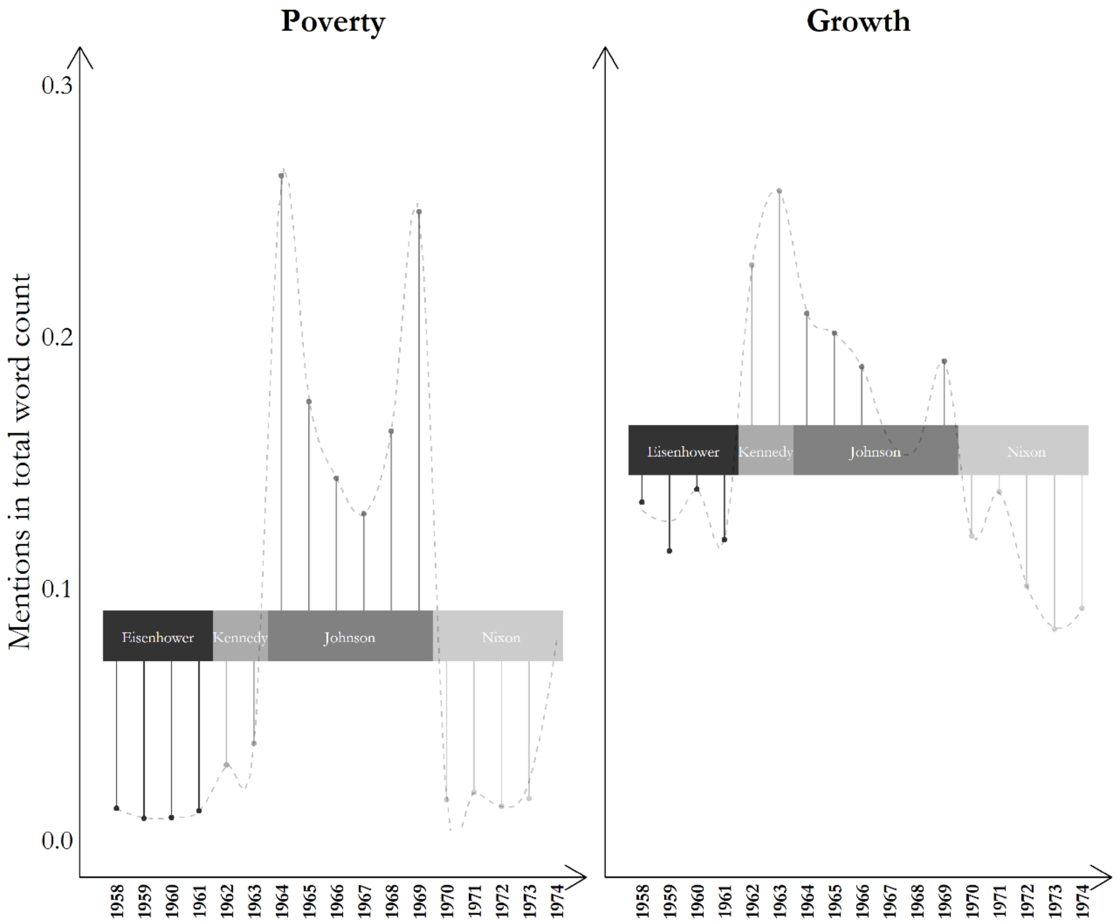
The most popular exemplars of the culture of poverty are the anthropologist Oscar Lewis' works (Lewis 1959, 1966, Streib et al. 2016). In 1959 the author published his book on poverty "Five Families - Mexican Case Studies in the Culture of Poverty" after a thorough examination of poverty families, using the most up to date surveys from behavioral psychology. "The culture of poverty" would soon become the theoretical frame that would define his career. Seven years after the book, he published an article in Scientific American delineating the main arguments in his theory. In his concise abstract, Lewis simply stated:

"Does membership in a group that has been poor for generations constitute belonging to a separate culture? A study of Puerto Ricans in both Puerto Rico and New York indicates that it does" (Lewis 1966, p. 19).

On the other side, Richard Cloward and Lloyd Ohlin (1960) *Delinquency and Opportunity* is a study of gangs' subculture that became an influential work on delinquency. It did not achieve much theoretical influence though, but practical. Their book supported the creation of the Ford Foundation's Gray Areas project of 1962 and the release of the Executive Order 10940 that established the President's Committee on Juvenile Delinquency and Youth Crime in 1961.

How could Kennedy's administration reconcile economic growth and these two branches of poverty knowledge was a question that had no immediate answer. The lack of trickle-down effects and the media coverage about poverty made economists reluctant to advance their views on economic growth without support of other social policies. The need for combining poverty and growth was noisy in the mid-60s. In the next figure, we can observe how the government responded the roar through its mentions of poverty and growth terms in the economic reports from 1958 to 1974:

Fig. 2.1 – Economic Reports of the President[21]



Source: Economic Reports from 1958 to 1974[22]

Growth was of limited concern in Eisenhower's and Nixon's Republican governments, but it widely appeared in the reports of the Democrats Kennedy and Johnson. As Tobin remembers: "An important issue for me arose in the 1960 presidential campaign. Kennedy and the Democrats were accusing Eisenhower and the Republicans of letting the economy stagnate and not grow fast enough. There was a lot of discussion about growth, and there was complete confusion in the political discussion […]" (Tobin Oral History 1999, p. 881). In a more contemporary comment, in 1965, he simply put: "At any rate, accelerating growth seemed to be a major theme." (CEA oral history p. 35).

Since WWII, America was in a constant run for growing, especially to grow more than its direct enemy, the Soviet Union (Holden and Biddle 2017, Collins 2000). Although

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[21] Named bar indicates average mentions to term during the period.
[22] Available at: https://www.presidency.ucsb.edu/documents/presidential-documents-archive-guidebook/the-economic-report-the-president-truman-1947 [accessed 09/07/2021]. For more information see Appendix 1.

Eisenhower had opted to a more conservative policy, putting Arthur Burns – a "sympathetic Republican loyalist" (Burton 2006, p. 177) – as head of the CEA and campaigning for stability, growth never left America's interest. The late 1950s were just a brief "republican interlude" in the growth mentality of the US government, according to Collins (2000).

The 1964 surprise then, was not in mentions to growth-related terms, but in mentions to poverty-related ones. In a pioneering manner, Heller's first signed report to President Johnson mentioned more poverty terminologies than growth words. This could be interpreted as a single man effort: Lyndon Johnson rose to power to save the poor. This is a cursory view. Kennedy's administration was aware of the poverty problem and, although it did not beat Johnson's reports in mentions, it was ahead of the Republican governments in the issue.

The link between poverty and economic growth had been formalized in the reports, but no technical detail had been advanced. In fact, for the most part, poverty remained a social problem that could be solved rapidly with intervention of social scientists and growth promoted by the economists. No research or quantification seemed necessary.

The apparently fast change in the framing of problems and campaign themes was a gradual change underneath. Government would probably have intervened in the problem of poverty before if Lee Harvey Oswald had not shot the president on that tragic November of 1963 in Texas. Walter Heller, in this regard, remembers struggling to convince Kennedy[23] about the relevance of a program on poverty and eventually receiving the approval:

"I got the green light from him [Ted Sorensen] on the poverty program. I had, after all, initiated that in May and June of 1963 in the sense of trying to get Kennedy interested. I had difficulty getting him on board, so to speak. Ted Sorensen had told me, "Keep at it, it's the kind of an issue we should sign on to, and it's a terribly important thing." I'd had sessions on it with Kennedy in October and again" (Heller Oral History, Interview I, p. 20)

Even before those sessions:

"I thought the only thing to do was to go directly to Kennedy and find out how he felt about it. And he said, 'Yes, Walter, I am definitely going to have something in the line of an attack on poverty in my program. I don't know what yet. But, yes, keep your boys at work, and come back to me in a couple of weeks.'" (Heller Oral History, Interview I, p. 21)

Lyndon Johnson played his part in boosting poverty programs into an "Unconditional War on Poverty". Walter Heller remembered that Johnson was spontaneously in favor of a program for the poor. According to Heller, Johnson said the

[23] Cherrier (2019) demonstrates Heller's skills to inform and convince the president of his economic ideas

following when receiving the information about the poverty program: "That's my kind of program; I'll find money for it one way or another. If I have to, I'll take away money from things to get money for people." (Heller I)

The previous name of the program was something way less impactful: "widening participation in prosperity" (see Sundquist Oral History, interview 1, p. 4). Kermit Gordon remembered that poverty was initially a condemned term, and Johnsons' advisers tried their best to avoid it. They believed the word could sound bad internationally or even offend low-income people:

"An interminable amount of time was spent in thinking up euphemisms for the poverty program with all this high-powered talent around the table. I think the reason it was called the War on poverty, the poverty program, was that nobody around the table, despite a lengthy effort to identify such a title, could think of any euphemism which didn't sound silly." (Gordon Oral History IV p. 3)

The War theme was contemporary as the Vietnam War was broadcasted daily, and Americans were already used to it. Lyndon Johnson presented the additional War with grandeur, knowing that it was central for his approval. The idea stuck into Americans' minds. He was not Kennedy's substitute; he was Lyndon Johnson: the - hopefully - winner of the War on Poverty.

Campaign themes aside, as a reminiscence of Kennedy's administration, growth was still central in the economic reports from 1964 to 1970. Walter Heller was still the chair of the economic Council, but Kermit Gordon and James Tobin had already been substituted. Heller's opinion on the role of growth was unaffected, even though he was politically smart in understanding the need for a poverty embedding for his Keynesian aspirations. That was really a matter of political acuteness from Heller and the CEA.

During the inauguration of poverty in the report, both the culture of poverty and delinquency studies made their way into Johnson's administration. The technical and numerical nature of economic discourse was complemented by its counterpart from the sociology. Poverty, however, was not a quantifiable problem, but still a campaign theme. Social intervention and action were the notions that came with poverty, not research and quantification. In the economic report of 1964, there were paragraphs that without context could readily be confused with one of Harrington's famous book:

"The poor inhabit a world scarcely recognizable, and rarely recognized, by the majority of their fellow Americans. It is a world apart, whose inhabitants are isolated from the mainstream of American life and alienated from its values. It is a world where Americans are literally concerned with day-to-day survival—a roof over their heads, where the next meal is coming from. It is a world where a minor illness is a major tragedy, where pride and privacy must be sacrificed to get help, where honesty can become a luxury and ambition a myth. Worst of all, the poverty of the fathers is visited upon the children" (US Economic Report 1964 p. 55)

"Poverty, as has been shown, has many faces. It is found in the North and in the South; in the East and in the West; on the farm and in the city. It is found among the young and among the old, among the employed and the unemployed. Its roots are many and its causes complex" (US Economic Report 1964 p. 77)

This is what O'Connor (2001) has named a Harrington-like language of the report. However, what is certain is that rebranding was easier on paper than in practice. Several agencies wanted to be the pillar of the War, being the main active agent in combating poverty.

The Department of Labor and the Department of Health Education and Welfare were the main contenders. But were they ready for mixing economics and action-oriented sociology? More important, were they prepared to put that mix in action right away? The answer at the time was: no. Johnson believed that current government agencies were unqualified to innovation once they would be divided between the new poverty effort and their normal activities.

The Office of Economic Opportunity (OEO) was rapidly gestated to the responsibility of administering the "War on Poverty" then. In the lack of an adequate place, the new agency was born in a "rather dingy office" in Washington[24] with the charge of "understanding the enemy" and creating a "strategy of attack" - although nobody actually knew what that meant at that time. In haste, the Economic Opportunity Act of 1964 that established the Office was vague in numerous ways; where should it be its physical location was one of them, the definition of poverty and how to face it were another two.

Section 202 of the economic opportunity act, where the definition of community action programs can be found, was written in a very unclear manner. Point a.3 of section 202, reads as follows: "[The term "community action program" means a program] which is developed, conducted, and administered with the maximum feasible participation of residents of the areas and members of the groups served". "Maximum feasible participation" was, without hyperbole, an inadequate choice of vocabulary and it has been discussed for its vagueness (US Economic Opportunity act 1964, p. 516). It even became a pun in Patrick Moynihan's book title years after: "Maximum feasible misunderstanding" (Moynihan 1969). In any case, the OEO was born, and it had to in a short time be capable of encouraging "maximum feasible participation" of the poor as part of its "strategy of attack".

All the haste and vagueness became a recipe for conflict, but also the recipe that would displace economists from their macroeconomic "ivory towers"[25]. Poverty was on its way out of the Council of Economic Advisers. Poverty would soon become more than a

[24] Probalby Robert Shriver's Conference Room. See Yarmolinski Interview II p. 5-6.
[25] It has already become standard to recall that when John Kennedy contacted James Tobin to join his team, Tobin answered: "I think you picked the wrong guy, I'm an Ivory Tower Economist".

campaign theme for economists, and intervention without research or quantification would prove to be unimplementable.

2.3. OEO born for conflict

Poverty, growth and economics were coming to a common agreement in the early 1960s. This agreement would be consolidated in the Office of Economic Opportunity. The office is a symbol of an era. It was an assortment of war-like ideas. Many of the well-succeeded concepts of the Cold War and WWII social sciences were there. The need for a different agency created an environment where every social scientist wanted space for its own worldview. As Haveman (1977), director of the Institute for Research on Poverty from 1971 to 1975 remembers: "Nearly every hypothesis regarding why the poor performed weakly in the labor market was reflected in some program" (Haveman 1977, p.6).

The agency, although having born in a dingy office, had a central role in the future war. The OEO was an action, coordination, researching, and evaluative facility. It was an immense innovative effort (March 1966, p. 115). Nothing like it had been tried before by the US government. Johnson gave it cabinet status that offered them freedom for innovation and quicker movements in Congress. In a way, the Office should work as an omniscient entity able to end poverty in about ten years! Sociologists run OEO's action, economists commanded evaluation and research, and politicians coordinated the effort. All these views together in the same place created considerable tension that would have only one winner: program evaluation. But before that, we have to understand what the conflicts inside OEO were and how evaluation could emerge as victorious.

For Johnson, the central piece for the idea to work out was a single man: Robert Sargent Shriver. Economists and sociologists could have ideas, but the fact is that government had to effectively fight poverty to achieve his campaign slogan. To that, someone had to pass ideas through Congress and be and active fighter in the War. Shriver was a middle-aged man who knew his way in politics. He was married to Eunice Kennedy, and thus was John Kennedy's brother-in-law. Since 1961, "Sarge" was ahead of the Peace Corps, making world tours solving political problems and implementing "community development" programs. He had both the credentials and the personal connections for the job.

Johnson trusted the man, as did Kennedy before him. On Thursday, the 2 of January of 1964, Johnson called Shriver to inform that he wanted to announce him as the head of the War on Poverty (see: Shriver oral history I). In fact, he wanted to announce Shriver in that same day. Shriver was caught by surprise. He had talked to Johnson at the Oval Office a few days before, but he had ineptly understood the president's hurry. He was still mourning Kennedy's death and had recently arrived from a world tour for the Peace Corps. Shriver asked for time to give an answer. Johnson gave him what an urged president can give: few hours. In the fourth call that day, in late afternoon, Johnson did not want to hear any other excuse from Shriver and just said in more eloquent words: "That's it. It's going to be you. I'll announce you soon. Be ready."[26] That was Johnson's conviction in the capacity of Shriver.

Shriver became responsible for organizing a task force that would put the War on Poverty on paper. Everything went fast. His first pick to help him was Adam Yarmolinski, ex-RAND researcher and at that moment serving Robert McNamara at the Department of Defense (see Yarmolinki Oral history). They assembled professionals from government agencies and from outside to complete the task force. As the president wanted the program to be his first serious move in congress, Shriver, Yarmolinsky and their crew outlined the "strategy of attack", including its central unity, the Office of Economic Opportunity, in no more than six months.

In that brief period, they had lots of questions to face, but two stood out: What exactly is poverty? How should they face it? The task force came up with pragmatical answers to those questions. For political reasons, an absolute line of income defined who the poor were. This absolute poverty can have an end, while relative poverty can go forever. It would not be politically smart to fight relative poverty[27].

Now, once they knew who the poor were, they had to decide how to help them. Simply, they had to start fighting. Johnson wanted the War on poverty and the OEO to be action-oriented. Research was secondary if even relevant. OEO had to be an agency aiming at acting directly on the grassroots of poverty. On this, the task force came up with a - soon to be noticed - reckless solution: the pioneer activities of Ford Foundation Gray Areas Project.

In the late 1950s, Ford Foundation Gray Areas Program was one of the earliest efforts to cope with the increasing awareness of problems surrounding juvenile delinquency and the urban crisis – inspired by Cloward and Ohlin (1960). It started as a series of grants provided to five cities: Boston, Philadelphia, Oakland, New Haven and Washington D.C.

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[26] Recording of the phone calls of that day are available at: https://discoverlbj.org/solr-search?q=LBJ%20and%20SARGENT%20SHRIVER&facet=40_s%3A%221964-02-01%22

[27] Molly Orshansky's research was behind the somewhat arbitrary definition of the line of income. See Huret (2018).

Almost everything about it was experimental as never something with its level of community engagement had been tried before (see O'Connor 1996). Still, participation of the communities in understanding and solving urban problems was its main goal and was demonstrating signs of success in the mid-60s.

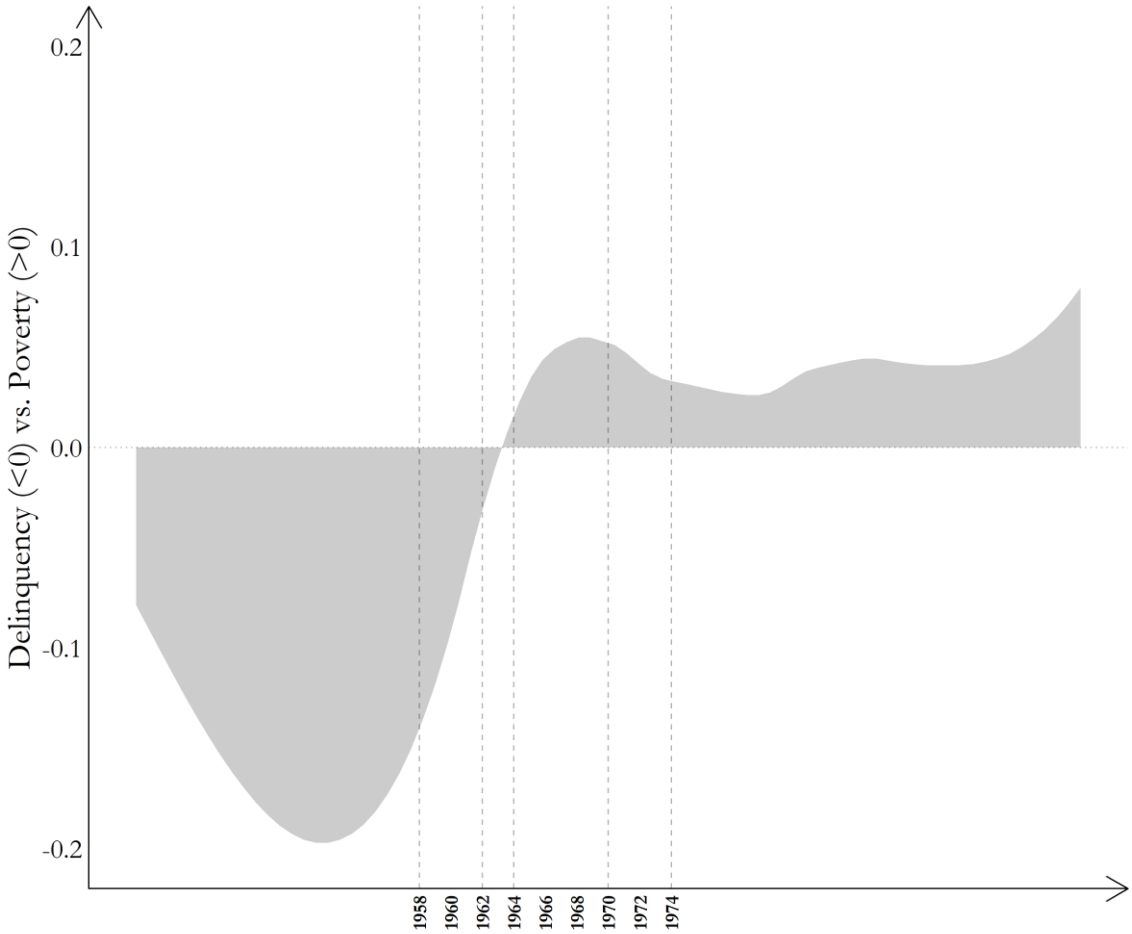
The members agreed that the community action programs (CAP) as embodied by Ford's ideal were the best way to attain the action goal. At least three reasons backed the decision. Beyond knowing community action quite well, given that the peace corps were involved in "community development" projects that worked in a similar vein, Shriver was a friend of Dick Lee, New Haven mayor, where Ford's Gray Areas had had its greatest success. In addition, among the task force recruits, the initial achievements of Ford's program put Paul Ylvisaker - director of the Gray Areas program - in an important position inside the group. These forces were enough for the community action program's pioneering trait prevail over its immatureness.

In the paper, community action is a straightforward task of empowering a community and let it make its own decisions. The poor - or whatever community in question - should not be taken for granted, and more important, they should not be co-opted in any manner. In this sense, community action programs are small-localized efforts. Each community must develop its unique democratic system of defining problems and finding solutions. Community action researchers were hopeful about this aspect as can be seen in the first edition of the Community Agencies Workbook:

"The community action agency provides the mewls whereby a community can take a fresh look in a coordinated fashion at the problem of poverty. It enables local citizens, local organizations, and local government to plan and act together. From their joint experiences and diverse perspectives, they try to find more effective ways to reduce or eliminate poverty." (US OEO 1965, p. 12).

What the War on poverty did was embrace this apparently simple idea and make it its own. The literature on "community action" that was of small scale and once concerned with delinquency, became in a brief period bigger and almost completely concerned with the broader problem of poverty. In JSTOR indexed articles, in the 1960s, mentions to poverty started to dominate papers mentioning "community action", while delinquency mentions lost space:

Fig. 2.2 – Poverty vs Delinquency mentions in "Community Action" Abstracts



Source: JSTOR[28]

It was all new and appealing. Although Shriver was very fond of the idea, he noticed that his budget was way over the necessary to implement hundreds – even thousands – of community agencies around the country (see Yarmolinsky I, p. 10). He had 500 million dollars to spend. His estimative was of spending 300 million dollars in community action programs. The task force had to find a way to use the leftover.

In his review of US governments of the 1960s, Plotnick (1975) compiled the five options of poverty policy available at the time: cash transfers, in-kind transfers, direct services, human capital programs, and community development programs. We have seen that community action programs were in the last category and represented the major part of the bill. Hand-outs werre readily discarded. Johnson never supported major cash transfers -

[28] Data built with JSTOR data for research (DFR), searched term: "community action". Analysis of abstracts done with tidyJSTOR, available at: https://github.com/arthurbnetto/tidyJSTOR. Poverty words accounted: "poverty"; "poor". Delinquency words accounted: "delinquency"; "delinquent". For more information see Appendix 1.

and this never became an option (Plotnick 1975). This left three options. In-kind transfers and direct services were used in a small scale. They were subsidies to help the consumption of certain goods or services, like food and health care.

The real competitors for the post of second major expenditure, then, were the remnant human capital programs. Without competition, it easily won the position. But human capital programs can themselves be divided into two other categories, training people or creating jobs, and this division became a theme of heated discussions within the task force and in the subsequent years. The secretary of Labor, Willard Wirtz, repeatedly contested training policies. He had been in Kennedy's administration and had the macroeconomic mentality of growth, employment and product. America needed more jobs. That was all for him.

Well, maybe that was not all. Wirtz has also been described as a "very jealous" bureaucrat. So, it must also be said that he knew that if more Jobs were America's future, the Department of Labor would be a central piece of the War on Poverty, whereas if training-programs were to be chosen, they would probably be divided among the Department of Health Education and Welfare and OEO, leaving the Department of Labor aside (which actually happened). Jealous or not, he was a strong voice inside the government whose opinion had to be considered. It took Shriver and his fellows some effort to overcome his thought.

A conflict was ensured for the rest of OEO's lifetime. The Department of Labor would be in disarray with the OEO since the beginning. Community action and job training were not Department of Labor's main responsibilities and would never be. What probably Shriver did not expect was that job training would also provoke buzz inside his agency. OEO's Community Action specialists also never approved training-programs. In their minds, job training programs were a top-down decision; there was no poor or community agency asking for that. This was purely a way of co-opting the poor in their scholar opinion.

Shriver tried to sell the idea for Community Action specialists that Job Training programs were a species of "department store" for community agencies. If the community noticed that the lack of training was a problem, it could check out OEO's catalogue and select one of its programs - like Job Corps or Head Start. In Shriver's view, job training programs of national extension were a way to protect community action from criticisms (Shriver Oral History IV p. 30)

The specialists never bought the idea. The OEO had then one more conflict to deal with, but that was not all. To complete the disputes with community action programs, following McNamara's successful implementation of RAND's planning programming budgeting system (PPBS) in the Department of Defense, a Johnsonian presidential directive enforced OEO to include a branch of research and evaluation. The Office of Research, Programming, Planning and Evaluation (RPP&E) was created to obey the regulations.

Right away the evaluation division noticed that evaluation was implementable on job trainings, but hardly so on community agencies. Whether community action programs were delivering results would be a hard to answer question during the subsequent years. Being a hard question would not be a problem if RPP&E did not play a far-reaching role in the agency, but that was not the case[29]. Joseph Kershaw, head of the economic department of RAND from 1948 to 1962 and at the time a Professor at Williams College, became the first director of the Office. Since his arrival, he was responsible for signing an important document. RPP&E and its director were the main authors of the War on Poverty five-year plans – directly analogues of the five-year defense plans. This was no small responsibility. The plan was the document that defined all poverty actions for the following period, and had to go through Congress and the Bureau of the Budget.

Kershaw invited Robert Levine to help him in the task of implementing system analysis at the OEO. Robert Levine was also a RAND employee and was eager to start a new challenge. Adapting PPBS to the need of the OEO would be it. PPBS had been inserted in all government agencies in 1965 through Johnson's presidential directive. The analytical technique had been inserted in the Defense agency through Robert McNamara in 61 and since then was presenting impressive results (See Einthoven 2019), but its capacity of adaptation to different problems was still a mystery.

The assimilation of PPBS varied greatly among all government agencies (Harper et al., 1969). This was due to several reasons, but mainly because most agencies already had their workflow of analysis. It would take time and effort to implement PPBS adequately for most of them. This was untrue for Kershaw and Levine's OEO. Recently created, it was fresh to use the most up to date metrics and tools from the Department of Defense and RAND. The choice of ex-RAND directors was a way of ensuring that. Harper et al. (1969) demonstrate that the OEO was the best agency in adopting the technique in the second half of the 1960s when compared to the other 15 governmental agencies.

Thus, while in other agencies community action programs could have lived an easy life without worrying itself with quantifiable metrics and single outcomes, community agencies were submitted to a rigid regime of evaluations in the OEO. This emerges in

[29] Forget (2011) discusses the clash between RPP&E and CAPS extensively

analyses and reminiscences of the period as a feeling that measurability was too demanding at the OEO (see Aaron 1978).

Imagine it, if you will, Community Programs were scattered all around the US with the intention of enhancing the quality of life of the communities. They want to increase self-esteem, empower individuals, make institutional changes. These are not exactly measurable outcomes, right? It comes as no surprise that RPP&E could not deal very well with that. Robert Levine admits that they found what was the "best way so far" only in 1970, but still he did not seem satisfied with the tools he had in hand:

"[…] to make institutional change is inherently a qualitative aspect. That's to say – an apple is inherently not quantitative – but you can count apples. So you can count institutional changes in detail, kinds of changes, causes of changes, and so forth. They're doing this now, and that's the best way I think we've come across to try to really evaluate Community Action" (Levine oral history p. 13)

Until this point, things seem to have been rough during OEO's formative period. Conflicts were popping everywhere. As if technical problems were scant, community action programs faced an additional more humane concern: political distrust. Mayors of major cities despised the concept. They accused OEO of inspiring riots all over the country. Already in 1965, in Congress, Shriver was having to dribble political conundrums explaining that his agency was not against politicians nor behind any protest. In the court hearing for increasing OEO's budget, Senator Murphy, asked him the following:

"May I ask a question? There was a comment in Los Angeles, which is the largest city in my State, made by the mayor. He said: 'Mayors all over the United States are being harassed by agitation promoted by Sargent Shriver's speeches urging those he calls the poor, in quotes, to insist upon control of local poverty programs. The Shriver organization can go ahead with the programs without the city having any voice in the programs at all and perhaps that is what is being planned, a real political boondoggle.' Would you care to comment on that? (US Expand the War on poverty 1965, p. 40)

Problems at OEO were enough for any agency to collapse. They were from all sorts of types: technical, ideological, political. And still, the agency survived. Years after the War on poverty ended, Shriver ponder on this. The point for him was that the programs were good. Time spoke for itself. Most of the War on Poverty programs had been living a long life when he was interviewed and Shriver did "believe that the longevity of them [spoke] well for the concept" (Shriver Oral History IV, p. 23)

Being proud and knowing that they were doing the right thing was powerless though, to avoid a restructuring of the war and the agency. The conflicts inside OEO were summed by an intellectual context that was aggressive towards any sociological consensus about the way of combating poverty. Patrick Moynihan, assistant secretary of Wirtz's Labor

Department and well-known figure in Washington politics, sparked a wave of social revolts and sociological debates after his 1965 report (US 1965), known as the "Moynihan Report" - but which the real title was "The Negro Family: The Case for National Action".

The report was written after Marquette Frye, an African American driver, fought with police near Los Angeles' Watts Neighborhood in August 1965. The report was a reflection on the subsequent days of the fight: The Watts Riots. Six days of uprising succeeded an acclaimed injury to a pregnant woman. Moynihan wrote about racial segregation and poverty in what can be said as an insensitive mode after those turmoiled days. Poverty was pathological in his report. The culture of poverty was taken to the extreme and the public received it with distaste.

The report became widely discussed inside and outside the academy. It was the spark to politicize the discussions about the culture of poverty. With this blatant insult to low-income families and African Americans, it did not take much to gradually bury any intention of applying the culture of poverty in governmental efforts.

While community action was not necessarily associated with the idea of dividing the poor under "deserving" and "undeserving" labels, it was closer to the culture of poverty than any job training program. The OEO had to be careful with its public appearances in an environment that was quick to bring academic ideas to politics and newspapers.

With every year that passed, it became clearer that the War on Poverty and the OEO had to be rebranded. Poverty was no more a mere campaign theme nor an exclusive specialty of sociologists, it was a complex and diverse phenomenon that deserved more than what economist offered through growth. The OEO would transform itself to abide to the modern times.

2.4. Governmental Think Tank for Experiments

Those were intense years until time for a change has come. Lyndon Johnson, the name behind the whole project, was worn out. The War on poverty was not going as expected, and the real War, on the other side of the ocean, was going even worse. In 1968 the Vietnam War was consuming all government resources, and still, it did not look as it was going to end neither well nor soon.

On the Sunday, the 31 of March 1968, from the Oval Office, President Lyndon Johnson postponed prime-time programs, appearing on television to inform that he would not run for re-election. Nevertheless, work still had to be done until the elections in

November of that year. Four days after Johnson's appearance on television, on the 4 of April, Martin Luther King was shot to death in a Memphis' motel. The United States went on distraught, and the president had to deal with numerous riots over that year. A half-staff flag flew in the country on the 7 of April, signaling respect but also that change was over-due. With a promise of ending the Vietnam War with honor and calming the situation at home, Nixon was elected at the end of that year.

This brings us back inside OEO's headquarters. Shriver resigned in the end of 1967, when Bertrand Harding replaced him. After the conflicts over community action and all the bad reputation of the culture of poverty that culminated from Moynihan's report, both topics silently lost space inside and outside the Office's walls. There were no political reasons to be associated with those controversial ideas. No substantial evidence or results were supporting their theories either.

The first sentence in OEO's report of 1969 had Nixon's following words:

"I believe that the goal of full economic opportunity for every American can be realized. I expect the Office of Economic Opportunity to play a central role in that achievement. With new organizational structures, new operating procedures, and a new sense of precision and direction, OEO can be one of the most creative and productive offices in the government. For here much of our social pioneering will be done. Here will begin many of our new adventures." (US OEO Report)

But what exactly were these new structures and operating procedures? In a single page of the report all the answers could be read. They were direct. They were simple. Experimentation and testing were the new role of the agency:

"The risk of wasted public money can be reduced substantially through documented testing of new ideas for social programs. The President has assigned to the Office of Economic Opportunity the role of experimenting with innovative approaches to the problems of poverty." (US OEO report 1969, p. 1)

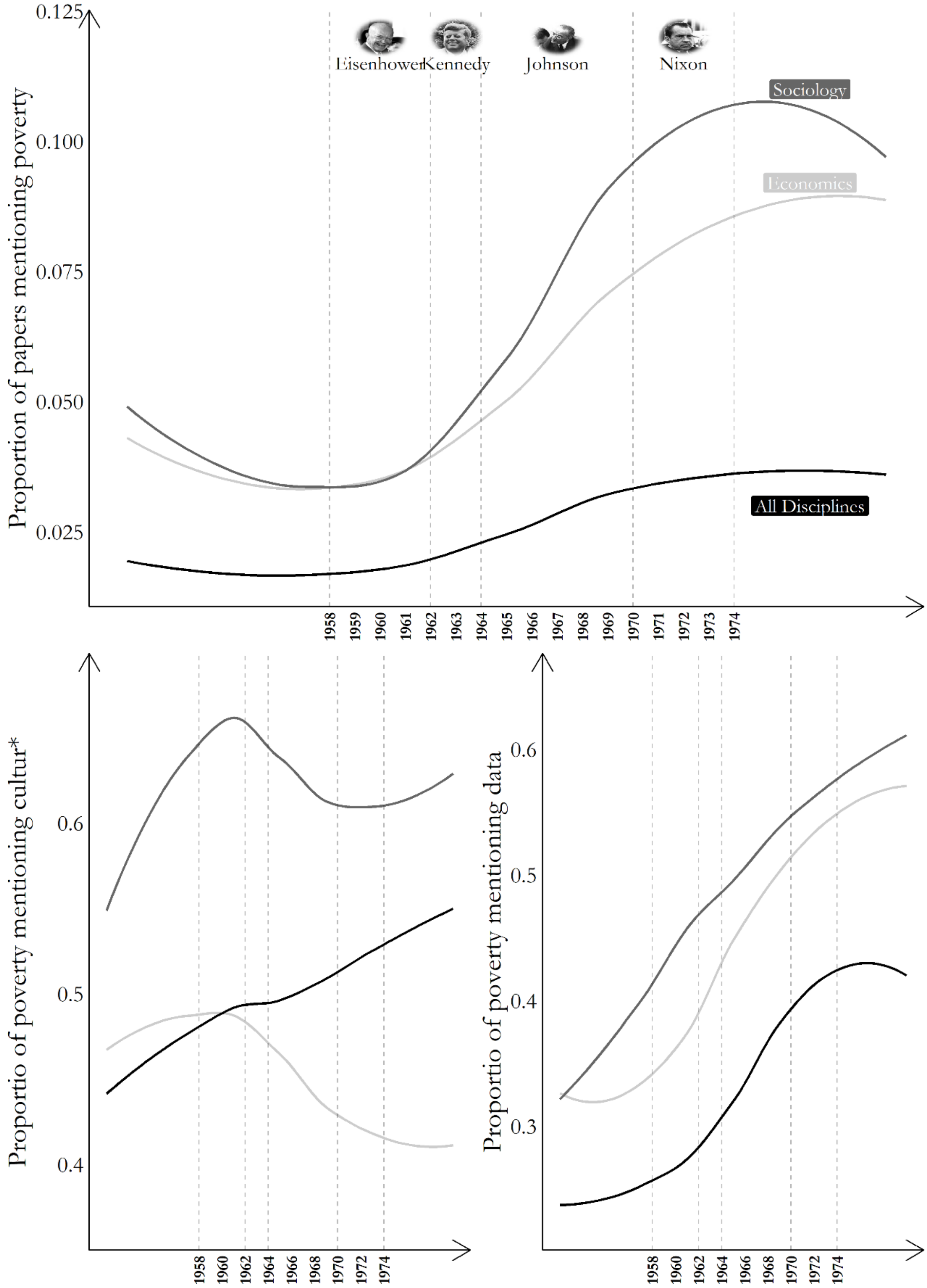
Action was no more OEO's focus:

"mature operating programs run by the Office of Economic Opportunity have been turned over to other Federal departments and agencies, leaving the Office of Economic Opportunity free to develop and demonstrate different ideas" (US OEO report 1969, p. 1)

Job training programs were mostly divided among the Department of Health Education and Welfare and Department of Labor. The previous Research, Planning, Programs, and Evaluation Office (RPP&E) became the Office of Planning, Research and Evaluation (PR&E) with an increased budget and new responsibilities. Leaving "program" outside its name and responsibilities, OEO "was strengthened" according to the report. Joseph Kershaw remained ahead of the evaluation and research activities.

As data from the poverty programs started to "materialize" in the late 1960s (see Levine oral history), culture gradually lost importance in discourses. In JSTOR-indexed articles, mentions to poverty increased both in economics and sociology since the early 1960s. However, they gradually diminished their user of culture-related terms and increasingly relied on data:

Fig. 2.3 – Cultur* vs Data in papers mentioning poverty



Source: JSTOR[30]

[30] Searched terms at www.jstor.org: poverty; poverty AND cultur*; poverty AND data. Cultur* is a wildcard that searches for all terms with the root cultur. This includes culture, cultural, cultures and so forth. For more information see Appendix 1.

What can be hypothesized is that this could be an artifact as data became increasingly available. Nevertheless, PPBS strict format evolved inside the agency into a more embracing culture of data. The OEO was a highly specialized unit inside the government. This was even more so during Nixon's term when the agency sought policy analysts with a Ph. D., especially in economics. Data and research were welcome there.

Since 1966 OEO had significant connections with universities and researchers. They were a few initiatives but had large-scale grants and important responsibilities. The contract with the Institute of Research on Poverty at the University of Wisconsin to support the advancement of the research on poverty exposes that. According to Evanson (1986), the institute started small at Wisconsin bolstered by the famous "Wisconsin idea", that impelled its economists to participate in politics and policy, and the influence of Robert Lampman, who, as we saw, had advised Walter Heller in the early sixties.

Harold Watts was the first director of the Institute at Wisconsin from 1966 to 1971 and had to coordinate the task of being a "national center for study of the nature, causes, and cures of poverty." Lampman had his back as a second in command. The institute had 30 persons as permanent staff in 1969 but grew in the following years, becoming a place of training and community gathering (Uhr 1986). During the 1960s and 1970s, numerous PhDs were trained there, and several conferences were organized[31].

Similarly, OEO supported other academic projects. Among them, the Panel Study for Income Dynamics (PSID) at the University of Michigan has to be under the spotlight. In 1966 and 1967, the OEO conducted a major survey of 30,000 households to study the characteristics of poverty. However, such a massive endeavor was hard to sustain for the following years, which led the Office to contact James Morgan at the University of Michigan Survey Research Center to continue the process, although with a sub-sample of the initial surveys (see McGonagle et al. 2012).

Academic research centers such as the Survey Research Center and the Institute for Research on Poverty are a contesting force to strict governmental procedures. Academics evade the rules of bureaucrats. For instance, James Morgan was reluctant to accept the role as director of the Survey Research Center for not agreeing with the previous research methods (see Sherburne 2017). The Institute for Research on Poverty only became a reality after imposing the "condition that the Institute exercise full authority in allocating grant funds to researchers, selecting research topics, and publishing the results" (Evanson 1986, p.2)

[31] For more information on Ph.Ds and conferences see Focus special issue of 1986 (Uhr 1986)

While PPBS lived its glory days in the early 1960s, especially inside McNamara's Department of Defense, in Nixon's more academic presidency, was in dissonance with a broad implementation of the methodology. In the *Public Administration Review* of the late 1960s, what could be read was a disappointment with PPBS and a foreseeable melancholic end (Botner 1970, Mosher 1969). New evaluation tools were on the horizon.

When Shriver left, new tools had the opportunity to emerge. The context was favorable: new data was materializing, PPBS was falling, culture talk was avoided, action was not the focus of the agency anymore. Adding up to that, new programs were on their way. In contrast to Johnson, Nixon was not afraid of hand-outs; cash-transfers were a real possibility in his government. Nixon's "Family Assitance Plan" was considerably discussed at the time and could be an aid for low-income families in the upcoming years. Still, Nixon and his agencies would not offer money without evidence that this would produce returns to society in a meaningful way.

This kind of discussion was already usual since the beginning of the 1960s, when Milton Friedman coined the term: "negative income tax" (Friedman 1962). It lost traction during Johnson's term, given his reluctance over hand-outs. Milton and Rose Friedman had been known as advocates of a cash transfer since the publication of *Capitalism and Freedom* in 1962, but economists more widely received the idea with caution. Most of them were worried about recoil effects on the job market. Still, no theory had proven sufficient to discern whether the preoccupation was valid until the late 1960s. If cash transfers would be a reality, some tests or experiments would have to be made.

Inside government, the Department of Health Education and Welfare analysts had historically opposed to Community Action as a waste of money. The idea of guaranteed income had survived inside that department even during Johnson's years. In the view of Alice Rivlin – an influential analyst inside the agency responsible for revamping the budgetary process and trained by Guy Orcutt -, community action and other Johnsonian solutions were just random projects to deal with poverty. There was no "systematic thinking" before the choice of the programs (See Rivlin 1972). A guaranteed income was a unified solution in her view that could pass through the test of data (see Huret 2018 Ch 7, p 30-32). In 1968 she had already developed the idea of a program called BIG (Basic Income Guarantee) that could be submitted to a national test: a randomized controlled trial.

The OEO, as the new data think tank of the government, took responsibility over the guaranteed income issue. The debate was growing stronger, and answers had to be provided. To the like of Rivlin, time had come to spend the money - otherwise spent on action - on finding answers. Large-scale social experiments were the most expensive kind of evaluation that one could think of and the kind of evaluation that was said to offer the most precise answer. As the budget could accommodate the project, that grew into a real strategy, and it was not a simple one. The realization of the largest randomized controlled trial ever done in America became the strategy of the Office to discover whether a negative income tax disincentivized labor.

The "Rural (North Carolina, Iowa) Income Maintenance Experiment" (1969-1973), the "Gary (Indiana) Income Maintenance Experiment" (1971-1974); and the "Seattle-Denver Income Maintenance Experiment" (1971-1978) were nation-wide programs of negative income tax estimated to cost up to 5 billion dollars in 1965 to the government. The randomized controlled trials themselves cost around 110 million dollars. This was about one fifth of the whole budget of the OEO when it started with Shriver. But this time it was money for research, and not action.

Such project would not be completed alone. While in Shriver's term there was a reluctance of using outside contractors to evaluate government programs, this was different in Nixon's period. The OEO changed its operational format and contracted outside evaluation firms in the late 1960s. With this, tt became besides a producer of research, also a consumer.

The most significant story in this operational change is the birth of Mathematica Inc's branch of policy research, which was the main responsible for the Negative Income Tax experiments. However, its history started earlier than that. Mathematica Inc emerged in 1958 as one of the several enterprises of the 1950s and 1960s that looked to provide external evaluations in business and defense problems. The success of RAND corporation leveraged the emergence of this kind of institution during the period. But it must be said that Mathematica was special in this milieu. Its initial fellows were Oskar Morgenstern along with several mathematicians from Princeton University: Albert Tucker, Ralph Gamory, Harlan Mills and Michel Balinski. Harold Kuhn, William Baumol and Richard Quandt also from Princeton joined soon after the foundation.

In 1968, Mathematica founded Mathematica Policy Research specifically to conduct the Negative Income Tax experiments.[32] Albert Rees, William Baumol – two respected Princeton economists of the time - and Heather Ross, a brave young scholar, signed a proposal of the experiment under the auspice of Mathematica. Once Mathematica was

[32] At that year, the Office of Economic Opportunity, led by Joseph Kershaw, made a Request for Proposals on a Negative Income Experiment after deciding the need for the experiment to be realized with the help of external agencies (see Wooldridge 2013 p. 28).

confirmed as responsible for the task, David Kershaw, the son of OEO's director Joseph Kershaw, became the president in charge of the freshly born policy research branch of Mathematica that managed the experiments.

There were reasons for bureaucrats to be reluctant with Mathematica's first experiment being one of such enormous size. Thus, OEO opted to subcontract Wisconsin's Institute of Research on Poverty as co-responsible for the experiments. This would mainly avoid concerns about money flowing directly to a for-profit company.

The Institute and Mathematica joined efforts to make the largest social experiment ever done in America: the New Jersey Negative Income Experiment. David Kershaw was the main director of the project, while William Baumol and Albert Rees from Mathematica and Robert Lampman and Harold Watts from the Institute for Research on Poverty were the leading investigators. Heather Ross, one of the initial proposers of the experiment to the OEO, was part of the investigation through her PhD. thesis - one the most expensive PhD. thesis ever done in economics, surpassing 5 million dollars at the time (Levitt and List 2009).[33]

Several negative income tax experiments followed the first one, and them, OEO, and poverty changed the course of the history of program evaluation towards economics. Poverty knowledge that was initially divided into Community Action and the Culture of poverty was suddenly Job Training programs and negative income taxes. Randomized controlled trials decided whether they were viable or not. Any qualitative judgments of the Shriver years were then gone.

But poverty knowledge was not the only idea being changed. The discipline of economics was also in movement. In the process of proposing and testing the negative income tax experiments, a network of research was being formed. Wisconsin was hiring staff in economics that had to be specialized in the evaluation of programs and was also training PhDs. Princeton Economics department was providing researchers through Mathematica, and Harvard was involved through Heather Ross. The Chicago Department, although indirectly involved in the experiments themselves, was also directly concerned with the results given that Friedman was considered the father of the negative income tax idea. This meant that the economics discipline mode globally had an eye over the whole thing.

But the experiments were unprecedent because economists had not dealt with randomized data up to that point. Randomized controlled trials or experiments were mainly a concern of other social scientists. Oakley (2000), in this regard, names few experiments during the first half of the 20th century in education, such as Hudelson (1928) and Pittman

[33] Edwin Kuh was her advisor at MIT with Robert Solow and Michael Piore completing the committee.

(1921). In criminology, in the 1930s a large-scale experiment was carried out for the first time: the Cambridge-Somerville youth study. And even in these fields, she argues, this kind of research can be said to be unconventional at that time.

As we saw in chapter one, econometrics was different field that dealt with what Haavelmo (1944) named "Passive Observations". Recalling the concept, what that means is that, for economists, data is gathered externally, and they do not have any control over the collection of data or the data generating process. They borrow data from institutions to their desks.

Regardless of whether economists' involvement mattered or not, what followed from 1968 to the early 1970s is known as the "Golden Age" of experiments (Oakley 2000, Rossi and Wright 1984)[34]. Large scale experiments were initiated all over the country. Beyond the negative income tax experiments, housing and health experiments were conducted.

There was a significant optimism about the programs and the evaluations. The experiments would polish negative income tax to its perfect form before implementing it to help the poor. It is not the point here to enter on the technical details of randomization and experiments again. It suffices to know that experiments divide two groups equal in everything observable and unobservable that they make different giving a "treatment" to only one of them. This difference attached to a single factor allows researchers to make causal claims: "the treatment causes…". With the negative income tax experiments, researchers would be able to say things like "a cash transfer of x, will cause a reduction in employment of y%" – once "cash transfer" would be the "treatment". Simple and precise, exactly the need of bureaucrats. No wonder researchers were optimistic about that.

But wait, there is a – not so - small puzzle here: there has never been a negative income tax in America since then. Something went wrong along the way with the Negative Income Tax experiments.

2.5. Evaluation beyond experiments – The birth of program evaluation in economics

Negative Income Tax experiments were huge endeavors. Thousands of individuals were being tested, hundreds of researchers were involved. Uncountable operational problems existed, starting with randomization. In some of the experiments, randomly picking

[34] Madaus and Stufflebeam (2000) name the period from 1958 to 1974 the "age of development" of evaluation, when large-scale experiments led the development of new knowledge on evaluation techniques.

individuals was not so easy as it seemed. And even if one could choose the right groups, some of the individuals would never show up or just disappear in the middle of the trial (what researchers call "attrition").

Statistical methods evolved during the period and techniques were developed to handle minor problems in the selection of groups. However, politicians and bureaucrats did and do not want to know about this. They want results, something that neither the Institute for Research on Poverty, Mathematica nor anyone could offer in a rapid manner. Their initial plans were of experiments that would last from 3 to 5 years. In the case of the Family Assistance Plan, discussions started in Congress in 1969. No major experiment was close to an end in that year. As Nathan (2000) pointed out: "As it turned out, the idea of a negative income tax as tested both in the New Jersey and the SIME/DIME [Seattle/Denver] studies seeped into the policy process long before the final results of the experiments were available.".

To complement, researchers were spending a lot of public money on the experiments. To be clear, those were experiments where the treatment was a "cash transfer". This "cash transfer" was occurring for the treated groups all months of the year over 3 to 5 years. In the eyes of politicians, it did not matter that it could save money in the future, this was money being spent right in front of their eyes without any practical return.

What researchers, bureaucrats, and politicians had then was a fascinating causal tool, but a very impractical one. It had a slow pace and cost a lot. It is not even necessary to consider that when results came out, they were inconclusive (researchers did not know whether cash transfers should be implemented after several years of research[35]). This happened in the mid-70s, and change had already come at that point.

Without achieving politicians' expectations, the OEO came to a sudden end in 1972. The programs and evaluations that remained active were delivered to other departments, mainly the Department of Health Education and Welfare and the Department of Labor. Evaluations were transferred to the Department of Health Education and Welfare 's Office of the Assistant Secretary for planning and evaluation and Department of Labor's Office of the Assistant Secretary for Policy, Evaluation and Research (ASPER). Those two were similar organizations that were responsible for evaluating the programs of their respective departments.

According to Greenberg and Robins (1986) and Greenberg et al. (1999), Social experiments became smaller and instead of testing new programs, they focused on analyzing

[35] See US GAO report of 1981

incremental changes in existing programs. The author affirm that long periods were no longer necessary to produce results; instead of lasting 3 to 10 years, experiments lasted 1 to 3 years in the period 1975-1983.

Even with these changes, experiments for the most part fade out in government. Governmental agencies and departments could contract firms such as Mathematica to conduct evaluations, but they would be no more responsible themselves for the endeavor. The institute of research on poverty, for instance, - considered part of OEO – stepped aside from negative income taxes after OEO closed its doors and had to find new modes of functioning. At the same time, Mathematica resorted to a grant from the Department of Health Education and Welfare to maintain its operations.

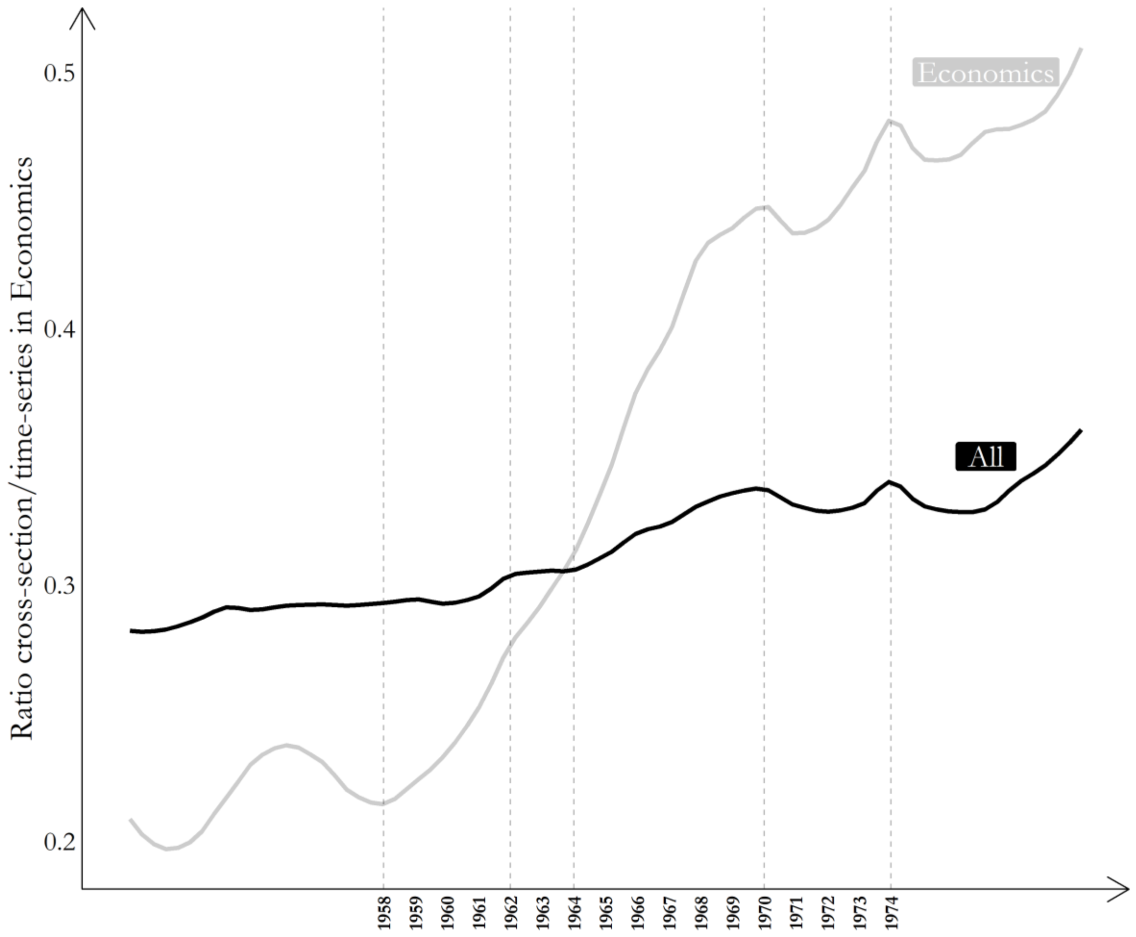
Time, however, had been enough for a safe network to be built. More precisely, policy evaluation had been professionalized. It had a life of its own beyond governmental demands. How to evaluate better had become a research question and would be part of universities from then on. Private institutions required evaluations for their programs and government was still able to hire minor evaluations.

This is not the most thrilling ending for such an intense journey, but it is a fact. Societies had been formed, academic journals released, graduate courses initiated (see Kimmel 1981, Rossi and Wright 1984). In the mid-70s, policy evaluation was a concern of in economics departments. The War on Poverty and the data and methods that came with it had sparked a technical and theoretical research agenda.

It had not been long since economists worried solely with time-series. That was their expertise. Econometrics *was* macroeconometrics. As discussed in chapter one, Hood and Koopmans (1953), in the founding days of the field, were worried mainly with measurement errors and simultaneity, known problems of aggregated data and time-series - widely described as *endogeneity* nowadays. There was no *omitted variable* or *unobservable* immediately in their sight. Even measurement errors were regarded as a secondary topic once there was little that could be done to interact with data collection.

This means that randomization, unobservable and omitted variables were a new topic for economists in the 1960s and 1970s. You would never open an econometric textbook and find a section called endogeneity divided into simultaneity, measurement errors and omitted variables. This is a modern approach, something that could only emerge with cross-section and panel data. In the economics' articles indexed in JSTOR, mentions to time-series were almost five times higher than mentions to cross-section in the 1950s. In a matter of 10 years, this changed dramatically. Cross-section was mentioned nearly one for every two mentions of time-series in the second half of the 1960s:

Fig. 2.4 - Cross-section vs. Time-Series



Source: JSTOR[36]

At this point, poverty knowledge, that had wandered through sociology and experiments, was crossing the realm of analytical policy evaluation. In economics, this is indistinguishable from the development of microeconometrics. The technical problems that lead to the failure of experiments and that came with the new datasets were the problems of young econometricians: How could one deal with panel data? How to avoid omitted fixed effects to influencing the estimators? Can we emulate randomization with econometric methods? What is the best way to deal with cross-sections? In which ways simultaneity is like the problem of unobservable?

[36] Searched terms at www.jstor.org: cross section; time series. For more information see Appendix 1.

Unlike money for evaluation, questions were plentiful. Microeconometrics came silently, but to stay. Economists understood that the data accumulated was perfect for solving economic theory questions, especially in labor economics. PSID would fast become the go-to source for any labor economist in the 1970s.

The new generation of economists faced the trial of having to solve the technical questions that came with the unavailability of experiments and an abundant number of panels and cross-sections. Cheaper, faster, and more reliable evaluations became the objective of researchers in government, private-sector and universities.

In the private sector, for instance, a few firms were established to conduct smaller-scale experiments. However, a gap in the market was also created for different evaluation approaches. Coming from the Office of Urban Affairs, in one interesting example, a host of young researchers created the Urban Institute. Microsimulation became its most well-known product (see O'connor 2001, ch. 9). Microeconometric techniques were developing in the outdoors.

In universities, those with ties to the experiments were quick to transmit the new questionings to their students. Albert Rees supervised or was in the committee of an outstanding group of students in the late 1960s and early 1970s. The most well-known are John Pencavel in 1969, Orley Ashenfelter in 1970, James Heckman and Ronald Oaxaca in 1971. We are talking about people who were behind difference-in-differences (Ashenfelter 1978), Oaxaca-blinder estimators (Oaxaca 1973), a Nobel winning model, the sample-selection model (Heckman 1979) and the consolidation of the Journal of Economic Literature (JEL) Codes (Pencavel in 1988[37]). The connection negative income tax – Mathematica – Rees – Princeton was an active force in the development of microeconometrics and economics.

Albert Rees was also a friend of George Shultz (see Shultz Oral History), Nixon's secretary of labor in 1969-1970. He was responsible for restructuring OEO's evaluation inside the Department of Labor. Under Shultz, evaluation and research were revamped. Shultz, "an intellectual conglomerate" [38], had been a noteworthy researcher himself before assuming positions in government. He had been part of the Chicago's Faculty of the 1960s together with Friedman, Gregg Lewis, and Rees. Shultz and his friend "Al" Rees had written an extensive research on labor markets that used the latest analytical tools emerging in the 1960s with the War on Poverty (Rees and Shultz 1970). At the time he was appointed

[37] See Cherrier (2017)

[38] Shultz's description at the department of labor's official history. Available at: https://www.dol.gov/general/aboutdol/history/dolchp07 [accessed 09/07/2021]

secretary of labor, he was the Dean of the Chicago Graduate Business School. His view for ASPER was that of a prominent research facility.

ASPER was set on 14^{th} and constitution in the same building as the Dep. of Labor in 1969[39]. It had "Very tall ceilings and was one of the first buildings built in Washington in the 1930s with central air conditioning." recalls Orley Ashenfelter. Beyond its magnificent look, the building was also close to the White House and the center of the Washington Riots of the 1960s. "And the fact is that the area around us impressed upon you that there was some urgency in trying to make those programs work." (Ashenfelter Oral History III).

It was 1972 when Orley Ashenfelter was called to become the first director of the "E in ASPER" – evaluation more precisely (see Hamermesh in Krueger 2014). This was an academic choice, even though George Shultz had already left the Dep. Of Labor and James Hodgson had assumed his position. Ashenfelter had just finished his PhD. in Princeton under the supervision of Rees and was a professor at Princeton for a few years at the time. He had no experience in government though. Still, he was part of a younger generation of PhDs trained in contact with cross-section and new labor economics tools. There were not many like him. Up to the 1970s, labor economics was intertwined with labor relations and was dominated by institutionalist approaches[40]. While analytical labor was maturing within government walls; few departments offered the training[41]. Princeton, Michigan, Columbia, Chicago, and Wisconsin were some of them because of their faculty's connection to government.

Following Ashenfelter in the command of the evaluation division of ASPER came George Johnson (1973-1974), Daniel Hamermesh (1974-1975), Frank Stafford (1976-77) and Alan Gustman (1978-79). All of them had connections to 'analytical labor' departments. Hamermesh was a 1969 Yale PhD. who taught at Princeton and was starting a new challenge at Michigan State. George Johnson was a 1966 Berkeley PhD. with a longtime career as Professor of Michigan. Gustman made his whole career at Dartmouth and Stafford was a 1968 Chicago PhD. teaching at Michigan.

"We were all very young at that time. What we did was introduce and spend money on serious evaluation of labor programs." Remembers Hamermesh (Hamermesh Interview I). That was impressive in a time when things were changing in government. "The experience

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[39] There are conflicting references to the year of foundation of ASPER. US ASPER 1980 points to 1969, while Krueger 2014, Ashenfelter and Hamermesh on interviews point to 1972.

[40] See Kaufman (1993, 2006) for accounts of the demise of institutionalism in labor disciplines and Rutherford (2011) for a broader review.

[41] As its being told, analytical labor economics was a small field mostly constrained to governmental organs until the 1970s. In universities it had a small reach that can be traced back to H Gregg Lewis in Chicago. For more references on the issue see: Rees (1976), Ashenfelter et al (1994) and Hamermesh (2020).

was quite exhilarating. Much to my surprise, the office was left to do its work without political interference" (Ashenfelter 2014, p. 2)

In between serious evaluation, these researchers also found time to do what they were trained to do: research. The Office started a series of Technical Analysis Papers (TAPs). Those were technical papers in labor theory and microeconometrics. Most of them were also published as working papers for economics' department and were later published by academic journals. They circulated freely to promote discussions. Their name though, was an inside joke:

"We called them technical analysis papers partly because at that time there were some unauthorized wiretaps and the Justice Department couldn't seem to keep track of. So when I would give up a talk on one of these papers. "this is TAP. Today I'm talking about TAP". this is true. I would say: "I would like the Justice Department, to know that the Labor Department actually know how many taps we have". (Ashenfelter Oral History II)

Jokes aside, the list of TAPs was impressive. Several researchers wrote them under grants from ASPER. Among them it is possible to find papers from James Heckman, George Johnson, Orley Ashenfelter, Lawrence Summers, Richard Freeman, Jacob Mincer and many more. Bobray Bordelon has made a preliminary list that counts more than 50 TAPs[42].

A list of all projects and reports of ASPER is available as well (US ASPER 1980). In the document it is possible to find 27 reports focusing "on the economic, social, and policy background"; 85 reports about "the labor market itself—the nature and extent of labor supply and demand; the way in which workers, their unions, and their employers respond to the ebb and flow in the labor market; and the consequences of the operation of the labor market for various groups of workers"; 94 reports "concerning the nature and impact of the Department of Labor's programs". And 41 reports about "how the Department could do a better job of administering its programs by developing better information for management and better methods for evaluating operations" (US ASPER 1980, p. X).

In the list it is possible to note that ASPER achieved a centrality that many economic departments could aspire but never have. Researchers from numerous universities and private institutes have contributed to ASPER research projects. All distinctive departments and researchers of analytical labor economics in the 1970s received some form of support from ASPER.

[42] The full list can be found in Appendix 2. I thank Bobray for his amazing librarian skills for the list.

The list has no large-scale experiments among the indexed projects though. On the contrary, most of the papers were technical developments of econometric techniques to deal with data in the absence of experimental data. The most well-known paper on the subject is: "Estimating the Effect of Training programs on earnings with longitudinal data" by Ashenfelter in 1974. The "study dealt with the effect of training programs on earnings and the difficulty of implementing an adequate experimental design to obtain a group against which to compare trainees reliably." (US ASPER 1980, p. 52)

The name may sound familiar, especially for labor economists, because it was published four years later in the *Review of Economics and Statistics* under the shorter title: "Estimating the Effect of Training Programs on Earnings" (Ashenfelter 1978). The paper is widely known to be the first presentation of the difference-in-differences estimator.

The difference-in-difference estimator is an exemplification of the transformation that economics and econometrics were facing. Without the money and fanciness of large-scale experiments, econometrics came back to the table as an unexpensive way of evaluating programs. However, the previous macroeconometric-laden theory was precarious to solve problems of panel data and cross-sections. Economists had then to embark on a journey to solve new technical problems.

Economists knew the capabilities of randomization. They had seen it in the previous years. "My own conclusion was that randomization was the only transparent and credible cure for this problem [dealing with unrandomized data]" said Ashenfelter (2014) when recalling about the time. The question then was how they could apply the same standards of randomized trials to unrandomized data. Diff-n-diff did that successfully.

Inside ASPER, then, researchers were enlightening and solving a concealed feature of passive observations: omitted variables. They were transforming their careers and their field in doing it: "This time in Washington led to a lot of stuff that we did later on and a lot of people paid attention to. So I would think about ASPER being this sort of breeding ground, if you will, for an awful lot of researchers in labor economics in that and subsequent periods." (Hamermesh 2020 - Appendix 11).

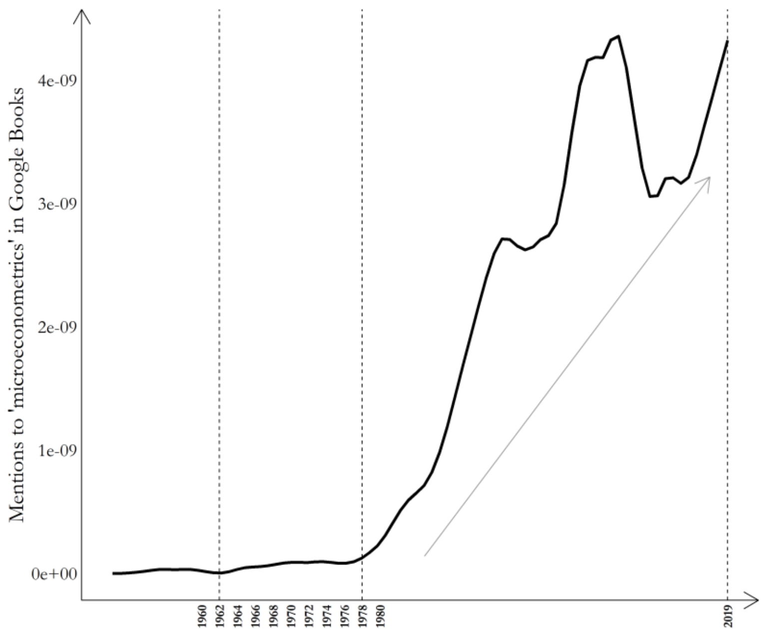
ASPER is, thus, the end of the long story of how econometrics made its way into policy evaluation and how it was shaped by it. That is the long story of the birth of omitted variables in econometrics and the emergence of microeconometrics. At the point where this story ends, microeconometrics, evaluation of programs and omitted variables had already become the bread and butter of economics departments all over the US.

2.6. Concluding remarks

In 1979, one year after Ashenfelter's paper was published, James Heckman identified a specific type of omitted variable, self-selection, and presented his solution as an *Econometrica* article titled "Sample Selection Bias as a Specification Error" (Heckman 1979, chapter 4). Instead of simulating randomization, though, Heckman modeled out the selection bias. Economics had changed. It had come a long way from its initial unknown incursion on the problem of poverty as a "campaign theme". Economics was dealing with cross-section, panel data, social issues, omitted variables, unobservables, and self-selection. Microeconometrics was a new area of research that enticed young researchers. It was the study of complex econometrics estimators that emerged from the necessity of researchers making evaluations in the absence of experimental data.

Governmental walls enclosed the formative years of microeconometrics, but when it was set free, being published in important journals by Heckman and Ashenfelter, it finally received its deserved token of an academic field and grew fast and strong. Mentions to "microeconometrics" in google books broke out after 1978/1979, marking the beginning of a new economic field:

Fig. 2.5 - Mentions to 'microeconometrics'



Source: Google Books[43]

[43] Data for fig. 5 available at: https://books.google.com/ngrams/graph?content=microeconometrics&year_start=1950&year_end=2019&corpus=26&smoothing=3. For more information see Appendix 1.

ASPER's research is the end of a history about how randomization became the gold standard for economists and how they learned to rely on their econometric methods instead of strict experiments. Labor economists from the 1970s and forward would face the same problems over and over again. Solving omitted variables and self-selection became the task of the microeconometrician while dealing with simultaneity stood the job of the macroeconometrician.

3. TO RANDOMIZE OR TO NOT RANDOMIZE? PRINCETON SOLUTIONS FOR SELECTION BIAS IN THE 1970S

3.1. Randomistas versus structural modelers

US concerns with poverty were on the rise in the 1960s, as discussed in chapter 2. Economists joined the effort of addressing them and participated in the early governmental organizations for evaluating programs and then brought them to universities. In Princeton, this was the road taken by Orley Ashenfleter and his students. As we argue in chapter 2 and will discuss more deeply in chapter 4, Ashenfelter brought program evaluation practices from the government to Princeton's Industrial Relations. He and his students gradually developed their research agenda to create a consensus around natural experiments.

This successful history contrasts with present-day views on microeconometrics. As James Heckman (2000, 2001) discuss, microeconometrics is far from having developed only through program evaluation or natural experiments. In addition to this, *structural modelers* developed their own methods to deal with omitted variables.

In the contemporary literature these two forces within microeconometrics became a clash between two communities: *randomistas* versus structural modelers (see: Deaton and Cartwright 2017, Heckman and Vitacyl 2005, Heckman and Urzua 2009, Imbens 2009, Heckman 2010, Angrist and Pischke 2010, Ravallion 2009, 2018). While randomistas represented by Orley Ashenfelter, David Card, Alan Krueger, Joshua Angrist and their students, especially Esther Duflo (1999 MIT PhD. advised by Angrist jointly with Abjit Banerjee, with whom she received the Nobel Prize of 2019) are said to defend the use of randomization as the only credible way of avoiding omitted variable bias, structural modelers represented by the Nobel prize winners, Heckman and Angus Deaton advocate the view that to exclude the bias one needs to know its root cause and model it out. These technicalities will be discussed in the next section, the main point here is highlighting that both communities have different solutions for the same problem, and this is the cause of their contemporary disagreement.

Looking from the previous history presented in this thesis and these three paragraphs it could be surmised that randomistas and structural modelers developed distinctly and had always had a dispute. In fact, this is the view presented in Heckman (2000, 2001). However, this is wrong. Randomization and structural modeling had a close relationship in the early days of omitted variables, at least conceptually and in Princeton, as we shall see.

Heckman and Ashenfelter have quite similar backgrounds. Both are labor economists with PhD. from Princeton. They are almost contemporaneous, the former is the youngest born in 1944, and the latter is two years older, born in 1942. Because of this minor difference in age and common interests, Heckman and Ashenfelter met at Princeton . Besides, they published together during the first years of their careers (Ashenfelter and Heckman 1971,1972,1973,1974). Consequently, during their initial careers, Heckman and Ashenfelter had similar knowledge and published similar research.

Taking some steps back on our narrative, we remember that during 1971 in the empirical labor economics front of the war on poverty, replicating the Vietnam battlefield, battles were being lost. Discrimination in the labor markets was statistically harder to be dealt with than previously expected. Social programs had data, but only on those who had participated in the programs. There were no control groups. Wage and employment data were only about those who worked and not about those who did not. How could economists deal with these samples if they did not describe the entire populations? What conclusions could be drawn from these biased samples? In contemporary econometric language, econometricians had still to discover a way to deal with selection bias and omitted variables.

It was in this context that in 1971 two Princeton young scholars joined the statistical battle against sample bias in labor economics. Encouraged by their Professor, Albert Rees, Ashenfelter and Heckman engaged in the evaluation of the several social programs implemented during the 1960s.

At that point, both joined the battle knowing that sample selection bias had a widespread solution: randomization. Randomization, however, was also known to be unimplementable for the type of data they were dealing with. Their task in the discrimination battle, as that of other empirical labor economists, became to discover a way to implement randomization to data about social programs and labor markets. What distinguishes their efforts is the fact that they came out of the fight decorated as the main solvers of the sample selection bias. By the end of the 1970s, they had implemented randomization to the labor data in two different manners: the sample selection model (Heckman 1979) and the difference in difference estimator (Ashenfelter 1978).

The question that emerges then is how Ashenfelter and Heckman reached different solutions even though both were colleagues and worked together in the beginning of their carrers? This chapter, while answering this question, intends to highlight some of the historical similarities and dissimilarities between randomistas and structural modelers which may enlighten the contemporary debate.

3.2. Randomization and Selection Bias

Selection-bias, from the point of view of other disciplines, is a specific type of the more general problem of sampling and non-sampling errors. These are errors that occur in data sets constructed through interviews of sampled populations and are common in a wide array of areas. Sampling errors occur when the sample does not represent the aimed population. When this error occurs because the interviewee had an omitted reason to participate or not participate (missings and truncation), the problem is called selection-bias. On the other hand, non-sampling errors are the errors that occur in the interview process such as errors of the interviewer, respondents and the researcher that built the questionnaire. Interviewers may err in asking the questions and in recording the answers, and they may even cheat when completing the interviews. Respondents may be unwilling or incapable of answering. Researchers may censor and truncate the possibility of answers when designing the answer pool.

In contemporary economics, sampling, non-sampling and selection biases all came to be known biases under the umbrella of omitted variables (see: Wooldridge 2002). Their consequence is identical to that of simultaneity. Omitted variables impair the assumption that the error term is random, as we see from:

$$y_i = \alpha_0 + \alpha_1 x_1 + \alpha_2 x_2 + \cdots + \alpha_i x_i + \beta k + \varepsilon_i \tag{3.1}$$

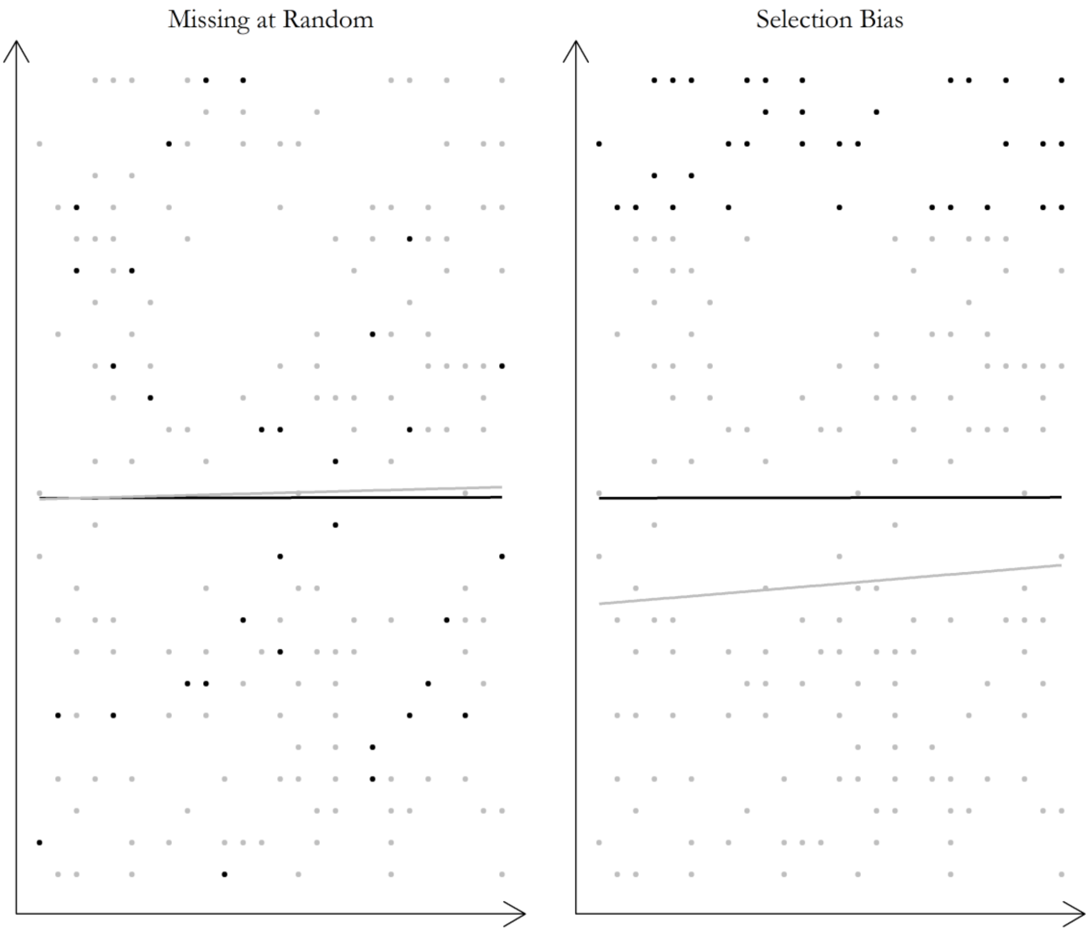
$$y_i = \alpha_0 + \alpha_1 x_1 + \alpha_2 x_2 + \cdots + \alpha_i x_i + u_i \tag{3.2}$$

In this situation, it is possible to see that in the second equation the term βk has been omitted. From the first equation w,e know that $cov(x_i, k) \neq 0$. As a consequence, in the second equation we must have $cov(x_i, u) \neq 0$. Therefore, the error term is correlated with the regressors. It is not possible to state that the regressors cause the explained variable because there is an omitted factor that may be the real cause.

In other disciplines, given the possibility of active engagement of the researcher in the collection of data, sampling and non-sampling error are corrected through random sampling[44]. Missingness at random does not bias the estimators, while any type of selection bis that omits characteristics does. This is observable through the following figures:

[44] See Antonakis et al. (2010) for technical details.

Fig. 3.1 – Missing at random vs. selection-bias



Source: Produced by the author

When data is missing at random, the estimated curves are almost identical. On the other hand, when data is missing for some omitted reason, the curves have drastically different inclinations. The black one estimated with the whole dataset remains horizontal, while the grey one that uses only the grey points has a positive inclination and is dislocated in the second figure. Hence, conclusions are biased. In econometrics, econometricians do not have control over the collection of data and cannot create an experiment that collects random samples. Graphically, this means that econometric data is almost always data of the kind of the graph in the right in Figure 4.1.

This is unfortunate, because random sampling is so common that old and renowned statistics' journals - such as the *Journal of the Statistical Society of London* and *Biometrika* – included mentions to "random sampling" already in their first volumes, in 1838 and 1901 respectively.

As a result, statisticians have been dealing with methods to solve failures in the collection of data for a long time, which could serve as inspiration for microeconometricians.

Beyond theoretical research in statistics, as a practical tool, randomization and experimentation have been applied in diverse fields long time ago. Experimentation and randomization have a long history that can be traced to the 17th century (Jamison 2017), with more relevant works being conducted in the 19th centurty. In this regard, Steven Levitt and John List (2008) point out Pasteur's experiments with sheep in the last quarter of the XIXth century. Ian Hacking (1988) and Judith Favereau (2014), on the other hand, highlight Charles Pierce's role in conducting randomized experimentation in psychology during the same period. While paternity of experimentation and randomization is an unresolved issue, researchers tend to agree that the 1920s marked the outburst of these methodologies (Favereu 2014; Levitt and List 2008; Diaz, Jimenez-Buedo and Teira 2010). Two major actors arise here: Ronald Fisher and Philip Wright. The former was responsible for establishing the role of random assignment (Fisher 1926, 1935); the latter played an important role in the definition of instrumental variables (Wright 1928, Stock and Trebbi 2003).

The usefulness of randomization, thus, allowed it to be implemented in distinct areas such as agriculture, education, and health. Randomization was early adopted in agriculture to test different cropping methods. Ronald Fisher himself was known as a biostatistician and worked on Rothamsted Experimental Station – an agricultural research institute – from 1919 to 1933. During this same period, Jerzy Neyman (1923[1990]), wrote in polish the article "On the Application of Probability Theory to Agricultural Experiments. Essay on Principles". According to the translators Dabrowska and Speed, Neyman "introduces a model for the analysis of field experiments conducted for the purpose of comparing a number of cropping varieties, which makes use of a double-indexed array of unknown potential yields, one index corresponding to varieties and the other to plots" (Neyman 1923[1990], p.1)

The same early movement can be seen in the health sciences. From Pasteur's vaccination experiments in the 19th century (see Latour 1982), randomization gradually became a functional tool for researchers in health areas. Claridge et al. (2005) affirm that during the first half of the 20th century randomized controlled trials became a reality in medical research.

In education, Ann Oakley (1998) highlights that in 1901, Edward Thorndike and Robert Woodworth (1901) were realizing an experiment to test increases in mental ability, an intercept of psychology and education research. This intercept - educational psychology - has a long history of experimentation. Levitt and List (2008) and Oakley (1998) add the case of

William McCall (1923) as another relevant experiment in the area during the first quarter of the 20th century. As a result, in the 1940s, technical discussions about experimental designs and statistics in educational and psychological research were already a common reality. *The Journal of Experimental Education* is an interesting manifestation of this reality, having its first volume released in September of 1932.

Even social scientists had their randomization solution for sampling and non-sampling error. Since the 1930s sociologists already advocated in favor of experimental methods in social sciences. Stuart Chapin (1931) and Ernest Greenwood (1945) are the most cited cases. However, Harold Gosnell's (1927) experiment about voter turnout can also be added to the hall of well succeeded social science's experiments. In the 1930s, as demonstrated by Harrington Brearley (1931)[45], at least 13 universities had courses in experimental sociology. Hence, sociology's early entrance in experimentation allowed researchers to be aware of self-selection and the problems related to individual heterogeneity. In this respect, in the late 1940s, Paul Wallin in the *Journal of American Sociology*, preceding the discussion in economics in almost 30 years, stated:

> "Many studies in psychology and sociology, as well as surveys of opinions, attitudes, or consumer preferences, require a sample of volunteers to serve as subjects or informants. This dependence on volunteer subjects is a problem because not all persons whose participation is solicited will consent. Some do so and others do not, the proportions falling in the two groups varying from study to study. Dependence on volunteers, therefore, has as a consequence the *self-selection* of the units comprising the sample. This violates the fundamental principles of sampling that the method used in selecting the sample be such that each person in the universe have an equal chance of being a part of it." (Wallin 1949, p. 539, emphasis added).

Still, from statistics to these applied experiments, they all dealt with imperfections in the collection of data. Hence, they coped with failures in active observation. Self-selection in econometrics is something that biases estimators in the exact same manner as non-random sampling in other disciplines, but the difference is that the problem could not have been controlled by the researcher. It was not the econometrician who made a mistake in the sampling procedure, but some external institution. As Trygve Haavelmo (1944) claimed in the 1940s, econometricians are *passive observers* who borrow data from institutions to conduct their research. They do not have control over their data. Econometricians are the ones accountable for non-experimental data (see Wold 1969, chapter 1).

Until the 1970s no explicit solution to self-selection had been found in economics. Solely problems surrounding it were having their solutions discovered. Censoring was solved by James Tobin (1958) in the 1950s. Unobservables in panel data were being tackled since the

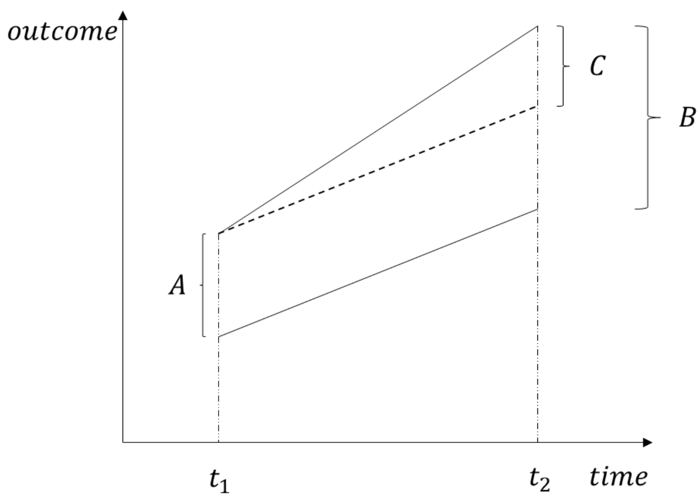
[45] Cited in Oakley (1998)

1960s (Dupont-Kieffer and Pirotte 2009). There were even economists leaving passive observations aside and engaging in the collection of data themselves, such as Heather Ross in the New Jersey Negative Income Experiments and experimental economists. However, there was still something missing for economist to claim victory over passive observations of individuals. There should be a manner of simulating randomization in their borrowed data. We can now see how Ashenfelter and Heckman proposed two different solutions for fulfilling this gap in the 1970s.

3.3. 1970s Princeton Solutions for the Selection Bias

Intuitively, Ashenfelter's difference-in-differences' method was presented in 1978 in "Estimating the Effect of Training Programs on Earnings" "creates" a counterfactual group from observations of treated and non-treated individuals in two different time periods. The idea is to simulate a randomized assignment of treatment and control where this randomization process has in fact not occurred. As Ashenfelter (1978) pointed out when discussing the main problems of program evaluation, there is an "extreme difficulty of implementing an adequate experimental design so as to obtain a group against which to reliably compare trainee." (Ashenfelter 1978, p. 47). Therefore, he proposes a "data collection system" that copes with the problem. Graphically, this means:

Fig. 3.2 – Difference in Differences



Source: produced by the author

What the figure demonstrates is the following definition. Without previous random assignment, it is possible to collect passive observations of two distinct groups -those who participated in a given program and those who not -, with different starting points. The interest, however, is not on the difference between these two groups, but in the difference of the counterfactual case, which is not apparently accessible. The diff-n-diff estimator allows to disclose this difference through the following equation:

$$Y_i = X_i'\alpha + \tau T_i + \varphi t_i + \beta(T_i t_i) + \varepsilon_i \tag{3.3}$$

The interaction term β of the dummy variables time T_i and treatment t_i captures the effect C, which is the difference between B and A - and, consequently, a difference of two differences that names the methodology. Without this interaction term, only the effects A and B would be accessible, and those do not account for the true effect of the program, given that it is not possible to exclude the presence of omitted variables causing the differences between them. The diff-n-diff estimator, thus, assumes the parallel trend of the two groups in order to simulate randomization.

Therefore, the method is assuming out the presence of omitted variables in the collected passive observations through its design. This designed simulation of randomization is what has recently granted this kind of methodology the name of quasi-experiments. They do not control their data nor explicitly use any form of randomization. However, through its design and assumptions, the methods can exclude omitted variables and allow causal claims.

Noticeably, Ashenfelter's insight draws a lot from the randomization literature. His method simulates randomization and is aimed towards the evaluation of programs. Heckman (1979), on the other hand, was concerned with a different kind of sample-selection, the misrepresentation of the aimed population in census' data. Technically, Heckman's first incursions in sample selection were concerns with missing data and incidental truncation in U.S. labor statistics.

He considered the problems of migrants, union members, and women's wage estimation. The example of women's wage estimation has since then become the canonical example in textbooks. Heckman observed that the wage is only observed for women who worked for wages[46]. Consequently, an omitted selection process of labor force participation biases the ordinary least squares estimators.

Intuitively, Heckman solved the problem with a two-step procedure. In the first step, Heckman proposes the use of an instrument to estimate a binary model of labor force participation, a probit model. In the second stage, a control variable is inserted in the original

[46] A formal definition of the model can be found in Wooldrige (2002) section 17.4.

equation for removing the selection bias from the unobservable decision of participating in the labor market.

Heckman's solution was based on previous econometrics research, noticeably Reuben Gronau's (1974) and Tobin's (1958) papers. The first presented the motivation for researching truncated and missing data as selection-bias. The second worked on the problem of censored data using probit estimation of the latent variable, a procedure that resembles Heckman's to the point of creating confusion even nowadays. Censoring and truncation are two common problems in econometric datasets, but only the second may create a relevant selection-bias. Therefore, Tobin's (1958) research can be said to be a hint into the use of binary models to model latent variables.

In contrast with Ashenfelter's proposal, Heckman's procedure does not simulate randomization, but "models out" the selection bias. The first stage of Heckman's procedure is the estimation of a model of decision-making. With the result of this model in hand, a second model for the observed variable is formulated. In the case of the labor market, for instance, the first model is a model of the decision of participating in the labor market and the second model a model of labor supply and demand equilibrium for determining optimum wage rates. From this point of view, it is interesting to observe that Heckman explicitly states that sample selection bias differs from other omitted variables in the sense that they can be theoretically formulated and distinctly estimated:

"This paper discusses the bias that results from using nonrandomly selected samples to estimate behavioral relationships as an ordinary specification bias that arises because of a missing data problem. *In contrast to the usual analysis of 'omitted variables' or specification error in econometrics, in the analysis of sample selection bias it is sometimes possible to estimate the variables which when omitted from a regression analysis give rise to the specification error*. The estimated values of the omitted variables can be used as regressors so that it is possible to estimate the behavioral functions of interest by simple methods." (Heckman 1979, p. 153, emphasis added)

Thus, intuitively and technically, the two solutions differ on their understanding of the cause of the omitted variable bias. Ashenfelter's does not assume the possibility of knowing the cause of the bias and opts to simulate randomization as a way of excluding all possible omitted variables. Heckman, on the other hand, has an implicit assumption that the cause of the bias is known and can be modeled out.

From this previous exposition it is possible to assert that Ashenfelter advocates in favor of randomization, while Heckman in favor of structural modeling. What this means, according to Qin (2013) and Heckman (2010), is that in Heckman's structural case, the workflow consists in specifying a theoretical model, identifying the causal claim excluding endogeneity bias, estimating the econometric model and finally testing the model, while

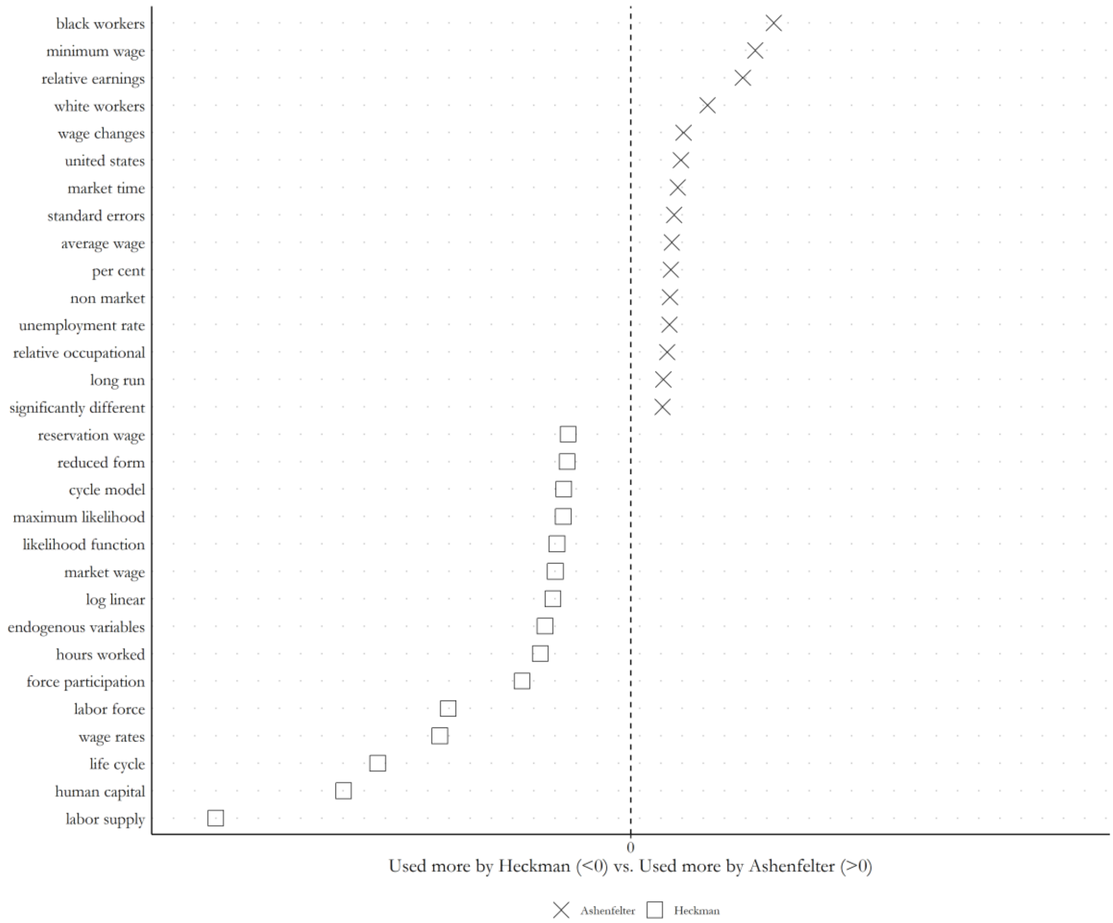
Ashenfelter's methodology may condone to some degree the theoretical aspect. As a result, Ashenfelter's proposal gave rise to a community known as "Randomistas". In their point of view, randomization is the gold standard of the evaluation of programs. There is not the necessity of modeling the problems when it is possible to utilize or simulate randomization.

This dichotomy between randomization and structural modeling has resulted in the heated contemporary debate alluded in the introduction of this chapter and that endorses the misconceived view that randomization and structural models have nothing in common. Usually, the most important point technical taken out of the debate is the timespan of application of the two methodologies. Structural models are said to be "ex-ante" evaluations, while randomistas apply solely "ex-post" evaluations.

Coming back to Ashenfelter and Heckman, their choices of vocabulary suggest this division of "modeling solution" versus "randomization" as well. In 1979 the cumulative research of Ashenfelter and Heckman[47] differed significantly in their most used words. Heckman pended in direction to modeling vocabulary, while Ashenfelter was prone to data terms. The following figure demonstrates the top words for each author in their papers from the beginning of their careers to the point where they proposed their methodologies:

[47] See appendix 3

Fig. 3.3 – Heckman and Ashenfelter main words comparison



Source: Heckman and Ashenfelter's paper list[48]

It is possible to observe that Heckman used the words "model" and "function" in bigger proportion than Ashenfelter. On the other hand, Ashenfelter presents in his research a higher presence of the words "data" and "table". Therefore, the bibliometric evidence endorses the argument presented earlier that the estimators differed in strategies. Heckman's estimator is a model out solution while Ashenfelter present a simulation of randomization. That is the conceptual difference, but how did both solve the same problem with two different versions? What happened in their careers from their first publications together to the end of the 1970s?

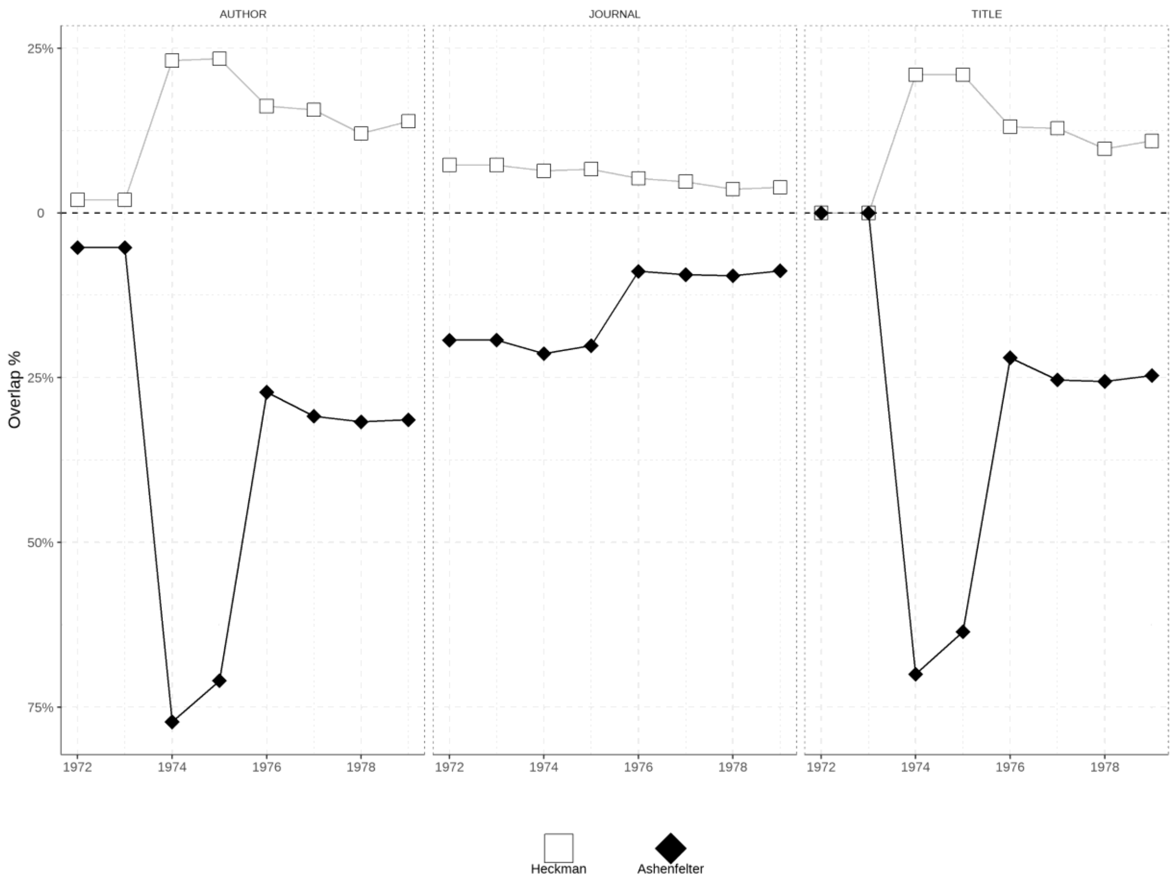
3.4. The same problem, different bibliometric trends

The answer to the previous questions can be traced through their different scientific inspirations and institutional connection throughout the years. For now, with a simple

[48] See appendix 3

intuitive exposition of the methods, it seems that Ashenfelter had influences from randomization and Heckman from econometrics[49]. However, this is a technical and ahistorical point of view. Is this perception confirmed in historical and sociological perspectives? The following figure demonstrates how their research differed over the years until the point they have proposed their methodologies:

Fig. 3.4 – Cumulative research differentiation



Source: Web of Science

The figure demonstrates the comparison of the cumulative related network of Heckman and Ashenfelter (See appendix 4 for more details). It demonstrates the percentage of related works included in each other's network. Simply, if both of them have 5 works in their networks and have 2 works in common, there would be an overlap of 40%. In another case, if one of them has 5 works in the network and the other has 10, being that 2 works are equal

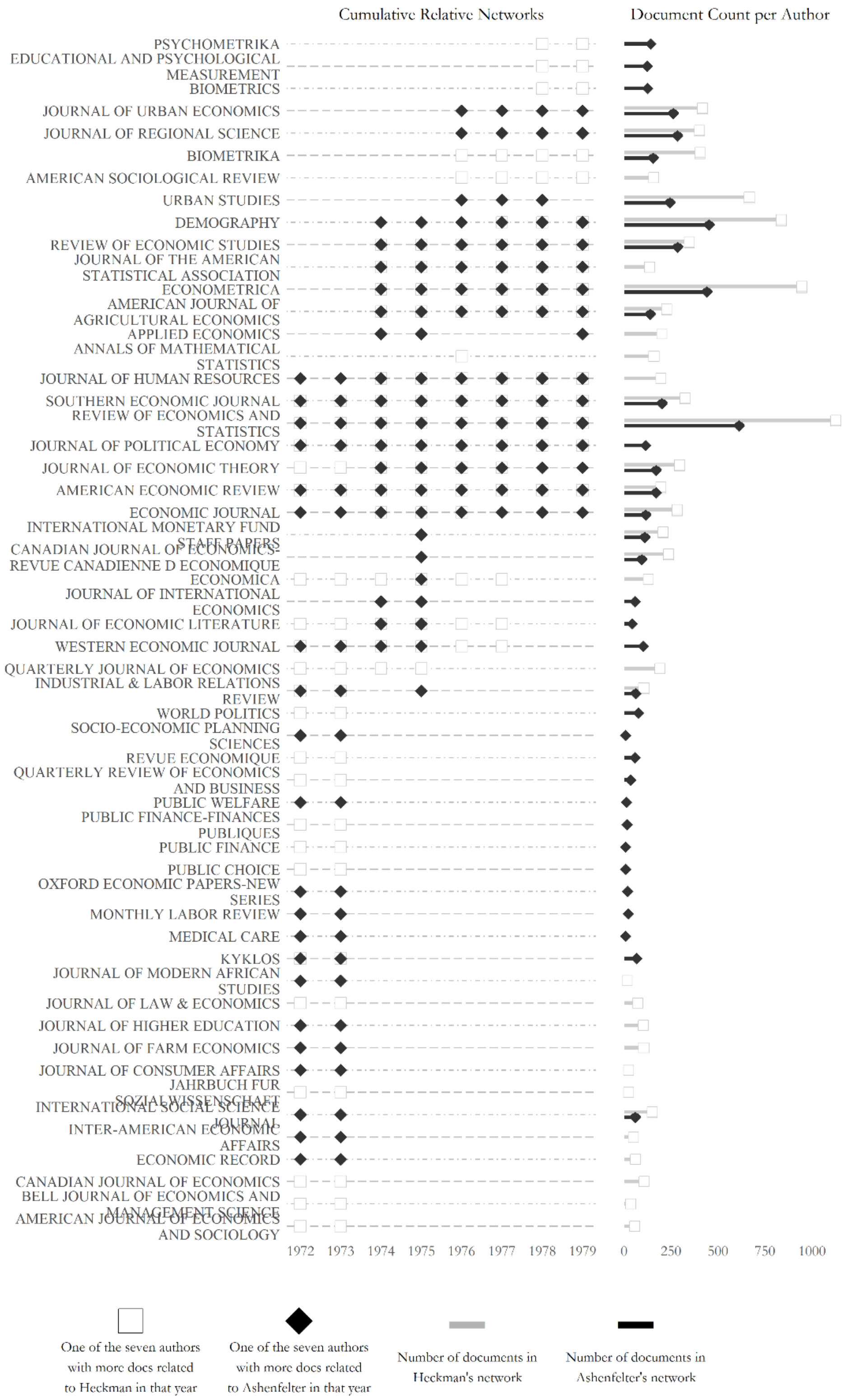
[49] Techcnically and ahistorically the sources of inspiration could be seen in other areas. Heckman (1996) explains the relationship between I.V. and Randomization, for instance.

in the two networks, we would have an overlap of 40% in the first and 20% in the second. The wider the difference in size of the two networks, the more overlap in the smaller network will appear.

Therefore, as it can be seen, in 1972 and 1973, their networks were almost completely different. Then, in the following two years, when they worked together, their researches converged to similar related networks. Ashenfelter's authors were more than 75% covered in Heckman's network. Even the titles covered became similar, they jumped from zero titles in their networks to almost 25% in Heckman's case and 70% in Ashenfelter's case.

More importantly, after the convergence in 73-74, we observe a new period of differentiation. The higher percentage presented in Ashenfelter's case represents that Heckman's related network was bigger in number of documents than Ashenfelter's. This means that Ashenfelter's related network reached only 35% of the same research scope of Heckman's, while Heckman reached 50% of Ashenfelter's research scope. This illustrates that Heckman was divergent, producing papers that escaped from their previous research topics. A more specific picture can be seen in the following figures:

Fig. 3.5 – comparison of Journals



Source: Web of Science

In this figure, it is possible to observe that the journals covered by the related networks were considerably different in the two first years. The convergence is clear in the second period, from 1973 to 1974. Even more interesting, is the differentiation that occurs from 1976 onwards, Heckman's main Journals started to include *Psychometrica*, *Biometrics*, and *Biometrika*. The first is a Journal concerned with the measurement and mathematization of human behavior. The second with the application of statistics to biology with dedicated papers on theoretical statistics. The third was mainly concerned with theoretical statistics.

Heckman had 197 titles of these three Journals in his network, while Ashenfelter had only 9 titles in his related network. Hence, theoretical statistics is far more relevant to Heckman's research than to Ashenfelter's. However, Heckman was not alone in doing works similar to those present in these Journals. Arthur Goldberger (1971), in the beginning of the 1970s, had already published a "survey of communalities" between econometrics and psychometrics in *Psychometrika*. Still, even though not alone, the bibliometric evidence suggests that Heckman research was similar to two important domains, the modeling of human behavior and theoretical statistics.

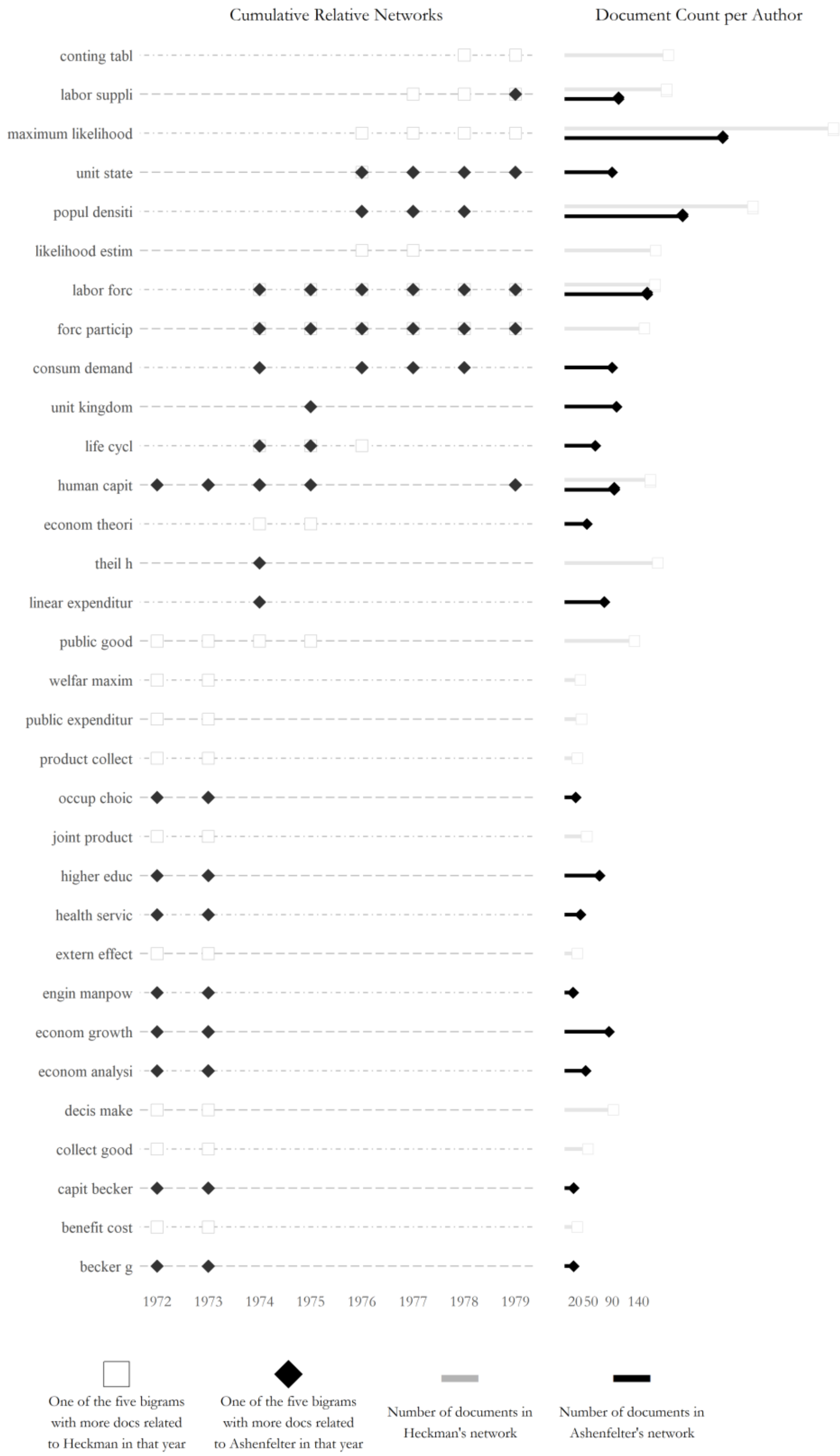
A common thread in these Journals during the 1970s was the use of likelihood methods and binary models, especially the logit model. In this respect, table 1 of appendix 5 demonstrates that Heckman's research resembled several papers of the psychometric community about categorical and incomplete data written between 1970 and 1976. From this point of view, Heckman's research had since the begginings of the 1970s, a relevant source of inspiration for the "model out" solution using the probit estimation that did not come from econometrics.

In contrast, Ashenfelter had a related network connected with political science and agriculture, as seen in Figure 4.5 by the prevalence of *American Political Science Review* and *The American Journal of Agricultural Economics* in his network starting in 1976. Theoretical statistics was not absent, but the bibliometric evidence points out that his research was more closely related to practical fields. Ashenfelter had 191 articles in his related network written in journals about either political science or agricultural economics, while Heckman has 91.

In table 2 of appendix 5, it is possible to notice that Ashenfelter's research had strong similarities with empirical estimation of the equilibrium of numerous different markets, such as food, health facilities and even shrimps. Moreover, different from Heckman, Ashenfelter's research resembled works from the 1950s and 1960s, even though most similarities were found with works form the 1970s as well.

From a technical analysis of the diff-and-diff methodology, it seemed that Ashenfelter's works were part of a research stream using randomization in agriculture and sociology. However, the bibliometric evidence suggests that his research, firstly, had more contact with the econometrics on those fields, with formulation of theoretical models and estimation of equilibrium. The following figure demonstrates that in the bigrams of the titles of the related networks, "economtr model" and "econom theori" were among the most found cases in Ashenfelter's network during the whole period:

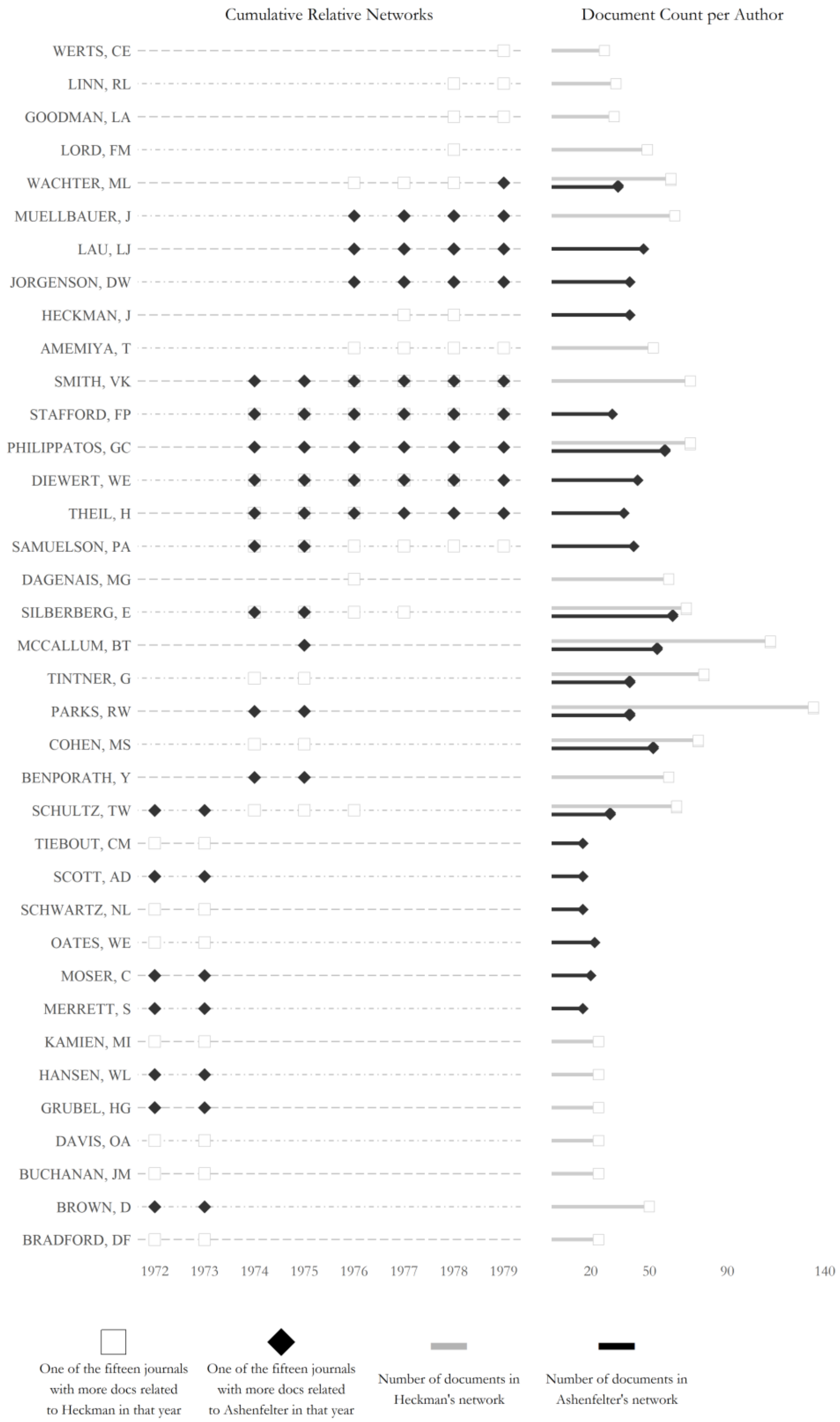
Fig. 3.6 – Comparison of Bigrams in the Titles of the Related Networks



Source: Web of Science

As we can see in Figure 4.6, Ashenfelter's research, although based on practical fields such as agriculture and political science, was still economic in essence, dealing with themes such as the phillips curve and human capital. Heckman, as expected, has its related network connected with labor (as in "labor market" and "forc partic") and strongly connected with topics in theoretical statistics and econometrics, especially likelihood methods. The same differentiation appears in the case of the authors belonging to their networks as we see in the next figure:

Fig. 3.7 – Authors Comparison



Source: Web of Science

In the case of authors, the differentiation of Heckman's network towards statistics is confirmed, with authors from statistics and mathematical statistics featuring in his network: Amemyia, Goodman, Lord and Dagenais (see also table 3 in appendix 5). In this regard, Lord and Goodman are two interesting appearances in Heckman's network. The first was famous for presenting the "Lord's paradox" that defined the possibility of finding two different solutions with the same dataset depending on the initial adjustment of the data (Lord 1967). Heckman's method, it must be remembered, is exactly that: a correction of the dataset. Therefore, since 1976 Heckman's research resembled discussions in statistics.

The second, as observed in table 3 of appendix 5, worked with different kinds of data such as surveys and dichotomous variables. In the same vein, Heckman's related network contains works of Amemyia and Dagenais about different types of data that were written since the beginning of the 1970s. Taking this into consideration, the evidence suggests that Heckman had also in econometrics inspiration for dealing with unusual types of data.

In contrast, bibliometric evidence on the authors of the related network of Ashenfelter suggests that his works resembled papers of Powell, Barten, Parks and Wachter (see table 4 in appendix 5). In this regard, Ashenfelter's work were similar to the research on the estimation of demand and supply once again. Moreover, his research resembled papers on theoretical microeconomic problems, such as the Slutsky conditions. From this point of view, Ashenfelter was embedded in a research that was far from being atheoretical.

This brief bibliometric exrcise does not confirms the first impressions acquired through the technical discussion of Heckman's and Ashenfelter's estimators. Ashenfelter had influences from randomization, but there is no evidence that he had them from theoretical discussions. His network is closely related to sociology and political science and from there is the most probable source of inspiration for the diff and diff estimator. Heckman, on the other side, was far from limited by econometrics discussions as one might think. His research network went further into technical statistical discussions.

3.5. Changing Contexts: 1968-1978

Both technical and bibliometric evidence indicate that their ideas gradually changed over time. This may be the case due to of one editorial detail: print time. While in the end of the 1960s Ashenfelter and Heckman were already writing their papers together, they would come to print only in 1974. The same is true for their estimators, both had already developed their ideas in 1974 but would publish their works in 1978 and 1979. This demonstrates that trusting on bibliometrics and technical exegesis might be erroneous in this case.

While bibliometrics is an interesting tool to understand *what* happened, it does not indicate *how* things have happened. The fact is that in the previous analysis differentiation occurred gradually, given that both authors had the same PhD. formation and they even published together until 1974. But this change is more complex when the historical facts come to the narrative.

Heckman and Ashenfelter were quite different individuals who, yet, briefly enjoyed each other's company while at Princeton. Their exchanges had deep impact in their future publications (Ashenfelter 2020 - Appendix 7, 8 and 9). Still, their personal contact was brief and happened before the bibliometric data suggests, since Heckman stayed in Princeton only from 1968 to 1971. After that, he left for Columbia and Chicago, while Ashenfelter stayed at Princeton, with a brief sojourn at the ASPER in 1972 (as seen in chapters 2).

3.5.1. Princeton years: Albert Rees and Late-night talks

In 1968, Chicago was already renowned. Economics aspired to be viewed as a science. Milton Freidman and George Stigler were the epitomes of the economist qua scientist. And the young PhD. student, James Heckman, opted to leave Chicago for Princeton. Put like that, it seems that this young scholar had made a mistake. However, from his personal point of view it was the right move to make.

Chicago's atmosphere was hard for an outsider. Heckman had a strong mathematical background, far better than his economics skills (Heckman Oral History, Heckman 2014a). That in a place where students were many and attention for basic or even controversial questions was scarce, made him want to leave. Protests and lack of mathematical propensity in the department may have had their roles as well (ibidem).

Be as it may, the fact is that Heckman choose Princeton as his new home. Princeton Industrial Relations Section (IRS) was small and familiar, contrasting with Chicago. The department was renewing itself. Institutionalists were leaving their place for a young generation of empirical professors. In 1966 Harry Kelejian had come to the department after his PhD at Chicago under the supervision of Marc Nerlove and Arnold Zellner. In 1968, Stanley Black (supervised by James Tobin) and Albert Rees (supervised by H. Gregg Lewis) were joining the department from Chicago and Yale respectively. Alan Blinder and Daniel

Hamermesh would join the department in the beginning of the 1970s after their PhDs. There, Heckman found not only a young faculty interested in empirical labor, but also a host of noteworthy students. Heckman had, in the near cohorts, colleagues such as Orley Ashenfelter, Ronald Oaxaca and John Pencavel who were then studying the new field of labor economics. It was a younger and exciting place to be a PhD. student.

In this environment, not only Heckman was thrilled, but all members of the new community. Something new was being built. Frederick Harbinson, who had been the director of the Industrial Relations Section for 15 years left the post for the newcomer Albert Rees. Rees had arrived in Princeton in 1966, along with Heckman, and brought with him enthusiasm for building an empirical community. Microdata was emerging rapidly and had not yet been used appropriately. Rees helped transforming the department in the home of the application of this microdata. In this regard, Rees kept renewing Princeton's faculty towards empiricism, being the responsible for enlisting Alan Blinder and Daniel Hamermesh, for instance.

However, his real impact in the renewing of the department was through supervision. Ress has had important supervising impacts in the research of Oaxaca, Pencavel, Ashenfelter and Heckman, since he was part of the PhD. committee of all of them. In the words of his students:

"He [Rees] was a very conscientious and courteous adviser of many students. Teaching was important to him and, in the 1970s, he authored the major textbook at that time in Labor Economics (1973). His gracious manner made him a popular teacher and colleague." (Ashenfelter and Pencavel 2010, p. 311)

"Rees knew his price theory, but he rooted his research in data and encouraged students to take data very seriously. His vision of economics, like that of many Chicago economists of his generation, was that it is only of value if it is practically useful. He was an honest, open, friendly person who encouraged curiosity and imposed high standards on those around him." (Heckman 2014, p. 309)

Both Heckman and Ashenfelter knew Rees well. In the case of Heckman, Rees impact stems from the fact that, when Heckman enrolled in Rees' labor economics class, he was the sole student to do so. Rees "wanted to cancel the class", but Heckman "pleaded with him that [he] wanted to learn labor. So [they] agreed to have a tutorial." (Heckman Oral History, p. 558). That created a bond between them. Heckman (2014, p. 309-310) remembers that Rees was important for his change from Chicago to Princeton, for presenting neoclassical labor economics in his third year of the PhD. program and for introducing him to the new array of microdata emerging in the 1960s and 1970s. Heckman even participated in the New Jersey Income Maintenance Experiment captained by Rees to help "enroll some of the first participants in the experiment".

In the case of Ashenfelter, the relationship was obvious and direct, given that Rees had been his PhD. supervisor. Their exchange during Ashenfelter's PhD. years even resulted in a co-organized conference about discrimination in the labor market in 1971 followed by its correspondent joint book a few years into the 1970s (Ashenfelter and Rees 1973).

As follows, both have had intense contact with Rees' empirical mindset. Rees knowledge of statistics and economic theory made him soon aware of the puzzles behind microdata and the collection of data. In Rees and Jacobs (1961) he was already aware of the biases caused by different selections of items to include in price indexes. Few years later, in 1966 he was commenting on the new data collected by the Department of Labor Statistics claiming for information on 'omitted variables":

"[a] problem exists in the unemployment statistics we have used for twenty-five years. How many of the unemployed cannot find work because the wage they are asking is unreasonably high relative to their abilities? No one knows, yet I cannot recall this issue ever being seriously raised in connection with the collection of unemployment statistics." (Rees 1966, p.462).

This concern comes from the "empirical" Chicago school where Rees had had his formation. H. Gregg Lewis supervised Albert Reees in his years at Chicago. Lewis, in contrast to Friedman and Stigler who were famous theoreticians – or at least better known for this virtue -, was an empiricist concerned with careful collection and analysis of data (see Rees 1976). His most well-known works are two exhaustive reviews of the literature on unions, dissecting statistical methodologies over all the works he had found on the issue.

As a result, in the 1970s, Rees was simply transmitting the empirical-labor DNA ahead. All his students had to credibly collect data and overcome its biases. Not by accident, Heckman, Ashenfelter and Oaxaca presented techniques for dealing with omitted variables. In this regard, until 1970, Ashenfelter and Heckman were on the same track: dealing with data-collection biases and applying labor economic theory to it. They discussed the problem in the hallways, with professors and even between them. As Heckman remembers:

"I was happy to be able to combine rigorous economic theory with microdata. […] I shared my enthusiasm for this general research program with Orley Ashenfelter, who was two years ahead of me in the graduate program and who took a job at Princeton in my final year of residence there. Ashenfelter's enthusiasm for economics, data and life was infectious. We would discuss topics into the night. We shared a common vision about applying economic theory to empirical work in labor economics. We wrote several papers estimating Slutsky income and substitution effects in labor supply. He also acquainted me with Becker's Economics of Discrimination and shared with me his research ideas on explaining the time series of racial wage and employment patterns."

Their connection went beyond night chats. During their Princeton years, therefore, they worked together on empirical labor problems with strict connection to Rees interests. In

Ashenfelter and Heckman (1971, 1972, 1974) they reviewed labor economic models motivated by the empirical research on the negative income experiments, in which Rees had had an important role (see Chapter 2). In Ashenfelter and Heckman (1973) they worked together on the measurement of the effects of antidiscrimination programs.

It is interesting to notice that in their work together they were already aware that data could hide information. Together in 1973, they highlighted that there are effects that "can be measured and effects that cannot". As a consequence, they engaged together in the formulation of a framework "to measure and interpret program effects". While neither the difference in difference nor the two-step procedure has been advanced in the paper, it is clear that the intention to overcome hidden information was among their common interests.

3.5.2. Columbia and OEO: theory and practice

Noticeably, up to 1971, both authors were in the same track. Things started to change during that year. In 1970 Heckman was finishing his PhD. and received an offer from Columbia. Given his interests in labor economic theory, Columbia seemed the perfect place to go. Gary Becker and Jacob Mincer should be there at the time he received the offer. Becker was coming back to Columbia after going to Chicago. However, as Heckman recalls:

> "I went there hoping to work with Gary Becker; as of March 1970 when I had to decide where to go, he was going back to Columbia, but he changed his mind! He left me high and dry. But with Jacob Mincer and a flock of his first rate, highly motivated, and intellectually engaged students. So even though I did not then work with him directly, I saw his legacy and the devotion of his scholars and students first hand." (Heckman 2014b, p. 22)

Still, his option for Columbia demonstrates that Heckman was eager to follow an academic path on economic theory. He wanted to learn from the best in the rising applied fields of labor economics, "new home economics" and economics of education (see Grossbard 2006): Mincer and Becker. Columbia had a famous labor workshop led by the two renowned scholars, where young scholars went nervously present their fledgling researches. Heckman attended the workshop without missing, even though Becker was not there anymore.

During his first year in New York, Heckman also received a proposal to participate in the activities of the NBER. He gladly accepted the offer, since the NBER was a growing institution with a blooming array of young scholars. It was also a break from the labor workshops. Heckman recalls having informal but intense discussion that went into the night when at the NBER (Heckman 2014). At the institution, he also met Finis Welch and Robert

Willis, with whom he had the opportunity of discussing his research. With Willis, Heckman wrote two papers during the 1970s.

Although always conscious of microdata problems, Heckman went to Columbia to develop his theoretical skills. According to himself, it was by accident that he became an econometrician:

> "At the end of my first year at Columbia, the chair of the department, Kelvin Lancaster, approached me to teach graduate econometrics because the resident econometrician was leaving and no one else in the department knew anything about econometrics. I took this as an occasion to teach myself Henri Theil's then-new Principles of Econometrics. His was a breakthrough book that introduced basic asymptotic theory into econometrics. Teaching this course enabled me to break into graduate teaching." (Heckman 2014)

This coincidence put Heckman on the road to his two-step procedure. In studying econometrics, Heckman reviewed the problems that he had already encountered: "In my thesis I faced the recurring problem of having missing wages for about half the women whose labor supply I was seeking to determine. Moreover the missing wages were associated with women not working (zero hours of work)." (Heckman 2014). After some research on the topic, Heckman decided to overcome the problem he had rediscovered. In 1972 he worked on a specific case of the problem: nonworking women (see Heckman 1974). This was the first rough form of his "model out" solution. At the time, he was concerned with the lack of theoretical reasons behind the methods being implemented to measure wage differential in with U.S. labor data. For him, theoretically, all women whether they worked or not should be accounted in labor market analysis.

As it can be observed, it was a theoretician looking through the lenses of econometrics to solve a problem yet without solution. He included, thus, a first theoretical step in the estimation of women wages that regarded their decision to participate or not in the labor market. With positive feedback, Heckman noticed that his framework could be generalized and already in 1972-73 he worked on the generalization of his methodology:

> "working at Columbia, I began to develop a general framework for organizing discrete, continuous, and joint discrete-continuous variables in a common framework. I realized that my method for using economic theory to produce counterfactual missing wages was more generally applicable. In early 1973, I wrote a paper called "Dummy Endogenous Variables" that spelled out a general framework for modeling interrelated discrete and continuous choice models." (Heckman 2014)

Heckman's unpublished version of his 2-step procedure circulated widely (Heckman 2014a, p. 317). However, it took him around seven years to see it published. Hence, as soon as 1973, Heckman had already developed his theoretical methodology for dealing with missing information. He frequently acknowledges the relevance of the interaction between the NBER and Columbia's Labor Workshop for developing his method. The interaction was unique and has never repeated itself after NBER moved to Cambridge in 1973 (see Grossbard 2006).

Ashenfelter, on the other hand, followed a different path, but also had an early version of his methodology. He was already part of Princeton's faculty since 1968, although he has finished his PhD. only in 1970. Given his connection to Albert Rees, who supervised him and was enthusiastic about empirical labor economics, Ashenfelter became the director of Princeton's Industrial Relations Section in 1971. He was not only acquainted with the peculiarities of Princeton, but he was also someone who could maintain the same empirical track that had become Princeton's gene in the 1960s.

Ashenfelter renew the department as he saw PhD. students and professors leave. Hamermesh went to Michigan state and Kelejian to New York University. Oaxaca and Pencavel, in the same manner as Heckman, left right after their PhDs to Western Ontario and Stanford respectively. As result, Ashenfelter brought to the department Farrel Bloch and Sharon P. Smith. However, similarly to Albert Rees, Ashenfelter's most lasting contribution to the department was not on his substitutions, but in a different area. Ashenfelter, in 1972, had the opportunity to create a network with the U.S. Department of Labor, as we shall see in more details in chapter 4.

During that year, Ashenfelter was on a leave as the director for working at ASPER. In his words: "in early 1972 I was offered a civil service position in the U.S. Department of Labor in which I was to direct an Office of Evaluation whose sole purpose was to ask and answer this [did training programs work?] and some related questions" (Ashenfelter 2014, p. 2).

As we have seen, ASPER had been created during Johnson's war on Poverty with the sole purpose of evaluating the overwhelming amount of data accumulated by its programs – mainly programs related to the Manpower Development and Training Act (MDTA) such as Head Start and Job Corps. According to Ashenfelter (2014), the office was a place where there was no political interference, and the sole purpose was to find a transparent manner of evaluating programs. There, randomization entered in Ashenfelter's research agenda.

Ashenfelter joined civil service for a year, where, beyond economists, he met several other researchers – especially from political science, education and sociology. All of them had the common notion that the active collection of data and randomization was the way to go. As Ashenfelter points out from his experience, he discovered during that year that randomization is an interesting tool to “whisper in the ears of kings”. The following quotation already seen in chapter 2 describes that: [50]

“a key reason why this procedure [difference-in-differences as a simulation of randomization] was so attractive to a bureaucrat in Washington, D.C., was that it was a transparent method that did not require elaborate explanation and was therefore an extremely credible way to report the results of what, in fact, was a complicated and difficult study” (Ashenfelter 2014)

This was the case because it does not make sense neither for researchers nor for policy makers to compare participants of training programs with comparison groups that are not counterfactual. There is no “credibility”. In the words of Ashenfelter, comparing with a non-counterfactual group made him aware of the necessity of simulating randomization with longitudinal data:

“The key thing learned from this comparison was that the program participants had lower earnings, both before and after the program, than the comparison group. This automatically made it clear that the analysis would not meet the highest standards for credibility. This also suggested that the participants should be compared with themselves instead of with the comparison group alone, and with longitudinal data that is precisely what was possible.” (Ashenfelter 2014)

As a consequence, Ashenfelter developed his difference-in-differences estimator already in 1973-74, circulated it in the U.S. labor department and used it to inform policymakers. He, as well, took some time to see his paper published as it was rejected sometimes before being printed (Ashenfelter 2020 - Appendix 7, 8 and 9). The now widely cited estimator was only published in 1978.

3.5.3. Establishing difference: Chicago and Princeton

According to the bibliometric evidence, 1974 is an important year for the authors. Both had already concluded most part of the research related to their estimators and were continuing their research agendas. In the case of Heckman, this meant leaving New York. In 1973, when the NBER was going to Cambridge, Heckman received a proposal from Becker to assume a position in the university of Chicago, which he accepted.

At Chicago, Heckman got tenured and received the task of teaching Gregg Lewis’ class on unionism already in 1974, only one year after his arrival. On the research side, Heckman expanded his research agenda quickly. He soon got his 1974 paper published and started to

[50] Ashenfelter’s experience was not innovative, and it has even reached the academy, especially in sociology. See for example: Latour (1982) and Druckman et al. (2006)

work on application of the methodology for different areas. At the same time, he formulated the general framework that would be published in 1979. From 1974 to 1979 Heckman published more than twenty papers – most of them as a single author. In Heckman's words:

"Chicago in the 1970s was an ideal environment for me to conduct research. One-on-one interactions and seminars improved my thinking on any project I undertook. I had developed an agenda in my first three years at Columbia, and I expanded on it and generalized it in my early years at Chicago." (Heckman 2014, p. 322)

The next table demonstrates the amount of JSTOR's mentions for "heckman" by subperiods, showing Heckman's reach in sociology, mathematics and statistics.

Table 3.1 – Mentions to Heckman by subperiods

Discipline	1969-1974	1974-1979
Economics	22	309
Sociology	7	58
Statistics	1	30
Mathematics	4	59

Source: JSTOR

As it can be observed, Heckman's work received immediate attention for its technical modeling in theoretical areas such as mathematics, statistics and mathematical sociology. In this regard, Heckman (2014) remembers meeting Burt Singer and James Coleman in the 1970s, with whom he worked together. After his initial contact with mathematical sociology, Heckman developed a lifelong interest by the subject, participating frequently in mathematical sociology workshops and writing with Coleman and Singer as a further development of his research agenda.

Heckman tried, in the 1980s, to apply his framework to problems where normality could not be assumed. The difficulties he faced led him to develop a nonparametric formulation of his method.

Columbia, hence, was the place where the idea was born, but Chicago was where Heckman disseminated his model and created a network outside economics. Columbia, on the one hand, offered him the opportunity of living intensely the field of labor economics and new home economics within the Columbia-NBER cluster. Chicago, on the other, offered him the opportunity of developing his research freely and even interacting with renowned scholars from other areas. Both were essential in the development of the two-step procedure.

However, it is interesting to notice that was in Chicago that Heckman established and generalized his framework as a "modeling out" procedure for the identification of causal relationships. In this sense, Chicago's context may have played an special role in this definition. As Heckman remembers: "The entire department [Chicago's] seemed to embrace Friedman's methodology of positive economics. […] No theory was worthwhile unless it survived a reality check.". In this context, Heckman was developing a research essential for the department. He was allowing theoretical questions that were before unanswerable to face a reality check for the first time. Friedman's positive methodology could then be applied in labor economics through Heckman.

Ashenfelter, on the other hand, finished his sojourn at the Labor Department and came back to his position as the director of Princeton's Industrial Relations Section in 1973. Starting that year, he centralized Princeton as a place of empirical labor economics. The section's research seminars were renewed after his comeback. Whereas during Albert Rees directorship most seminars were held by Princeton Scholars, from 1973 onward, research seminars started to count with several renowned scholars from outside Princeton, as is going to be discussed in chapter 4. Presenters came from several places, but specially from University of California LA and the University of Chicago.

Table 3.2 – Seminar presentation by institutions

Institution	**Presentations (1973-1979)**
University of California, LA	6
University of Chicago	5
MIT	4
Harvard University	4
Cornell University	4
University of Wisconsin	4

Source: Princeton's Industrial Relations Section Reports

Ashenfelter managed to centralize the discussion of labor economics and evaluation of Programs in the university of Princeton. Beyond renewing the seminars, Ashenfelter organized two conferences on the evalution of Programs in 1974 and 1976 respectively, counting with the presence of numerous scholars and researchers form the U.S. labor department. More on the conferences will be discussed in the next chapter, for now it is important to notice that these efforts of creating bonds with governmental organs were relevant for two main reasons: first, access to data; second, learning how to translate academic research to practical purposes.

Amidst his responsibilities as Director of the section, Ashenfelter managed to position himself as an important researcher as well. From 1974 forward, he specialized himself in the evaluation of programs, becoming one of the first economists of the area. This meant embracing randomization and creating a "credible" language for communicating academic discovers to policymakers.

From 1974 to 1978, Ashenfelter published six papers in the working papers series of the Industrial Relations Section that would become important published articles. Among the papers, in 1974 and 1976, Ashenfelter published his works on the evaluation of the Manpower Development and Training Act programs. Also, in 76 and 77 he published evaluation of other programs such as employment tax credit and the income maintenance experiments. In all works, simple and understandable economic analysis were what Ashenfelter tried to offer. In his 1977 evaluation of the employment tax credit, for instance, his primary concern was to translate the confuse pages of the employment tax act to a simpler economic language: "To do this I first set out in simplified form the accounting details of how ETC is designed to operate in terms of the change in the wage rate it may be expected to induce." (Ashenfelter 1977, p. 1)

3.6. Concluding Remarks

Historical facts confirm what has been demonstrated by the bibliometric and technical evidence: Heckman and Ashenfelter differed their researches due to their different point of views, theoretical and applied respectively. However, while technical and bibliometric evidence point to a gradual modification of ideas, historical facts demonstrate that already in the first years after their PhD. they had already developed their estimators.

Nevertheless, although contrasting on their conclusions about timing, all evidence point to the same history. Both Authors were concerned with the same problem during part of their careers: biases in the collection of econometric microdata. This is seen in the bibliometric evidence in their almost identical related networks during the period they published together. This is also seen by the historical fact that both learned about this issue through albert Rees, with whom they had important contact.

In this regard, it is interesting to notice that what is now a clear clash, was historically only a change of context. Both authors had the same concerns, but during the period from 1968 to 1974 they moved to completely different contexts. Heckman went to Columbia where he worked on his theoretical skills and became an econometrician by accident. Ashenfelter stayed at Princeton and briefly worked at the Labor Department where he had contact with the credibility of randomization. Quickly, in their new contexts, they proposed their solutions that took five to seven years to come to be published.

Ashenfelter and Heckman established their research agendas from 1974 to 1979. Heckman engaged in the task of conciliating heterogeneity in economic behavior with econometrics and Ashenfelter engaged in the consolidation of the field of evaluation of economic programs as is going to be seen in the next chapter.

4. AN ACADEMIC HARBOR FOR GOVERNMENTAL AFFAIRS: HOW PRINCETON INDUSTRIAL RELATIONS SECTION LEGITIMIZED PROGRAM EVALUATION IN ECONOMICS?

4.1. The Offer

In contrast with the contemporary look of Princeton's department of economics where the Industrial Relations Section (IRS) is now placed, from the 1960s to the 2000s labor economics was still finding its place and IRS settled in a much more modest site. By the end of "rows and rows of books" (Card 2020) at Princeton's Firestone Library basement, there was a door. Unadvised students who ended up behind that door, saw a counter on the right where the administrative assistants sat, and a bulletin board filled with notes on the left (Altonji 2020). A small set of unevenly distributed offices surrounded an elevated table at the center (ibidem). The smell of recently made coffee and the sound of typing machines permeated the air. Maybe a few young scholars would be discussing sketched calculations by the table, the center of a companionship that helped in their breakthroughs in the following years.

This was the 1970s Industrial Relations Section of the library, where research on industrial relations should be conducted. Time, however, was gradually transforming the subject matter of the section into the newly rising field of labor economics. The old institutionalist approaches of the Industrial Relations were being substituted by the "analytical labor economics" of the 1960s and 1970s, as we discussed in the previous chapters (see als Rees 1976 and Kaufman 1993, 2004, 2006)[51]. The name was the only thing that lasted.

In the largest office, with bookshelves stuffed with books and journals (Card 2020), Orley Ashenfelter - the section director - could be found. In the late 1970s, a decade after his PhD. and still in his early 1930s, Ashenfelter was the only tenured economics professor on the section's staff (Robinson 2016, p. 139). Frederick Harbison, senior faculty nearing his retirement, had lost interest in labor markets and had moved to the Woodrow Wilson School. Albert Rees, Ashenfelter's advisor and section director, had gone to follow his career first in the government and then as provost of Princeton university. He would hardly show up at the section anymore. Harry Kelejian and Daniel Hamermesh left Princeton for different universities.

[51] For an account about the demise od instutional economics and industrial relations see: Kaufman (1993, 2004, 2006)

Ashenfelter had a particular background. Albert Rees, his advisor, had been advised by H. Gregg Lewis, who saw his advisor as "the father of analytical economics" (Rees 1976) and one of the few economists trained in analytical methods in labor economics. As Rees stated: "the overwhelming majority of analytical labor economists [by 1976] have been his [Lewis] colleagues, his students, or the students of his students - his intellectual brothers, children, and grandchildren." (Rees 1976)

In the late 1970s, following Rees's views, Ashenfelter was one of a kind. He was almost alone in the list of tenured analytical labor economists in a context where this expertise was getting gradually more demanded. Economics was in the middle of a transformation. Post-war economics was gradually and constantly becoming more applied (Backhouse and Cherrier 2016, Panhans and Singleton 2016, Claveau and Gingras 2016). As we saw in the previous chapter, data was accumulating fast and had to be dealt with. Social scientists stepped forward in this process, especially economists (Huret 2010, Forget 2010, Fleury 2010, Rossi and Wright 1984, Oakley 2000). The insertion of IBM mainframes in universities' computer centers and analytical governmental organs since the 1960s made the necessity for analytically trained social scientists even greater. Programming languages such as FORTRAN, ASSEMBLY, COBOL, and the use of punch cards should not be exotic knowledge in economics anymore (Renfro 2011, Backhouse and Cherrier 2016).

Analytical labor economics embodied such change (Levit and List 2008, Angrist and Pischke 2010, Panhans and Singleton 2016). Labor economics until the 1960s was mostly led by institutional economists (Kaufman 1993). In what concerned its applied methodology, statistics and econometrics were unusual and when applied were used to study unions (see Lewis 1963). Thus, Kaufman (1993) affirms that the “case study ‘go and see’ approach pioneered by the early institutionalists” dominated until the 1960s.

From the 1960s forward, however, Macroeconomic datasets of employment and income were were increasingly becoming more reliable and accessible, being used in banks and governmental agencies (Chapter 2, Dupont-Kieffer and Pirotte 2010). Moreover, as discussed in chapter 2, the 1960s saw an uprise in country-wide field experiments on labor topics, the Negative Income Experiments being the largest ones, but more were on their way. In the 1970s, the first panel surveys took shape, such as the Panel Study of Income Dynamics. A late 1960s PhD. student on labor would proudly carry his thesis in a box full of Punch Cards through campus (Hochman 2018, Card 2020). The raw computational power of thousands of pen and paper calculations was now on those tiny holes in the cards. Weeks of calculations became days, maybe hours.

As the government was the source of most social sciences data, going from the computer center of universities to the government was a route often taken by analytical labor economists. Before being IRS's director, Ashenfelter took that route. Albert Rees, his advisor, was a close friend of George Shultz, who had been Secretary of Labor in the early 1970s (chapter 2, Ashenfelter 2020 - Appendix 7, 8 and 9). Shultz indirectly indicated Ashenfelter for the directorship of the Office of the Assistant Secretary for Policy, Evaluation, and Research (ASPER). He accepted the offer, and his choice affected not only his career, but also the course of Princeton's Industrial Relations Section and of economics at large. Daniel Hamermesh assumed his position as IRS director.

Ashenfelter stayed only one year in Washington and then went back to Princeton. From then on, inspired by his time in D.C., Ashenfelter did his job as a researcher at and the director of the IRS. He followed his interests and enlisted students interested in the same problems as himself. The rest of this chapter tells in a chronological manner how Ashenfelter and his students did no more than the ordinary business of their academic lives, publishing in top-tier journals and pushing forward their discoveries, but still were capable of transforming economics. This is a history about how day-to-day activities as flights and choice of hometowns, done by "normal PhDs students"[52] and researchers – who have difficulties and rely on friendship and companionship - can be as impactful as any revolution. It is, thus, a micro history of an economics department and seeks to contribute to understanding how knowledge is produced in the grassroots of the departments.

This paper follows closely the accounts of several actors in the history of the IRS to tell this history. A total of nine interviews were conducted in 2020 with Orley Ashenfelter (APPENDIX 7, 8 and 9), David Card, Joshua Angrist (APPENDIX 6), Michael Ransom, Gary Solon (APPENDIX 10), Joseph Altonji and Daniel Hamermesh (APPENDIX 11) to understand how the IRS dealt with "natural experiments" and program evaluation. Inside Princeton's IRS, as told in the interviews, program evaluation saw no "shift", "revolution", nor "turn" as we shall see in the coming pages.[53]

[52] Normal in the sense of Kuhn (1996), as in researchers who follow a settled paradigm of methods and theories and work to accumulate knowledge in it.
[53] These are terms used in contemporary contributions to the history of economic thought, as discussed in the first chapter See: Panhans and Singleton 2016, Biddle and Hamermesh 2017, Cherrier and Backhouse 2016, Claveau and Gingras's (2016), De Vroey and Pensieroso 2017, Levit and List 2008, Angrist and Pischke 2010.

4.2. The Washington-IRS connection

When Ashenfelter went to Washington in 1972, "Washington wasn't expensive." (Ashenfelter 2020 - Appendix 7, 8 and 9). Richard Nixon's government employees had inherited lifted salary caps from Lyndon Johnson and "real estate prices were nothing like they are now." He and his wife could afford a "beautiful house in Georgetown" for their family. "In the back bedroom where the kids were, they could actually see the Washington Monument." (ibidem) It was as nice as Washington could be.

Ashenfelter would walk to the Labor Department "in 14th and constitution". ASPER was a research and evaluation facility, as the name suggests. As director, Ashenfelter's task was to organize and incentivize researchers to deal with the overwhelming amount of data flowing from government's experiments on the labor market and get economic insight out of it, as we showed in chapter 2.

During his year in Washington DC, Ashenfelter organized ASPER's technical analysis papers (TAPs) and hired top researchers in analytical labor to write them. The facility became central in the development of labor economics as numerous researchers in the field had at least one contract or grant under ASPER or a paper written for the TAP series during the 1970s. Also, it was in DC that Ashenfelter initiated his work on the difference in differences estimator, which culminated in his 1978 publication (Ashenfelter 1978).

The young Ashenfelter's sojourn at Washington was then productive academically and institutionally. On the academy side, the difference in differences estimator simplicity was enticing and Ashenfelter inserted the field of program evaluation definitely into economics with his paper. According to Ashenfelter himself:

"[…] a key reason why this procedure [difference in differences] was so attractive to a bureaucrat in Washington, D.C., was that it was a transparent method that did not require elaborate explanation and was therefore an extremely credible way to report the results of what, in fact, was a complicated and difficult study. From a technical point of view, a difference-in-differences study controls for fixed effects for individuals, and thus heterogeneity across people, and for fixed effects for time periods, and thus variability over time. It was meant, in short, not to be a method, but instead a way to display the results of a complex data analysis in a transparent and credible fashion." (Ashenfelter 2014)

Ashenfelter's convinceability emanated from one important technical-philosophical aspect: randomness. According to Guido Imbens (2014), econometricians have historically focused on choice rather than chance. While Imbens view may be contested when applied to econometrics as a whole (as discussed in Chapter 1), it is interesting to notice that at least from 1950s up to the 1970s randomness has played a secondary role in labor as displayed in the reviews made by Gregg Lewis (1963, 1986). The use of econometric methods to deal with labor economics' problems had been up to that point an approach directed towards modeling agents' behaviors. Thus, the starting point of analysis for labor econometricians was in general the assumption that agents actively influence the outcome, normally by behaving optimally. In contrast, statistics literature focuses on chance and hence assumes that the units of analyses are passive. These units do not actively influence the outcome. They do not choose the level of treatment they receive for instance. Randomness, in this setting, plays a pivotal role.

Ashenfelter's perception was that for the bureaucrats, passive units of observation were easier to be explained because no optimal behavior had to be interpreted (Ashenfelter 2014, 2020). Bureaucrats are not necessarily concerned about how agents behave or why their policies work, but instead on whether their policies work or not. Ashenfelter understood the need for pragmatism in the evaluation of policy and was not afraid of embracing it. If modeling agents was not necessary, so be it.

Putting technical specificities aside, Ashenfelter's sojourn in Washington was also institutionally productive. His directorship initiated a communication channel between government policy evaluation and the academy that was not usual at the time, at least in labor economics. In ASPER, his task was to ensure a constant flow of researchers to the government (Chapter 2). Up to that point, research facilities in government had not had much success beyond the Department of Defense (Berman forthcoming).

The issue is that researchers usually do not want to be bureaucrats in the government according to Ashenfelter (2020). Thus he "used to have a policy that: I ask you to work four days a week, the fifth day you come to the office to do your own research. Take on a problem that you are interested in. That was the way to try to get academics to come and be bureaucrats." (Ashenfelter 2020 - Appendix 7, 8 and 9) ASPER was able to engage a significant number of researchers this way, but according to Ashenfelter at the cost of efficiency. Losing a full day of work was not the best deal.

The problem was easy to identify to Ashenfelter (2020). Incentives were misallocated. Academy has "all the students who need to write dissertations." and has "the faculty who want to publish papers". In the government, however, "It's very difficult for people who are regular bureaucrats [to do the research]." (ibidem).

Although Georgetown and Princeton are not perfectly alike, lifestyle was one of the factors that put Ashenfelter back at the IRS. Still, Ashenfelter left Washington, but Washington's evaluation of programs followed him. After Washington, he "completely changed [his] research agenda to some extent." (Ashenfelter 2020 - Appendix 7, 8 and 9)

Interest in traditional labor economics was kept alive, but he had "changed it more to this idea of public policy… trying to do quantitative work in the public policy area." (ibidem) Until his time at Washington, Ashenfelter studied mostly unions impact and researched labor supply models (chapter 4). The idea that stuck in Ashenfelter's head after Washington was that "the problems come from the government. The data could come from them. The funding can come from the government. And we supply the people from the university because we're the ones that have the motivation to publish the papers." (ibidem)

To connect academy and government, he followed a simple path: conferences. If the problem was about joining researchers and bureaucrats, why not put both in a room with the same objective? Ashenfelter used his contacts in Princeton and Washington to set up a series of conferences about the evaluation of public policies.

While Ashenfelter was still in ASPER, Daniel Hamermesh was the main organizer from Princeton of the first joint conference between the Industrial Relations Section and ASPER in 1973[54]. "Labor in Nonprofit Industry and Government" was held at Princeton University on May 7-8, 1973[55]. A book with papers and comments was published in 1975 (Hamermesh 1975).

Ashenfelter took the directorship from Hamermesh in 1974 and organized two more joint conferences with ASPER and the IRS in 1974 and 1976, which would be the last ones given the sudden end of ASPER as discussed in chapter 2. Wolpin summarized the content of the first conference in his review of Ashenfelter and James Blum edited volume that came out in 1977 (Ashenfelter and Blum 1977):

> "The opening piece by Finis Welch contains his most recent effort on the employment effects of minimum wages and is followed with one of the first studies of the Federal Contract Compliance Program by Orley Ashenfelter and James Heckman. It continues with a predominantly theoretic essay on public employment programs by George Johnson and a statistical evaluation of the Black Lung Benefits Program by Morris Goldstein and Robert Smith. In the final piece a model of job sorting with imperfect information by Dale Mortenson is used to explore the issue of societal gains to public as opposed to purely private employment services." (Wolpin 1978)

Beyond the authors of the papers, discussants included Robert Flanagan, T Aldrich Finegan, Gregg Lewis, and John Pencavel. For Wolpin: "In sum, Evaluating the Labor-Market Effects of Social Programs is a fine collection of essays comprising the best examples of which I am aware of policy-specific labor-market research." (Wolpin 1978)

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[54] Hamermesh not only replaced Ashenfelter as IRS's director but also assumed his position in Washington in 1974 when he left ASPER. They basically changed positions two times in a row.

[55] The participants were Melvin Reder, Alvin Klevorick, Orley Ashenfelter, Ronald Ehrenberg, Burton Weisbrod, Richard Freeman, George Johnson, John Burton Jr, Charles Krider, Jack Stieber, Donald Frey, Hirschel Kasper, Daniel Hamermesh, Paul Gerhart

Farrel Bloch helped in the organization of the 1976 conference: "Evaluating manpower training programs". This conference brought names from government and academy together. Ashenfelter, Stafford, Stromsdorfer and Hamermesh had been directors of ASPER and all of them collaborated in the conference. In the conference, important papers such as the second version of Ashenfelter's difference and difference estimator were discussed. The academy and the government had had a successful partnership in the ASPER-IRS conferences.

During this period, the influx of PhD. students was high at the IRS. At least two Princeton students from each cohort from 1973 to 1982 opted for the IRS, and, in 1983, nine students went to be advised by IRS professors (Robinson 2016). The new labor economics being offered at Princeton attracted them. Princeton had this different set of policy evaluation where modeling agents was not mandatory. Choice and chance should be chosen according to the problem in hand. This was Ashenfelter's lesson learned in Washington.

Most students knew that choosing Princeton's IRS meant being directly or indirectly advised by Ashenfelter. As the sole tenured professor at the section, Ashenfelter was responsible for teaching and advising any students interested in labor. He was responsible for the Labor course at the time while visiting Professors would come and go to help him in the section. In the 1970s, the list of visiting Professors included: Stephen J. Nickell, Charles Mulvey, Robert C. Vowels, Card Metcalf, H. Gregg Lewis, Ransom L. Wachter, Walter Y. Oi, Yoram Weiss, Barry R. Chiswick, Burton A. Weisbrod (Robinson 2016). Those were names from around the world, coming from the UK, Israel and the US.

ASPER's activities have ended in the late 1970s being sent to other departments inside the government (Chapter 2). At this point, however, the interaction had already transformed the IRS. The industrial relations section was now a different breed of labor economics within the department. The problems in the conferences were "the kind of problems that you wouldn't come up with if you're a regular economist sitting in your office." (Ashenfelter 2020 - Appendix 7, 8 and 9) Ashenfelter was now concerned with dealing with government-produced data and was fully aware of the problems that then current econometric methods had when interpreted by governmental bureaucrats. Under Ashenfelter, the IRS would be an institution for training economists in the econometric methods that best suited the necessities of government: *credible* methods[56].

[56] This is an ex-post rationalization inserted by the actors in their accounts of this history. Angrist and Pischke (2010) use the concept of a "credibility revolution" to retell the history of natural experiments. Angrist, Ashenfelter and Card have used the term during their interviews used in this chapter. I opted to include the

Randomization and credibility became important inside IRS. Since the early 1970s, Ashenfelter advocated that randomization was the only transparent and credible method for dealing with bureaucrats. His "plea for randomized trials, appeared in a paper presented at the Industrial Relations Research Association Meetings in 1974." (Ashenfelter 2020 - Appendix 7, 8 and 9, Ashenfelter 1974) From then on, IRS would live an econometric tradition of credibility mostly through randomization. According to Gary Solon, Ashnfelter's advisees from Princeton's 1977 cohort would combine labor economics and econometrics:

"By the time I was there, there was already a very well-established tradition that Ashenfelter students whose first field was empirical labor economics were quite regularly treating econometrics as their second field and that was a strong tradition." (Solon 2020 - Appendix 10)

4.3. The IRS Tradition: Econometrics and computers

Michael Ransom embarked on this same tradition. He was not from the same cohort as that of Solon. He was one year ahead, starting his PhD. in 1978. During his undergraduate studies, Ransom had worked under the auspice of James McDonald at Brigham Young University. His work was on the statistics and econometrics of fitting income distributions. Labor, then, "was a natural choice" (Ransom 2020), and he was a natural candidate for the econometrics-heavy labor that Ashenfelter was developing.

In Princeton, Ransom attended the labor course taught by Ashenfelter and ended up working for him afterwards. Soon, he received a carrel in the library right outside the IRS as Ashenfelter became his formal PhD. advisor. While his carrel was not spacious, it prevented Ransom from having to deal with the "stunk of Ashenfelter's cigar" (Card 2020) - Ashenfelter usually swan in the morning and then would come back to the section to have a Cubano (ibidem). Ransom was not inside the section but could jump in easily to grab some coffee or listen to the discussions at the table.

Together with the classes of Angus Deaton (microeconomics), Richard Quandt (optimization), and Gregory Chow (econometrics), Ashenfelter's labor class popular at Princeton. "A pretty good chunk of [Ransom's] group of fellow students were taking the course." (Ransom 2020) David Card, Ransom's close friend, certainly was. Card was one of the stars of the class, according to Ransom. "It was pretty clear that he knew what was going on. If you had questions, you went to him." (ibidem) Card also went on to be advised by Ashenfelter. Thus by the third year of their PhDs, "Dave" and "Mike" – how themselves

idea here, although I advise the reader to be careful in the interpretation of the concept and the extent that the idea was present when the events broke out in the 1980s and 1990s.

refer to each other – got an upgrade from their carrels to shared offices inside the section. Senior and younger students in labor economics would be there. By the time the duo arrived there, Joseph Altonji, Mark Plant and Laurie Bassi also had offices at the section.

For those many students working on computational problems, it was usual to spend a lot of time at the computer center. As seen, in the late 1960s, Princeton had installed mainframe computers and made them available for the university community. Although Apple presented personal computers to the world in 1977, it took some time for them to spread out. Personal computers would be a reality only in the second half of the 1980s and even so on a small scale (Card 2020, Angrist 2020 - Appendix 6). In the late 1970s and early 1980s, IBM 360s and 370s were the reality, and economics graduate students would spend a lot of time with their punch cards (Hochman 2018).

At Princeton, economics graduate students took advantage of a strong computational support. Richard Quandt and Steve Goldfeldt, Princeton professors at the time, "had this big nonlinear optimization package that you could do maximum likelihood estimation." (Card 2020) The name was GQOPT, G for Goldfeldt, Q for Quandt and OPT for optimization. Quandt's current description of GQOPT is the following:

> "GQOPT is a general purpose numerical computation package, particularly adapted to the needs of economists, econometricians, and operation researchers. Its original core consists of numerical optimization, but over the years many other modules and computational methods have been added, ranging from utility routines to numerical integration, nonparametric statistics, time series analysis, etc. It was originally designed to work on IBM mainframes. Some quite early versions were also available for Prime, Burroughs and Control Data computers."[57]

While theory and optimization may have represented a significant part of labor economics, Ashenfelter's analytical labor was becoming famous for its applied side. In the 1980s, "there was no Internet connection or anything like that. Right? So if you wanted to do computer programming, you had to go down there [the computer center]." (Card 2020)

Thus, going down there meant that you could solve optimization problems a lot faster and access otherwise unavailable data. Judith Rowe, the associate director for social science users of the computer center, was an early advocate of machine-readable governmental data, starting to publish papers on the subject already in the second half of the 1970s. As she states (Rowe 1978), governmental data was, until the 1970s, produced mainly for internal analysts and thus, even though available through the "Freedom of Information Act", were hardly accessible for outsiders. Moreover, printed sheets of tabulated or non-

[57] http://www.quandt.com/handbook720/frames1.htm [accessed 09/07/2021]

tabulated were inefficient for everyone else involved in using them: computer center workers, librarians, students, researchers and so forth.

"…neither the files themselves nor the accompanying documentation, a sine qua non for analyzing the data, are ever properly edited. For example, the data may contain "wild" or unexplained punches; the documentation or codebook which explains the meaning of each code and the location of each data item within the data records may be hand-written, incomplete or poorly duplicated. While none of this precludes the use of the data within the agency, it severely impacts analysis by an outside user" (Rowe 1978, p. 196)

Together with social science researchers, Judith Rowe made strong efforts to push forward the use of machine-readable data. They would identify valuable datasets and make their access easier for future needs. This meant the whole process, from acquisition to cleaning the data. Having a computer center doing this work was a privilege not always acknowledged, but that contributed to make economics at Princeton a center for applied research. This made the social science computer center a place of exchange and learning:

"if everybody is there working on the computer, you will learn a lot from those other people because you just have to sit and wait for the jobs to run. And you have a lot of time. I was one of the first people to learn SAS in economics because there was a grad student in sociology that I talked to and I was doing stuff on some stupid program. And he said: 'well, I'm using this SAS thing.' This was really cool. And I basically switched over." (Card 2020)

Even though Princeton was a top-tier university with resources such as the computer center, a lot of intangible pressure was involved in earning a PhD. from the IRS. Ashenfelter was a busy advisor: aside from being director of the section, he was a renowned researcher in labor, he published periodically in high-tier journals, and had his undergraduate and graduate teacher appointments. Ashenfelter would not "work through three drafts of your paper" (Altonji 2020). "He wasn't somebody who was going to go in and fix every problem in your paper." (ibidem) He was the type of advisor who was good with ideas and keeping students' enthusiasm high.

To become a PhD. from Princeton IRS, and especially when being advised by Ashenfelter, required that you conduct a high-level innovative research on your own. Ashenfelter treated his students as colleagues. This was so true that presenting a thesis paper on a labor seminar meant receiving harsh criticism as any other senior labor economist would, as Gary Solon remembered in his interview. For him, presenting at the weekly seminar was his best way to receive feedback on his thesis:

"[Ashenfelter] did the same thing he did if Jacob Mincer came and gave a seminar: he's just gone at it. You know, 'what's wrong with this study?' and 'How could we improve this?' […] If he had criticism of our work, he just let it flow. I value that. The hard part was getting him to read or hear, just getting the opportunities to get the criticism. But there were ways to do it and giving seminars was a very good way to do it." (Solon 2020 - Appendix 10)

Leading researchers would come to present their papers at Princeton's Dickinson Hall where the department of economics was located:

"every week we were all gathered together more often with an outside speaker than an internal one (but we had both) and we had this very serious and intense 90-minute conversation about whatever research was being presented. Ashenfelter, of course, was very vocal. He was kind of the leader of the audience in that. But by listening to his reactions to what was being presented, we were kind of learning at his feet about his way of thinking about research. And by that time, he was very interested in: "should we actually believe the answers this person is offering". So there was always a focus on, actually, the research design and whether the conclusion being offered was really what we ought to believe or were there alternative interpretations, were better ways to get more convincing evidence. So that was a terrific way to be learning our craft: in that workshop which was meeting pretty much every week." (Solon 2020 - Appendix 10)

Interesting times were the 1980s for the PhD. students at the industrial relations section. They would have the opportunity to have classes with important applied researchers, use a supportive computer center and weekly listen to intense academic discussions. This was everything a "grad student" could ask for at that time, but there is no denial that there was a toil. Core theory classes were known to be hard – as they usually are in graduate programs -, as were the main econometric courses, and uncertainty weigh heavy on newcomers. Ransom, for instance, recalls having to go frequently to his friend "Dave" Card to get help with Hugo Sonnenschein's theory classes (Ransom 2020).

Companionship, then, was essential to cope with the hardships of a PhD. The simple day-to-day activities, although common in any PhD., also played an important role in uncovering the problems of program evaluation, according to the interviewed researchers: "The industrial section was full of people and not random people but people that you could talk to and exchange ideas and discuss maybe the seminar that you saw yesterday or something that was going on in your classes or your work." (Angrist 2020 - Appendix 6) The elevated table, at the center of IRS basement room, was the heart of the friendship. If someone was there, it meant anyone could go there and have a chat. Joshua Angrist remembers fondly that calculations would be sketched on any paper available and maybe someone would come out of their office to help. If Orley – how all interviewees call him - happened to be in an inspired day, he would go by the table and "hold forth [...]. Everyone would come out of their offices to hear him doing his thing. And sometimes it was just a comedy routine, basically." (Solon 2020 - Appendix 10) "It was a critical mass of people at similar career stages who were in it together and were all committed to doing empirical labor economics in the Ashenfelter fashion in which we were relying heavily on modern econometric methods." (ibidem)

The administrative assistants, Irene Rowe and Dorothy Sylvester, were important for the students as well (Altonji 2020). They were helpful to the extent they could be. They did the hard work of typing papers, but also cared for the students, mending holes in sweaters for instance. One interesting part of their activities was not letting Ashenfelter forget his compromises with the students:

"Every now and then, there'd be this episode where Orley was not getting his recommendation letters out for those of us who were on the job market and the secretaries would go on strike on our behalf [laughs]. If Orley wasn't getting his letters out, they basically just refused to do any other work for him." (Solon 2020 - Appendix 10)

From computer centers to companionship, as told by the interviewees, all these factors contributed to lead IRS to important publications and even breakthroughs. Its students went to top-tier departments after their PhDs, or as the time was standard, even before defending their thesis. Cornell, Columbia, Harvard, MIT, Berkeley, Yale, and Chicago are some of the universities which hired IRS alumni from the 1970s and 1980s (Robinson 2016).

Both IRS topics and quality of research transformed their working papers in published research in major outlets for the economist profession, especially in the early 1980s. They mixed econometrics and labor, but it also presented the original scent of experiments and policy evaluation. Randomness and model agents divided the attention of IRS students. From 1975 to 1985 the share of IRS working papers being published in *American Economic Review*, *Quarterly Journal of Economics*, *Journal of Political Economy* and *Review of Economics and Statistics* went from near 10% to 25%.

To make IRS success greater and more visible, Ashenfelter became the leading editor of the *American Economic Review* (AER) in 1985 and was invited to co-edit with Richard Layard the *Handbook of Labor Economics*, the book that set the tone of everything that had been done in labor economics up to that point.

"it really appealed to me because what we really needed was a textbook. We needed a graduate textbook and no one was going to really write it. I wasn't going to write a graduate textbook. No one was going to write one. We didn't have one. So the original idea was: "let's try to put together what would serve as a textbook". And we went through and got all these topics and tried to cover all the things where there was research that we could really teach about." (Ashenfelter 2020 - Appendix 7, 8 and 9)

In the twenty-two chapters presented in Volumes 1 and 2 of the *Handbook* (Ashenfelter and Layard 1987a, 1987b), five were written by researchers that had a direct connection either to ASPER or Princeton IRS: Daniel Hamermesh, Frank Stafford, James Heckman, John Pencavel, and Henry Farber. Chapter 20 "Union relative Wage Effects" was written by H. Gregg Lewis, who was the precursor of Ashenfelter's analytical labor lineage according to Rees (1976).

Those were accomplishments that not only promoted Ashenfelter but the section as well. The question then was: how to keep moving forward? Ashenfelter had brought randomness and governmental problems together to an economics department. But, he was accumulating other time-consuming responsibilities and advancing policy evaluation on different fronts.

4.4. Time to Expand

A lot of politics is involved in expanding a department or a section of a department in a university. It is not trivial to convince your peers that your section deserves more than theirs. In the 1970s, IRS policy was that of bringing people from government or outside institutions to be visiting scholars and present seminars, but in the 1980s, the needs of the IRS were more specific. What was being taught to IRS PhD students was not only traditional labor economics but policy evaluation as well. IRS had now to bring people to be permanent contributors to the section that could help in this specific topic.

Labor economics had advanced a lot by the 1980s, beyond Princeton several institutions were now building their teams of labor economics. Columbia had always been important through Jacob Mincer (Teixeira 2014). Chicago had Gary Becker and the newcomer James Heckman (Emmet 2010). Wisconsin founded the Institute for Research on Poverty in the 1970s and had by the 1980s formed several PhDs specialized in poverty and policy evaluation (Evanson 1986). Michigan had helped in the creation of the Panel Study of Income Dynamics and trained PhDs in econometric methods to deal with panel data and policy evaluation (McGonagle et al 2013).

But still, it was not exactly easy to find qualified candidates to fit exactly the needs of the IRS. Chicago trained students towards a labor economics focused on modeling agents, Columbia had a focus on education and Home Economics (Grossbard 2006), for instance. Thus, consciously or not, the policy of the section became an endogenous one. Visiting professors of the section started to be former students of the section. During the 1980s, Ronald Oaxaca, Joseph Altonji, John Ham, Clifford Reid, Michael Ransom and Mark Plant had appointments as visting professors at the IRS – all of them IRS alumni. This did not happen in the 1970s (Robinson 2016).

An essential part of their visiting appointments was just being there at basement. Following the increased interest in analytical labor, the section was enlarged during the 1980s and a few more offices were available. Learning from everyone at the section was part of the training as interviewees remember.

The simple fact of studying at the section meant, therefore, having an opportunity to learn something from experienced researchers in policy evaluation. To be clear, they were not only experienced but researchers who had followed closely Ashenfelter's course on Labor and thus had experience on Ashenfelter's methods. They were some of the few fellows following the Gregg-Lewis - Albert Ress - Ashenfelter lineage.

Endogeneity, however, is not well accepted in most economics departments, and this was no different in Princeton. Visiting professors could come from inside, but this was not true for assistant or associate professors. The cohorts that started their PhDs in the late 1970s early 1980s and were finishing their PhDs in the first half of the decade had to deal with the fact that Princeton was not an option.

David Card, with an impressive curriculum, was looking for a job in 1982. He received two interesting proposals, Stanford and Chicago Business School, different from his colleagues who struggled to receive offers due to the US recession of the early 1980s (Card 2020). He chose the second, although it came with a stuffed workload of teaching, more than four classes.

His appointment however did not last long. In 1983, Ashenfelter received an exciting offer from MIT (Card 2020, Ashenfelter 2020 - Appendix 7, 8 and 9). Its economics department was starting to specialize in labor and policy evaluation at that time, and Ashenfelter was a bet to start well.

However, amidst the formation of the new team at MIT, IRS was also expanding its faculty. David Card had been one of the best of his class and had been advised by Ashenfelter himself. He had published his thesis very well and was known to be both well versed in theory and econometrics.

The situation "was crazy" according to Ashenfelter (2020). People realized that Princeton should have hired Card a long time ago. Ashenfelter approached Card with an offer, that reduced his teaching workload and managed put him back at the IRS. Card's wife worked as an assistant professor at Columbia, which was closer to Princeton than Chicago.

Card's decision to go back to his alma matter in 1983 was not the most complex one (Card 2020).

Ashenfelter, in the end, decided to stay at Princeton. MIT was not going to have him; instead they hired Henry Farber who was also an IRS PhD.. In the aftermath of the negotiations, Princeton had one more economics professor at the section beyond Ashenfelter and a very dedicated one. Card's wife was in New York and would be teaching until late at night most days; this allowed Card to stay at the section until midnight most days of the week (Card 2020). Card as a professor was a great addition to the table time. "Like, after eight o'clock or so, we used to sit and kind of chat after dinner and have a cup of coffee before everybody went back to work. […] We used to kind of sit around and talk about what was going on." (Card 2020) Card was pretty much always there at night (Card 2020).

Ashenfelter's workload at the section was reduced during the 1980s. Besides Card's arrival, Harvey Rosen achieved the status of professor of economics at the section (Robinson 2016). From 1979 to 1983, Ashenfelter supervised alone eight PhD. thesis in labor economics including Card's and taught by himself the whole labor course. After Card's arrival, in the second half of the 1980s, Ashenfelter did not supervise by himself anymore. Card would typically be his co-advisor or the other way around, and the labor course started to be divided by the two of them.

The joint supervision was facilitated because Card and Ashenfelter were complimentary advisors (Card 2020). Ashenfelter was the supervisor that helped with ideas, broad discussions and keeping the enthusiasm. He was the one who pushed the credibility effort forward. Card, on the other hand, was the detail supervisor. He was the one that would go through drafts of the paper and sketches of calculations with the advisees (ibidem).

Moreover, Ashenfelter and Card had their research agendas aligned, as Ashenfelter used to have with all his advisees. Joint publication between Ashenfelter and his students were common in the 1980s[58]. With Card, Ashenfelter shared the interest in training programs and advancing econometric methodologies to evaluate them. Since Ashenfelter's 1978 difference in difference paper, the comparison between experiments and econometric methods became an even more common concern among IRS members. Gary Solon, in 1982, wrote with Ashenfelter about the limitation of longitudinal data, for instance (Solon and Ashenfelter 1982). Card, in 1985, published with Ashenfelter an acclaimed paper that marked for them how assumption-laden were their econometric methods (Card and Ashenfelter 1985).

[58] In Ashenfelter's 26 publications from 1980 to 1989, twelve were written with students of the IRS.

These publications mark a transition in the mindset of the section. Experiments had always been supported at the section as a mean for credibility in the analyses, but until the 1980s no paper from the members of the section had explicitly contested the econometric results of traditional labor economics'. Modeling agents and randomness had never competed as opposed methodologies in their papers.

Ashenfelter and Card took a step forward in questioning the credibility of the techniques they had been using for years. Basically, they were frustrated with the tools they had in hand. In the 1985 paper this issue appeared as follows:

"As we shall see, this method is no substitute for a properly designed experimental test of the effectiveness of training, but it does provide some evidence on the empirical consistency of the estimated program effects. In the absence of experimental data, there seems to be no alternative to the adoption of this or similar methods of program evaluation, since we find that small differences in model specification can lead to remarkable differences in the estimated impact of training. Hopefully, the accuracy of these methods may eventually be the subject of experimental testing" (Card and Ashenfelter 1985, p. 649)

Card was even clearer when talking about the paper in his interview:

"That was very frustrating because you know, we basically found out that depending on a couple of assumptions you made about how the timing of selection, you could get quite different answers on the size of the training program effect. And there didn't seem to be an easy way to choose between the two choices." (Card 2020)

However, their frustration was also motivation for advancing econometrics. What could econometricianss learn from experiments? How could they apply experimentation to passive observations? How experiments and econometric methods compared to each other? Were they substitutes or not?

Robert LaLonde, who was the first joint supervision by Ashenfelter and Card (1981-1985) knew what his advisors were frustrated with. Ashenfelter, then, suggested that he followed up on the problem (Ashenfelter 2020 - Appendix 7, 8 and 9). And he did.

LaLonde wrote a chapter of his thesis with the sole objective of answering roughly the following question: can we reproduce the results of experiments with econometric methods? He used an existing experiment and compared the estimates with econometric methods. In his words: "Econometricians intend their empirical studies to reproduce the results of experiments that use random assignment without incurring their costs. One way, then, to evaluate econometric methods is to compare them against experimentally determined results." (LaLonde 1986, p.604)

The procedure of comparing experimental and econometric results came to be known by close friends as 'lalondizing' (Ashenfelter 2020 - Appendix 7, 8 and 9). The paper resulting from the thesis was not exactly surprising, although innovative (Card 2020). The comparison, which is common nowadays, had never been applied and the results were valuable[59]. He concluded that policymakers should be aware of the biases that came with the application of econometric methods: "Even though I was unable to evaluate all nonexperimental methods, this evidence suggests that policymakers should be aware that the available nonexperimental evaluations of employment and training programs may contain large and unknown biases resulting from specification errors." (LaLonde 1986, p. 617).

LaLonde presented a new methodology for the generation to come[60]. Lalondizing became a credibility check. The task for future IRS PhDs' was to uncover the differences in the econometrics of experiments and traditional labor economics. But before that, IRS had still to expand more.

During the period that LaLonde made his thesis, Princeton Economics department dismissed a new wave of professors that would not be tenured. The department was famous for having a high turnover (Ashenfelter 2020 - Appendix 7, 8 and 9). IRS, in this situation, had one vacancy available. Princeton started analyzing curricula at the end of 1986 and organized the Job Market talks for happening after the holidays of that year.

Meanwhile, after Christmas of 1986, New Orleans was the destination of the economists who attended the ASSA meetings. They started on the 28th of December of that year at the New Orleans Marriott. Ashenfelter would have a busy weekend, participating in five sessions. In some he would be the president and in other he would be presenting papers. David Card also joined the meeting as President of one section.

In the job market that year, Krueger flew to New Orleans for the conference after visiting his parents in New Jersey for Christmas. He did not know about the Princeton vacancy and was not on the list of candidates. He also did not have any formal appointment at the meeting, no paper to present, or session to preside. He probably did not imagine, though, that the meeting would start already in the flight to New Orleans.

Krueger scheduled the same flight as Ashenfelter and his family. The flight was full, and Ashenfelter did not manage to get three seats for him, his wife, Ginna, and his daughter, Tracey (Ashenfelter 2020 - Appendix 7, 8 and 9). He sat alone while Ginna and Tracey sat side by side. Krueger ended up sitting next to Ginna. Ashenfelter recalled the following moments:

"And so my wife started talking to him. She basically interviewed him. She says "What are you doing?". He explains what he's doing. And then she says: "You know my husband, Orley Ashenfelter?". "Yes, I never met

[59] It later became more common, starting to be repeated by Franker and Maynard (1987) for instance.
[60] His idea, in fact, keeps being replicated until nowadays. See: Gordon, Moakler, Zettelmeyer (2021) for instance.

him" - he replied. And she says: "well, are you being interviewed by Princeton?" And he said: "No, I'm not". "And Would you like to be?" "Oh, yes, I would". Now, my wife comes over me and she says: "we need to change seats. I met this young man and he is just not getting interviewed. And I answered: "you know what? He's probably no good. That's why we're not interviewing him." But it was my wife, so look what I did. I got to change seats and I started talking to him and I thought: "well, this is crazy. I'm here interviewing the guy". So then I just called up the interviewing group and I said: "we should talk to this guy. I don't know why he's not on the list and we should stick him into the list and interview him. And the next thing i know is we hired him." (Ashenfelter 2020 - Appendix 7, 8 and 9)

Krueger had impressive credentials. He was a PhD. candidate from Harvard studying under Lawrence Summers. Summers was a contemporary of Ashenfelter who was highly esteemed in the professions. In 1987, Summers was the first Economist to win the Alan Waterman award awarded by the National Science Foundation to recognize "outstanding young researchers in any field of science or engineering":

"For outstanding contributions to economic research on unemployment, taxation of capital, savings behavior and macroeconomic activity. His work combines powerful analytic insights and imaginative econometric methods aimed at subjects of fundamental National importance."[61]

Econometrics was Krueger's main quality, then. He accepted the opportunity offered in the plane and later that year he presented "Agency and Ownership" in his job market talk at Princeton, a paper that would eventually come out in the *Quarterly Journal of Economics*' (Krueger 1991). He was an excellent candidate and overcame people from the very own IRS, like Joshua Angrist (Angrist 2020 - Appendix 6). Card, Kruger, Ashenfelter became the central trio supporting IRS PhDs. They were the ones who would always be at the firestone basement in the next few years.

A stronger IRS, at least in the sense of having more available researchers and publishing impactful research, was a reality in the second half of the 1980s. In addition to LaLonde's successful research (which ended up published in AER in 1986), Card and Ashenfelter were also publishing well their research. Together, they wrote "Using the Longitudinal Structure of Earnings to Estimate the Effect of Training Programs" for the *Review of Economic and* Statistics in 1985 (Card and Ashenfelter 1985). Alone, Card published "Efficient Contracts with Costly Adjustment: Short Run Employment Determination for Airline Mechanics" in the *American Economic Review* and "An Empirical Model of Wage Indexation Provisions in Union Contracts" in the *Journal of Political Economy* in 1985 and 1986 (Card, 1985, 1986). Ashenfelter, on the other hand, in the 1986-1987 biennium published seven papers, two in the *American Economic Review,* one in the *Quarterly Journal of Economics,* and

[61] Official Alan Waterman Award website:
https://www.nsf.gov/od/waterman/waterman_recipients.jsp#1987 [accessed 09/07/2021]

one in the *Journal of Political Economy* (Ashenfelter 1987a, 1987b, Ashenfelter and Hannan 1986, Ashenfelter and Card 1986, Ashenfelter and Sullivan 1987, Ashenfelter and Oaxaca 1986, Brown and Ashenfelter 1987). In short, Princeton's evaluation of programs and labor economics deserved not only attention but also to be further developed and disseminated from the point of view of academic stamps.

4.5. Technical standardization

Along with Card, Krueger changed the department's supervision scenario. Ashenfelter had been successful in engaging both of them in the randomization mentality. As a result, Ashenfelter reinforced the formula that had worked with Robert LaLonde and delegated supervision responsibilities to Card and Krueger. After Lalonde, the pair Ashenfelter/Card supervised Janet Currie (1988 PhD), Joshua Angrist (1989 PhD.), Thomas Lemieux (1989 PhD) and John-Steffen Pischke (1992 PhD.). Card also solo supervised Brian McCall (1988 PhD.), John Budd (1991 PhD.) and Christopher Schmidt (1991 PhD.). Krueger advised in this period Anamaria Lusardi's PhD (1992) by himself and Phillip Levine (1990 PhD) jointly with Card. Ashenfelter did not solo supervise in the second half of the 1980s.

David Card (2020) called this cohort the "golden generation". At the time, Princeton was a top choice for economics PhDs, if not in all fields indeed in labor economics. Other departments "had not built up their labor groups yet" (Card 2020). Thus, the top students interested in labor had a great chance of ending up at the firestone basement.

Times were different as well. Professors had more influence in choosing students than they do nowadays. Joshua Angrist, for instance, met Orley Ashenfelter in 1982. Ashenfelter was the outside examiner for his undergraduate thesis at Oberlin College. He wrote "Sample Selection Bias and the nature of unemployment" (Angrist 1982) and made a good impression on Ashenfelter, who sometime after the defense wrote him a letter inviting him to join Princeton's graduate program (Angrist 2020 - Appendix 6).

Angrist did not accept the offer and instead went to Israel where he joined the master's program at Hebrew University and later became part of the Israeli military forces for two years. He met his wife, Mira, and lots of things changed. He decided it was time to go back to the US in 1985. Then, he wrote Ashenfelter a letter saying: "remember me? You said I could come to Princeton. Can I still do that?" (Angrist 2020 - Appendix 6). Ashenfelter, in fact, did not forget and manage to get Angrist a position as a PhD student at Princeton.

Angrist fit the section perfectly. He had been interested in the econometrics of policy evaluation since his undergraduate studies. As the section moved to more econometric-oriented questions, so did the students, and Angrist represented that move. In the 1970s and early 1980s policy evaluation was about applying econometric tools to training programs and panel data. In the 1980s, Angrist's cohort decade (1985-1989), it was time to determine what exactly was the econometrics of policy evaluation.

A detour on the literature of the history of econometrics is interesting to understand how this fits the wider context of econometrics. In the early 1980s, econometrics as a tool for *causal inference* was being questioned. Even though econometricians had already started to cope with non-aggregated data in the 1960s and 1970s (Dupont-Kieffer and Pirotte 2010, Nerlove 2002, Chapter 2) and self-selection had already been tackled in econometrics (chapter 4), 1970s and 1980s econometrics was worn out inside and outside academy as a tool for causal identification.

According to Qin (1993, 2013), after a brief period of agreement going from the 1950s to the 1970s, econometrics started to demonstrate controversy signals. Frisch (1970) bluntly stated that the econometrics he helped create had become "playometrics". Econometrics had become highly abstract, and its results were frequently contestable. The relationship between the models and the world was a blurred. Leontief (1971) criticized econometrics, affirming that most of its models were "relegated to the stockpile without any practical application or after only a perfunctory demonstration exercise" (Leontief 1971, p. 3). In the American Economic Review, Juster (1970) claimed for a change in the production of economics knowledge:

"We can land men on the moon, reduce the incidence of diseases like polio and smallpox to virtually zero, and intercept a potential enemy missile within minutes of its appearance in the atmosphere because we can predict a great many physical consequences with extraordinary precision; but we could not predict the effects of the 1968 tax increase, we could not even agree on the degree of monetary restraint in 1969, let alone predict its quantitative impact, we were unable to foresee the debilitating effects of our present welfare system […] we invest very large sums on a program designed to enrich the educational environment for ghetto youngsters without knowing whether or not the beneficial effects are wholly transitory, and we spend $50 billion a year on education without knowing whether half or twice that amount is optimal or whether what we do spend is efficiently allocated." (Juster 1970, p. 142)

The state of affairs got worse during the 1970s. Economists could not predict the oil crisis of the period and their capacity of forecasting economic activity was severely challenged. In this context, in 1976, the Lucas Critique came harshly out (Lucas 1976). According to Heckman (2000), the incapacity of predicting the oil crisis of 1973 marked what was understood as an empirical failure of the current econometric approach. The result was an increased criticism to econometrics. Entering in the 1980s, papers were full of bitter oppositions to econometrics. In the *History of Political Economy* special issue about econometrics, the 1980s are the decade when happened “convergence of econometrics, history of economics, and philosophy of science” due to the accruing questioning upon econometric methodology (Boumans and Dupont-Kieffer 2010, p. 11). Stock (2010) indicates the trio Leamer (1983), Hendry (1980), and Sims (1980) as three of the most notorious critiques to econometrics in 1980s. The respective titles of their works indicate the harsh tone of their expositions: "Let's Take the Con Out of Econometrics"; "Econometrics: Alchemy or Science?", "Macroeconometrics and Reality".

Although the three papers are considerably different among themselves, they were all critiques, claiming that econometrics should be reinvented because at its current state, it was incapable of reproducing the result expected. It is important to understand, however, that underneath this view, the similarity may be understood more specifically. Econometric methods based on modelling agent's optimal behavior failed to predict and explain. Causality did not emerge from those methods and had to be reviewed.

Moreover, while dealing with nonexperimental data, Hendry, Sims and Leamer proposed different methodologies. The authors conceded that where experimental data could be available, randomization was the gold standard for causal identification.

Coming back from our detour, Angrist's generation was formed under this understanding of econometrics. Causality as-if-random permeated the air. In macroeconometrics, though, it was unachievable. Vector autoregressive, London School of Economics and Bayesian approaches evolved to treat causality in time series then (Qin 1993, 2013, 2015a, 2015b). In microeconoemtrics, randomization and causality were more tangible but had yet to be developed further. Lots of work was ahead for future microeconometricians. Resilience, creativity, and motivation – expected qualities in PhD. candidates – had to be cherished in this context, and those were characteristics that Angrist cultivated:

"I loved being at Princeton. Even though it was very difficult. The studies were difficult. I had been out of school for a while. My Hebrew University experience wasn't very helpful to be a student. And then the army, of course, and I didn't really have the math backgrounds or the theory. I thought I was a math major in college, but I wasn't even close to what was needed. So I had to take some remedial courses and get a tutor. It was a struggle. But I loved labor and I took Ashenfelter's class. I actually took it twice. And then I got very interested in program and policy evaluation, because a lot of people in the industrial relations section were working on that. And I was excited by that." (Angrist 2020 - Appendix 6)

It was in this mood that Angrist’s cohort and others from the 1980s had to embark on analytical labor economics. It was an uncertain time for econometrics and a flourishing time for labor economics. Data and questions were abundant; what lacked were foundations.

However, given the increasingly applied nature of both economiocs and labor economnics (Cherrier and Backhouse 2016, Panhans and Singleton 2016) the answers would at least partially have to emerge from applied problems. After LaLonde, randomization achieved such a high status in the section that any opportunity to apply it was valued.

Field experiments, however, were not an option due to their cost and stigma from the 1970s (Chapter 2). Randomization, then, had to find another way to be implemented. A set of new questions and ideas had to emerge. At the time, there was no way to randomize data without acting on the data generating process. Data was either random or not. there was no *as-if* random data in economics[62]. There was no almost (or *quasi*) experiment. How could randomness circumvent the use of agent modeling? That was one rampant problem for program evaluation.

There is no documentation of this discussion being explicit at the time, but it can be inferred that it was commented on the elevated table of the IRS in the late 1980s. Card and Ashenfelter published a widely cited paper in 1985 (Card and Ashenfelter 1985) where they discuss the need of credible alternatives to "genuine experiments". They reused Ashenfelter's difference in differences to create a credible estimator for the non-experimental data available on the results of training programs. Credibility and randomization were at the core of their paper:

"The rise and fall of successive federal training programs underscores the need for credible and continuous evaluation of these programs. Yet, apart from the results of one genuine experiment,' these training programs must still be analyzed by non-experimental methods, even some two decades after they were first initiated. […] In order to make any progress a comparison group of workers must be generated to control for economy-wide movements in earnings during and after the training period. In addition, it is clear by now that participants in training programs do not represent a random sample of the eligible population" (Card and Ashenfelter 1985, p. 648)

In this context, Ashenfelter and Card were watchful for any developments in causal inference for non-experimental data. They would read papers from diverse fields in search for the credibility that they thought lacked in economics (Card 2020). In one of these excursions outside the realm of economics, Angrist thesis' topic appeared. Ashenfelter read a paper in the *New England Journal of Medicine* about the Vietnam draft lottery and its impact on mortality (Hearst, Newman & Hulley 1986) and noticed that the methodology could be replicated in economics:

[62] In psychology, the situation was a bit different given that regression discontinuity designs had already been used there: see Cook (2008).

"Orley came in one day after having seen a news story about using the draft lottery to study the effects of Vietnam era military service on longterm health. Orley said somebody should do that for their earnings. And I said: "Yeah, I'm going to do that" [laughs]. And I went and went over to the library and got that journal article and that was getting into my thesis. I was a second-year student." (Angrist 2020 - Appendix 6)

Since 1986, then, everyone at the section knew that Angrist was working on a problem with *naturally* randomized data. This was original in economics. Although Angrist was following the IRS tradition, nothing like that had been done yet at the section. In fact, nothing to the like had been extensively studied in economics beyond Ashenfelter's presentation of the difference in differences estimator. Ashenfelter's diff-and-diff creates an as-if-random estimator from pre and post observations. It creates a ghost counterfactual. The draft, on the other hand, does not have to create a counterfactual as it randomizes observations pre-treatment. There is an actual randomness in the observations.

In common, both methods are concerned with uncovering the causal effects of a "treatment". Their goal is to replicate the credibility of randomized controlled trials. Nowadays, this task in economics is widely known as quasi-experimentation or natural-experimentation[63]. In the 1980s and early 1990s, though, these nomenclatures were unusual. Researchers had yet to uncover the commonalities of different methods to put them under the same name.

In fact, the methodologies had yet to spread for the aggregation of the methodologies to be worth for the community. Before 1985 "natural experiments" appeared in a few noticeable sources. Thomas Juster (1974) used the term in a broader sense, meaning something closer to observational data. In contrast, a few years later, Mark Rosenzweig and Kenneth Wolpin (1980) applied a very modern natural experiment approach, twins, in their paper "Testing the quantity-quality fertility model: The use of twins as a natural experiment". In 1985, Robert Deacon and Jon Sonstelie (1985) also presented a modern interpretation of natural experiments. They noticed that a sudden increase in gasoline prices offered an excellent opportunity to study the value of time. Would you rather wait in line for a lower price or go to the next gas station with higher prices?

Anyhow, these are exceptions. Few papers used the terms until 1985. Many applied the now known quasi-experimental methods without naming them even after the mid-1980s. Inside the IRS, for instance, David Card had just become interested in migration problems in 1990 (Card 2020). The topic was new in labor economics and had been pioneered by George Borjas and Barry Chiswick a few years earlier (Borjas 1982, 1983, Chiswick 1978).

[63] Noawadays, quasi-experimental approaches are known alternatives for structural model: see Heckman (2000, 2001, 2010) and the next chapter.

Card, by chance, noticed an interesting case study. In the 1980s, Miami saw a significant influx of Cuban Immigrants, known as the Mariel Boatlift. More than 100,000 Cubans flew from a severe crisis in the Cuban Economy. *Marielitos* waited at the Mariel Harbor for anyone who could pick them up and leave them in Florida. This unexpected influx of immigrants was also a sudden increase in unskilled labor force in one specific place: Miami. The situation was perfect for studying changes in employment and wages using Ashenfelter's Difference in Differences approach.

Card's paper did not use the terminology (Card 1990). Maybe if it had, it could have impressed the reviewer who did not like the paper. "Luckily, the editor of the *Industrial and Labor Relations Review*, Don Cullen, liked the paper and accepted over the objections of the referees, who hated it" (Card 2020). The paper is now known as one of the first instances of a natural experiment for studying the labor market, as Daniel Hamermesh emphasized in his interview (Hamermesh 2020 - Appendix 11).

Card was not the only one aware of the natural experiment strategy though. Lawrence Summers, Alan Krueger's supervisor, had written in 1982 a paper with Kim Clark about labor force participation (Clark and Summers 1982). In the introduction, they wrote: "During World War II, the level of female employment and participation rose precipitously. We examine the aftermath of the conflict to see whether the war had a positive or negative impact on subsequent female participation." (Clark and Summers 1982)

Other former students from IRS were also using natural phenomena to study economics problems. For example, John Bound and Gary Solon wrote a paper on the value of twin-based estimates of the return to schooling (Bound and Solon 1998). But Solon had been since his PhD. in 1983 a researcher on the importance of different econometric methods and measurements.

The section was stacked with influences then. Ashenfelter was the pioneer of Diff-n-diff and captained the search for credible methodologies. Robert LaLonde and Gary Solon, IRS's alumni, wrote papers on the values of natural experiments and experimental methods. Card and Krueger, tenured professors at the section, had both direct contact with natural experiments. Angrist was well served.

However, none of these references remarked explicitly the technical core of their methodologies. Randomization was their aspiration, certainly. But in econometric terms, how were they applying randomization? What was the technical commonality between difference in differences, twins, boatlifts, and drafts? They implicitly knew that their papers fit together but had not yet explicitly described their commonality.

Angrist's research clarified their "family resemblance". Angrist searched for the root of his methodology and explained it clearly in his thesis and publication. Angrist published his research in several places (Angrist 1990, 1991), but noticeably in the *American Economics Review*, where his paper came under the title "Lifetime Earnings and the Vietnam Era Draft Lottery: Evidence from Social Security Administrative Records". There he stated:

"The research reported here overcomes such statistical problems [omitted variables] by using the Vietnam era draft lotteries to set up a natural experiment that randomly influenced who served in the military. […] The assumptions underlying this procedure [the lottery] are those that justify instrumental variables estimation". (Angrist 1990, p. 313)

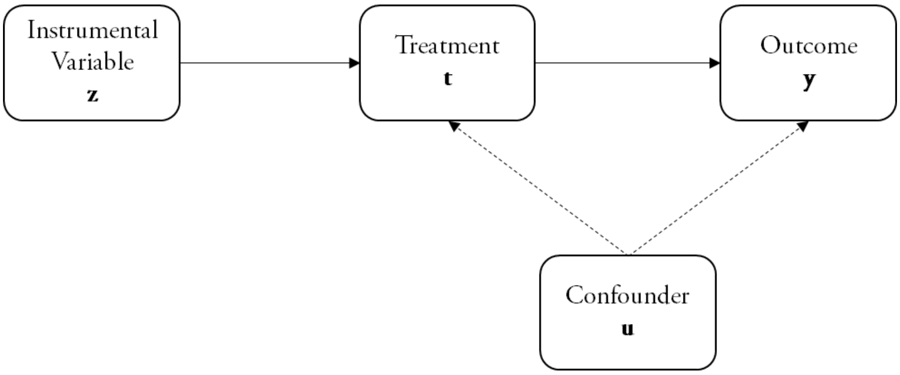
Angrist's research was the first to thoroughly explain the relationship between the natural randomization of his observations and instrumental variables (IV). While the IV route is commonly understood nowadays[64], the method was unusual in microeconometrics at that time.

Two step procedures were commonly used to solve simultaneity, especially in supply and demand models. The problem and its solution were inescapable in economics, which readily made them available in textbooks in the early days of econometrics. The didactic formulation endured, and it remains the main framework of explanation for IV in modern textbooks.

We have already technically described simultaneity in Chapter 1. Now, it is interesting to remember that in a system of equations, where there are two or more variables that simultaneously cause each other, there is an endogeneity bias. The most common example, and the one that tormented economists for decades, is the case of equilibrium between supply and demanddiscussed in chapter 1.[65] But, when getting outside the supply and demand framework, in the policy evaluation setting, the confounder bias comes from a different type of endogeneity problem: omitted variables. The omitted variable bias can be understood with the use of a Directed Acyclic Graph – technique which has been spreading fast (see: Pearl 2017):

[64] As seen in chapter 1, until the 1980s, instrumental variables were a known methodology to solve simultaneity biases. Omitted variables and measurement errors were dismissed. For more on the history of IV, see: Qin (2015a, 2015b), Panhans and Singleton (2016), and Stock and Trebbi (2003).
[65] In the 1910s, before the advent of solutions for simultaneity, Moore found a positive demand curve and was criticized by his colleagues (Morgan 1990 Ch. 5).

Fig. 4.1 – Instrumental Variables Directed Acyclic Graph (DAG)



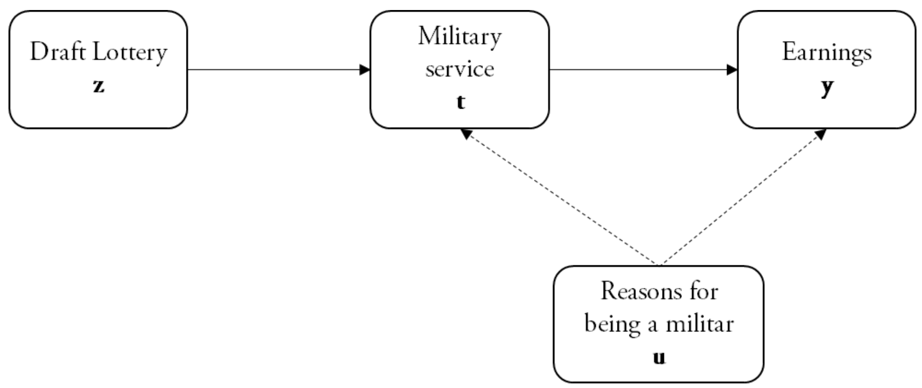
Source: Produced by the author

In the image above we notice that it is impossible to know whether the treatment or the confounder caused the outcome as both are connected to y. In this situation, the instrument z comes in hand as it is known that it is related to y only through the treatment t. Angrist's step was to notice that Ashenfelter's randomness could be transcribed as an instrumental variable. Any random variable connected to the treatment is incontestably unconnected to the confounder, by its own random nature. Since the 1990s, his solution has also spread to textbooks (see Angrist and Pischke 2009, for instance). In his words:

"The draft lottery facilitates estimation of (1) because functions of randomly assigned lottery numbers provide instrumental variables that are correlated with s_i, but orthogonal to the error term, u_{it}. For example, one such instrument is a dummy variable, d_i, that equals one if the *i*th individual was draft eligible" (Angrist 1990)

In order to illustrate his solution, the following figure illustrates how the random nature of the draft allows it to be connected only to military service, making it possible to exclude any confounder.

Fig. 4.2 – Angrist (1990) translated to a Directed Acyclic Graph (DAG)



Source: Produced by the author

In less technical terms, Angrist clarified that randomness could be a powerful rhetoric when applied together with instrumental variables. Any random variable connected to the treatment will most likely be unconnected to a confounder. Finding this kind of instrument is not easy, but according to John Antonakis and colleagues (2010) "the time spent to find instruments is an investment that will serve science and society in good stead because the estimated parameters of the model will be consistent" (Antonakis et al. 2010, p. 1103).

Instrumental variables facilitated the spread of quasi-experimentation. The method became an umbrella capable of explaining econometrically numerous different randomization procedures. The randomness once extraneous to economics and econometrics and embraced by Ashenfelter had now an econometric formulation. As with Ashenfelter's difference-in-differences paper, Angrist thesis was a milestone in the use of randomness and causal identification in econometrics.

It would not take long for different randomization techniques to be translated to the IV framework. In this regard, Thomas Cook (2008) demonstrates how regression discontinuity designs were adopted in the 1990s in economics after a long journey in psychology since the 1960s. In the 1990s, experiments and natural experiments were translated to the language of instrumental variables by Guido Imbens, Angrist and Donald Rubin. In several joint works the authors presented how Average Treatment Effects could be described as IV, they presented Local Average Treatment Effect to deal with the noncompliance problem and they generalized IV connecting it to the potential outcomes framework (see: Imbens and Angrist 1994 and Imbens, Angrist and Rubin 1996).

4.6. A Decade of Recognition

In the early 2000s, when reviewing the literature for the Handbook of Labor Economics, Angrist and Krueger wrote the chapter "Empirical strategies in Labor Economics" that focused exclusively on techniques adopting randomness. In their words, the chapter reviewed "four identification strategies that are commonly used to answer causal questions in contemporary economics" (Angrist and Krueger 1999, p. 1281). Those strategies were: "control for confounding variables, fixed-effects and difference-in-differences, instrumental variables, and regression discontinuity methods" (Angrist and Krueger 1999, p. 1284)

At this point, natural experiments and causal identification were fields of their own. They had their own chapter in the *Handbook of Labor Economics*, but also prestige and dissent. The 1990s had been special for the methodologies proposed by Ashenfelter, Card, Krueger and their students. Angrist's central chapter of his thesis was published in the *American Economic Review* in 1990 (Angrist 1990). In 1991, Angrist published with Krueger in the *Quarterly Journal of Economics* a now renowned paper titled "Does Compulsory School Attendance Affect Schooling and Earnings?" (Angrist and Krueger 1991). These two papers in the most important economic periodicals helped disseminate instrumental variables, natural experiments, and causal identification in economics. Angrist and Krueger's paper accumulate more than 3000 citations according to Google Scholar, and Angrist's thesis paper has more than 1200 citations[66].

In the polemics side, their innovation had also been applied to core economic problems. In 1994, Card and Krueger wrote a paper on the effect of minimum wage on employment called "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania" (Card and Krueger 1994). Their paper was straightforward as already in the abstract they claimed: "On April 1, 1992, New Jersey's minimum wage rose from $4.25 to $5.05 per hour. To evaluate the impact of the law we surveyed 410 fast-food restaurants in New Jersey and eastern Pennsylvania before and after the rise. (…) We find no indication that the rise in the minimum wage reduced employment." (Card and Krueger 1994)

[66] Citation count of June 2021.

The results were counter-intuitive, at least for theoretical economics at the time (Leoard 2000, Krueger 2015). Economic theory had always proposed a negative trade-off effect between minimum wage and employment. The paper was published in the *American Economic Review* as well, and it certainly made a buzz. The results were discussed all over US and it brought causal identification to scrutiny. This is the headline of an article in the New York Times in 1993: "Conversations/David Card and Alan Krueger; Two Economists Catch Clinton's Eye By Bucking the Common Wisdom". In the article reads:

"The two, David Card, 37 years old, and Alan Krueger, 32, are economists at Princeton University and pioneers in a new kind of survey research. Sometimes together, sometimes with collaborators have found that testing the conventional wisdom on trendy topics like the minimum wage, mandated benefits and immigration can yield a rich harvest of unexpected results. Quite often, they argue, the received wisdom is simply wrong." (Nasar 1993)

In 1995, unafraid of facing criticism and contesting received wisdom, Card and Krueger published *Myth and Measurement* (Card and Krueger 1995), a book where they reviewed the literature on the minimum wage. While reviewing, they also made strong claims about making causal claims in economics and how economic myths could be overcome.

The book was received with mixed feelings, but nevertheless with buzz. Minimum wage research was once again stamped in newspapers all over the country. In the Industrial Relations Review, five reviews by senior scholars were published: Charles Brown, Richard Freeman, Daniel Hamermesh, Paul Osterman and Finis Welch.

The content of their reviews is secondary here; what is relevant is the effect that their book and their research had in economics. The book was a vehicle for discussing not only minimum wage theory, but empirical methods. Card and Krueger, in 2017, when participating in a symposium in honor of their book recalled that their book was also a claim for "credible design-driven empirical research" (Card and Krueger 2017).

During the 1990s, Ashenfelter's difference in difference, Angrist's instrumental variables, potential outcomes, regressions discontinuity, and matching techniques all came together in econometrics to become "natural experiments" (Angrist and Kruegre 1999). Minimum wage helped to boost their ideas forward, but there's no denying that the methods were at the forefront of their success and not only the content. Ashenfelter and Card would win prizes in the 1990s for their methodologies, not for the analysis of their papers. In 1995 Card received one of the highest prizes in economics, the Clark Medal, which stated the following:

"David Card's research is distinguished by a high degree of econometric sophistication and the development of new and highly relevant empirical data. His findings have initiated revaluations of important issues in labor demand and supply and the impacts of unionization. His pioneering development of natural experiments has inspired a new generation of empirical labor economists. The John Bates Clark Medal for 1995 is awarded to Card in recognition of these impressive achievements." [67]

A few years later, the techniques that had received the academic token of approval from the Clark committee was also awarded by the Institute of Labor Economics (IZA). Orley Ashenfelter won the IZA prize of 2003. The committee was formed by George A. Akerlof, Solon S. Becker James J. Heckman, Gerard Pfann, and Klaus F. Zimmermann. Together they choose Ashenfelter for:

"his ingenuity in devising clever ways to derive and test hypotheses of economic models, his exceptional creativity in using and collecting data, and his originality in pioneering the natural experiment methodology. Setting off the development of methods for empirical tests of labor market models Ashenfelter's scholarly contributions have fundamentally transformed the analysis of labor markets. In a number of seminal articles he has broken new ground in various core areas of labor economics including research on trade unions, wages and employment, the analysis of labor supply, and the study of discrimination, education and training."

In the 2000s, natural experiments were staple in economics. Ashenfelter, and his disciples had already been recognized. Their techniques had been published and discussed in prestigious journals and conferences. But it all started with Ashenfelter's affinity with randomness in the 1970s Labor Department and his efforts to make it viable.

[67] See: https://www.aeaweb.org/about-aea/honors-awards/bates-clark/Card-card [accessed 09/07/2021]

5. CONCLUSION

Randomization is only possible in special cases. The researcher must control the collection of data to be able to randomize groups of treatment and control. The lesser the control over the data generating process, the farther from randomization the researcher gets. Non-randomly generated *passive observations* are probably one of the worst-case scenarios. And since the 1950s, economists know that this is exactly the kind of data they have to deal with. Haavelmo, Koopmans and Hood in seminal publications in the 1940s and 1950s presented *the problem of passive observations* in economics and discussed how it is ubiquitous in econometrics.

For them, *the problem of passive observation* encompasses all biases that hamper causal claims. Econometricians since Haavelmo know that to control these different biases the only tool would be randomization. However, they also know that active observation and control are exactly what lacks for them. As Koopmans (1979) affirmed: economists are the meteorologists of economic life. This means that, like the meteorologist, they have to search for ways to clean the data without controlling the observations.

This search to the problem of passive observations was gradual and focused on different aspects through time. Looking from the perspective of contemporary econometric textbook, that problem became endogeneity biases, and economists dealt sequentially over time with measurement errors, simultaneity, and omitted variables.

As already discussed, Haavelmo's (1944) efforts, followed by Koopmans (1950) and Hood and Koopmans (1953), showed a method for dealing with passive observations, and in doing so, they equaled passive observations to simultaneity. They then claimed for simultaneous equation models, but also argued that econometricians should be accountable for passive observations.

Their works reached a wide audience and the economists' accountability for passive observations spread out. In 1972, in his "Thirteen Critical Points in Contemporary Economic Theory: An Interpretation", Oskar Morgenstern affirmed that the control of economic variables was the only empirical problem among the other twelve that were theoretical (Morgenstern 1972). Thus, in a few decades, the problem of passive observations was already established at the core of econometrics.

In the 1970s, this meant that accumulated passive observations about the society had to be explained by the economist in their armchairs: "If you want to understand passive observations, you have to listen to my econometric methods" (Latour p. 260).

These 1970s *new* passive observations introduced a different challenge for economists. While measurement errors and simultaneity were known issues of macroeconomic data sets, panel data brought a distinct conundrum to the surface: omitted variables. Once again, causality would not be identifiable, as it had happened earlier with measurement errors and simultaneity. Econometricians had discovered a distinct facet of passive observations that propelled them to further develop their own field of research. From this point of view, this thesis affirms that microeconometrics has developed from the widening perspective of passive observation problem, that went from passive observations as simultaneity to passive observations as omitted variables.

While this affirmation is interesting when looking from a distant point of view, in the grassroots of economics and program evaluation, terms such as passive observation, omitted variable and simultaneity were not the main concern. Actually, in the early 1970s the real problem was way more palpable: poverty.

When looking at the headquarters of US government, the connection between passive observation and econometrics is blurred. Poverty, economics, and program evaluation intertwined in a non-obvious way to give rise to microeconometrics. In the grassroots of economics, microeconometrics was not a theoretical problem, but an applied one.

In the 1960s and 1970s, *poverty knowledge* in the US government had numerous phases. It went from unknown and qualitative to fashionable and measurable. It lost its fashion and witnessed the questioning of its measurability. It went from a social science problem to an economics problem and then to a multidisciplinary endeavor. Poverty knowledge was bred for fifteen years inside government walls until program evaluation was set free to develop inside universities. Program evaluation in economics was then born and promoted microeconometrics to a new status.

During these years, numerous real conflicts among practitioners occurred. Initially, economics had to insert poverty in its agenda then dominated by concerns with economic growth. This happened through the creation of the Office of Economic Opportunity (OEO) in the early 1960s and the unwind of the War on Poverty of John Kennedy and Lyndon Johnson. The office was an assortment of war-like ideas. Concepts from the Cold War and WWII social sciences were all there.

With every year that passed, the many different ideas inside the office created conflicts and impasses. It became increasingly clearer that the War on Poverty and the OEO had to be rebranded. Rebranding happened in Nixon's government. The office became an evaluation and research facility. What followed were major social experiments: the negative income tax experiments. Poverty knowledge that was initially action-oriented (divided into Community Action and the Culture of poverty) suddenly became Job Training programs and negative income taxes. Randomized controlled trials decided whether they were viable or not.

The discipline of economics followed closely this movement as economists were directly involved in the experiments. In the process of proposing and testing negative income tax esxperiments, a network of research was formed. Wisconsin, Princeton, Harvard, and Chicago were some of the departments interested in the results of the experiments.

Statistical methods evolved during the period and techniques were developed to handle minor problems in the selection of groups in the experimentats. However, politicians and bureaucrats did and do not want to know about this. They wanted fast results and the research facilities could not offer that to them. Experiments faded out as an expensive and slow methodology.

Nevertheless, a new discipline was being born. Societies had been formed, academic journals released, graduate courses initiated. In the mid-70s, the data and methods that came with it had sparked a technical and theoretical research agenda inside economics department. It had not been long since economists worried mostly with time-series and simultaneity. Accountability for passive observation had put them on an applied problem that changed the discipline.

Randomization, unobservable and omitted variables were a renewed topic for economists in the 1960s and 1970s. In universities, those with ties to the experiments were quick to transmit the new questionings to their students. Albert Rees, who was one of the main researchers in the negative income tax experiments, supervised or was in the committee of an important group of students in the late 1960s and early 1970s. The most well-known are John Pencavel in 1969, Orley Ashenfelter in 1970, James Heckman and Ronald Oaxaca in 1971.

Heckman and Ashenfelter have an important role in the history of microeconometrics. What distinguishes their efforts is the fact that they came to be known as the main solvers of the sample selection bias, the specific omitted variable problem behind the rise of microeconometrics. By the end of the 1970s, they had implemented randomization to labor data in two different manners: the sample selection model (Heckman 1979) and the difference in difference estimator (Ashenfelter 1978).

Heckman and Ashenfelter have quite similar backgrounds. Both are labor economists and PhD. Alumni from Princeton. They in fact met at Princeton and published together during the initial years of their careers (Ashenfelter and Heckman 1971,1972,1973,1974).

Their close relationship contrasts with our current notion of microecnometrics. In the contemporary literature two communities have been clashing recently: *randomistas* and structural modelers. While randomistas, group to which Ashenfelter pertains, are said to defend the use of randomization as the only credible way of avoiding omitted variable bias, structural modelers, community in which Heckman belongs to, advocate the view that to exclude the bias one needs to know its root cause and model it out.

The present-day view on two opposing groups leads to an erroneous understanding that microeconometrics has developed in a departmentalized way. However, when looking closer to Heckman and Ashenfelter, we notice that this is a misconception. Randomization and structural modeling had a close relationship in the early days of omitted variables in Princeton, at least conceptually. The interesting point that arises from their relationship is that both communities have different solutions for the same problem, and this is the cause of their contemporary disagreement.

Both technical and bibliometric evidence indicate that Heckman's and Ashenfelter's ideas about omitted variables gradually changed over time, and historical facts confirm that. They differed their research due to their different point of views, theoretical and applied respectively. Both Authors were concerned with the same problem during part of their careers: biases in the collection of econometric microdata. This is seen in the bibliometric evidence that shows that they had almost identical related networks during the period they published together. This is also seen in the historical fact that both learned about this issue through Albert Rees, with whom they had an important relationship.

In this regard, it is interesting to notice that what is now a clear clash, was historically only a change of context. Both authors had the same concerns, but during the period from 1968 to 1974 they moved to different contexts. Heckman went to Columbia and. Ashenfelter stayed at Princeton. Then, Ashenfelter and Heckman established their research agendas from 1974 to 1979. The former focusing on randomization and the latter on modeling agent's behaviors, but both dealing with the same problem: omitted variables.

Thus, by putting the government problems side by side with the development of Heckman's and Ashenfelter's research agendas, we noticed that governmental walls enclosed the formative years of microeconometrics, but when it was set free, being published in important journals by Heckman and Ashenfelter, it finally received its deserved token of an academic field and grew fast and strong.

In this regard, Ashenfelter's difference-in-difference estimator epitomizes the transformation in econometrics. Without the pretentiousness of large-scale experiments, econometrics came back as an unexpensive way of evaluating programs. Economists had then to embark on a journey to solve new technical problems. The question then was how they could apply the same standards of randomized trials to unrandomized data. Ashenfelter's Diff-n-diff did that successfully in Washington DC when he was the director of ASPER.

Ashenfelter stayed only one year in Washington and then went back to Princeton. Then, stimulated by his time in ASPER, Ashenfelter continued his academic career as a researcher and the director of the IRS. Ashenfelter and his students did the ordinary business of their academic lives, but still transformed economics in a meaningful way. When looking from the lenses of a micro history, the happenings in an economics department are not as revolutionary as one might think from a broader perspective but are still impressive.

A total of nine interviews were conducted in 2020 with Orley Ashenfelter, David Card, Joshua Angrist, Michael Ransom, Gary Solon, Joseph Altonji and Daniel Hamermesh to understand how the IRS dealt with "natural experiments" and program evaluation. Inside Princeton's IRS, as told in the interviews, program evaluation saw no "shift", "revolution", nor "turn" as saw in this thesis.

The fact is that, during the 1990s, several developments in econometrics came together to become "natural experiments:" Ashenfelter's difference in difference, Angrist's instrumental variables, potential outcomes, regressions discontinuity, and matching techniques. This happened gradually and incrementally. Their techniques were published and discussed in prestigious journals and conferences throughout the years. They even faced criticisms and hardships. And more important, it all started with econometricians accountability for passive observations and Ashenfelter's affinity with randomness in the Labor Department in the 1970s.

Much has yet to be explored in the history of microeconometrics. Princeton was certainly not the only department developing the field, and institutions such as Chicago, Columbia, Wisconsin, and so forth must have their points of view discussed. Nevertheless, this thesis focused on uncovering the rise of natural experiments in economics and there is no doubt from what we discussed and from our current notion of the field that Princeton's Industrial Relations Section was a major agent in the development of these techniques.

This thesis built a history of microeconometrics that faced the hardships of the lack of secondary research and has thus explored different manners of filling this gap. Oral histories, bibliometrics and natural language processing were among the methodologies applied during the development of the arguments. The thesis thus also contributes to the development of diverse historiographical tools in the history of economics. To my knowledge, related networks have not been used in the literature to identify similarity between works and authors, and this thesis presents the first effort of the kind. Related networks are thus a fledgling methodology that should be studied further because similarity between ideas and practices is a growing concern among historians to which few technical methodologies are available beyond text interpretation.

Moreover, counting mentions to names and words is still underused in the history of science. Digital humanities focuses on bibliometric techniques rather than natural language processing. In the history of economic thought, natural language processing has been used to apply machine learning techniques and not so much to understand visually minor changes to specific words and arrays of words. In chapter 2, I demonstrate how the use of these visual tools can aid in writing history and can be combined with other historical methodologies to complement the arguments. Oral history and computational methods are not opposing techniques, but complimentary as seen especially in chapter 2.

Oral histories have still much to be developed inside the history of economic thought community. Although they have limitations due to the fact that actors have flawed memories and could be deceptive in their narratives, I do believe that a combination of oral histories is capable of being useful as a historical source. Biases tend to diminish with a bigger dataset of interviews. In this thesis, although with a selected few, I accumulated almost 20h of interviews with them that was capable of showing what was consistent and inconsistent in the narratives. There should be more discussion about oral history in the community. Doing an interview and working with it is, I discovered, considerably different than learning the theory.

Historiographical tools and methodologies are, unfortunately, not taught for young scholars in the history of economics, who have to grasp them by themselves. Oral history, natural language processing, and bibliometrics are all important to be taught. In this regard, this thesis aimed to widen the possibilities and was also behind the development of initiatives to incentivize the use of these methodologies. In the course of this Ph.D. I developed an R Package to handle JSTOR data for research (DfR). I've also made available online a simple web scraping website to count numbers of mentions to a word or term in JSTOR. Interviews with both audio and transcripts are available by contacting me and the interviewees. Some of them are already available in an appendix of the thesis.

Still, it is a fact that all these methodologies have flaws and that this thesis is just an initial effort to using them for studying the history of microeconometrics. Thus, in the years to come, I intend to do more oral histories and, especially, research the archives of the institutions for further understanding the history of microeconometrics. Visiting archives and conducting more interviews was a goal of this thesis early on, but was something that the COVID-19 pandemic hampered. Nevertheless, the amount of information gathered during the development of this thesis was more than enough to sustain the arguments here presented.

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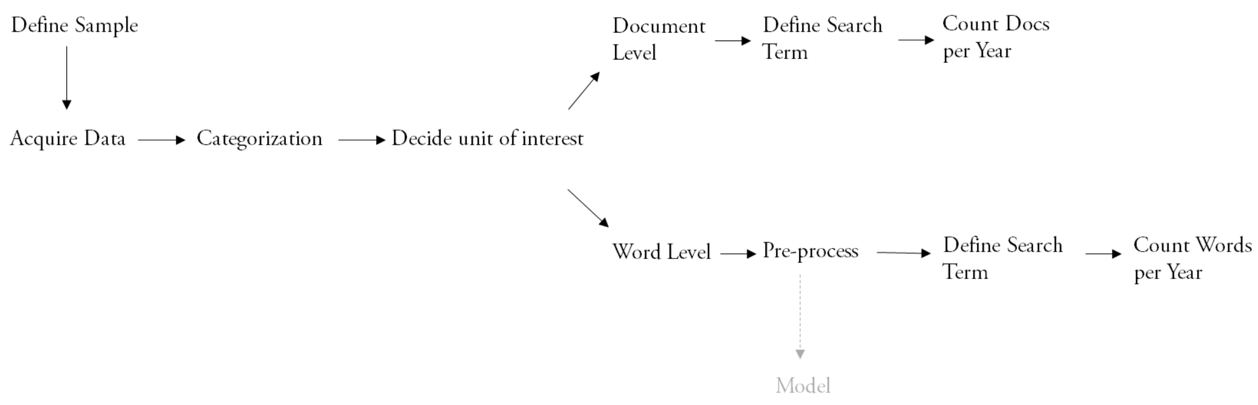
Appendix 1 – Text mining

The use of computational methods in the history of science, and especially in the history of economic thought, has seen an upsurge in the last decade. Text mining and bibliometrics have been the two most used sources of information in this period. Researchers have applied mostly machine learning tools but also simpler methodologies. Overviews of the methodologies can be found in numerous places. Wilkerson and Casas (2017), grimmer and Stewart (2013) and Nguyen et al (2020) provide interesting introductions to computational methods for text mining. The Journal of Economic Methodology special issue of 2018 has a long discussion about the upsurge of computational methods in the history of economic thought and their impacts and constraints.

This appendix deals with the application of simple, even though enlightening methodologies. Namely, word counts, and document counts over time (see Hoover 2004 for an early and leading reference on the issue). Every day, researchers perform searches in scientific databases such as google scholar, JSTOR and ideas.repec. Their results are normally presented by relevance (according to the algorithm calculation of best match for the search). This makes sense, given that researchers are normally concerned with finding references. But what if the researcher is interested in the evolution of that search over time?

Well, it is totally possible to organize the results per date, although in any of those databases the researcher would not be able to view how many papers are available for each year. Is this transformation that this appendix is concerned with. How is it possible to use research databases for analyzing time-series of words and documents? The following workflow extends the data science strategy for the creation of the time-series of interest.

Fig. A.1– Text mining time-series workflow



Source: done by the author

The first step, then, is the simple step already common to every researcher: defining a broad sample of documents for search. More practically, this means choosing a database like JSTOR for social scientists or philpapers for philosophers and, if necessary, constraining it to search terms. Even though this may be overlooked, this is an important step. No database is complete, and they define the representativeness of the search. A search for the term “poverty” has different meanings in JSTOR, philpapers and Google Scholar, for instance.

Moreover, it is the defined sample that will normalize the results. The number of documents and words change over years, normally because of the private nature of this institutions. JSTOR, for instance, holds contracts with publishers that hampers it from providing access to recent research. This means that the sample is biased for final years. Normalizing the result by dividing by total document count helps with the issue, but it is still important to realize exploratory data analysis (EDA) for knowing the characteristics of the sample and avoiding unknown biases.

In the sequence, the researcher must acquire the data through web scrap (manual or computational), APIs or online services. This paper uses four different samples, acquired in different forms: Economic Reports of the President, JSTOR abstracts for the search “community action”, Google Ngrams and JSTOR database as a whole. How each data has been acquired will be explained briefly soon, but first it is important to not skip the third step: categorization. After defining the sample, it is necessary to categorize it accordingly to the interest of the historical research. Normally this means grouping them by years, but this could be also different grouping strategies.

Finally, with a categorized (divided by year) sample, it is possible to define the unit of interest, words of documents. This might sound confusing, but it is simples. When realizing a search in a database, researcher see the number of documents that fit the search criteria. A search for “poverty” in JSTOR yields 527.822 results[68]. This means that JSTOR found 527.822 documents that have the word poverty at least one time on them, not that the word was counted that amount of times.

If we were interested in a word count, we could then download all papers - or all abstracts - to count how many times a specific term has appeared. In the case of “poverty”, we could be interested in “malnutrition” or “school” for instance. When counting words, differently from when dealing directly with document count, data needs to be pre-processed. This means that texts have to be tokenized: transformed in unigrams, bigrams or trigrams

[68] Searcvh realized on 11/19/2020

through the method of "bag of words". In R, programming language utilized for this paper, tidytext and tm are two of the most used packages for this task. For technical reference see: https://CRAN.R-project.org/package=tm and https://CRAN.R-project.org/package=tidytext. An interesting technical introduction for text mining and text analysis in R is provided by: Silge and Robinson (2017) and Jockers (2014).

In a more technical analysis, after pre-processing the data, researcher may follow the route of modeling for different insights. Those would be mostly machine learning models such as topic models and supervised models. In this paper, these methods were not used.

Samples, acquisition of data and treatment

Economic Reports of the President – Fig. 2.1

All reports from 1958 to 1974 were downloaded as PDF files from: https://www.presidency.ucsb.edu/documents/presidential-documents-archive-guidebook/the-economic-report-the-president-truman-1947 [accessed on 11/19/2020]. PDFs were read using R version 4.0.2 (2020-06-22) with the help of the following packages: tm, tidyverse, tidytext and pdftools. Visualizations were produced using ggplot2, patchwork and extrafont. All packages were available at the The Comprehensive R Archive Network (CRAN). Code is available under contact with the author.

The time-series were created by counting the number of hits in the following string arrays: poverty, poor, opportunity and community; growth, development. Both counts were normalized by the total number of words for each year.

JSTOR Abstracts – Fig. 2.2

Figure 2 was built using data from JSTOR data for research tool. jstor.org/dfr provides access to .xml files containing metadata, with abstracts, for a given search. The searched term was "community action". No quotation marks were used when realizing the search at JSTOR. XMLs were read using R version 4.0.2 (2020-06-22) with the help of the following packages: tidyJSTOR and tidyverse. Vizualizations were created using ggplot2 and extrafont. tidyJSTOR is an R package created by the author for reading JSTOR dfr data. Available at: https://github.com/arthurbnetto/tidyJSTOR.

The time-series were created by counting the number of hits of the following string arrays in the abstracts: poverty, poor; delinquent, delinquency. Both counts were normalized by the total number of words for each year.

Google ngrams – Fig. 2.5

Figure 5 is a simple reproduction of the plot available at https://books.google.com/ngrams/graph?content=microeconometrics&year_start=1950&year_end=2019&corpus=26&smoothing=3&direct_url=t1%3B%2Cmicroeconometrics%3B%2Cc0. For more information about Google Ngram Viewer see: Lin et al (2012) and Michel et al (2010). The plot was reproduced with R version 4.0.2 (2020-06-22) and ggplot2.

JSTOR documents – Fig 1.1, 1.2, 2.3 and 2.4

Figures 3 and 4 were created through the count of documents for searches in the JSTOR website. Terms were searched for individualized years and annotated. For instance, "poverty" for from 1950 to 1950 in the economics discipline, then from 1951 to 1951 and so forth. Results were normalized by the total number of documents in JSTOR for the discipline in question. This was acquired through a search for an empty string in jstor.org/dfr. A gentle web scrap was behind the acquisition of data [number of hists for a certain term each year], with a request time of 30 seconds to 60 seconds, with a mean time of 45 sec.

Plots, web scrap and data treatment were made using R version 4.0.2 (2020-06-22) with the help of the following packages: tidyverse, rvest, reshape2, lubridate, data.table, extrafont and ggplot2. All packages were available at the The Comprehensive R Archive Network (CRAN). Code is available under contact with the author.

Appendix 2- ASPER Technical Analysis Papers

Measuring the Effect of the Federal Government on the Change in the Labor Market Position of Black Male Workers Relative to White Male Workers: 1966 to 1970	1973-04
Using Estimates of Income and Substitution Parameters to Predict the Work Incentive Effects of Various Income Maintenance Programs: A Brief Exposition and Partial Survey of the Empirical Literature	1973-06
Minimum wage legislation in the United States	1973
Minimum Wage Legislation in the United States: Comment. II. Minimum Wage Legislation in the United States: Reply	1976-05
Compensating Wage Differentials and Hazardous Work	1973-08
Estimating the Benefits of Job Banks as a Computerized Record Keeping System	1973-08
Effect of unemployment insurance laws and administration on unemployment rates	1973
Neighborhood Youth Corps: An Impact Evaluation	1973-09
Economic considerations for manpower revenue sharing	1973
Progress Report on the Development of Continuous Performance Information on the Impact of the Manpower Development and Training Act	1973-10
Effect of Manpower Training on Earnings: Preliminary Results	1974
Predicted Impact of the Black Lung Benefits Program on the Coal Industry	1973-12
Evaluation of the WIN 2 program	1974
Further Evidence on the Impact of the WIN II Program	1975-01
Revised Estimates of WIN Job Placements in FY 1973 and FY 1974	1974-10
Revised Estimates of WIN Job Placements in FY 1973 and FY 1974. Appendix: Sampling Procedures	1974-10
Impact of the WIN 2 program on welfare costs and recipient rates	1975
Economic Incentives and Occupational Safety	1974-04
Some evidence on the effect of manpower training programs on the Black/white wage differential	1974
Economics of job satisfaction	1974

Economist's View of Job Satisfaction and Worker Alienation	1975-01
Land Reclamation Requirements and Their Estimated Effects on the Coal Industry	1975-01
Statistical Theory of Discrimination in Labor Markets, Appendix A	1975-01
Indexing for Inflation: Some Formal Models and an Informal Policy Proposal	1975-05
Econometric Studies of Labor Demand and Their Application to Policy Analysis	1975-06
Stability of the racial unemployment differential	1975
Unemployment insurance, duration of unemployment, and subsequent wage gain	1975
Unemployment Effects of Minimum Wages	1976-02
Critique of tax based cost/benefit ratios	1976
Labor market displacement effect in the analysis of the net impact of manpower training programs	1976
Constant-Utility Index Numbers of Real Wages	1976-06
Effect of direct taxes and other factors on money wage changes in U.S. manufacturing	1977
Costs of Defined Benefit Pension Plans and Firm Adjustments	1977-04
Influence of Fertility on the Labor Force Behavior of Married Women	1976-04
Bias in the Estimates of Treatment Effects in Quasi-Experimental Evaluations	1977-08
Tax Base of the U.S. Unemployment Insurance Tax: An Empirical Analysis	1978-10
Unemployment insurance tax and labor turnover	1978
Three Paths from Disability to Poverty	1978-10
Distribution of Unemployment Insurance Benefits and Costs	1978-10
Labor Force Transitions and Unemployment	1978-10
Labor force participation--timing vs. persistence	1979-01
Demographic Composition of Cyclical Variations in Employment	1979-01
Compliance with Standards, Abatement of Violations and Effectiveness of OSHA Safety Inspections	1981
Study of the number of persons with records of arrest or conviction in the labor force	1978

Concepts and Measures of Structural Unemployment	1979-03
Implications for fiscal substitution and occupational displacement under expanded CETA Title VI	1979-03
Effect of Minimum Wages on the Youth Labor Market: An Expanded Model	1979-07
Person and organization characteristics involved in CETA program effectiveness: issues in need of research	1979-08
Nature of the youth employment problem: a review paper	1980-03
Economic Benefits from Manpower Training Programs	1976-11
Effect of the UI System on Labor Force Behavior	1978-01
Effect of the UI System on Labor Force Behavior (superceded)	1977-08

Appendix 3 – Heckman's and Ashenfelter's papers processed

Heckman's papers

Borjas and Heckman (1978) - Labor Supply Estimates for Public Policy Evaluation. *NBER Working Paper* No. 299

Butler and Heckman (1977) - The Governments impact on the Labor Market Status of Black Americans. *NBER Working Paper* 173

Heckman, J. (1974) .Effects of Child-Care Programs on Women's Work Effort. *Journal of Political Economy*, Vol. 82, No. 2, Part 2: Marriage, Family Human Capital, and Fertility, pp. S136-S163

Heckman, J. (1974) Life Cycle Consumption and Labor Supply: An Explanation of the Relationship betweenIncome and Consumption Over the Life Cycle *The American Economic Review*, Vol. 64, No. 1 (Mar., 1974), pp. 188-194

Heckman, J. (1974) Review- Problems and Issues in Current Econometric Practice by Karl Brunner. *Journal of Economic Literature*, Vol. 12, No. 4 (Dec., 1974), pp. 1342-1343

Heckman, J. (1974) Shadow Prices, Market Wages, and Labor Supply *Econometrica*, Vol. 42, No. 4 (Jul., 1974), pp. 679-694

Heckman, J. (1976) A Life-Cycle Model of Earnings, Learning, and Consumption *Journal of Political Economy*, Vol. 84, No. 4, Part 2: Essays in Labor Economics in Honor of H. Gregg Lewis (Aug., 1976), pp. S11-S44

Heckman, J. (1976) The Common Structure of Statistical Models of Truncation, Sample Selection and Limited Dependent Variables and a Simple Estimator for Such Models. *NBER Annals of Economic and Social Measurement*, Volume 5, number 4

Heckman, J. (1978) A Partial Survey of Recent Research on The Labor Supply of Women *The American Economic Review*, Vol. 68, No. 2, Papers and Proceedings of the Ninetieth Annual Meeting of the American Economic Association (May, 1978), pp. 200-207

Heckman, J. (1978) Dummy Endogenous Variables in a Simultaneous Equation System *Econometrica*, Vol. 46, No. 4 (Jul., 1978), pp. 931-959

Heckman, J. and Willis, R. (1975) A Beta-logistic Model for the Analysis of Sequential Labor Force Participation by Married Women. *NBER Working Paper* 112

Heckman, J. and Willis, R. (1977) A Beta-logistic Model for the Analysis of Sequential Labor Force Participation by Married Women *Journal of Political Economy*, Vol. 85, No. 1 (Feb., 1977), pp. 27-58

Heckman, J. and Willis, R. (1979) The Distribution of Lifetime Labor Force Participation of Married Women: Reply to Mincer and Ofek. *Journal of Political Economy*, Vol. 87, No. 1 (Feb., 1979), pp. 203-211

Heckman, J. and Wolpin, K. (1976) Does the Contract Compliance Program Work? An Analysis of Chicago Data *ILR Review*, Vol. 29, No. 4 (Jul., 1976), pp. 544-564

Heckman, J. (1976) Introduction. *NBER Annals of Economic and Social Measurement*, Volume 5, number 4

Ashenfelter's papers

Abbot, M. and Ashenfelter, O. (1976) Labour Supply, Commodity Demand and the Allocation of Time *The Review of Economic Studies*, Vol. 43, No. 3 (Oct., 1976), pp. 389-411

Ashenfelter and Heckman (1974) - Measuring the Effect of Antidiscrimination Program. *Working Paper (Princeton Industrisl relations Section)* 50.

Ashenfelter, O (1978) What Is Involuntary Unemployment? *Proceedings of the American Philosophical Society*, Vol. 122, No. 3 (Jun. 9, 1978),pp. 135-138

Ashenfelter, O. (1970) Changes in Labor Market Discrimination Over Time. *The Journal of Human Resources*, Vol. 5, No. 4 (Autumn, 1970), pp. 403-430

Ashenfelter, O. (1971) The Effect of Unionization on Wages in the Public Sector: The Case of Fire Fighters *ILR Review*, Vol. 24, No. 2 (Jan., 1971), pp. 191-202

Ashenfelter, O. (1972)Racial Discrimination and Trade Unionism *Journal of Political Economy*, Vol. 80, No. 3, Part 1 (May - Jun., 1972), pp. 435-464

Ashenfelter, O. (1973). Child Quality and the Demand for Children: Comment *Journal of Political Economy*, Vol. 81, No. 2, Part 2: New Economic Approaches toFertility (Mar. - Apr., 1973), pp. S96-S98

Ashenfelter, O. (1975) Manpower Training and Earnings. *Monthly Labor Review*, Vol. 98, No. 4 (April 1975), pp. 46-48

Ashenfelter, O. (1976) A Process Evaluation of the Contract Compliance Program in Nonconstruction Industry: Comment. *ILR Review*, Vol. 29, No. 4 (Jul., 1976), pp. 577-580

Ashenfelter, O. (1977) A Model of Unemployment Insurance and the Work Test]: Comment. *ILR Review*, Vol. 30, No. 4 (Jul., 1977), pp. 467-468

Ashenfelter, O. (1977) Will the Real Conventional Theory of Income Distribution Please Stand Up? *Social Science Quarterly*, Vol. 58, No. 1 (JUNE, 1977), pp. 147-150

Ashenfelter, O. (1978) Estimating the Effect of Training Programs on Earnings. *The Review of Economics and Statistics*, Vol. 60, No. 1 (Feb., 1978), pp. 47-57

Ashenfelter, O. and Ham, J. (1979) Education, Unemployment, and Earnings *Journal of Political Economy*, Vol. 87, No. 5, Part 2: Education and IncomeDistribution (Oct., 1979), pp. S99-S116

Ashenfelter, O. and Heckman, J. (1974) The Estimation of Income and Substitution Effects in a Model of Family Labor Supply *Econometrica*, Vol. 42, No. 1 (Jan., 1974), pp. 73-85

Ashenfelter, O. and Johnson, G. (1969) Bargaining Theory, Trade Unions, and Industrial Strike Activity *The American Economic Review*, Vol. 59, No. 1 (1969), pp. 35-49

Ashenfelter, O. and Johnson, G. (1972) Unionism, Relative Wages, and Labor Quality in U.S. Manufacturing Industries *International Economic Review*, Vol. 13, No. 3 (Oct., 1972), pp. 488-508

Ashenfelter, O. and Kelley, S (1975) Determinants of Participation in Presidential Elections *The Journal of Law & Economics*, Vol. 18, No. 3, Economic Analysis of PoliticalBehavior: Universities-National Bureau Conference Series Number 29 (Dec., 1975), pp. 695-733

Ashenfelter, O. and Mooney, J. (1969) Some Evidence on the Private Returns to Graduate Education *Southern Economic Journal*, Vol. 35, No. 3 (Jan., 1969), pp. 247-256

Ashenfelter, O. and Peirce, W. (1966) Industrial Conflict: The Power of Prediction *ILR Review*, Vol. 20, No. 1 (Oct., 1966), pp. 92-95

Ashenfelter, O. and Pencavel, J. (1969) American Trade Union Growth: 1900-1960 *The Quarterly Journal of Economics*, Vol. 83, No. 3 (Aug., 1969), pp. 434-448

Ashenfelter, O. and Pencavel, J. (1975) Wage Changes and the Frequency of Wage Settlements *Economica*, New Series, Vol. 42, No. 166 (May, 1975), pp. 162-170

Ashenfelter, O. and Smith, R. (1979) Compliance with the Minimum Wage Law *Journal of Political Economy*, Vol. 87, No. 2 (Apr., 1979), pp. 333-350

Appendix 4 – Related Network Definition

A related network is built using documents that cite at least one reference of the source document (https://images.webofknowledge.com/images/help/WOS/hp_related_records.html). Web of Science allows to search for related records, finding papers which cite at least one item of the cited references list of a paper. Thus, for instance, Heckman's related network concerns the papers which cited at least one reference of his papers from 1971 to 1979. However, papers after the year of publication also appear as related, but they were excluded from the research given that the intention was to observe with what and who the research resembled before publishing the papers.

The search, hence, was realized in the following manner. All Heckman's paper from 1971 (his first publication) to 1979 (year of the self-selection model) were selected. In the sequence, the cited references were observed and searches for related records were realized. Finally, documents that were outside the timespan were excluded. For 1978 papers, all records after 1978 were excluded. For 1975 papers, all records after 1975 were excluded and so on. The result was a network of related records especially suited to determine how the researches developed, demonstrating with what and who the research resembled up to that point in time. This offers an indication of who were the researchers involved with the same concerns and in what directions the researches were going.

The visualizations intend to demonstrate the evolution of the networks. Therefore, 1971 represent the (before 1971) related records of the author's 1971 papers. 1972 records represent the (before 1972) related records of the author's 1972 and 1971 papers. 1973 records represent the (before 1973) related records of the author's 1973, 1972 and 1971 papers and so on. The related networks, thus, are cumulative.

Related networks, instead of a generational citation network (the references of the references of a paper), are wider and aggregate more papers. Thus, they may offer a clearer picture of possible works, authors and journals involved in a research agenda. Moreoover, for the period in question, citation patterns were different from current practices. As a result, generational citation networks yield small datasets that hamper a conclusive analysis.

Appendix 5 – Related Network Tables

Table 1 – Heckman's Network - Most Relevant Articles in selected Journals

Title	Author	Journal	Year	n
ANALYSIS OF CONTINGENCY-TABLES WITH INCOMPLETELY CLASSIFIED DATA	CHEN T;FIENBERG SE	BIOMETRICS	1976	3
ANALYSIS OF MULTIDIMENSIONAL CONTINGENCY TABLES WHEN SOME VARIABLES ARE POSTERIOR TO OTHERS - MODIFIED PATH ANALYSIS APPROACH	GOODMAN LA	BIOMETRIKA	1973	3
EMPIRICAL BAYES POINT ESTIMATES OF LATENT TRAIT SCORES WITHOUT KNOWLEDGE OF TRAIT DISTRIBUTION	MEREDITH W;KEARNS J	PSYCHOMETRIKA	1973	3
IMPROVED LIKELIHOOD RATIO TESTS FOR COMPLETE CONTINGENCY-TABLES	WILLIAMS DA	BIOMETRIKA	1976	3
LOG-LINEAR MODELS FOR FREQUENCY TABLES WITH ORDERED CLASSIFICATIONS	HABERMAN SJ	BIOMETRICS	1974	3
STEPWISE PROCEDURE FOR 2 POPULATION BAYES DECISION RULES USING DISCRETE VARIABLES	LACHIN JM	BIOMETRICS	1973	3
2-WAY CONTINGENCY-TABLES FOR COMPLEX SAMPLING SCHEMES	SHUSTER JJ;DOWNING DJ	BIOMETRIKA	1976	2
ALLOCATION OF RESOURCES IN POPULATION SCREENING - DECISION THEORY MODEL	GOLDSTEIN H	BIOMETRICS	1972	2
ANALOGIES BETWEEN MULTIPLICATIVE MODELS IN CONTINGENCY-TABLES AND COVARIANCE SELECTION	WERMUTH N	BIOMETRICS	1976	2
ANALYSIS FOR COMPOUNDED FUNCTIONS OF CATEGORICAL DATA	FORTHOFER RN;KOCH GG	BIOMETRICS	1973	2
ANALYSIS OF CATEGORICAL DATA FROM MIXED MODELS	KOCH GG;REINFURT DW	BIOMETRICS	1971	2
ANALYSIS OF INCOMPLETE MULTI-WAY CONTINGENCY TABLES	FIENBERG SE	BIOMETRICS	1972	2
APPROXIMATION TO MAXIMUM MODULUS OF TRIVARIATE-T WITH A COMPARISON TO EXACT VALUES	DUTT JE;MATTES KD;SOMS AP;TAO LC	BIOMETRICS	1976	2
BOUNDS FOR DISTRIBUTION FUNCTIONS OF EXTREME LATENT ROOTS OF A SAMPLE COVARIANCE MATRIX	MUIRHEAD RJ	BIOMETRIKA	1974	2
CLASSIFICATION OF MULTIDIMENSIONAL CONTINGENCE TABLES	VICTOR N	BIOMETRICS	1972	2
ECONOMETRICS AND PSYCHOMETRICS - SURVEY OF COMMUNALITIES	GOLDBERGER AS	PSYCHOMETRIKA	1971	2
EFFECTS OF COLLAPSING MULTIDIMENSIONAL CONTINGENCY TABLES	BISHOP YMM	BIOMETRICS	1971	2
GENERAL MODEL FOR FREE-RESPONSE DATA	SAMEJIMA F	PSYCHOMETRIKA	1972	2
INTERACTION TESTS FOR 2 BY S BY T CONTINGENCY TABLES	GART JJ	BIOMETRIKA	1972	2
LINEAR MODEL ANALYSIS OF CATEGORICAL DATA WITH INCOMPLETE RESPONSE VECTORS	KOCH GG;REINFURT DW;IMREY PB	BIOMETRICS	1972	2
METHODS FOR EVALUATING EMPIRICAL BAYES POINT ESTIMATES OF LATENT TRAIT SCORES	KEARNS J;MEREDITH W	PSYCHOMETRIKA	1975	2
MIXED QUASI-INDEPENDENT MODELS FOR CATEGORICAL DATA	GAIL MH	BIOMETRICS	1972	2
MODEL SEARCH AMONG MULTIPLICATIVE MODELS	WERMUTH N	BIOMETRICS	1976	2
MULTIVARIATE PAIRED COMPARISONS - EXTENSION OF A UNIVARIATE MODEL AND ASSOCIATED ESTIMATION AND TEST PROCEDURES	DAVIDSON RR;BRADLEY RA	BIOMETRIKA	1969	2
PROPERTIES OF DIAGNOSTIC DATA DISTRIBUTIONS	DAWID AP	BIOMETRICS	1976	2
SOME MODELS FOR INDIVIDUAL-GROUP COMPARISONS AND GROUP BEHAVIOR	FIENBERG SE;LARNTZ FK	PSYCHOMETRIKA	1971	2

STATISTICAL DISTRIBUTIONS IN UNIVARIATE AND MULTIVARIATE EDGEWORTH POPULATIONS	DAVIS AW	BIOMETRIKA	1976	2

Table 2 – Ashenfelter's Network - most Relevant Articles in selected Journals

Title	Author	Journal	Year	n
POST-WAR CONSUMPTION IN CANADA - A 1ST LOOK AT THE AGGREGATES	POWELL A	CANADIAN JOURNAL OF ECONOMICS \& POLITICAL SCIENCE	1965	3
AN INTERTEMPORAL ANALYSIS OF NATURE OF DEMAND FOR FOOD PRODUCTS	HARMSTON FK;HINO H	AMERICAN JOURNAL OF AGRICULTURAL ECONOMICS	1970	2
COMMUNITY HEALTH FACILITIES AND SERVICES - MANPOWER DIMENSIONS	BALL DS;WILSON JW	AMERICAN JOURNAL OF AGRICULTURAL ECONOMICS	1968	2
CONTROL THEORY FOR AGRICULTURAL POLICY - METHODS AND PROBLEMS IN OPERATIONAL MODELS	BURT OR	AMERICAN JOURNAL OF AGRICULTURAL ECONOMICS	1969	2
DISCUSSION OF BOUTWELL,WK	HALLBERG MC	AMERICAN JOURNAL OF AGRICULTURAL ECONOMICS	1968	2
ECONOMETRIC ANALYSIS OF SHRIMP EX-VESSEL PRICES, 1950-1968	DOLL JP	AMERICAN JOURNAL OF AGRICULTURAL ECONOMICS	1972	2
EMPIRICAL STUDIES OF DEMAND	HOOD WMC	CANADIAN JOURNAL OF ECONOMICS \& POLITICAL SCIENCE	1955	2
ESTIMATING DEMAND FOR AN ON-FARM RECREATIONAL SERVICE	MILAM RL;PASOUR EC	AMERICAN JOURNAL OF AGRICULTURAL ECONOMICS	1970	2
ESTIMATION OF DEMAND FOR FOOD AND OTHER PRODUCTS ASSUMING ORDINALLY SEPARABLE UTILITY	BOUTWELL WK;SIMMONS RL	AMERICAN JOURNAL OF AGRICULTURAL ECONOMICS	1968	2
ESTIMATION OF DEMAND PARAMETERS UNDER CONSUMER BUDGETING - APPLICATION TO ARGENTINA	JANVRY AD;BIERI J;NUNEZ A	AMERICAN JOURNAL OF AGRICULTURAL ECONOMICS	1972	2
GINI RATIOS - SOME CONSIDERATIONS AFFECTING THEIR INTERPRETATION	BENSON RA	AMERICAN JOURNAL OF AGRICULTURAL ECONOMICS	1970	2
INSTITUTIONS AND RISING ECONOMIC VALUE OF MAN	SCHULTZ TW	AMERICAN JOURNAL OF AGRICULTURAL ECONOMICS	1968	2
MATHEMATICAL POLITICAL THEORY	TAYLOR M	BRITISH JOURNAL OF POLITICAL SCIENCE	1971	2
ON PROBLEM OF DEGREES OF FREEDOM IN ANALYSIS OF CONSUMER BEHAVIOR	DEJANVRY A;BIERI J	AMERICAN JOURNAL OF AGRICULTURAL ECONOMICS	1968	2
PRICES AND DEMANDS FOR INPUT CHARACTERISTICS	LADD GW;MARTIN MB	AMERICAN JOURNAL OF AGRICULTURAL ECONOMICS	1976	2
SCHOOLING AND AGRICULTURAL MINIMUM-WAGE	GALLASCH HF;GARDNER BL	AMERICAN JOURNAL OF AGRICULTURAL ECONOMICS	1978	2
SIMULTANEOUS-EQUATION MODEL OF SPATIAL EQUILIBRIUM AND ITS APPLICATION TO BROILER MARKETS	LEE TC;SEAVER SK	AMERICAN JOURNAL OF AGRICULTURAL ECONOMICS	1971	2
SOME SUGGESTIONS FOR DEVELOPING NEW MODELS FROM EXISTING MODELS	HAIDACHER RC	AMERICAN JOURNAL OF AGRICULTURAL ECONOMICS	1970	2

Table 3 – Heckman's Network - most Relevant Articles of selected Authors

Title	Author	Journal	Year	n
A THRESHOLD REGRESSION MODEL	DAGENAIS MG	ECONOMETRICA	1969	3
ANALYSIS OF MULTIDIMENSIONAL CONTINGENCY TABLES WHEN SOME VARIABLES ARE POSTERIOR TO OTHERS - MODIFIED PATH ANALYSIS APPROACH	GOODMAN LA	BIOMETRIKA	1973	3
MAXIMUM LIKELIHOOD, MINIMUM CHI-SQUARE AND NONLINEAR WEIGHTED LEAST-SQUARES ESTIMATOR IN GENERAL QUALITATIVE RESPONSE MODEL	AMEMIYA T	JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION	1976	3
MODIFIED MULTIPLE REGRESSION APPROACH TO ANALYSIS OF DICHOTOMOUS VARIABLES	GOODMAN LA	AMERICAN SOCIOLOGICAL REVIEW	1972	3
MULTIVARIATE REGRESSION AND SIMULTANEOUS EQUATION MODELS WHEN DEPENDENT VARIABLES ARE TRUNCATED NORMAL	AMEMIYA T	ECONOMETRICA	1974	3
NOTE ON THE TREATMENT OF CERTAIN PROBLEMS OF DISCONTINUITY IN MULTIPLE-REGRESSION	DAGENAIS MG	BULLETIN OF THE INTERNATIONAL STATISTICAL INSTITUTE	1963	3
QUALITATIVE RESPONSE MODELS	AMEMIYA T	ANNALS OF ECONOMIC AND SOCIAL MEASUREMENT	1975	3
REGRESSION-ANALYSIS WHEN DEPENDENT VARIABLE IS TRUNCATED NORMAL	AMEMIYA T	ECONOMETRICA	1973	3
THE SHORT-RUN DETERMINATION OF OUTPUT AND SHIPMENTS IN THE NORTH AMERICAN-NEWSPRINT PAPER-INDUSTRY	DAGENAIS MG	YALE ECONOMIC ESSAYS	1964	3
ANALYSIS OF MULTIDIMENSIONAL CONTINGENCY TABLES - STEPWISE PROCEDURES AND DIRECT ESTIMATION METHODS FOR BUILDING MODELS FOR MULTIPLE CLASSIFICATIONS	GOODMAN LA	TECHNOMETRICS	1971	2
APPLICATION OF A THRESHOLD REGRESSION MODEL TO HOUSEHOLD PURCHASES OF AUTOMOBILES	DAGENAIS MG	REVIEW OF ECONOMICS AND STATISTICS	1975	2
EMPIRICAL BAYES PROCEDURE FOR FINDING AN INTERVAL ESTIMATE	LORD FM;CRESSIE N	SANKHYA-THE INDIAN JOURNAL OF STATISTICS SERIES B	1975	2
GENERAL MODEL FOR ANALYSIS OF SURVEYS	GOODMAN LA	AMERICAN JOURNAL OF SOCIOLOGY	1972	2
GUIDED AND UNGUIDED METHODS FOR SELECTION OF MODELS FOR A SET OF T MULTIDIMENSIONAL CONTINGENCY TABLES	GOODMAN LA	JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION	1973	2
MATHEMATICAL-METHODS FOR THE STUDY OF SYSTEMS OF GROUPS	GOODMAN LA	AMERICAN JOURNAL OF SOCIOLOGY	1964	2
MAXIMUM PRINCIPLES IN ANALYTICAL ECONOMICS	SAMUELSON PA	AMERICAN ECONOMIC REVIEW	1972	2
PARTITIONING OF CHI-SQUARE, ANALYSIS OF MARGINAL CONTINGENCY TABLES, AND ESTIMATION OF EXPECTED FREQUENCIES IN MULTIDIMENSIONAL CONTINGENCY TABLES	GOODMAN LA	JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION	1971	2
RELATIONSHIP BETWEEN 2 STATISTICS PERTAINING TO TESTS OF 3-FACTOR INTERACTION IN CONTINGENCY-TABLES	GOODMAN LA	JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION	1975	2

SIGNIFICANCE TEST FOR A PARTIAL CORRELATION CORRECTED FOR ATTENUATION	LORD FM	EDUCATIONAL AND PSYCHOLOGICAL MEASUREMENT	1974	2
SIMPLE SIMULTANEOUS TEST PROCEDURE FOR QUASI-INDEPENDENCE IN CONTINGENCY TABLES	GOODMAN LA	JOURNAL OF THE ROYAL STATISTICAL SOCIETY SERIES C-APPLIED STATISTICS	1971	2
STATISTICAL-METHODS FOR MOVER-STAYER MODEL	GOODMAN LA	JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION	1961	2

Table 4 – Ashenfelter's Network - most relevant articles of selected Authors

Title	Author	Journal	Year	n
LABOR SUPPLY MODEL FOR SECONDARY WORKERS	WACHTER ML	REVIEW OF ECONOMICS AND STATISTICS	1972	4
A COMPLETE SYSTEM OF CONSUMER DEMAND EQUATIONS FOR AUSTRALIAN ECONOMY FITTED BY A MODEL OF ADDITIVE PREFERENCES	POWELL A	ECONOMETRICA	1966	3
AITKEN ESTIMATORS AS A TOOL IN ALLOCATING PREDETERMINED AGGREGATES	POWELL A	JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION	1969	3
CONSUMER DEMAND-FUNCTIONS UNDER CONDITIONS OF ALMOST ADDITIVE PREFERENCES	BARTEN AP	ECONOMETRICA	1964	3
EVIDENCE ON SLUTSKY CONDITIONS FOR DEMAND EQUATIONS	BARTEN AP	REVIEW OF ECONOMICS AND STATISTICS	1967	3
MULTI-SECTORAL ANALYSIS OF CONSUMER DEMAND IN POST-WAR PERIOD	POWELL AA;VANHOA T	SOUTHERN ECONOMIC JOURNAL	1968	3
POST-WAR CONSUMPTION IN CANADA - A 1ST LOOK AT THE AGGREGATES	POWELL A	CANADIAN JOURNAL OF ECONOMICS \& POLITICAL SCIENCE	1965	3
SYSTEMS OF DEMAND EQUATIONS - AN EMPIRICAL COMPARISON OF ALTERNATIVE FUNCTIONAL FORMS	PARKS RW	ECONOMETRICA	1969	3
EFFICIENT ESTIMATION OF A SYSTEM OF REGRESSION EQUATIONS WHEN DISTURBANCES ARE BOTH SERIALLY AND CONTEMPORANEOUSLY CORRELATED	PARKS RW	JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION	1967	2
ESTIMATING DEMAND EQUATIONS	BARTEN AP	ECONOMETRICA	1968	2
ESTIMATION OF RELATIONSHIPS INVOLVING QUALITATIVE VARIABLES	THEIL H	AMERICAN JOURNAL OF SOCIOLOGY	1970	2
HOW DOES MARGINAL UTILITY OF INCOME CHANGE WHEN REAL INCOME CHANGES	THEIL H;BROOKS RB	EUROPEAN ECONOMIC REVIEW	1970	2
HOW TO WORRY ABOUT INCREASED EXPENDITURES	THEIL H	ACCOUNTING REVIEW	1969	2
MAXIMUM LIKELIHOOD ESTIMATION OF A COMPLETE SYSTEM OF DEMAND EQUATIONS	BARTEN AP	EUROPEAN ECONOMIC REVIEW	1969	2
MAXIMUM LIKELIHOOD ESTIMATION OF LINEAR EXPENDITURE SYSTEM	PARKS RW	JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION	1971	2
NEW APPROACH TO EQUILIBRIUM LABOUR FORCE	WACHTER ML	ECONOMICA	1974	2

NOTE ON A CLASS OF UTILITY AND PRODUCTION FUNCTIONS YIELDING EVERYWHERE DIFFERENTIABLE DEMAND FUNCTIONS	BARTEN AP;KLOEK T;LEMPERS FB	REVIEW OF ECONOMIC STUDIES	1969	2
RELATIVE WAGE EQUATIONS FOR US MANUFACTURING INDUSTRIES 1947-1967	WACHTER ML	REVIEW OF ECONOMICS AND STATISTICS	1970	2
SOME DEVELOPMENTS OF ECONOMIC-THOUGHT IN THE NETHERLANDS	THEIL H	AMERICAN ECONOMIC REVIEW	1964	2
THE DEMAND FOR PRODUCTION FACTORS AND THE PRICE SENSITIVITY OF INPUT-OUTPUT PREDICTIONS	THEIL H;TILANUS CB	INTERNATIONAL ECONOMIC REVIEW	1964	2
THE INFORMATION APPROACH TO DEMAND ANALYSIS	THEIL H	ECONOMETRICA	1965	2
USE OF INFORMATION THEORY CONCEPTS IN ANALYSIS OF FINANCIAL STATEMENTS	THEIL H	MANAGEMENT SCIENCE SERIES A-THEORY	1969	2

Appendix 6 – Interview with Joshua Angrist: 19-05-2020

Interviewer: Arthur Brackmann Netto
Platform: Zoom
Recorder: Easy Recorder
Transcripts: Sonix.ai and minor revisions

[00:00:00] [Greetings and Presentations]

Introduction

Arthur: [00:00:35] So let's start. I'll start with some very broad questions to warm up. So how was being a graduate student during the 80s. I mean, what were the main concerns and problems of econometrics and labor economics? Do you remember something about that?

Angrist: [00:00:56] Yeah, I remember it pretty well. I started graduate school in 1985 and then I was very happy and very, very lucky to get to go to Princeton. I've told the story before. I was a college student at Oberlin College, a little college in Ohio near Pittsburgh, where I grew up. Oberlin has an honors program. The honors program allows college seniors who are majoring in economics to write a thesis. So you feel like a graduate student. You do independent work, you do research, you read journal articles instead of textbooks, and you write a thesis and you get an office. You know, there are no Ph.D. Students at Oberlin College. So, intellectually, you're the elite there if you're an honors student. I was already interested in labor. One of my advisers there, my undergraduate advisor, was a labor economist named Hirschel Kasper. And he was always encouraging me. And so I wrote a thesis called "Sample Selection Bias and the nature of unemployment". It wasn't too focused, but it was loosely motivated by work going on at the time or shortly before in the 70s about the fact that you don't see wages for unemployed workers and you want to estimate a reservation wage. Very much in the spirit of Heckman and Gronau. But I was also interested in modeling labor supply. And then there was this problem that you don't see wages for people who don't work. I was reading a lot of Orley Ashenfelter's papers, so I was reading Heckman and Ashenfelter and other Classic papers - now we know they're classics, I probably didn't realize that - from the 70s and early 80s. But I was also very lucky because Oberlin has an honors program feature where they invite an outside examiner. It's a small college and the faculty there are not well known researchers. They mostly concentrate on teaching and they're good teachers. But at that time, Oberlin was sending a lot of undergraduates to Ph.D. Programs. I don't think that's true so much now. But anyway, long story short, my outside examiner was Orley Ashenfelter and he came to Oberlin for a few days. This must have been in 1982. And so that's when I met him. We got along very well, and he gave me an oral exam and I also had a written exam and my thesis. I had to defend it like a seminar - it was very exciting. Not something many undergraduates get to do. And then afterwards, Orley wrote me a letter saying: if you want to go to graduate school, why don't you come to Princeton? I had other interests at the time. I thought I might do it someday, but in the meantime, I moved to Israel.

Arthur: [00:05:07] Yeah. You went to the army, right?

Angrist: [00:05:09] Well, first I was a Masters student at Hebrew University. I didn't like that program very much, but I liked Israel and I met my wife, the woman who would become my wife. Still my wife. My first wife, Mira. I was there with my best friend from high school. We decided to join the army and become Israeli citizens. We served in the Army for two years.

Then when I was getting out of the army, I had to decide what was I going to do? So my sort of default was to go back to Hebrew University, where I had been a Masters student. And then I would just do my Ph.D. There. But in the spring of 1985, it must've been late winter or early spring, I wrote Orley an old fashioned letter, saying: "remember me? You said I could come to Princeton. Can I still do that?" And he said: "Sure! Come in the fall." That was it. And I said: "well, that's wonderful Orley. But I'm married now..." and he said: "Don't worry about it. I'll give you a job. You'll be fine." Years later, I talked to Dave Card about that, and I said: "Dave, you know the way Orley recruited me to Princeton? I wish I could do that. You know, I wish I could find students and think, well, that's a good student and just tell them to come down to MIT and take care of one"

Arthur: [00:06:49] But that's kind of like Krueger's story, right?

Angrist: [00:06:54] That's very similar. Serendipitous, he met Orley's wife on an airplane.

Arthur: [00:06:59] Yeah.

Angrist: [00:07:02] Gina Ashenfelter knew a lot about our world and she recognized that Alan was very talented when she sat next to him on a flight down to New Orleans for the AEA meetings. And so she made the match with Orley. But Orley was very decisive and he was a good judge of economic talent and motivation. But Card told me: "well, actually, Orley didn't really have the authority to do that!" So that's how I went to Princeton. And I was so happy to be at Princeton. I loved being at Princeton, even though it was very difficult. The studies were difficult. I had been out of school for a while. My Hebrew University experience wasn't very helpful, and then the army, of course, and I didn't really have the math backgrounds or the theory. I thought I was a math major in college, but I wasn't even close to having what was needed. So I had to take some remedial courses and get a tutor. It was a struggle. But I loved labor and I took Orley's (grad labor) class. I actually took it twice. And I got very interested in program and policy evaluation, because a lot of people in the industrial relations section were working on that and I was excited by that. It was empirical and had a conceptual side. It seemed important and fun. And I already knew as an undergraduate that I like doing empirical work. My undergraduate thesis was empirical: I was running regressions and estimating Heckman style structural models, which was more challenging, and I liked the whole empirical enterprise, it was sort of like being in a lab.

Arthur: [00:08:56] So if you go back to your years as a grad student beginning in Princeton, what is the picture of the industrial relations sections that comes to your mind?

Industrial Relations Section as a Student

Angrist: [00:09:11] Well, physically, the IRS wasn't much. I remember it well. You know, it's nicer now. Have you been to Princeton? Did you get there?

Arthur: [00:09:18] No, I was going there now, but I had to postpone my plans probably next year.

Angrist: [00:09:25] Well, now they have a nice building for the economics department and the Industrial Relations Section is part of that. It's an older building that was renovated and it's super comfortable and attractive. But when I was a grad student, the economics program was off in one old building, which wasn't very nice. But the Industrial Relations Section was in the basement of the library, Firestone Library, a big university library, a landmark in

Princeton. You know, like the library at Harvard, it's a big, elegant facility. Scholars come to work there and it has a wonderful industrial relations collection. It has lots of material related to industrial relations, which is why the industrial relations section was there. That must have evolved in the days of Al Rees; you should talk to Orley about that period.

Arthur: [00:10:37] Yeah. Yeah. I'm talking to him frequently to get those points.

Angrist: [00:10:44] Good. I talked to Orley yesterday. He's a great guy.

Arthur: [00:10:50] I'll talk to him Friday. I guess. I hope.

Angrist: [00:10:53] He's a lot of fun to talk to.

Arthur: [00:10:55] Yeah, he is.

Angrist: [00:10:56] He can tell a lot of good stories. When I came, they had enough space for graduate students as well as faculty. Orley had a big office and there were some other faculty offices. Dave Card was there and there were a lot of visitors coming through. And then there were graduate students like me and my classmates included many people who went on to distinguished careers like Janet Currie, who was Janet Neelin at the time, and John DiNardo, Tom Lemieux, and Duane Benjamin. Brian McCall was my office mate. We had visitors that were recent grads like Joe Altonji and Jon Ham. They would pass through and give a seminar and hang out. There were always interesting new people there. And Dave Card was there all the time.

Arthur: [00:12:18] Yeah, I talked to David and he said that he was like there all the time during the night. He didn't get out there. And he said he used to talk to you guys a lot. You remember something about that?

Angrist: [00:12:33] Totally. You know, that was a big part of what was good about it. I'm afraid that the new world that we're heading for won't have that, you know. Even before Corona,, people were not in their offices the way they oce were. But when I was a graduate student, the industrial section was full of people and not random people but people that you could talk to and exchange ideas and discuss maybe the seminar that you saw yesterday or something that was going on in your classes or your work. And there was kind of a table where you could sit. I remember it was elevated. We would sit up there. And David, would maybe be there and you'd say: "David, what do you think of this idea?" And then he would get out some paper and sketch it out for you correctly [laughs].

Arthur: [00:13:36] That's good.

Angrist: [00:13:38] You could always discuss your work with David and also Orley. Orley was not 24/7 in there, but he would come in most days. And also I think Princeton invented the labor lunch.

Arthur: [00:14:01] The labor lunch?

Angrist: [00:14:02] Yeah, I'm pretty sure. You could ask David if he sees it that way (or ask Orley). At MIT now, the labor lunch is a very organized thing. We get 40 or 50 people and we provide lunch. In those days, it was a brown bag lunch. You know what a brown bag is?

Arthur: [00:14:25] Yeah, I know. We had a brown bag seminar at Duke for the History of Economic thought. But it's not 40 or 50 people because history of economics is really smaller. But still, we have one.

Angrist: [00:14:42] And who provides the food?

Arthur: [00:14:43] Oh, I arrived at a bad time because now we have to bring our own brown bags. But they used to provide lunches before.

Angrist: [00:14:53] Well, I think now that there's another recession probably the students will be told to bring their own food. In those days, it wasn't about the food and it was a relatively small group. It was just the Section faculty and grad students. Orley had this big, big table in his office that could seat a dozen people. That's where it was. You'd present your work in progress to those around the table and people discuss it. You know, in those days, it was paper handouts. Not very formal presentations. And it was just an amazing thing. Even now, looking back on it, I think how lucky I was that I ended up there when I did. I would ride my bike or walk up from the graduate student dorms where I lived with my wife and I would be happy to be going up there and spending the day. I would go home in the middle sometimes, but still come back in the evening. I knew Card would be there.

Arthur: [00:16:15] I mean, talking to you guys, it seems that it was a really nice time to be there. And David Card has said that your generation from 85 to nineteen 1990, called you guys the Golden Generation. So what do you remember about your colleagues? And what did you guys do besides working and you know, you lunched together? What did you do in Princeton?

Angrist: [00:16:46] We socialized some, though people had different interests. Orley, as you probably know, loves wine. And there was some occasional wine drinking there. I'm not a wine lover myself. But at the time, I was a beer drinker and we'd go out for beer and pizza sometimes. Faculty and the students would go together, and if there was a seminar speaker, we would take the speaker out. The graduate students were always invited to. That's less common now. When we have a speaker at MIT, more often than not, there are no students that go to take him out. Theres a lot of fretting about fairness in which students get to go, so no one goes. And I think that's too bad.

Arthur: [00:17:47] But you should rotate. I dont know. Bring one or two.

Angrist: [00:17:50] Well, anyway, it's part of a big cultural change. In those days that the industrial relations section was unquestionably a mix of faculty and students. There wasn't this clear dividing line between faculty and students. We, the students, understood that we were faculty in training and we tried to conduct ourselves like the scholars and the faculty we admired. We focused intensely on our research. And we weren't shy about talking to (seminar) speakers. There really wasn't much social distance between the faculty and students.

Arthur: [00:18:36] So do you feel like you could say to Orley or to David that they were wrong about something?

Angrist: [00:18:44] Yeah, I used to argue with Card about things. Usually he was right was right, but not always [laughs]. I also taught for my advisors - that was part of my training. I was a TA for Orley in undergrad metrics, and I was a TA for David in undergrad micro. That was a big part of my learning experience: to not only be their students, but to produce an educational experience with them. I learned a lot from that about how to organize my time and how to conduct myself around undergraduates. I developed my own style, which is somewhat different from theirs. But I still learned a lot from them. That was intense because, you know, TAing was a job: there's a job to do. Lots and lots of undergraduate at Princeton, you know? Give recitations, get their problem sets graded, there's a lot of logistics. I learned how to manage that - it's like, you know, running a little business. I don't know if you've done any t.a. work...

Arthur: [00:20:15] Yeah, I have done some,

Angrist: [00:20:17] Especially if you're doing like a large undergrad class like micro or metrics.

Arthur: [00:20:22] Yeah. Normally in those classes there's lots of work.

Angrist: [00:20:27] Now with the online instruction .... this year, we transitioned to that and we worked hard on it. I think my TAs got a lot out of it and they're benefiting from the fact that I had a good TA experience as a graduate student. I had good role models. I can be a good role model for them. But anyway, yeah, David and I would argue sometimes about things, both research and teaching, and that was fine. There's sort of more distance now. Maybe it's because I'm older [laughs].

Arthur: [00:21:06] And what do you remember about classes? You said you took Orley's class twice. And who taught econometrics at the time?

Angrist: [00:21:18] Well, the classes were very heterogeneous and some were not very good and I even complained once. Much of the core macro and micro classes we're not very well thought out and the instructors did a poor job. Some from inexperience, which is understandable, but some from indifference.

Arthur: [00:21:38] Do you mind saying who they were?

Angrist: [00:21:40] Well, I don't think I really want to dump on people. But, you can look it up if you want [laughs]. But some were good. For example, Hugo Sonnenschein - who went on to be president of University of Chicago and is a distinguished theorist - was a gifted instructor and he taught the most amazing core micro class. That was a standout. I had some wonderful teachers, including Orley and David. Orley taught grad labor. Orley is not the super organized instructor. I am a much more structured instructor. But Orley was full of ideas. He would come in and talk about something that he had seen in the news and say "there's a project there". That's how I ended up studying the draft lottery and doing my Ph.D. thesis. Orley came in one day after having seen a news story about using the draft lottery to study the effects of Vietnam era military service on long-term health. Orley said somebody should do that for their earnings. And I said: "Yeah, I'm going to do that" [laughs]. And I went over to the library and got that journal article and started my thesis. I was a second year student. Orley was full of ideas like that and so was David. I think that also changed. I often have a hard time convincing students that, you know, I have a good project idea.

Arthur: [00:23:51] That's strange...

Angrist: [00:23:53] It is. I don't know where it comes from exactly. Maybe it's the process that gets you into graduate school in our hyper-selective world. On the one hand, you're very talented, but you're also very conservative and risk-averse. Anyway. I liked Orley's class so much that I sat through it twice and I was also very lucky that David taught an applied econometrics class. That was wonderful. You know, not in detail, but in spirit, it's a lot like a class that I teach now for 20 plus years, applied econometrics for the applied micro students. And that was just great. I mean, David is a great teacher on any given day, but the material there was also so tuned to my interests. At the time, David was doing a lot of econometrically sophisticated work that was really on the cutting edge. Key papers of his from that period include Card and Sullivan 88 on training program effects on employment. We also studied Gary Chamberlain's 1980 analysis-of-covariance-with-qualitative-data. And Abowd and Card were exploring method of moment's estimation of models that mix labor supply with a model for measurement. They had two very exciting papers. And then there was the stuff on training programs that I was just super-excited by, a lot of good stuff. LaLonde’s thesis especially, which became LaLonde's 1986 paper on comparing experimental and econometric estimates of training effects. I loved that paper so much. I saw that as a model for the kind of work I wanted to do.

Arthur: [00:26:21] Do you remember if other people at the industrial relations section took Bob LaLonde as an example of work to be done?

Angrist: [00:26:34] Well, Orley and David were working on related problems. Orley had his 1978 paper and he had long been interested in training evaluations. And then he had work written with David – Ashenfelter and Card 85. Were there other graduate students working on it? I don't remember. I'm sure there were. I think I was the one after Bob. Bob wasn't there anymore by the time I got there.

Arthur: [00:27:17] Did you meet him?

Angrist: [00:27:17] Yeah, I think I first met him when he gave a talk. And then when I was on the job market in 1989, the schools I was most likely to go to were Harvard Econ and Chicago business. Bob was one of the big attractions at Chicago. Chicago Business School at the time was just amazing for labor. It had a LaLonde, Kevin Murphy and Bob Topel. I should mention, one other very good class I had was with Angus Deaton. He also taught an Applied Metrics class. It was wonderful; he’s a great teacher. And again, the material was very aligned with my interests. I ended up going in a somewhat different direction. But I learned a lot from him.

Arthur: [00:28:10] It's very interesting to hear from you that you liked Deaton's class because nowadays we know there is a clash between, I don't know, randomisation and structural models. So you like Deaton's class...

Angrist: [00:28:27] Well, Deaton is a great teacher and a great economist. And you can learn a lot from him [laughs]. But also Deaton's views evolved, I think. At that time, there weren't that many randomized trials. Orley and David were promoting the idea that we need randomized trials for training programs. Bob's work was kind of the vanguard of that. But the randomized trial thing gotget to development economics through Esther Duflo, who was my student. And, you know, Deaton has been reacting to her revolution. That's all much later. I'm a 1989 Ph.D., Esther I believe is 99

Arthur: [00:29:29] And J-PAL is only in 2010. Something like that, right?

Angrist: [00:29:36] I don't know the year it was founded. But it's got to be quite a while after Esther got her Ph.D.

Arthur: [00:29:42] Yeah.

Angrist: [00:29:44] Could be 2010.

Arthur: [00:29:49] Yeah. I mean, what it seems to me is that Princeton had a tendency towards natural experiments. That's also because of some circumstances of the period, because there was no money for experiments...

Angrist: [00:30:08] Well, there were some experiments. I remember we had some graduate students testing Rubenstein's bargaining model, I think maybe Orley was working on that. But those were lab experiments.

Arthur: [00:30:21] Yeah, no, I'm talking about like big field experiments.

Angrist: [00:30:24] Yeah. That was hard to do. And graduate students didn't imagine that they could do that themselves. You could analyze the data, say, from the RAND (health insurance experiment) or from the SIME-DIME (Seattle-Denver Income Maintenance Experiment). And Orley had a student, Mark Plant, who was very interested in program evaluation as well. He didn't have an academic career. His thesis was on SIME-DIME and welfare dependence. Ashenfelter & Plant had a nice paper on attrition in SIME-DIME. And Mark had a solo paper on a dynamic model of welfare dependence. But, yeah, we weren't really imagining we were going to do those big field experiments ourselves. Even if we thought that maybe that was the way to go. But we were laying the intellectual foundation

Arthur: [00:31:30] What's interesting is that in the 70s, some economists run big field experiments, but things didn't work out. And then we went through a different road...

Angrist: [00:31:42] I don't know if things didn't work out. I mean, the RAND experiment was important and SIME-DIME was important. But the experiments were probably not very well designed from a data collection point of view. So even though they were randomized trials, they didn't think through issues like: "You know what? It's gonna be hard to find people. There's going to be an attrition problem. If earnings is self-reported, that's a problem. People are going to misrepresent because they want to get a payment." One thing that happened in the JPAL era is that applied micro economists became much better at running randomized trials. And we became more like clinical trialists in the medical world, where there's people who really develop expertise in that. The other thing is - and this is maybe where my work stands out - I got the idea that there are a lot of natural experiments out there where there is a source of random assignment like the draft lottery and later on charter school lotteries and stuff like that, and the work using quarter birth. This could be treated as something between a randomized trial and an observational study. And the key intellectual tool there was instrumental variables. In that part of my story, Alan is important.

Arthur: [00:33:26] We are probably getting there soon.

Angrist: [00:33:28] Yeah, well, Alan himself did many other things. He’s probably not best-remembered for the work with me. But I remember him best for that!

Arthur: [00:33:37] So do you remember if the industrial relations sections or the economic department had relationship with other departments in Princeton?

Angrist: [00:33:50] Not much at that time. Not as far as I could tell. I had a couple friends who were grad students in Psych. But it wasn't an intellectual thing.

Arthur: [00:33:55] And with outsiders, I don't know, NBER, the Labor Department, Something like that or even other, you know?

Angrist: [00:34:01] Orley had actually worked in the US Labor Department. That's how he got interested in training evaluation. The NBER was part of our world mostly because we wanted to participate in those meetings. Rven as a graduate student, I think I was going already to some of the meetings. Maybe I submitted a paper or two. Certainly as an assistant professor at Harvard, I was already involved with that. But the NBER was important to us. It is a center of empirical work. I think we had a little hubris: we thought that at Princeton we did better empirical work than the NBER crowd and especially better than Harvard. But I ended up going to Harvard! [laughs].

Arthur: [00:35:04] And Alan came from Harvard right?

Angrist: [00:35:08] Yeah, Alan got his PhD from Harvard. So, I don't want to make too big a deal out of the case against Harvard [laughs]. Hank Farber at MIT was also an important person in the universe of industrial relations section graduates.

Arthur: [00:35:36] So Orley was your advisor for the Ph.D.?

Angrist: [00:35:41] Yeah, I had three advisers. Orley, Dave Card, and Whitney Newey. I was very lucky that Whitney was an assistant professor at Princeton during the years I was a graduate student. On the technical side, I got a lot of training from Whitney. I used to spend hours with him. He wasn't in the Industrial Relations Section. He was in the economics building. I would walk over there many afternoons and hang out with Whitney. We shared a love for ice cream, so we'd go get ice cream [laughs]. We'd work problems together or he would just explain things to me.

Thesis

Arthur: [00:36:32] So how do you think your research fit to the whole package of labor economics at the time? What do you think about it?

Angrist: [00:36:48] What do you mean?

Arthur: [00:36:49] I mean, was it different from what other people were doing? What do you felt about it?

Angrist: [00:36:59] Well, I guess if you had to say what my big idea was, I'd say I had a sort of an epiphany. When I started working on the draft lottery, I didn't think of it as instrumental variables, I had not thought about instrumental variables at all. I thought, well, here's this problem. There's some randomness. And what I want to do is compare people. Let's try to get their lottery numbers. That was my problem. And then there was the question of: "well, how am I going to get lottery numbers because that depends on birthdays and birthdays are not easily obtained" So I'm going to need some data that has exact birth dates.

So I started thinking about what i could do with sample means. And I remember I worked it out on like a big piece of graph paper. I worked it out very slowly, step by step. I realized that if I just knew what the average earnings and the average probability of service for each birthday, I wouldn't need the micro data, I could just run the regression using the averages. So I proved that and showed it to David. We discussed it. "What would be the assumptions that you would need for that to work?" And it's clear that you needed to believe that the averages converged to the population means. And once I kind of thought that through, I realized, well, that's what instrumental variables do, so this must be an IV procedure. And that led me to my 1991 paper on labor supply, which uses averages to estimate labor supply models. But that was also based on something Orley had published in 1984. He had said you could use averages to estimate intertemporal substitution effects. But he had not made the connection with IV. Then I kind of figured out all these connections as part of my thesis work and that led me to sort of specialize in the econometrics of IV. And I got a lot of the more detailed technical side of that by talking to Whitney as well as David. Eventually I worked out the relevant theory. And then I got entranced with the idea that there are probably a lot of instruments that are useful. And I started to keep an eye out for them.

Arthur: [00:39:40] Do you think that before your connection with IV, when people talked about randomisation or talked about, I don't know, Orley's method the difference and difference. They didn't make the connection?

Angrist: [00:39:54] diff and diff is not IV. I don't think Orley had figured out that what the New England Journal guys were doing with the draft lottery was IV. And in fact, Angus Deaton had a paper on group data circa 1985, in Journal of Econometrics, which is essentially the estimation procedure I was proposing, but he didn't connect with IV in that paper either.

Krueger

Arthur: [00:40:34] So all this was happening in 86 to 89 and Alan arrived in Princeton in 87. Right?

Angrist: [00:40:47] Yeah.... Yes. The Deaton paper is called "panel data from a time series of cross sections" and it's cited in my 1991 paper. That's my first publication. I think I was still a graduate student when I wrote it.

Arthur: [00:41:07] Yeah, I know all of them [laughs].

Angrist: [00:41:11] And yeah, I was very proud of that. It was hard to get that, you know, accepted. And I worked hard on it. And I had some other papers that had been rejected already. So it was good to have success... Publication has only gotten harder over the years I feel. Did you see the essay I wrote about Alan kind of tracing our history?

Arthur: [00:41:41] The 2001 paper?

Angrist: [00:41:49] I wrote an essay, posted on marginal revolution blog, on the one year anniversary of Alan's death.

Arthur: [00:41:54] Oh, yeah. Yeah, I've read it.

Angrist: [00:41:56] And I gave the history a little bit.

Arthur: [00:41:59] Yeah. Yeah. And of this was posted on March 23.

Angrist: [00:42:02] Yeah. Yeah. You saw that? I saw some.

Arthur: [00:42:07] I mean, Orley sent me.

Angrist: [00:42:10] What's that?

Arthur: [00:42:11] Orley sent me this essay.

Angrist: [00:42:15] The post tells the story of my times with Alan. It has all the dates when Alan came and stuff. I don't know if we need to go through it.

Arthur: [00:42:31] I have just a small question. I know that Alan went to Princeton (for his first job) because it seems that Ashenfelter had a proposal from M.I.T., so they were looking for someone. And then Alan got there. Do you know something about that?

Angrist: [00:42:54] I don't. It sounds vaguely familiar. You should ask either Card or Farber.

Arthur: [00:43:00] Yes. Because my point is that it seems that they were renewing the labor in M.I.T....

Angrist: [00:45:10] All right.

Arthur: [00:45:12] So I was talking about the fact that M.I.T. was renewing its labor researchers in late 80s and you got there in mid 90s. So can you tell me something about that?

Angrist: [00:45:30] Well, I got my Ph.D. in 89. Actually, MIT didn't offer me a job when I was on the job market. I think it's the only school I talked that did not. So it's interesting that I ended up spending most of my career there. I had a disastrous job talk there. Didn't go well.

Arthur: [00:45:55] Yeah?

Angrist: [00:45:56] I remember feeling deflated, disappointed in my own performance.

Arthur: [00:46:06] But it was a topic hard for you or something?

Angrist: [00:46:10] No. It was my job talk. It was the draft lottery paper. It did not go well there. It went very badly. You know, I didn't know they were thinking of hiring Orley. That's possible. It's likely. Hank surely would have wanted that. But then I went to Harvard for two years. And then I moved back to Israel to be a faculty member at Hebrew U for five years and I didn't come to MIT till 96. But I did visit MIT in 94.

Professional Life

Arthur: [00:46:50] And during this period, I know that you wrote some papers with Alan. How you communicated with him?

Angrist: [00:46:59] Well, the work with Alan started when I was a graduate student. And we wrote the quarter-of-birth papers. On on the returns to schooling in the QJE and the two-sample IV paper in the JASA. And then Alan and I also started a project on the effects of World War Two military service. That paper took a long time to get published, partly because we became distracted by the other quarter-birth papers and partly because it was rejected in the second round (which is always devastating), at the AER. Anyway, Alan and I were able to communicate. We had e-mail and we had faxes. I would come in the summer and spend time in America. Alan visited me in Israel.

Arthur: [00:48:01] Yet another you talked about emails. Normally people talk a lot about the computer center at Princeton. Do you remember something about the computer?

Angrist: [00:48:10] When I was a graduate student, the computer center is where your computing was done. And also when I was an assistant professor at Harvard, there was a computer center in the basement of Science Center, a building in Harvard next to Littauer. Princeton had a computer center on Prospect Street. And that was, you know, a few blocks or a block or so away from the main quad, and you would run mostly you would run your jobs over there, especially if they involve magnetic tape. So larger data sets were on magnetic tape. Have you ever seen the big nine-track reels?

Arthur: [00:48:50] Yeah, I have seen.

Angrist: [00:48:51] Yeah, I used to keep those as a souvenir, but eventually we had to move our offices and they're very bulky so I got rid of them. But, you know, that was sort of a thing that we had to know how to get the information off the tape. You'd have to load the tape up. In Harvard you loaded the tape on the drive yourself. So you had to learn how to thread it and get it to work and make sure it didn't get stuck and stuff. In Princeton there were machine room operators, people who worked in a clean machine room and you would hand on the tape and you could store the tape there and the tapes were identified by numbers. You could send them a message like a text message to load up a particular tape. And you had to tell them whether you were just reading the tape or writing to the tape, because if you were writing to the tape... In those days, these are IBM type machines, even thoug they weren't all made by IBM. But it was an IBM standard. There was something called a right ring and that was designed to prevent you from mistakenly erasing tape or writing over. So you had to put an actual plastic ring inside the tape to enable 'write'. So there was this whole process that was kind of cumbersome of using magnetic tape. Then we had high capacity disks that we've paid a lot of money for. I remember at Princeton, we called them res-pacs. I don't know why. We leased these essentially, and they were thought of as massive storage. I think they had 500 megabytes of storage. We paid a lot of money to have room on those or to own one or lease one. And that was a big benefit to graduate students could use that for their research. And I have tons of data on the IRS220 res-pac for years. Eventually, I get it off of there.

Arthur: [00:51:01] And when did you start to use a personal computer? Do you remember?

Angrist: [00:51:07] For computing?

Arthur: [00:51:08] Yeah.

Angrist: [00:51:10] I mean, as a graduate student, I was using personal computers for word processing. But not actual statistical computing. I was trained to use mainframe computers and so that's what I did. I used SAS and SAS was very much built around mainframes. You could run SAS on a PC, but you didn't want to do that if you were in a hurry. I was slow to make the transition. When I moved to Harvard, I used a VAX mini-computer. It was not a desktop, but it was a smaller, cheaper machine than an IBM mainframe. And then at Hebrew University, they also had VAXes. And it wasn't really, I think, till late 90s that I started in w/PCs. Even then, I'm not sure [laughs] when I made the transition.

Arthur: [00:52:25] So when you were at Harvard and at Hebrew University, you kept researching about IVs and communicating with people in Princeton or you were doing your own thing there?

Angrist: [00:52:40] No, I continued to work with Alan and I wrote eight papers with Alan. Some of them were written by the time I was at MIT, like the Journal of Economic Perspectives survey of IV. I was already a professor at MIT in 2001. The one Alan and Imbens and I wrote on IV was probably also done when I was at MIT. Well, the working paper was 1995. But I was on sabbatical that year at MIT so I probably saw a lot of Alan, and Guido was at Harvard at the time. I think - no, by then Guido was at UCLA. But anyway, I Guido and I were good friends, and we were always in touch with each other.

Arthur: [00:53:41] Well, when you met Guido?

Angrist: [00:53:47] I was only at Harvard for two years. He came in my second year as an assistant professor. And I have to thank Gary Chamberlain for that. Gary was my closest colleague. Unfortunately, Gary died a few months ago. Very sad. Gary had a big influence on me. I taught an econometrics class with him at Harvard and that was a big part of developing my thinking about how econometrics should be taught. I kind of do it somewhat differently now. But I learned a lot from doing it with Gary. One thing we did is a mixed graduate-undergraduate course, and that's something I still like to do. But, anyway, Guido came to Harvard. I was two academic years there (1989-90 and 90-91). Then I left for Israel in the summer of 91. Guido and I overlapped that one year. But that was enough for us to begin working together. He got very interested in the IV stuff that I was doing as applications, and we started to talk about the theory and how to interpret it. We knew there was a problem. It was not clear what to make of IV with heterogeneous potential outcomes. Heckman had shown there was a problem, but he hadn't solved the problem. I have to thank Gary for bringing Guido to Harvard because I didn't like Guido's job talk. I thought it was boring [laughs]. I didn't think we should hire him. And Gary said: "No, he's good. You'll see." Of course, Gary was right about that [laughs].

MIT

Arthur: [00:55:33] Yeah. It's about experience. He already knew what to look for... So can you tell me a little bit about the M.I.T. department in the 90s. When you arrived there, there was already a labor research group or no?

Angrist: [00:55:53] Well, I visited M.I.T. in 94/95 and then I came as a full time professor in 96. Hank by then was gone. I don't recall when exactly he went to Princeton, but he wasn't there when I came. Michael Piore was active. And Steve Pischke, who I knew from Princeton, was there.

Arthur: [00:56:18] He was your colleague, Right?

Angrist: [00:56:23] Steve and I worked well together as teachers and advisers. We didn't write anything at that time. But later we wrote "Mostly h=Harmless" and "Mastering Metrics". But Steve and I taught together and we were doing that very happily.

Ending – Center of Gravity

Arthur: [00:56:45] So what do you think is the place of MIT and Princeton, in labor economics during the 90s? Because it seems that in the 80s, Princeton was the big place to be. So in the 90s, things started to change. What do you think about that?

Angrist: [00:57:04] I think there's something to that, Arthur. I mean, one thing is, the competition for graduate students seemed to get more intense. So, for whatever reason, there were fewer good students to go around. So it got to be hard to fill all the seats at Princeton and at MIT and Harvard and Chicago, Stanford, Yale. And the way that the seats were being filled was with foreign students. Which is fine. But Labor had always been more of a US field. I guess some fields are seen as more portable like theory and econometric theory. Both micro theory and econometric theory. But maybe also macro. Those seem to appeal to foreign students more than labor. At least at that time. So, you know, there were fewer good students to go around. Then MIT and Harvard seem to have an advantage over Princeton in terms of recruiting Ph.D. Students. So that was part of the shift.

Arthur: [00:58:18] But do you know why? Because in the 80s, it seems that Princeton had this advantage.

Angrist: [00:58:26] I think the shift has to do with the fact that the graduate programs got to be, you know, at least 50 percent foreign. I don't know if that was true in the 80s. You could look into that. You know, there's this thing about fields like public finance and labor, those tend to attract American born Ph.D. Students.

Arthur: [00:58:58] Today, labor is it's still a U.S. field or no?

Angrist: [00:59:09] Well, these things have evolved. But I think what happened is the part of labor that involves foreign countries became development economics. The development economics that you see everywhere around you today didn't exist. And so people who might have done labor end up today doing development. And labor is still, I think, probably disproportionately American students, and because there aren't that many American students to go around, you know, Princeton, is it somewhat of a disadvantage. So that's on the Ph.D. recruiting side. And then Cambridge was doing very well. Harvard and MIT were doing well with faculty recruiting. The center of gravity shifted a little bit towards Cambridge.

Arthur: [01:00:11] So, you arrived at a period when the center of gravity had already changed. Do you think? At MIT...

Angrist: [01:00:20] No, no, no. It wasn't clear. I arrived in Harvard in 89. You mean at MIT?

Arthur: [01:00:28] Yeah.

Angrist: [01:00:30] The change was now visible.

Arthur: [01:00:46] I guess that was pretty much the main questions I had. Is there anything else that you would like to say about labor, about your colleagues, about anything?

Angrist: [01:00:58] I feel very lucky, I've had a great career doing something I love that was a gift given to me by many people.

[01:01:10] [Yes. If I have any other question, can I contact you? Sure. Sure. I mean, I'll probably have because I'm interviewing lots of people now and then initial interview.

[01:01:22] There are new questions that come and maybe in some time I'll get in touch and I'll probably enjoy more.

[01:01:29] I hope you can get there and visit.

[01:01:31] Yeah. Yeah. And I'll probably send you a copy of some of the papers of the.

[01:01:36] I'd like to see it. Hopefully you can come visit in the fall.

[01:01:39] Yeah, hopefully.

[01:01:40] I hope everything gets better. Yeah. Too there. So thank you very much Professor.

[01:01:45] Well, OK. Nice talking with you. Have a nice day. Bye bye. Thank you. Bye.]

Appendix 7 – Interview with Orley Ashenfelter (part I): 08-05-2020

Interviewer: Arthur Brackman Netto
Platform: Zoom
Recorder: Easy Recorder
Transcripts: sonix.ai and minor revisions

[00:05:46] [Greetings and Presentations]

Introduction

Arthur: [00:08:39] Yeah, it's a very strange situation. I mean, I was in the States and I had to come back now in Brazil. OK, so let me present myself. I'm a Ph.D. Student at the University of Sao Paulo. I'm a fellow of the Hope Center at Duke. I mean, I was utill this situation. Now, it's kind of strange because I'm back in Brazil and my research concerns the history of micro econometrics, policy evaluation, applied labor economics, experiments and things like that. Some institutions that I'm interested are Wisconsin's Institute for Research on Poverty, Princeton's industrial relations section, Mathematica, Rand Corporation, the Office of Economic Evaluation, Michigan Survey Research Center and some people that I'm researching are Albert Rees, you, James Hackeman, David Card, Alan Krueger, Joshua Angrist. Some people from Wisconsin like Robert Lampman, Harold Watts, some people from Michigan like James Morgan. My idea is to put all those things in a box and try to explain what's happenedin the 70s and 80s that made micro econometrics of thing. And it's a very spread story that I'm trying to see the links here.

Ashenfelter: [00:10:07] Yeah, it did spread. And, you know, it was surprising to me because it spread to Europe, too. And then after that, to many other places. Yeah.

Arthur: [00:10:20] Yeah, I'm focused on the United States, but I know that it spreads quickly too to Europe too. So my plan is to talk to you about some broad topics on labor, economics and econometrics of the 70s and 80s, some characteristics of the industrial relations sections of the 70s and 80s, and some points about your career as a researcher, supervisor and teacher. Oh, is there something else that you like to say before we start?

Ashenfelter: [00:10:54] No, that's fine. Before. Before we go, it tell me a little bit. I was curious when you mentioned the Hope Center. Is this connected to the archives they have there? What exactly is it?

Arthur: [00:11:05] Yeah. It is connected. I mean, the hope center is the one who administer the economist's papers archives. So they are parts of that. It's a common project of the university, that the Hope Center and the library have there. It's very interesting. I mean, Albert Rees's archives are there, and Gregg Lewis is, too. So I've been there. It's really a great place to be.

Ashenfelter: [00:11:44] I've never actually been there. I might. I think I'd enjoy it.

Arthur: [00:11:47] Yeah, you might. There are pretty interesting things and lots of interesting guys to read there. There's everything you might imagine. I mean, Albert Rees didn't have much, but there are guys where it's like hundreds of boxes of files. So it's lots of information.

Ashenfelter: [00:12:09] Yeah. A very, very broad career because you get much more than just teach at a university. And he taught at more than one university. And Lewis is interesting, too, because, of course, he ended up doing that was the final. The other thing is, of course, back in those days we used to use paper. (laughs)

Early Labor Economics

Arthur: [00:12:30] Yeah (laughs). So let me start with some very broad question. That is, how was being a graduate student during the late 60s and early 70s in labor economics and micro econometrics? I mean, we didn't even had the term micro econometric. So how how was it to be studying something that was not in shape yet?

Ashenfelter: [00:13:02] That's interesting. I went to Princeton, really because of the Industrial relations section. It is quite old. It dates back to the 1920s and it has a history of long term interest in labor problems and active connection in public policy. So people like Richard Lester was actively involved in the creation of the Social Security system and many other things. We used to have a photo with a nice picture of Lester in his prime with Eleanor Roosevelt, he was a friend of Franklin Roosevelt's wife and was very active in social warfare (?). So it was there [IRS]. It was endowed, already had an endowment. So, well financed. J. Douglas Brown really was the founder of the way it is and raised a lot of the money. He ultimately became dean of the faculty and was for a short time Provost at Princeton. He had a lot of connections. There was an interest in it. The other thing you might want to be aware of is that the original foundation is really due the Rockefeller family. I'm going to tell you something that you can try to track down. It would be fascinating if you could. The Rockefeller family owned a massive amount of American industry in the 1920s. We don't know how much of it was done with corporate entities, that it's really unclear how you uncover the ownership structure. [A person you could chat with about this, and you might want to, is Albert Rees's son. He has two sons, both of whom do labor economics. One is at University of Colorado, at Denver, Daniel. The other one is named John. And I think he's at Southern Colorado State University. John is actually a labor historian. That's what he actually does. He's had some connection with how this all works. You know, he knows much more about it than I do. I haven't talked to John for a long time. Dan is very active. Writes a lot of papers. John works in history, so it a different sort of thing.] But the Rockefeller family kind of thought that they had management that was sympathetic to workers. There was a massive strike in a mine in Colorado that resulted in some violence. And they became concerned that a worker meaningful relationships were in bad shape. So they actually founded several industrial relations section. We're not the only one. We're the only one that really remains sort of. They started one in Princeton. Gave money for it. They started one at M.I.T. They started one in Chicago. They started one at Queen's University in Kingston, Ontario, and they started one in Caltech.

Arthur: [00:16:55] That's very interesting.

Ashenfelter: [00:16:57] Yeah, it is interesting. Now, actually, if you do a Google search, you'll find there is still an industrial action section at Cal Tech. I walked by it once. It's in a building. I have no idea what it does. I'm sure there are no faculty. I don't know what they do. The one in Ontario was folded into their public policy school. The one in Chicago, I think, was folded into their business school. And the one at MIT was also forwarded to the Sloan School. Princeton is unusual. We don't have a business school, it could be folded into the Woodrow Wilson School. But it never has been. It's been kept as a separate enterprise. It has a long history of people who were involved with the Induatrial Relation section become the administrators. David Lee, who is one of our faculty members, was until recently the provost at Princeton. So it has a kind of a story. It's got a history that continues. I think will continue long after I'm gone. I really went there for that. And those people were much more traditional labor economists. They did quantitative work, but not very elaborate quantitative work. Now, just as I arrived there, Albert Rees arrived from Chicago and he did more quantitative work. He was just getting into it. He did this book with George Shultz. [George Shultz is still alive, 99 years old, and had a very elaborate career also.] And that book is econometric. It uses microdata, runs regressions in the level of the individual firm. It's hardly ever noted by anyone anymore. But if you look at it, it's a very modern-looking piece of work. And it has some interesting findings in it. It didn't make much of an impact. Partly because when Rees arrived at Princeton, all the empirical work had pretty much been done. The study was of companies in Chicago and by that time, George Shultz had left the university. So he really wasn't actively involved. I think it was 1972 and Richard Nixon appointed him secretary of labor. Here's another story, George Shultz is famous for being secretary of state, secretary of Treasury, but he was also secretary of labor and that was his first job in the Nixon administration. That administration, people do not realize this, was probably the most qualified we have ever had. It was so qualified that they wouldn't even do some of the things Nixon expected them to. (laughs) When Archibald Cox, who was investigating Nixon, when he got too close Nixon told the attorney general at that time - Named (?) Richardson, a the Massachusetts Republican - told him to fire Cox and Richardson said he wouldn't. Instead, he resigned. Donald Ruckelshaus was a deputy attorney general. And so Nixon said: "okay, you fire Archibald Cox." Ruckelshaus resigned. He wouldn't do it. That's the last resignation of any cabinet member. They had such principle that they simply would not do what he asked them to do. The man who did it, amazingly enough, the man who finally fired Archibald Cox was Robert Bork. Wellknown to economists who do anti-trust. And he was nominated to the Supreme Court. He did not get onto the Supreme Court. And I think it's pretty clear a lot of people had a lot of reasons they didn't like - Democrats and Republicans. So, George Schultz, what he did - And this is important for the background of your material - George appointed a remarkable group of people into the Labor Department. That original group was just remarkable. For example, there was a think of this assistant secretary for manpower. He appointed a man called Arnold Weber to that job. Arnold Weber was a labor economist who was at Northwestern University that George knew, who subsequently went back to Northwestern and became the president of Northwestern. All that people George hired in Labor Department. And at one point, George had already left labor Dep., but basically he was responsible for hiring me. I went to work for the Labor Dpt. That's where I got interested in program evaluation. We had access to incredible data. They called me up and said, we'd like to hire you as the director of the Office of Program Evaluation. I said: "what do you need?" I took me down there and they had a guy who had worked for the Social Security industry. He had an incredible dataset. It was just not like anything I've ever seen in my life. What he had was there were training programs that were very provocative.

Arthur: [00:22:40] Just a second. Just a second, Professor. I think I'm not recording. Did you receive something?

[00:22:49] [...]

Ashenfelter: [00:23:54] Rees, of course, was a great friend of George Shultz and colleague at Chicago, and Shultz had put in place this phenomenal group of people, competent people in the Labor Department. I don't think it's ever been like that since. And they hired me and they did it really because this guy, his name was Farber, not my colleague Hank Farber, but David Farber. He had been at the Social Security Administration and at the Labor

Department they wanted to really engage in some evaluation. They were just keen on trying to figure out whether these programs work. That they had adopted some of these programs after the riots in 1968. And people had hoped to find out whether or not they were really increasing the earnings of poor people or poor people who have been displaced. So what happened is this guy had access to the Manpower Administration's data. And the data contained a Social Security number for all these workers. We'd gone through the program. And what this guy, Farber, had done is he had gone to the Social Security Administration [now we do this on a regular basis] and he had obtained from them anonymous but linked data. So it was the first example of these linked data between individual firms and workers. And it was linked in addition to whether they had gone through this training program. So suddenly that was it. That's where difference and differences come from actually. I started doing that work. It was a giant regression and basically it was just a regression with fixed effects for individuals. I generated a pcomparison group from continuous work history sample. We had fixed effects for individuals. We had time effects. Then the interaction between the argument[?] Of the program and the time effect in the post period was the term of the program. What was interesting about is when I tried to present this. So here I am in Washington, now become a bureaucrat. I'm self-serving actually. I've become a bureaucrat and I'm trying to present this to me. Well, I say the word regression, everybody in the room: "What? What are you talking about?". So I decided there had to be a better way to present this and I realized that with a balanced sample, difference and differences was exactly what I was doing. That difference in differences is a regression that has in it time dummies, person dummies and the interaction between who's using the program and what the time period is. So you calculate the difference between the control group and treatment group in the pre-period. You calculate the difference between the treatment and the control group for the post period and then you subtract those differences. It came about basically as a way to explain to ordinary people what I was finding. It's amazing to me now, because I look at papers, hundreds and hundreds of papers and people say we're using the difference in differences method [laughs], but it was never a method. It was a way to display something. It was way to say. It has taken a life of its own. It still does have that advantage of displaying things. It is easy way to display things. Anyway, so it was an interaction. I was at the section then I went to Washington, and then I came back to the section and I brought with me basically the same interests that I'd had at the Labor Department. I was interested in all the different topics, you know, minimum wage - that was something we were supposed to evaluate -, training programs - we were suposed to evaluate them. These were all kind of things that I was supposed to do in the government, and they became things that I wanted to do at the university. And so that was was more or less the start of the way I think about it. Then I think students came along and it was kind of a golden age. There were a lot of students that were learning quantitative methods and suddenly the labor area especially was full of great data. At the same time, the Office of Economic Opportunity was starting off, and that's really connected to Wisconsin. I didn't have any connection to that. That's kind of the Wisconsin example. They actually got something that was then called the Survey of Economic Opportunity, which still surveys as the basis for the PSID. And that was a survey commissioned by the Office of Economic Opportunity. And it was basically just a supplement to the current population survey, that's been deducted now since the 1940s. The interesting thing about that is: you know who the first director of the Office of Economic Opportunity was? Donald Rumsfeld. Donald Rumsfeld was also a friend of George Shultz's. He was a very capable guy. Unfortunately, he went to Iraq and became a leader in Iraq. He pushed it too far. So he's still alive, but he disappeared. He was a congressman from Illinois. Schultz, of course, was at the University of Chicago. So these people are actually all connected. And many of them are connected back to George Shultz.

Arthur: [00:29:56] Ok.

Ashenfelter: [00:29:58] Very surprising the way. I still see George occasionally. Yeah, he's 99 years old.

Arthur: [00:30:06] Maybe I'll interview him.

Ashenfelter: [00:30:09] [off-topic] [I think you could. Yeah. He lives in San Francisco. I have an apartment here down in the [?]. He lives up on the hill. But yeah, you could think it would be interesting. Yeah. I don't know how long you can get with him. Let me see the way to do that. You know, I'll tell you what I'll do. I will e-mail John Taylor. Was it the Hoover Institution so George. I don't know if he actually goes to the Hoover Institution anymore. The last time I saw him, he was in a wheelchair. He's getting he's becoming frail. But I'll tell you, his mind is fine. His mind is just fine. If if the Hoover people could set that up for you, they're interested in archives or know why they're basically, you know, library. What I'll do is I put an e-mail John Taylor, who is a friend of George's and a friend of mine at Hoover. And I'm gonna copy, you know, I'm going to explain that you were interested in this topic, OK? Would it be possible for him to help set up a zoo interview with Jordan? OK, that would be great.]

Arthur: [00:31:19] But before that, I have some other questions for you.

Ashenfelter: [00:31:23] Yeah, I'm sure.

Arthur: [00:31:27] I guess we have to go back a little in time because you went to the 70s and when you went to the Office of Economic Opportunity. But I would like to know a bit about your days in Princeton in the beginning. So could you please elaborate on your relationship with professor Albert Rees? I know that he was a director of the center.

Princeton as a Grad Student (descriptions, Rees, Colleagues, etc.)

Ashenfelter: [00:31:58] He was a teacher of mine. So he came to Princeton. Actually, he came to Princeton about the same time. I did. Yeah, I was a student there. And he moved from Chicago. He took me. I was Amelia's student. So he connected me to the Chicago people, too, in a way, because Albert was actually - Al, they called him Al. Al was highly regarded by people. He'd been chairman of the department at Chicago and he left there really for personal reasons. He got divorced. And turned out he married someone who worked in the office at the department. And it was awkward, I think. So he looked around for another job. W. Bowen, who's also a teacher of mine, was labor economist [his career was really fast. When he finished graduate school in three years and was on the faculty and then he was on the faculty for quite a short time. He wrote papers, quite a few papers and a big book on labor force participation. But he very quickly moved into administration and really never left it] So Bowen really recruited Rees. And I think he recruited him because he thought he would be a good colleague. And then very quickly, Bowen moved on. He was not really a colleague so much anymore, as an administrator of the university.He did a lot of things, Bowen. I mean, if you look now at JSTOR. When he left Princeton, he went to Mellon[?] Foundation and one of the problems that he felt was that the cost of libraries is ridiculous and the cost of trying to keep copies of journals was incredibly expensive. So he started JSTOR.You know, that's why you have it. That's why everybody in the world now has access to all these journals.

Arthur: [00:34:11] Yeah. And what do you remember about your colleagues at the time? I mean, there were some special guys in Labor like Heckmann, John Pencavel and Ronald Oaxaca. Do you remember your relationship with them?

Ashenfelter: [00:34:29] I was a little older. I think Pencavel came as a graduate student and he and I were graduate student colleagues really. He finished fairly quickly. Moved. He was a student of Al Rees and moved on quickly to Stanford. Jim Heckman was there also. Heckman actually followed Rees. So, you may not know this, Heckman was originally a graduate student at Chicago and he flawed his exams. [laughs]

Arthur: [00:35:07] Yeah, I know. I know that. [laughs].

Ashenfelter: [00:35:20] I think it's OK to talk about it now. Back then, he didn't like. Rees moved to Chicago and Heckman decided he wanted to look into moving too. [So I started remembering it.] He came to visit Princeton and I was a student and they put me in touch with him. And we stayed up all night talking about economics, literally all night. So that was the first time I met him. He came as a student a little after I was a student. There were other people there then, like Ron Oaxaca. He was a student. Actually, Ron Oaxaca is retired. He's older than I am. Ron was in the military. His name is it's spelled way OAXACA. Easy to get confused because it's a it's not really a Spanish name, that's an indian name. It's from Mexico. It's an state in Mexico named for the Indians that were there. I should say, the Mexicans or whatever. I don't know what they call the Mexican. We call it native-americans. Native-Whatever-they-are. (laughs). Ron was my first graduate student. I'm very proud of that. He wanted to work. Al Rees said that he should work on the male-female pay differences. And because a guy named Henry Santorum[?] had done that in Chicago before, but no regressions. And Ron was an econometrician. So we sat down together and said: "well, how would you really do this if you were trying to measure pay differences". So that came up. And then he used the survey of economic opportunity to actually use that original dataset for his analysis, as I recall. He might have used the Census Center. I guess it's the 1960 census became available as a form of microdata. That it was a lot of things going on. Computers were becoming way more capable of handling data. Lots of micro data was coming out. There was the census. As a student, I analyzed micro data on individual firms from the Equal Employment Opportunity Commission. They require firms to state how many workers they have from each race group and sex group in occupational categories. And I actually analyze that for the [?]. I was hired by them. This is as graduate student. So a lot of things are happening. And suddenly there were these massive datasets. And then at the same time, we had computers that could deal with. Without the computer none of this could have occurred.

Arthur: [00:38:14] Did Princeton already had the computer center?

Ashenfelter: [00:38:22] Yes. Yes, it did. Mainframe computers. I did all my work at the mainframe computer. Yeah. Yeah. I think that IBM 360, maybe, Pascal. Today, of course, it wouldn't have as much power as my laptop. [laughs] So, yeah, we had a computer center of our own.

Arthur: [00:38:46] Oh, I've talked to David Card this week and he said that computer center was really important for him. And I was wondering whether it was important for you too.

Ashenfelter: [00:38:58] It was critical. Absolutely critical. Without the computer center, I couldn't have done anything. Right. When I analyzed the Equal Employment Opportunity Commission data, it came on tapes. It was employment data for the whole country. I had to pay money actually to use the mainframe computer. They wouldnt let me untill eight o'clock at night and they ran all night long. [laughs]

Arthur: [00:39:22] But something that we'd do in minutes today, seconds maybe. [laughs]

Ashenfelter: [00:39:34] It broke down once and I don't know what happened exactly. And what we did is, we were trying to get tabulation of all these numbers and at that time you learned how to program what you would want. And the thing broke down about halfway through the job, just eight or nine hours of computing. And I had not written any backup system and we didn't have a thing where we saved the data that we'd already accumulated at a certain point like, say, every hour. I hadn't done it. It was all gone. I said: "Oh, no, here we go again". The whole thing all over again. I learned a lesson. We wrote some backups of these so you didn't lose everything. It was crazy. I mean, I used to pay real money. I had money from the government and the Computer Ceter charged me for doing this. My e-mail account, by the way, c6789@princeton, that's my original mainframe account. When they tried to get me to use another name, I said no. I have spent half a million dollars on computer money under that account. I'm keeping that forever.

Microeconometric Problems (Self-selection, Heckman…)

Arthur: [00:40:49] That makes sense. So I know you had some late night talks with Jim Heckman and you published together some papers in the beginning of your career. So what do you remember about those papers and about those talks? Do you remember discussing omitted variables or these kinds of problems in microdata.

Ashenfelter: [00:41:15] You know what? What Jim did, that helped me so much, is Jim was really interested in economic theory. I had two kind of mentors in that regard. Al Rees, who was very good at labor economics, but he was trained in the old school of how you do economic theory, which is basic. I could do it the way they did it. They use graphs, geometry, and they had a way of thinking about the problem in their heads that allowed them to use geometry. They didn't do any calculus or anything like that. So the foundations of economics, that Paul Samuelson wrote, that hadn't really creeped into labor economics. So there were two people that influenced me. One was Jim Heckman and the other was a man named George Johnson. George was visiting us from the University of Michigan. He had gone to Berkeley. He was a product of their industrial relations center, which was a very active place [still there]. But he was a theorist. He really was influenced heavily by Dan McFadden and some other people. So he came to Princeton. In fact, I wrote a paper with him on marketing theory, which was really an eye opener for me. I did the empirical material work. He was not an empirical guy, but he really thought he had a way of thinking about marketing that he could write down in a model. It was really interesting the way he could do it. So he was kind of like a person like Joe Stiglitz. He could think about how to create a formal model or what people could basically understand was a tractable way to do things. So he influenced me and Jim did, too. Jim was especially interested in economics of the household. So he really got me interested in how to use utility theory and demand theory, and I basically filled out my education in that area. Today, if you look back, most of us who came out of that period, Jim Heckman, John Pencavel. David Card, actually, also. Most of us are pretty good theorists. We can actually go to economic theory or teach it. In fact, we all did. I actually taught the graduate students microeconomics at one point. That's not really true anymorewith labor economists. They mostly don't do that. It was kind of Chicago microeconomic theory. So, yeah, he had a big effect on me. He was a very big influence. I think I had a big effect on him. You know, I was very interested in public policy. And Jim was less interested. But I think he became much more interested in public policy as a result of our association, because I would always say: "why? Why are we doing this? We're economists. We don't just study economics for its own sake. It's like engineering. You don't really do it for its own sake. We do it because we're going to try to change something, to improve something, try to understand the way something of the world works." [he uses this all the time now, whn he teach his students]. I always say to a student who wrote a paper on something, I gave the two words: "so what? But why do I care what you did?"

Arthur: [00:45:11] As a graduate student, I have already heard that one before. That's a very useful question to do to graduate students.

Ashenfelter: [00:45:19] It is, yes. We were having an oral exam just before. One of the questions about his paper was the so what part introduction. Right. Which is: why are we doing this? Why should they care about it? I got that really from the government where there were really serious problems to solve, social problems and others.

IRS Institutional Network

Arthur: [00:45:54] And do you remember about the relationship of the industrial relations section and other universities or places like NBER, Mathematica, the Office of Economic Opportunity?

Ashenfelter: [00:46:07] Yeah. Yeah. Al Rees actually was the lead investigator on the negative income tax experiment, which was at Mathematica. He was he was heavily involved in that project. We weren't always connected. For example, Mathematica was a bigger company, the part of it that does the program evaluation is called NPR still around. There was also there were also connections, sort of indirect, with New York. There is a New York firm that specializes in doing randomized trials, cant remember the name right now. [Maybe Brooking's Vermont. What? Brookings Institution.No, it's not Brooking's. It's in New York, actually. It's called. I figure I'll come to me.] It really got started because one of the things that I really did change in Washington, as I came to realize that the data we had was great, the control groups we had were not ideal... So I came to conclude... I wrote a little paper published in the Industrial Relations Research Association. A paper I wrote which had a huge influence on other things. It was a paper published in the Proceedings of the Industrial Relations Research Association about 1972, maybe 1971, it was paper I gave. Basically, what I concluded in the paper was that we needed to do randomized trials. And I think I called it "The case for randomized trials in program evaluation". I did a little sample design work. I showed them what they needed to get in order to do sample design. We had commissioned some work on sample design, and that was just because I felt that the control groups were not convincing enough. So that was extremely early that I proposed these randomized trials. And I left the government. It wasn't anything I could do, but I had pushed the people in the Labor Department, in an entire administration into trying to do these randomized trials. So believe it or not, they did them. They did them on the national support work program, Which was. [Hold on. Give me a second and I'll look up the name company. In fact, they started a company just for this purpose. ... MPRC. That actually stands for -they don't use that the words anymore, but it stands for - Manpower Development Research Corporation. Yet they don't use that manpower anymore.

Arthur: [00:49:57] Ok.

Ashenfelter: [00:49:58] About that. That company was started just to do the randomized trials for the supported work program. But that was really the initiation, probably amongst the very earliest of the trading programs that were done by randomized trials. Before that, there were randomised trials done for negative income tax and also some housing experiments and also some medical care experiments. But that was really the first. And I thought that we needed the randomized trials because I had done so much work with control groups and I just was suspicious that the control groups were not good enough.

Arthur: [00:50:39] OK.

Ashenfelter: [00:50:42] I look at now! Now they have a registry for randomized trials![laughs] There's so many, you register them.

IRS as a faculty member

Arthur: [00:50:51] Yeah. I mean, that's how things are now. So you became part of the faculty in 68, right?

Ashenfelter: [00:51:01] Yes.

Arthur: [00:51:02] And do you remember what were responsibilities and what you did?

Ashenfelter: [00:51:08] Yeah. I teach Woodrow Wilson School. The main thing I remember teaching, with some very good students was in the Woodrow Wilson School, I taught econometrics. So that was perfect for me, because I was doing econometrics, the students in the Woodrow Wilson School were masters degree students and they were interested in public policy. So that was a perfect combination. I wanted to teach you econometrics and i'm interested in public policy these. We put these two together and see if we can figure how to use econometrics to study public policy. So I did that for several years. I also taught in the graduate program. I would have taught the graduate course of labor economics. I think at that time it was only one semester. So I probably taught the whole thing. Now is two semesters. And I teach part of it still. And I would have probably taught undergraduate labor economics sometimes, but that would be the main things I taught. At one time I taught - at least one - Graduate students, first semester micro course. Basically utility theory and demand theory, kind of core chicago-style stuff. [You know, it's it's hard to get. Stanford did that for a while, too. There's an attraction to having applied people teach the theory [laughs] You've got to make it so that they get the theory that we think that they really might use.

Arthur: [00:52:54] [laughs] Yeah. So, can you say to me, if you had other job offerings, or you decided to stay in Princeton? Right. Ever.

Ashenfelter: [00:53:10] You know, I was offered several times to leave Princeton. The most attractive probably was once when MIT wanted to hire me.

Arthur: [00:53:20] That's in the 1986/987 when Kruger...

Ashenfelter: [00:53:24] Right, the late 80s. I think they had come to realize that they were weak in quantitative labor economics. And so they wanted to hire someone to fill into that field. They ended up getting good people. Right. They they hired my student Hank Farber instead. Well, I think the deal was they were going to hire him, but they decided they should hire me too. Get the whole package. I did not take the offer and he ultimately did. He move there and he really built up a labor economics program there, we would partially hire him back. And then they hired Josh Angrist, who was one of our students. And I think he's sort of the core of quantitative labor people there. And then they've added David [?], other people as well. So now they're extremely strong. I mean they used to be weak in that field, but then they did the right thing: Built it up, hired our students. So we don't really have the edge now in the field. Now they are very strong. They're strong everywhere. Chicago has good people, MIT has good people, Berkeley has really strong group. But at that time back then, there weren't so many competitors, it was a little bit easier.

Arthur: [00:54:45] Yeah, but what M.I.T. did in the late 80s is similar to what Princeton was doing the early 70s, right? Hiring some people to teach labor and micro econometrics. So you became the director in 71, right?

Ashenfelter: [00:55:04] You're talking about?

Arthur: [00:55:05] The industrial relations section.

Ashenfelter: [00:55:07] Yes. Well, you know, that's the way the directors are rotated. So the deal was, when Farber moved to Princeton, up to that time, the director was not rotating and more or less, kept the job for a while, but that was largely because we didn't have so many senior people that were free to do the job. When Farber arrived, we made a different arrangement and the arrangement was that from that point on, since we did not have another senior guy take the job, that we would rotate who was the director. So you'll see, if you look at them, you'll see that I was either the director or multiple times after that. But you'll see that it went to Farber and then I forget who was basically Alan Krueger was a director at one time. Cecilia Rouse was a director one time. David Lee was a director one time. Alex Moss was the director at one time. We basically tried to rotate. The kind of idea of it is that it's a collegial arrangement and what the director does: I always thought of it as the director it's a kind of like a football coach, you know, it's not like a general.[laughs]

Arthur: [00:56:28] Is it your football or my football? Because my football is soccer.[laughs]

Ashenfelter: [00:56:38] I've taken my football. [laughs] But I think, well, the coach probably is the same in your soccer. Your football is also the same, right. The coach, you can't really make the players do things. You sort of suggest it to them [laughs] I think that's the best way to do it. And so ever since then, we've rotated through directors. So there's been lots of practice. Some take the administration seriously, which can be very good, especially in difficult times. Difficult times is very helpful to have someone actually likes to do administrative work.

[00:57:22] [internet failure – farewell by e-mail]

Appendix 8 – Interview with Orley Ashenfelter (part II): 14-05-2020

Interviewer: Arthur Brackman Netto
Platform: Zoom
Recorder: Easy Recorder
Transcripts: sonix.ai and minor revisions

[00:02:20] [Greetings]
About George Shultz

Ashenfelter: [00:04:36] You're lucky with George Shultz. Just to give you the background, Schultz, after doing many things, ended up in the Hoover Institution in Stanford. And John Taylor is there also. And they have something in common, they both were in the Marines and George was in the Marines in second world war. John was marines too, but he didn't serve anything. This was it. Then they both went to Princeton, ironically. And George was quite well to the Princeton. People that he knows well, I think also went there, some quite famous people. But anyway, I sent you that note about his senior thesis, which is remarkable. They had it to do in the 1930s. And it's a remarkable document. But it's an early example of program evaluation, basically a program that was designed to help farmers in Tennessee and he did his own data collection which he is very proud of it. Went down there and Livingstone[?] Farm and checked his books. And so he has affiliations with three university [?] In Stanford. And he gave money to each of them. In each case, the funds that he gave was support for students who might be engaged in primary data collection. So he gave his fund to Princeton for undergraduates, a fund to M.I.T. for grad students. And I don't know what the fund is for in Chicago, [?] with the money. So he has a long history of those connections. I think if you can get an interview, if we can arrange an interview with you on. It probably won't be long, and it keep be short. It can't be long. He's ninety nine years old. He's still pretty valuable. He speaks slowly, very precisely. But I think if you start with his senior thesis and go on from there, I think you'll touch a chord that will make him look like...

Arthur: [00:06:53] This is his B.A thesis, right?

Ashenfelter: [00:06:59] Yeah. You know a lot American universities don't do that. But that's one of Princeton's strengths. They make all of the undergraduates... And they still do. And I supervise some of them. They can be quite remarkable. They do things you would hardly ever believe. I mean, they're like Ph.D. thesis.

Arthur: [00:07:24] Yeah, we still do have them here in Brazil. We have them.

Ashenfelter: [00:07:32] OK. Not sure how long this room goes. It Is probably automatically set for an hour, so I guess that's what we got.

Arthur: [00:07:39] OK.

Ashenfelter: [00:07:41] An hour seems like a lot, but if we run out of time. We can talk again.

About IRS Colleagues

Arthur: [00:07:44] Ok, OK. So we stopped when you were talking about Albert Rees leaving the role as director of the center and then, in 71, I guess you became the director. So you were talking about the the functions of the director.

Ashenfelter: [00:08:13] I'm goint to record this to us.

[00:08:23] ...

[00:08:25] ...

Ashenfelter: [00:08:28] Right. Albert Rees. Yes, I became the director. I think that's what[?]. I really forgotten what Al did. He either went to work... I think he left for a government position. I think he worked for Gerald Ford at a place called the Council on Wage and Price Stability. And then I was the director. At that time, we didn't have very many faculty compared to what we have now. We always had some. When he came back, he ended up as provost of the university. So I don't think he was director for very long. Maybe not at all before he came back. And then from there, he went to become president of the Sloan Foundation. I don't think he retired from Sloan, maybe he died while he was at the Sloan Foundation, although he would be coming close to retirement. He wasn't so old when he died. I'm not sure how old he was, but he was active as a director in our department. The young people, not necessarily labor economists working in the field, including visitors, of course, like George Johnson. There had been visitors before, like Aldrich Finnegan. Although he wasn't a visitor. He pass there often, he was a faculty and one time he was a visitor. And we're a pretty small group of mostly visitors and graduate students. And then people that were kind of connected to it. A quite important guy for some of the students, especially Jim Heckman, was Harry Kalejian. Harry was a student of Art Goldberg at Wisconsin. He was an econometrician. And his way of thinking about econometrics, (where everything we do is sort of conditioning on variables. The regression is like that, but it's more general versions of it) that's actually what I think inspired Jim to think about the selection bias issue. The conditional expectation out of a particular ration[?] function, even if the error term is correlated with the right hand side variables, [makes that] maybe to work out with that conditional expectation wasn't adjusted to just the regression. Anyway, Harry was a very influential person that way. He was very good at teaching, very clear minded. He's still alive. He left Princeton, went to University, Maryland. He's a very clear thinking guy. He was active. [also] A guy named Stan Black was very active. He did more macro economics. We had a thing called the Systems Analysis of the Labor Market Project[?], where government money would come for us and it was really just to do quantitative work and labor economics. There were a lot of papers and if you look at the earliest papers of the industrial relation section, many of them... One of them is the macro model, actually, of the U.S.

Arthur: [00:12:24] Yeah, I was doing some Biblliometrics with the working papers and I have seen some of them. Yeah.

Ashenfelter: [00:12:31] Yeah. Those are the early ones. I forget, when is the first one? What's the date on it, you know?

Arthur: [00:12:37] Oh I don't know exactly, but it's early 70s I guess.

Ashenfelter: [00:12:43] I may have started those, I cant remember when we started them. I think they started before I went to the government, but I'm not actually sure. Maybe early 70s. It could be in the 60s, actually.

Arthur: [00:12:59] Yes, it could. I'm not sure. So a lot of people left the industrial relations section during the 70s. Harry Kalejian left in 73. Daniel Hamermesh left in 73. Frederick

Harbisson left in 76, and Albert Rees left in 79. Do you remember being worried about that or thinking about, like, how to substitute those guys?

Ashenfelter: [00:13:43] To hire new people? Well, we're definitely much smaller than we are right now, especially in senior people, but we would always hire junior faculty. You know, we had a lot of other good people. Johnny Abowd was there. I actually firget when he came, early 80s maybe. I wrote a couple of papers on him. So we did have people who showed up. The departures are interesting. I mean, Harbisson basically just switched fields and moved to the Woodrow Wilson School. He had become interested in economic development. He lost interest in Labor markets, really. And so he moved. Rees took a government positions. Harry moved to Maryland and I'm not a hundred percent sure why. It's possible that he didn't get tenure at Princeton. I'm not sure. The department may not have promoted him. Hamermesh left because he decided that he wanted to get promoted and he wanted to do it very quickly. So he moved to Michigan State because they made him an offer. Princeton wouldn't respond to that offer. So that was his departure. They all left for different reasons. Well, I should also add, it's very common at Princeton University they have a lot of turnover. It's amazing. In my department, the industrail relations section doesn't have that much turnover. I think David Card moved to Berkeley. He's the only person, really senior person, that left to go to another university. If people leave, it's for other reasons. Well, Alan Krueger, my colleague, died. But typically it's not go to another university. The two groups that have very low turnover are the game theorists and labor economists. But the rest in department has always had an amazing turnover. I mean, so many people that I know in other departments actually were colleagues of mine, including, for example, John Taylor at Stanford. He was at Princeton, actually. He taught at Columbia, then he taught at Princeton and he moved. But I mean, the list of people that have taught at Princeton is remarkable. At Harvard, Ken Rogoff, John Campbell, those were all at Princeton at one time or another.

Office of Economic Evaluation - DOL

Arthur: [00:16:25] Ok. So, in 72, you went to the Office of Economic Evaluation, right? So how did they contact you?

Ashenfelter: [00:16:38] I think it must have been indirectly connected to George Schultz. What happened is Schultz brought into the Labor Department in 1970, or 68 I guess it was... I think I mentioned this earlier, the Nixon cabinet was of extremely high quality. Most of them... I think the first black cabinet member was Nixon who appointed. Elliot Richardson, as I said, was the attorney general. A Boston Brahmin[?] guy, who resigned rather than fire Archibald Cox. And George was secretary of labor. And he brought in a lot of very good people. Many of them either academics or at least very sensitive to academia. Arnold Weber[?] was Assistant Secretary for Manpower, which was the biggest department [of the labor department]. Arnie ultimately became president of Northwestern University. So he's quite a distinguished guy. And then those people got quite good bureaucrats. Those people at a little bit lower level than the top political appointments. And that's stayed with the department for quite a while. I mean, it was funny, you know. It changed. There were good salaries, too. It changed and then that stuck there for quite a while. So that office of evaluation that we set up. But what I did? I had two things that I did that there were truly bureaucratic. One was we set up an office evaluation and hired people. I hired Bob Smith, who at that time was, I think, the university of Connecticut. He ultimately got a job at Cornell. He's still there. I hired Morris Goldstein. Still a very good friend. Morris was at the International Monetary Fund and had been a labor economist, but couldn't get a job as a labor economist. So he went to the IMF. He's ultimately assistant to the director. But we got an intergovernmental personnel transfer for him. So we didn't have to pay for him. We got the IMF to pay for him to come to the Labor Department. You can still do that today, as an interpersonal transfer. [I hired] consultants, really distinguished people. Ron Ehrenberg consulted a day a week for a while. Dale Mortenson wrote a paper on job matching for us and the employment service. So we had kind of influx of...They used to call it the Harvard at[? harvardly?] Government. I was kind of annoying because I was at Princeton. But there were a lot of people like that around and a lot of activity. The person who replaced me when I left was George Johnson, the man who'd been a visitor from Michigan to Princeton in an earlier time. He actually became the director of the office. He's a very intellectual guy. Nothing like a political person. And I think he was followed maybe by Dan Hamermesh. I think [name?]... It went on for quite a few years, actually, that academics came into that kind of job. The other thing I got was money. The way they did that is the secretary issued an order that I wrote, Secretary [name?]. And the order, basically took one percent of the operating funds [or of the research funds, I guess it was a research fund] associated with each unit of the department. And that that money had to be used for evaluation. That was like a law. We passed a law that said: "you have to spend money on evaluating programs". I don't know what ever became of that, but it was quite useful. It helped us because it meant that we also had the funds, if we wanted to use outside funds, either to hire people or to do collection of data or do something like that. There were evaluation offices in each of the departments inside Labor Department. And I, in a way, could influence those people. There was a guy, bless his heart, his name was Howard Rosen[?], at the manpower administration that I kept pushing on to do a really good program evaluation of training programs, which become fairly substantial at that time (which is something I really had been hired for). And Howard did. He followed up on that. He ended up with this funding, this company, which still exists, called the Manpower Development Research Corporation (MPRC). That was that was a company set up to do randomized trials. People thinking about randomized trials now, in economics, as big, fairly common. They were extremely rare at that time. That company was set up in the 1970s to actually do randomized trials. And they did do one. I think they hired Jim Hackeman as a consultant. So they did do a big randomized trial on some training program. You know, of course, in those days there wasn't very much experience in the field on doing randomized trials. So lots of mistakes were made. We know many examples where the randomization failed. You know why that is? You know, when you go on the field, it's not like being in a laboratory. Laboratory chemicals don't talk back to you. They do what you tell'em. If you put it in [?] one it stays in [?] one. Field trials are more complicated. We just always think of the evaluation part is, too. the actual program evaluation, but there is also process evaluation. The process evaluation is really about making sure that the program is operating. Making sure that if the program was designed for poor people, that poor people get the money. This issue has cropped up again recently in the government because we just handed out two trillion dollars - kind of hard to believe it - for response to the COVID 19. And there has been incredible resistance to an inspector general who would be the person evaluating the program. And almost all people, Republicans and Democrats, think that we should have some way of evaluating how the program operates. But in fact, nothing has been put in place to do that. And I don't think we will. I think there'll be a lot of corruption and a lot of problems with it. We'll never find out what was wrong with it. But I'm more concerned about right now is that we open up. I know Brazil is actually starting to get a fairly good run on that COVID virus too. We're at nearly 90000 deaths. Some of the states have not had much virus. Some had a lot. Staten island and New York have had a huge amount. But ideally, we have 50 states and we could learn from the way that they behave because they're all behaving differently. Some of them are opening up completely. Some of them never really didn't close. Some of them are closed down. I don't believe there's anybody in the federal government that's engaged in that kind of evaluation. And there's a huge externality from it, because what you learn from state A is it's good knowledge for all the other 49 states. The same idea, by the way, with our training programsand how were things at the Labor Department. We were aware that, you know, what we learned it's something that others could use as well. So there's like in learning there's an externality. So that that organization existed for quite a long time, The evaluation department. At the time it was set up, we actually had three groups. We had the evaluation department. We had the research office. And there was also a Programming and planning office, which was really more like a Overall budget kind of thing. It was written by a friend of mine, Jim Bloom. He's very, very good. I think he and I edited book together at one point. By the time I got there, he was a very experienced bureaucrat, although he was an economist and he was extremely helpful to me. I didn't know anything about how to operate the government. People are always offering you gifts to the government, which are really not gifts [laughs]."Why don't you hire this secretary?" That's a sure give away the things that's a terrible person! [laughs] I would have been a little naive and think: "well, that's not such a bad idea. That's really great. I want to go out and recruit." But it is a terrible idea. Bureacrats will do that to each other. But it's no mean spirited. But, you know, if I've got a crummy employee, I want to get rid of them. And the easiest way is to get someone else to hire.

Arthur: [00:27:29] But the Office of Evaluation was part of the Office of Economic Opportunity, right?

Ashenfelter: [00:27:37] No, it was a part of the Department of Labor.

Arthur: [00:27:39] Oh, yeah. Oh, OK.

Ashenfelter: [00:27:42] We were all under the Dep of Labor. The Office evaluation was at Department Labor. This Office of research was there. There was also an Office of Econ Opportunity at the same time, they were separate. They had some good people in there. They set up these various experiments early on, the NIT experiment. They had a housing experiment. They did a healthcare experiment. These were all big. They were randomized trials and they were done in the field on a large scale. The Labor Department was not doing any, but I pushed people. And finally after I left, actually they did. They didn't have this randomized trial training program. There may have been some other smaller things to do.

Arthur: [00:28:38] The thing that I think that happened because the Office of Economic Opportunity was ended in 73 and it went to the Department of Labor. So I think some of the functionalities of the Office of Economic Opportunity went to the Department of Labor. Probably some of the randomise...

Ashenfelter: [00:28:59] Yeah, right. Just after my time in the department, I was there 72, 73. It could be that some of the people were moved over there, whoever was left at the OEO. I don't remember. I was not aware of that. I know that that office disappeared, but I don't know what happened to it. It was set up as an independent organization and dated back to Lyndon Johnson and the War on Poverty.

Arthur: [00:29:27] Yeah. Nixon ended the Office of economic opportunity because it tought it was not necessary anymore.

Ashenfelter: [00:29:36] Yeah, well, a lot of that is about politics. I mean, to give an example, this department, the office of evaluation in the Labor Department that still existed all the way up to 1980. In fact, I remember quite a distinguished guy was the director of it in the

Reagan administration. That was John Razien[?]. John Razien[?] was a Ph.D. in economics. I forgot where he'd been teaching, but he was a Ph.D. from UCLA. He ended up at the Hoover Institution and he actually ended up as the director of the Hoover Institution. But he had been director of the Office of Evaluation, probably only administrative job he held before he was director of the Hoover Institution. So even at the beginning of the Reagan administration, that office still existed. After that, it disappeared. And then in the Obama administration, they tried to restart it. I wrote a little paper about the history of the Office of Evaluation. You've may have that in the industrial labor relations review. They had a little conference. But I think that's all, you know. With Trump winning, I think that's probably all been eliminated. No, actually, no. But he's standing up. He got the support of labor, but he's not been very supportive of labor. A little bit surprising. They don't seem to know the difference. I guess laborers are just incapable of... Somehow they're seemingly voting against their own interests and I don't quite understand it. That's not politics as usual, that's for sure. Oh, Mr. Trump, I think he knows he was assured to be re-elected, but this wreckage is, I think has changed that quite dramatically.

Arthur: [00:31:50] Yeah...So the office of Evaluation was at the Department of Labor and you. Do you remember having relationship with people from other fields besides economics? I don't know, sociology or other fields? Statistics.

Ashenfelter: [00:32:12] Well, we mostly used economists, but there were people who were hired who in the three offices who came from public policy schools. One of the places that we look to hire people in each of the three offices were the public policy schools, the Woodrow Wilson School, the public policy school at Carnegie Mellon, which we liked the students from there. They tended to be very technical, quantitative. And then later, there was the University of Chicago which has a school too [?] Kennedy School. So those were kind of prime. And I think there are some other schools, too, of public policy. A lot of them are fairly recent by the comparison with the ones I mentioned. I would say public policy schools and economists [were what we hired]. We didn't normally look to hire people from sociology or local science, although there may have been some people.

Arthur: [00:33:19] Yeah, that's interesting because the Office of Economic Opportunity, they used to hire a lot more sociologists and people from other fields besides economics.

Ashenfelter: [00:33:28] I think you're right. Although they typically hired sociologists who were coming out of the demography area and so were more quantitative. There was a difference of concern. There was always some concern about: How well off, people who were members of these various programs, they felt as opposed to how much money they got. We were pretty concentrated on... Well, it wasn't necessarily always money, but it was something like that. So, for example, it was the occupational safety and health. We were not concerned about how happy they were on their jobs. We were more concerned about whether they got hurt. So occupational safety & health was about that. Employment Standards Administration was about when you were entitled to overtime, when you got paid your overtime. This is a running problem, by the way, if you look at it. I get the daily labor report and there are continuous lawsuits. I mean, literally every day there's the result of a lawsuit over some firm that was supposed to pay overtime to workers, but did it not in fact pay it, and some settlement has been engineered. It's a kind of an amazing thing that you have to enforce that. You would think that, you know, once the law is there, people... but, you know, they have different ways of calculating things. So some of these were just pure enforcement things. I think what was really special about it is that the training programs were really meant to be human capital, but they were meant to be human capital for people that had already left school. So these were retraining programs. A lot of people had a lot of hope for them as a second chance for people, for women. So, for example, the Manpower Administration Corporation project had what was called a national support work program run as an experiment. And they actually deliberately selected four Groups to study because they were groups that might be interest in employment, but hard for them to get jobs. So the four groups were ex-offenders, youth offenders (that's, you know, how junior delinquency used to be called: kids, young people who had trouble with law), and women, typically single women with children and with no spouse present. And they actually studied those groups, especially because the interest really was, could we design a program that would be like a second chance for them? You know, the evidence was pretty interesting because it did seem to be a very good second chance for the for the women, not so much for the other groups.

Arthur: [00:36:50] Ok. A pretty naive question. Where was the office of Evaluation and how was it (The building)?

Ashenfelter: [00:37:04] a wonderful old building, it was in the original Department of Labor, which is in 14th and constitution. It's a gorgeous old building. One of the first buildings built. I forget who's in it now, but still if you go to Washington, you can take a look at it. I'm not sure who's in it. The Labor Department is not in it anymore. But it was a gorgeous old building and it's a little difficult to describe the situation. It's very close to the White House, not far from the Treasury, but not close to the Congress. So we're on the other end of Constitution Avenue. We're on the White House yet not the end where the Congress is. And I think some people felt that was a mistake just because... In early times, the White House was kind of a place where the Labor Department gravitated. Later on, I think they thought they should be trying to operate, particularly with members of Congress. But it was in this beautiful building. I love that building, very tall ceilings. It was one of the first buildings built in Washington in the 1930s with central air conditioning. That's a pretty important thing. You know, the climate gets really hot and humid in Washington in the summer. So that's the origin of why they all leave in the summer. It was miserable. In fact, the British used to get tropical duty pay for having to be in Washington, D.C., as they considered a tropical environment. It's pretty hot and it's very humid. I'm sure you understand what I mean [laughs]

Arthur: [00:38:45] Yeah. I'm already used to it [laughs].

Ashenfelter: [00:38:49] So a little interesting thing for you to hear in this. 14th and Constitution was right near the White House, but also very near to the rioting damage that had taken place in the late 60s. A big part of Washington, D.C., was nearly burned down again by riots. A lot of people don't realize that. That's really, to some extent, that convulsion is to some extent the origin of this interest in training programs. The idea was that we had people who didn't have jobs in there, out looting. I mean, the scale of it is quite remarkable. I mean, for example, there's a very famous hotel I don't stay in it anymore, it's so expensive. Only two blocks from the Labor Department. That hotel was shut up. They closed it. It was empty for a decade. It's now, you know, six hundred dollars a night. It had been a very famous one prior to that. The whole area was kind of in this state of devastation. If you visit Washington today it's not recognizable from what had happened during that period. But I think those riots in the 1960s (mostly African-American rioters), that set off an alarm bell that people said: "you know, there's something really wrong here and we need to try to fix it". You know, there was kind of a convulsion. 1964 was the passage of the Civil Rights Act. And that was really the first time that most Americans came to grips with the fact that we kind had ended slavery but no one had ever made any attempt to try to ensure that African-

Americans were fully integrated into the U.S. economy. So a lot of it was motivated really by this. A lot of the programs were motivated by those riots. And the fact is that, you know, the area around us - even though we were in the beautiful old building - impressed upon you that there was some urgency, some urgency in trying to make these programs work.

Arthur: [00:41:21] That must have been a problem also because some evaluations cannot be done so fast, right? You need data, you need time.

Ashenfelter: [00:41:31] That's the biggest problem. You get somebody who says: "OK, I've got a great idea for a program, let's run it". And they want to know the answer six months later. Well [laughs], the program was supposed to increase your earnings by learning something and that is not going to happen in six months. So, yes, this was always a problem: long term evaluation and sticking to it. So that is why we see now a lot of economists using administrative data and records. That was why we were so concerned to be able to do that, because you could do longer term analysis. That has had mixed success in a way. A lot of money was spent on trying to quickly collect data and that meant, you know, doing things by asking people questions, not doing administrative data. But administrative data is not really foolproof either. It has its own problems. A lot of times things are not covered. So I would say that our data collection is always been historically one of the biggest single problems associated with the program. You think it's a good program, but then you need to have a way to capture what the outcome is. And with the data collection system in general, you have to wait. You just going to have to be patient. So you need a longer term commitment to evaluation.[?]. But, you know, it's funny thing I was looking today, I would have a long conversation, couple e-mails and some Zoom's with Angus Deaton about the Spanish flu (we're trying to make some analysis of what went on there). [And it's quite remarkable, people had been using are . It's a little surprising, you know. So it would have been nice if someone had done a really good.] And of course, it was 1918, Right? So penicillin hadn't been investigated. They didn't even know what a virus was. [So to some extent, but not much. I mean, their data on things back in that period.] There were several waves of the Spanish pandemic. There was a wave in the summer and then it went away and it came back. But the one thing they haven't recognized is the susceptibility and mortality effects of the flu were inverse to what they are today by age. The people between 15 and 35 were the ones that most died. People that were over 55 had almost no mortality effects at all. It's exact opposite of today. People say: "well, the virus is different". I understand that. But "the virus is different" it's not an explanation.[laughs] Why is the virus different? I mean, biologist say: "well, the virus is different". Sure. Right. I don't need you for that. I need to explain why it's different.

Arthur: [00:45:02] This part I can understand. That's OK. That that I don't need to be a biologist for that.

Ashenfelter: [00:45:08] Right. That part is easy. So that's interesting. So long term evaluations are surprising. I mean, the reason I'm studing the mortality period back then is that I've been trying to work on evaluating economic effects of our greatest natural experiment, which was prohibition. That was a period between 1920 and 1933 when the manufacturers sale, import or export of intoxicating spirits was illegal in the United States. You're not you're not allowed to... Theoretically, they stoped people from drinking. There is one very good book called Economic Effects of Prohibition, written by quite distinguished economists of his time, Clark Warburton, who has finished his Ph.D. thesis at Columbia in 1932. And what's interesting is his evaluation, of course, ends before the end of prohibition. And then no one ever took up that trouble to study what happened after Prohibition. We really have an experiment where you have a pre-Period, which is quite muddy. It's muddied because when Prohibition started it was also the First World War and the Spanish flu epidemic. And then, of course, after when it ended, it was the Great Depression. But no one has paid much attention to what happened afterward, and it's quite remarkable. So I got interested in in. I was thinking, well, this is probably one of the few ways we have to study what the effect of the legalization of marijuana or other drugs would be: to see what happened during Prohibition. It looks pretty similar by the away. Anyway, go ahead. I got off the track.

Back to the IRS

Arthur: [00:47:04] Yeah. OK, so Office of Evaluation. And in 73 or 74, you came back to Princeton, right? So, in seventy four and seventy six, you organized two different conferences at Princeton with people from the Labor Department. Those were people you met during your days at the Office of Evaluation. And so you came back with the idea of creating some connection or how was it? Why those conferences?

Ashenfelter: [00:47:39] Yeah, that's right. The idea was that we would be doing... I mean, I completely changed my research agenda to some extent. And I still have great interest in traditional labor economics, but I changed it more to this idea of public policy: trying to do quantitative work in the public policy area. And the idea was that... this has actually come to pass. The idea was: the government didn't really have an easy way to employ the quantitative people they needed. We have all the students who needed to write dissertations. We have faculty who want to publish papers. We have all the incentives to do the research that should be done in the government. The government doesn't really have it there. It's very difficult for people who are regular bureaucrats [to do the research]. I used to do this when I hired them, I used to have a policy that: I ask you to work four days a week, the fifth day you come to the office to do your own research. Take on a problem that you are interested in. That was the way to try to get academics to come and be bureaucrats. But that's a pretty inefficient way to do it. So my idea was that the problems come from the government. The data could come from them. The funding can come from the government. And we supply the people from the university, because we're the ones that have the motivation to publish the papers. Also the motivation was to make sure it was published so there'd be some follow up. So people could see what had been done. A lot of times you'll see there's some evaluation of something, but it never gets published. It disappears, right? Somehow it gets lost. And the idea was to get that into the public record and try to make it a very transparent public analysis and to creat the standards so that academics wanted to actually do it. Now it's almost like the reverse. If you look at the poverty lab, this cluster around Cambridge, Mass. Poverty Lab is kind of the same idea, only it's more like a government organization. It's more like as if there's a piece of the government in the university. But it's a similar kind of notion of trying to motivate [bureacrats]. They use lots of administrative records as well. I mean, in a way that has come to pass. It has taken a very long time. But we started doing that with these conferences that were very successful in the mid 70s, organized really around papers that had come out of [this setup]. You know, sometimes they were special papers like: there's one paper in one of the books that we did on sample design. There's actually two papers on this. It's a Bayesian analysis to figure out how much data do we need to collect in order to shed some light on. Those are the kind of problems that you wouldn't come up with if you're a regular economist sitting in your office. As an Economist sitting in the office, you just figure: "well, I better go find a data set and going to make a dataset". So our [entity?] was a little different. We thought: "well, if we are gonna do this, we should try to figure out all the technology associated with [?]. Yeah, that was a great moment. And a lot of wonderful people came to those conferences and also wrote papers that went on and affected their own lives.

Arthur: [00:51:17] Yeah. It seems to be a really different situation that was happening in Princeton when compared to other universities. This connection with the Labor Department in those conferences and probably data that was coming to Princeton. So there were lots of papers about public policy being done there. So we talked already that you had to deal with some leaves during the 70s and then from 79 to 83, it seems that you had kind of a lack of colleagues teaching labor and maybe microeconometrics. If we look into data, you supervised a lot from 79 to 83 and mostly alone, it seems you didn't have someone to supervise with. So how was that period from 79 to 83?

Ashenfelter: [00:52:22] Actually, I was happy with it. We had good students who were interested in the Problems. And then the other thing that made it easier, I guess, is that the way I was doing labor economics was much closer to the way that economics is more generally done. So, for example, having econometricians nearby, you know. And now, of course, a lot of motivation for people who do econometrics really comes out of the problems that were posed back in that same period. Labor economics problems. So now we have people who are talking about the design of experiments. They're thinking about selection bias problems which were just always endemic to everything. So a lot of what became econometrics... I used to think of training students as having a skill that they were labor economists, but also had a secondary skill or maybe even a primary skill. In those days, it was almost always econometrics. So, someone like Heckmann would be someone who is doing labor, economics and econometrics. Someone like Jon Hamm would be someone who's doing labor economics and econometrics. And I think he sees himself actually as more of an econometrician, than a labor economist. But you could see even their students like the [?] on my faculty as a student od Jim Heckman's. But basically, he is often motivated to by practical problems, labor type problems in doing the econometrics that he does. He was interested in a parametric problem. Dick Quandt, for example - He's still around, he's 90 years old, in very good health. I just had a conversation with him yesterday. I have to admit, this is being in a lockdown for your 90th birthday. You have to ask yourself. I lived 90 years for this? [laughs]

Arthur: [00:54:41] It must be it must be strange.

Ashenfelter: [00:54:48] He was on my dissertation. And he was always very helpful in that regard. He left Princeton, too. He's Hungarian, native Hungarian. And he left really when the Iron Curtain fell because he was so excited about working and consulting with the Mellon Foundation. But yeah. So then we had a string of good econometricians who we could work with. And even later we would have people that would be working in areas where labor economics was connected to another field. A very good example is environmental economics, which early on was kind of dominated by resources for the future[?] Type of people who are interested in the population effects. The program evaluation of some kind of environmental problem. That was a guy like Mike Greenstone who's now at Chicago, very distinguished guy. He's a really conservative environmental economist, but he started off as a labor economist. And what he was studying was the effect of the EPA, Environmental Protection Act, on employment. So this was an issue of how much was it costing in employment? He had started doing it as a labor economist. But now basically everything he works on is environmental. He has a recent paper talking about whther the lock down makes sense.

Arthur: [00:56:40] And who taught econometrics during that period? Do you remember?

Ashenfelter: [00:56:47] Let's see who did. Quandt. Quandt was certainly doing it. Steve Goldfeldt. He was a macro guy, really, money and macro, but he would have taught it also. He died quite young. Harry Kelejian was there of course until he left. There must have been a string of other people. I honestly don't remember, but we used to think of our students as they could teach applied econometrics as well as labor economics and added a dimension to them. That was, you know, back at a time when that wasn't so common. There was always a demand for econometricians. The fact that they do labor economics is something special. Josh Angrist is a good example of that. Angrist, I would say he is a labor economist, but basically a lot of what he's known for is methodology. I mean, everything from like his paper with Krueger to sample instrumental variables or this business about the average treatment effect and all the discussion of that. That's a sort of taking program evaluation into a more technical area. He certainly has changed the terminology everyone uses. I don't change my mind [laughs]. But he was a good example. His dissertation was really an example of something that came out of my time in the Labor Department. It was fascinating. One of the problems we had was, at the end of the Vietnam War, we brought soldiers home. Vietnam war was a pretty big deal. A lot have died, American soldiers in Vietnam, 50000 died. So we brought em home. And the history of veterans coming into the workforce is that they have huge unemployment. that was true after the Second World War and it was true after the Vietnam War. So there was quite a lot of research - Labor Department funded as well - on: first of all, you could just tabulate and see that the veterans had low... This is kind of a tragedy. These guys have gone off and fought for their country and they come home when they can't get a job. So one of the first questions we had was what effect did being a veteran have on your economic prospects? At that time that the volunteer army had not yet come in. So here we were conscripting you - basically making you serve as a slave then. So presumably you watched[?] Earnings from that. But then you come home and there's a permanent effect. So that was like a big issue. And there were some doctors out here who were concerned about mental health problems among Vietnam veterans. So they were the ones who originally figured out that because of the lottery - And this is talk about a Princeton connection, a lot of people don't know this. So the first thing we had in the Vietnam War that was trying to make it fair was we had a lottery for whether or not you would have to serve. The person who proposed that, by the way, was David Radford who was a colleague of mine in Princeton. That was his Ph.D. thesis. He worked with Canaro at Stanford. And he actually had a book - there's industrial action section monograph - in which he proposes a lottery. They actually did it. And, of course, what that meant was that we had a field experiment that really was a randomized trial. And these doctors have taken advantage of it and then Angrist did the same thing. I'll never forget. He just came out here to San Francisco and it was in the 70s - 70s or 80s - So it was like Marvin Gaye heard on [recording error] associate with the Vietnam War. [laughs] Anyway, he was playing these songs in the industrial relation section. I still remember that. [laughs]. He himself had been in the military in Israel. So he had been a sergeant for a period. So he is quite familiar with that. Anyway, there were a lot of examples like that. But he would have been heavily involved with econometricians to. So that was our plan.

About David Card

Arthur: [01:02:06] So in the 80s David Card joined the department, right? He had just finished his Ph.D...

Ashenfelter: [01:02:18] Yeah. I think that the desire to get a new person was pretty strong. What happened was we had this policy like most departments by that time. Princeton was not event a separate economics department until 1960. Don't you know that? Up until 1960, we had a Department of Economics and Sociology. They were in the same department. In 1960, the president of the university decided to change things. He wanted a good economics department. And he split them off and then he went out and he hired most of the Johns Hopkins Department. Richard Musgrave, Fritz Machlup. These were all senior people at Hopkins, Machlup stayed and ultimately die. Musgrave moved on to Harvard. I forget. Oh, Arthur Lewis he hired from Manchester. He was a distinguished guy, won the Nobel prize. These were all part of our department at that time. But the department had just been started. It's really remarkable. The industrial relations section.was always there. There had also always been a connection to the math department because of Harold Khun. You know, all these people like John Nash and so on. They were students of Al Tucker, who is in the math department but did applied mathematics. So there was always some connection to game theory and mathematics. And there was the industrial action section in Labor Economics. And then [??] In the middle. He basically went on hire, spend a lot of money. And we didn't have a department really until 1960. So at the beginning, we were not uncomfortable hiring our own students because typically our graduate students couldn't get really good jobs. So we'd have a really good student and say, well, this is ridiculous. I mean, that's this guy's better than anybody we could hire from Harvard. Why don't we hire him? Then we went to the policy of: "no, we can't do that". Which is more or less the policy now. And Card was an example. He actually took a job at the Chicago Business School. And then people realize that, you know, this was crazy: we should have hired him. He should be at Princeton. So everybody agreed to do that and we hired him. He is probably still my closest friend. A lot younger than I am. He is a remarkable economist. His path was a little bit like mine, too. For example, he taught to graduate the core micro at one point, just as I did. He is a very good econometrician. Outstanding, in fact. Super sharp guy, very smart guy. He was a great hire for us. And then the next one was Farber. We basically got him because MIT tried to hire card, and then Card wondered as his response that Princeton should hire Farber [laughs]. So that happened to. But at that point was the three of us. And then we always had Junior faculty. Jim Brown was there for a while. I still keep in touch with him. With the three of us, we had a kind of a more basic core people to teach. In fact, Farber had always had graduate students in MIT. So he's still very active with teaching graduate students. I think card is too. Card now is at Berkeley, and they have a pretty big group there, too. I was just on their Zoom yesterday. They had a mini seminar. I'm still on their list. I didn't show my face, but I climbed on. At one point, someone showed up in chat saying: "you haven't turned off your audio". [laughs] I guess they knew I was on. Because I was at S Francisco where there's fire trucks and noise. It's never really quiet here. Actually today I came into a separate room and closed all the windows. So it's not so bad. It's still a nice city, but nevertheless,

Arthur: [01:07:03] [off topic]

Arthur: [01:08:50] So Card went to Princeton in 83, I guess. Let me see it. 83?

Ashenfelter: [01:09:01] I always get confused. You know, when I meet former graduate students, I always think that they know each other. But of course, no, they don't.

Arthur: [01:09:10] I was going to ask you about Robert LaLonde.

Ashenfelter: [01:09:15] Yeah. Yeah. LaLonde was one of our students.

Arthur: [01:09:17] Yeah. I think he was the first student that you and Card supervised together. And he had a pretty important paper in 86, comparing evaluation with other econometric methods. So do you remember something about that?

Ashenfelter: [01:09:36] Oh, yeah. He was a wonderful guy. Wonderful guy. He died of.... We had a memorial for him, but we did before he died. I don't really believe in memorials after you die. You have to do a memorial before you die. We had a very nice conference with papers in his honor. He was very touched. He had an unknown disease. At first people thought he had Parkinson's, but he didn't have Parkinson's. The National Institute of Health has an unknown disease department and he qualified and spent a week there and they declared: yes, he had a unique disease. And that's bad news, by the way, because you're definitely not going to be able to cure it if no one's ever had.

Arthur: [01:10:22] That is really unlucky. Really, really terrible.

Ashenfelter: [01:10:26] Yeah. And he was such a sweet guy. I mean, he was just a nice person. He took advantage of a great opportunity. He took advantage of this national support work program - that I explained to you about - that had randomized trials. So what he did: we used to call it the "lalondizing" [?]. So the issue became: could you reproduce what you'd get from a randomized trial using sophisticated control groups and econometrics. The paper was important because it clearly showed that even specification tests at some level, they have very little power to tell you whether or not your econometric model is any good. So it was very influential. It was influential for two reasons. One, I think it was an important paper because it was shocking to people - a surprise - in introducing more scientific attitude toward empirical work. The other reason is that Bob went to work in the government. So his first job was the University of Chicago Business School and one of his friends there with a guy named Mike Moossa[?], who during I guess it was the Reagan administration, went on the Council of Economic Advisors and he hired Bob LaLonde to come to Washington. And so in this issue of trying to do program evaluations and randomized trials, Bob was there to actually kind of seeing the extent to which that might have an effect. And I think it had an effect in the government in terms of what they thought was a credible evaluation of the programs they were doing. So it went one step further because it really started to the government with the support to work program that was randomized trial, then it went to the academia where Bob used it to see whether or not he could generate the same results, with econometric methods and then it went back to the government. So it was a I think it was influential for both of those reasons.

Arthur: [01:12:51] And do you think it was influential for the department having those students doing that kind of work?

Ashenfelter: [01:13:01] You know, it's interesting question. I think. It is fairly common now. I mean, what he did is now almost second nature to the people who do randomized trials in the field. Right? So the first thing they show you is a table about whether or not the control and treatment groups are balanced. Right? That comes straight out of LaLonde's paper. Now, the first thing you do is you ask: "have I matched the treatment and the control group on these characteristics?" That's not enough, right? It doesn't really prove that you randomized. But that's the first step. In a lot of things like that, that sort of procedures and methods, people are probably not quoting LaLonde. What's happened is the way people do those things have seeped into the profession. It's kind of like it melded into people's minds. And it just had a long term influence, even though we don't talk about exactly where it came from. So, yes, he had a big influence. And he had a lot of students. I usually don't know them, but he first was at Chicago school and then went to Michigan State for a while and then he came back to Chiocago - he was from Chicago by the way, he went to undergrad in chicago. He came back Chicago was in the public policy school there. In the public policy school, he had quite a few students of his own. And occasionally now someone will come up to me and say: "I was a student of Bob LaLonde". And I say: "oh, really? That is wonderful" because he was just a wonderful guy. This is really a sad story because he was not very old.

The Golden Generation – late 80s IRS

Arthur: [01:14:56] Yeah, I know... So I'm asking, did the influence of LaLonde's paper because Card called the next generation, the 87,88,89,90s the golden generation of Princeton. So you have Janet Currie, Josh Angrist, Thomas Lemieux, Steffen-Pischke, Brian McCall, John Budd and some guys like that. So he said "Oh this generation was really good". And I was wondering whether maybe LaLonde was the first guy of this generation.

Ashenfelter: [01:15:32] That's a good question. Chronologically I guess he is. He mightvery well. It's possible... That it's quite possible or Angrist maybe. Yeah, they're all from that period. Even Budd, for example, the idea there was that he would be more traditional industrial relations as well as labor economist. So they all had to know their own things, as they did. They all became very well known people. What did card call them?

Arthur: [01:16:13] Something like the gold generation or something like that.

Ashenfelter: [01:16:27] Yeah, yeah, yeah.[laughs]

Arthur: [01:16:30] It's something like that. I took a note when he said that...

Ashenfelter: [01:16:43] He may remember better than I do. But it certainly was a very special time. It two things going on. We had good students. We were also uniquely... We probably had a very strong group and most other departments still had not built up their labor groups. So we didn't really have as much competition for students. That was a factor. So they were good and they were probably the best that were interested in these kind of topics. And the placements were easier because we didn't have much competition. And then there was the "low lying fruit", as they say. There were just an awful lot of good problems for people to work on that had not been touched yet. Now, I look at what students do: It's tough. The scale which people work is Pretty impressive.

Arthur: [01:17:59] It's it's different. I guess it's different. I mean, people had to come up with ideas. And now I don't know. There seems to be fewer problems, but I don't know. We have the same problem in history.

Ashenfelter: [01:18:18] Yeah. It is a little different. It is hard to say. It's it is a little different. Now you're doing new techniques. I think the emphasis on big data has become pretty overwhelming. I mean, it's pretty hard to... It's one of the reasons I've had to kind of stay away a little bit in my ancient[?] Days. So I got more involved with what I consider to be interesting data collection like this big project I have on collecting data on wage rates at McDonald's restaurants. I find that interesting. It's a way to get a vision of what's happening to labor markets that are pretty much just market based. With the exception of things like the minimum wage, right? They're not administered markets and they let you have a comparison across countries as well as across states. [uncomprehensible...] Charles University, I went there when they first opened. And of course, it's a beautiful city, Prague.

And a couple of our students, John Svejnar and Randy [?] were very active in developing the economics department there. So I had been on television there one night. They have a thing they call Hyde Park. I don't really understand it. It's a hyper corner, I think they call it. I guess they're all aware that in England, Hyde Park Corner is where you have outspoken people. It's an English thing. You can go there. Well, I guess you don't do it now because of the cold. But generally speaking, you go there and you can criticize anybody or anything except the queen and say anything about it [laughs]. Anyway, so the television program is named after it. And it's in English, although I think what they do is they have you in the background in English people who understand English, and then they kind of have that translate. Any way, the first question they asked me was, what do you think happened to the wage rates of McDonald's as a result of the Covid 19? I don't know right now, but in August, I hopefully will. So anyway, so it has changed a little. I think partly it's harder to get the low lying fruit. And the other thing is not same for the students. We had a very good student this year who was really old school but didn't find it easy to get jobs. I was surprised. His name is David Arnold. I would have thought he would have been very attractive to a very, very good department. He worked on big data. But at the same time, some real economic theory. He is working onthe effect of mergers on wages. Basically if two firms merge that are not in the same labor market, you don't expect much back. But if two firms merge and they are in the same labor market, then that reduces the competition for workers. I think he did a spectacular job with it. [now, I actually instead of working with the government, I work with lawyers. Just yesterday.] People don't realize how much explicit collusion there is in American labor markets. I was shocked by this. For example, there is a lawsuit - this is public - There's a lawsuit going on right now between a company that operates across the country. They filed this lawsuit before the Covid experience. But nurses are now a pretty important topic. The way hospitals work is they hire temporary nurses, which can move around. And there's a big company that hires these temporary nurses. And then there are a lot of subcontractors. Several of the subcontractors sued the big company on the grounds that they were engaged in explicit no poaching agreements with the supplier. And the no poach agreements are amazing. They basically say things like the if we if you if we have a subcontract with you, you will not try to hire any nurses who work for us forever. There's not even a period not like in the next six months or anything. It's just forever. Right. But slavery is not a course. Admittedly, that doesn't mean that they can't work for somebody else. But I would have thought that these are would clearly be legal. And yet they're obviously going along. And it only came came out because the one of the subcontractors, I guess, was not going along with the deal and so filed it, filed a lawsuit. And finally, the kind of thing I think is I think that a lot of it going on and it's not. No one's paying attention to it. I think it's a it's low lying fruit. But. It's very hard to study because it's secret.

Arthur: [01:24:39] Yeah. In Brazil. In Brazil, we have this problem. Almost the whole health system here. It's way subcontractor. So both physicians, doctors, nurses, everybody. It's like a subcontract. So they they have a they have some kind of firm. And then the hospital hires the firm. It's almost the whole system works this way here.

Ashenfelter: [01:25:04] So do they have. No. No. Could not compete around, you know, poaching deals yet.

Arthur: [01:25:09] Death parts. I'm not sure. I'm really not sure. But maybe I'm not sure we'll be a great paper.

Ashenfelter: [01:25:18] Yeah. Yeah, I doubt that that's the problem is to find out, you know.

Arthur: [01:25:24] Yeah. I mean, this kind of thing I can find out because my girlfriend, my father, my modern day up, they are all doctors.

Ashenfelter: [01:25:31] So I kind of know something about that. And I'll check. Check out. Okay. Oh, I got it. Oh, check. Yeah. I love the paper. Okay, check that.

Editor of the AER

Arthur: [01:25:41] So coming back to ourinterview. I'll check this. But I have some question before. In 85, you became the editor of the AER, right? So in the 70s when you were finishing your Ph.D. there was no really clear to labor economics and not so clearly micro econometrics. And in 85, you were editor of the American Economic Review.So what what do you feel is represented to the field?.

Ashenfelter: [01:26:22] You mean my appointment as the editor?

Arthur: [01:26:24] Yes.

Ashenfelter: [01:26:27] You know, that's interesting because... I became editor of the AER, although I always had a coediting system, so my coediting system was not very many editors. I thought of it as three other editors. We had a theorist, macro and then all other Micro fields. That was the way I tought about it. That gave us some more flexibility. You wouldn't have to handle a collegue's paper and those kinds of things. You know? Other journals followed that lead. For example, Sherwin Rosen became very active at JPE and he watched what we did at the AER. I'll never forget when we published a paper at the AER of Will Nordhaus on climate change. Really early days paper. And he wanted to have some color graphs to show what would happen in color to the temperatures. And I said: "well, if you can help raise some money. Sure! I'll do it." And I'll never forget Sherwin when he saw that he was just blown away: "I can't believe you have color stuff in your journal" [laughs]. Where do I been all this time anyway? And then, of course, Larry Katz was at the QJE. So there are quite a lot of people coming along.

Arthur: [01:28:07] [Off-topic]

Arthur: [01:30:16] So we were talking about the AER…

Ashenfelter: [01:30:20] Yeah. So that was... It was an unusual experience. I edited it for a very long time. I loved it, actually - and I found coeditors who I think liked it too -, because you can learn a lot to see what was going on. Of course, you also could influence the field or influence economics by publishing papers that might not otherwise get published. And it's a fascinating thing indeed. I think journals operate a little differently now. We were a maybe little less career oriented. And so the journals weren't like the things you had to do to get tenure somewhere. They were also about distributing your work. Now, if you want to distribute your work you put it in NBER working paper. When it comes out to the journals, it's way too late for anybody to pay much attention. So that was a great moment for me. [recording error] Coeditors, the first three were John Taylor, I think, I had Bob Haveman and John Riley, who was at UCLA. They're all great people. We would meet once a year. We had a good time and talk about... And we started implementing things that had never been done in journals before. So, for example, we started implementing. things about about sharing data. You know, opening up the data that you had. We implemented some rules about who would handle a paper so that you wouldn't handle a colleague's paper. Some kind of conflict of interest things. Things got started there and it is going on now. In fact, I think it's kind of gone on quite a long ways in terms of what the AEA does. Maybe more than they actually really, really, genuinely accomplish. But when we did that it was a fascinating thing. The papers I remember best were not mainly labor economics. For example, I remember by Larry Ausubel[??] on credit card debt, that referees didn't like it and in a way, it's a monumentous paper. Basically, what he showed was that most credit cards have interest bearing debt on them. At that time, if you said to an economist... I remember being in a meeting and to go around the table. It was a bunch o labor economists. And I said: "does anybody have a credit card with an outstanding debt on it?" Because it's a 20 percent interest rate and it's: "no way, no one would do that". And one guy said: "well, yeah, I do. I've gone through a terrible divorce, and I'm really short of money." And we were like sitting there completely goggle eyed. "You're paying 20 percent?". The rest of us at the table said: "you know what? Maybe we should lend the money to you." [laughs] That was amazing, and Larry... The best part about him - I met him later. I may have met him when he was an undergraduate in Princeton. He's a theorist. He doesn't do that kind of work at all. But he was told at Northwestern when he was on the faculty that they would give him a summer salary unless he did an empirical project. He did this project of all things. I had a Federal Reserve referee, I remember. He didn't believe it. They have their flow of funds numbers, which didn't show everything like this. Which was just wrong. So Larry... I sometimes chastised the behavioral economists. I think this paper is probably the first really good behavior economics paper. It really shows that what you would expect from standard theory for some reason doesn't work with interest rates. But there were other examples. The other one, labor economics, was George Borjas. He tells this story. I had two referees, one who hated the paper. The other one thought it was good. And so I read the paper and I thought it was good, too. That was the beginning of his papers on using the roy model in migration topics, which was, I thought, extremely original. I mean, there hadn't really up to that point - a little shocking to hear this - But in the first edition, the first volume of the Handbook of Labor Economics, we didn't have a chapter on migration. It's just shocking, right? You think: How? And the answer is because there was no work on it. No one had done any work on it at that time. So George really started doing the work. And so that was a very important paper, I think. It's clear that some of those papers wouldn't have gotten published, I think, without having a real editor. I mean, I didn't just take referees off their word. And nowadays it's got worse because typically most editors don't do anything really. They get often three referees. Which I would never have done that. Way too many. Just think about this. Picking random with 10 percent chance of acceptance, get acceptance from all three it's almost impossible. So I think the way that the journals look now it's not altogether clear to me. Jim Heckman has a whole paper about this. How publishing in the top five doesn't...

The Handbook of Labor Economics

Arthur: [01:36:41] Yeah. It's a nice paper. So my next question was about the handbook of Labor Economics. So you edited it in 86, right? How you got to the point you thought that: "Oh, now it's the point where we have to organize what is labor economics and make the first volume of it."?

Ashenfelter: [01:37:07] That's a very good question. You know what really did that was partly they [Ken Arrow and Intrillgator] asked me. But it really appealed to me because what we really needed was a textbook. We needed a graduate textbook and no one was going to really write it. I wasn't going to write a graduate textbook. No one was going to write one. We didn't have one. So the original idea was: "let's try to put together what would serve as a textbook". And we went through and got all these topics and tried to cover all the things where there was research that we could really teach about. And that's how it came about. No, it came out in 86, but each one of the handbook's was preceded by a conference which had two purposes: One was to give feedback to the papers and the other was to set a deadline [laughs]. The deadline was probably the most important.[laughs]

Arthur: [01:38:24] Deadlines are important.

Ashenfelter: [01:38:26] Very, very tight deadline. So we had a deadline. And I think that conference might have been in 84. So, in 84, there was a lot of work going to be done in the next 10 years and much more empirical. So if you look at the handbooks, they do tend that way. They are more theoretical in the beginning and then they tend to become much more empirical as you get to the later handbooks. And I got Lord Layard to be my co-author because he is very reliable. And also he had a real connection at that time with the macro part of labor. But also, I was trying to encourage... At that time, we didn't have so much labour economics going on outside of the US. So the goal was to try to get more of it. And so that was sort of what Richard and I worked on. I just saw him recently, actually, in London. Not only is he a lord, he's married to a woman who's a baroness. I think that's what they call a lord that's appointed by the queen when they're a woman. They're both lords on their own, so to speak. Her name is Molly. I forget her last name. Pretty nice person. Is so funny that both of them always have these, you know, kind of a mission. But in 1984, his big mission was unemployment. They had the million men march on unemployment in England. But now, Molly is, I talked to her, she's on to allowing assisted suicide and medical marijuana. I guess it is illegal in England.[laughs]

Ashenfelter: [01:40:45] [Ok. Well, that's pretty good. Yeah. Eighty year old woman who's record on medical marijuana and allowing a suicide lock. Could only have those. Some American states have both. Yeah.

Arthur: [01:41:01] Here in Brazil, we have. We don't have them.

Ashenfelter: [01:41:04] So, yeah.]

About Alan Krueger

Arthur: [01:41:07] Next question is. So 85 AER, 86 handbook of labor economics then 87, Professor Alan Krueger joined the department. So I've heard from cards that you had an offer to go to M.I.T., right?

Ashenfelter: [01:41:26] Yes.

Arthur: [01:41:27] And then you hired Krueger. So can you tell me a little bit about this story?

Ashenfelter: [01:41:35] Well, Krueger would tell this story. It's a very funny story. Krueger was a Harvard ph.d. Student and the meetings were in New Orleans. Alan didn't use to like to tell this story, but then later [record error]. In fact, is in his book, Rockonomics. You know, he committed suicide over a year ago. He was a good friend. And I'm still angry with him for doing that. What a waste [sighs]. Yeah, it is a true story. I'll tell you a story. It's amazing. He was on the same airplanes. So my wife and my daughter and I were on the airplane to New Orleans from New York. And alan was on the same airplane. Alan is from New Jersey, so he was probably visiting his parents. And we couldn't sit together, we didn't have three together. So we had my wife and my daughter, and it turned out Alan was sitting next to her on the airplane on the way to New Orleans. And so my wife started talking to him. She basically interviewed him. She says "What are you doing?". He explains what he's doing. And the she says: "You know my husband, Orley Ashenfelter?". "Yes, I never met him" - he replied. And she says: "well, are you being interviewed by Princeton?" And he said: "No, I'm not". "And Would you like to be?" "Oh, yes, I would". Now, my wife comes over me and she says: "we need to change seats. I met this young man and he is just not getting interviewed. And I answered: "you know what? He's probably no good. That's why we're not interviewing him." But it was my wife, so look what I did. I got to change seats and I started talking to him and I thought: "well, this is crazy. I'm here interviewing the guy". So then I just called up the interviewing group and I said: "we should talk to this guy. I don't know why he's not on the list and we should stick him into the list and interview him. And the next thing i know is we hired him. [laughs] That is literally the case. My wife had sat next to him on the airplane. He tells the story in Rockonomics because he has a section in that book about luck. You know, the exposure that a rock musician gets. And what we're going to say after that. And then we were doing papers together. We did have one recently, in fact, and I did read his rockonomics book. I'm still mystified about why that why he committed suicide [sighs].

MIT Job Offer

Arthur: [01:44:47] Well, I guess this is a common feeling when something like that happens. Unfortunately [sighs]. Anyways, you had the M.I.T. job offer, so you decided to stay. Do you remember why?

Ashenfelter: [01:45:22] Well, Farber was at MIT. So I would have had a colleague for sure. I think ultimately the reason I've stayed in Princeton for so long and many people hang out is the university support. It's a little hard to explain. First of all, it has a big endowment. There's a history of the industrial relations section at the university that you nver see administrators are aware of. I think it's like a lot of universities. If you have something that's special and doing well, it's kind of your obligation to keep it going and doing well. So it's very difficult to change that. Because the things that are good, the universities are going to hold on to them the best they can. So we've always had a remarkable support at the university. I mean, we have phenomenal facilities. There was surely no limit on salaries and the rest or all finance and the usual way. We're just treated like economists. The ability to do the research that let you collect data early on, that was not common in most departments. When you hired an economist, you didn't expect them to insist on money for data. That's no longer true. But it was true in the early days. There is also a collegial environment. The director it's actually not a permanent job. After Farber came, we made it one that circulates, so lots of people have been director. It's mostly collegial and it operates like a laboratory, you might say, that was the that people always thought about it. And, you know, as you get bigger, it's maybe a little bit less collegial. Of course, it's harder for everybody talk to each other all the time. Right now, we have a Zoom each week, but it's very difficult otherwise to do that. So actually, I just didn't think I'd do better work if it really had pulled down to. I think in retrospect, I probably would by that time, eventually have had access to better students. We have a hard time competing with them. It wasn't so difficult back in the 70s and 80s to compete with Harvard and MIT. It's very difficult now to compete with them for graduate students. But other than that, I never thought I would do any better work. So I was always driven by how hard is it to do the work. I thought I wouldn't do any better work. So I was happy to stay at Princeton. Of course, it's a nice place. Nice town. Very easy place to live. We discovered that... You know, when I went to Washington in 1972, Washington wasn't expensive. And

Lyndon Johnson had given Richard Nixon a present when he left, and the present was that the salary caps on administrators and government bureaucrats were lifted. So actually, salaries were pretty good. So you had a good salary and real estate prices were nothing like what they are now. So we rented a place, a beautiful house in Georgetown. I actually could walk to the Labor Department, because it was in 14th and constitution. It was a long walk, and it wasn't very pleasant in the summer, but I could walk. And in the back bedroom where the kids were, they could actually see the Washington Monument. We rented this house - it was just gorgeous - in Georgetown, an area that was totally urbanized: had restaurants within a few blocks, we had a local grocery store where actually you could send your kid is a guy would take credit. You had your name on a piece of paper. I'll never forget it because my name was Ashenfelter, and it came after Alsop, who was a very famous journalist. They would say: "what's your name?" "Ashenfelter, right after Alsop". Like famous people there in a grocery store - that doesn't exist anymore - and you could send a 10 year old kid up there and they just give them that stuff and write down their name on them. And then at the end of the month, you paid. It was an amazingly nice life. And my wife and I have lived in the suburbs before that. So when we left to Washington, my wife said: "you know, this is fantastic". And most towns were difficult with crime all over the place. But there wasn't crime in downtown Princeton. So our first reaction was: "why don't we move to the center of Princeton?" It is not expensive. It's not exactly Georgetown, that's for sure, but it's a lot more like it. So that lifestyle just changed us completely. So that Washington experience changed another thing for me that I prefer to be in an urban area. Now, New York is too much of an urban area [laughs]. I mean, that's even true for me when I go to Europe. I like, for example, a smaller town in France to Paris. I like Bristol better than London. You know, it's that kind of thing. Germany is nice because except for Berlin, there aren't really big cities. Italy is mostly small cities. You know, Florence and Venice are not really big cities. They are nice. So I kind of like urban, but not over the top. Not too urban.

Arthur: [01:51:52] So I guess my last question is, at least for now, because we are getting into two hours of talk already, and I still have lots of questions about the difference and difference method.

Ashenfelter: [01:52:04] Right. And it's probably a good time to quit because my Daughter it's coming over for a walk.

The trio: Ashenfelter, Card and Krueger

Arthur: [01:52:08] So my last question for now would be: you stayed at Princeton and then you had you, Card and Kruger. For an outsider, it looks like you became a big trio teaching labor economics and micro econometrics. So is this true? How do you feel about you three there?

Ashenfelter: [01:52:35] It was a golden moment, really. That's true. It was golden. The thing is, we had some confidence, enough people. We actually thought the way we were doing things was the right way to do. That you should have a design for your analysis. You should be trying to get credible measures of the causal effects that you're interested in. We thought that and we were self-reinforcing and the university supported us. We had everything going for us. It was like a little moment when everything was going for us. And I never forget it. The best example of this fo all is probably the book that Card and Krueger did on minimum wages. Card and I had actually worked on this earlier. There is a paper out there somewhere that Card and I did. Someone found it the other day. It's a clever idea. And what it was, is someone approached me from the National Retail Merchants Association to try to get credible evidence that minimum wage had an effect on employment. So what we did - this is before the Card and Krueger book: "Well, here's a simple design. We have longitudinal data (Panel data). Let's take all the people who were making in the year when the minimum wage increase comes into effect. Let's take all the people who are making less than minimum wage and see what happens to them in the next year when the minimum wage comes in. There was at least one paper by David Wise that implied that they would all lose their jobs. If you think there's a productivity number for each person and the wage is exactly equal to that, then if you shave off the bottom, they have all to go away. So we did the analysis and it turned out that if you take up what's the change in employment for people that are below the minimum wage prior to the increased and then take the employment change for those people who are above the minimum wage in the year after the minimum goes into effects - so presumably that wouldn't affect them -, the employment change was the same! This was a sign that the minimum wage could do anything. So we suddenly thought: "oh, wow! This is not good. No one is ever going to believe this". The paper is there. It exists. But we never did anything about that. And that could have happened as early as 1980. Anyway, the papers out there and it is around somewhere. So then, when they decided they wanted to study the increased minimum wage in New Jersey, I think the idea for doing that was already implanted in Card's mind and Krueger's as well. And so then they decided that they were going to do this kind of experiment where they collected data. And what I think was special about that time is they were working furiously. When they wrote the book, they were just like in there day and night. But what was special about it is that it never occurred to them at all that if they just designed the analysis, reported exactly what they found - honestly - that they would get criticized. It never occurred to them because we were like in a bubble. We were in this bubble of the industrial relation section where if you get a really good analysis and you found that whatever economists thought was true isn't - which at that time is every damn textbook in the country - then fine, we would change: we would change economics [laughs].

Arthur: [01:56:57] It is not exactly like that.[laughs]

Ashenfelter: [01:57:00] We learned the hard way. [laughs] You know, I encouraged them. I mean, I felt a little bit of bad about it because I was in that same bubble with them. Right. We were all in the same bubble. And I was encouraging them: "Well, if the results don't show an effect, well, fine. That's what you want to publish". Of course, there were many more chapters in that book. But here's the weirdest part about this. So during the beginning of the Bush administration... Bush was a very smart politician. And one of the interesting smart things about him is that he would get off the table any issue the Democrats could run on that would make him feel uncomfortable. One of them was the minimum wage. So the Congress had hearings on the minimum wage just after this paper come out. And, of course, the Republicans were organizing this phenomenal "get high minimum wage thing" There's a labor economist named David Neumark, in Irvine now. Someone I know who is actually a pretty decent person, but he testified in front of this congressional committee. I watched on television. It was just unbelievable what he said. They had basically got him to say: "no, no, no legitimate economist could ever claim that minimum wage didn't decrease employment." He went on and on with this. Here's the part that's amusing about this. I guess that was the last time after those hearings that the federal minimum wage was increased in America. And those Republicans voted for it. George Bush told them. Get your buddy in there and vote for it. I don't want this as an issue in the next election. So they held these horrible hearings. If you saw a tape... - I don't know if David Neumark has save one. You can probably imagine that if you mentioned the word David Neumark in front of David Card, you'd get a very good response [laughs]. And Krueger hated it. And it is true that what he did was really, in some ways, unforgivable. I'm a more forgiving person, I guess. So now, of course, people have reproduced much of what they found. So we were a little bubble and a lot of students, you know, benefited from that bubble too.

Arthur: [01:59:39] [Yes, it did. That's great to hear because, I mean, that's history, you know. People think sometimes that things in science or in economics are easy and they are not so easy discovering things. And it's not easy and making people believe. It's not making believe. But I don't know, convincing that you're right. It's like a process. It's not all dramatic. And that's a good. Right.

Ashenfelter: [02:00:07] And it's complicated, too. So we. We. Yeah.

Arthur: [02:00:12] Yeah. I mean, now everything looks easy.

Ashenfelter: [02:00:14] Yeah. Before that, even though it's not easy. No, not at all.

Arthur: [02:00:21] [Closure and Farewell]

Appendix 9 – Interview with Orley Ashenfelter (part III): 22-05-2020

Interviewer: Arthur Brackman Netto
Platform: Zoom
Recorder: Easy Recorder
Transcripts: sonix.ai and minor revisions

[00:00:36] [Greetings]
Difference in Differences

Ashenfelter: [00:07:24] Well, okay, where were we...

Arthur: [00:07:27] Yeah. I mean, for today, I guess we have already talked about lots of things. And for today I wanted to talk about the difference and differences and how you wrote the paper and things around it.

Ashenfelter: [00:07:46] Difference in differences. Yeah, yes. Actually, that's an interesting... The original work on that was the reason that I went to the Labor Department and there was this guy named Farber -I think I mentioned him before. They brought from the social security administration and he knew how to link the data, administrative records or earnings. And because he been in Social security, He had the end[?] To do this. So the idea was that when someone went through a training program, which at that time they were controversial... But also a deep concern about whether they worked. He actually had produced the first real evidence on it. And what happened was that each trainee had a Social Security number, and he linked the administrative data from the Social Security Administration to it. So that meant that he had a longitudinal record for each trainee. Typically, then, the trainees were not so young. They were young, but they were typically people who had lost jobs but had worked in the past. So there was a history of them. They weren't just people coming out of school and it was supposed to be job retraining. So you linked them and he had a record for each person over time. But then there's something called the continuous work history sample, which was essentially a random sample of the US population, which he used for control group. And he had done work making comparisons. And what had happened that I think got people after me to try to get me to move Labor Department was that he had concluded that training programs harmed the trainees. That if you compared the people who went to train to those who didn't, after training, typically they were lower relative to where they had been than the control group. So it looked like the difference between the trainees and the controls was negative. So they showed me these data. What was going around Washington that training programs were not just bad, but they were harming the trainees. It seemed unlikely, but, you know, it's always possible that somehow it is. So that's how they got me interested. And that's how I got interested. I wondered: "how in the world could this be?" And that leads to a long story about the difference and differences in two dimensions. One: So I had the data, year by year on trainees, prior to training and during training and after training. It wouldn't be surprising if during training their earnings were lower because they're not working. They were going to the training program. But what I discovered was that - and it wouldn't surprise people probably - that the trainees had as group (cause they were selected for this) lower earnings levels than the control group. So the average for the trainees was less... What people hadn't thought about was the fact that in the years immediately prior to training - typically the reason they went into training is because - they had gone through a bad event. So their earnings dropped relative to the earnings of the controls. They dropped in the period prior to the training. So this is some people call the ashenfelter dip. This was this. And of course, that meant that, if you did a comparison of the period prior to the training for the trainees to after and then do it for the controls, the trainees were adversely affected. So that means there would be a bounceback, actually, and it would overstate training effects. So what you had was if you look at the level, the trainees were lower than the control groups, perfectly sensible. If you look at the change, it depended in what base you'd used. In that paper, I did this with regression. I had a regression model, which had in it, typically, a year effect, a person effect and then the interaction between when training took place and the time period. So I discovered then a basic problem, which is that - depends on what group you look at, but generally speaking -, if you took a period prior to the decline in earnings for the trainees and compared it to their earnings after training and then subtracted off the same change for the controls, we generally found a positive effect of the training program, not a negative effect. But there were these two potential biases. One, that the controls had a higher level on average, and also this ashenfelter dip thing. I tried to present this in the government. My idea was to present this to people. And it turned out that, if you said the word "regression" their eyes glazed over and nobody could understand what you were talking about.[laughs]. So I figure out a better way to say it, which is that if you have a balanced sample of the time dimension or you take out the year effect, or the person effect, that's actually the same thing as taking that average difference. Calculate the average pre and the average post for the trainees. That's a difference. And then subtract from that the average post in the average free for the controls. That's a difference. That difference in differences it is exactly the same thing as what the regression coefficient would get if you had this training dummy. So it turned out that everybody can understand that [laughs]. So everybody learned that what you have to do is not to improve it more, you have to take an average. Then you have to subtract two times. Anyone can understand that. So that is really the origin of the difference in Differences. I was actually running these as regressions. The origin of these difference ndifferences was really an expository device. And the expository device became a kind of a code word for a message that now everybody talks about. They're using the difference of difference design. I merely know what that means. I think it probably as an expository device, it was very, very helpful. And it may even be helpful as a code framework. You know, when you say: "use difference in difference approach". But, of course, it's equally applicable to situations where you don't have a balanced sample. There you really need to read it as a regression, because you wont have the same balance. Let's just say I've got the same number of years treat... You'd have to do it for each group that was balanced, than average them together. So it would be more complicated. It is much simpler to it as a regression. In a way, a lot of examples that are said to be difference in Differences are really regression examples. I think most people actually do their computations using a regression framework, but then they just call it a difference in Differences. It is much easier to explain to people [the subtraction], even though you're running a regression with thousands of observations. So that's really where that came about. I think it changed a lot of attitudes about training programs. I think it really, in some ways rescued the public. It was a different world back then because people really did share information across government agencies. There were seminars. We actually had a working paper series in the labor department. I'll never forget it, cause it is kind of a joke. I called them technical analysis papers. These were papers that economists wrote and it could be technical. And we would circulate freely. We called them technical analysis papers partly because at that time there were some unauthorized wiretaps and the Justice Department couldn't seem to keep track of. So when I would give up a talk on one of these papers. "this is TAP. Today I'm talking about TAP". this is true. I would say: "I would like the Justice Department, to know that the Labor Department actually know how many taps we have". [laughs] As you think back on it, It's kind of hard to believe people would do that today.[laughs] So that was one of the reasons I liked to call on them Technical Analysis papers. We'd say tap number 1, tap number five, and so on. We couldn't do wiretaps. So, I like that way.[laughs] So that was the start and I wrote that one paper. I forgot where it was published. And then there was a sequel to that paper. [record error] If you went far enough back to the training history, you could get a pretty stable assessment of the training effect. Later, I teamed up with David Card and we worked on a second paper. My situation in the earlier paper was one where I took a problem where it wasn't apparent what the answer was. if you did a simple analysis, you would definitely easily get a wrong answer. If you just did a comparison of pre to post for the trainees, you get too big an effect. If you did a comparison of controls and treatments, you get too small an effect. So I kind just found this system for using longitudinal data and fixed that so that we could try to get a stable estimate. In the meantime, the training programs had evolved. At the beginning they were actually trying to retrain people. Despite the fact that the average earnings were lower than the control group, normally they were looking for people who would benefit from the training. People who were selecting the trainees were doing that. The selection scheme was to benefit the trainees. As the years went by, in the 70s, the training programs really turned into more into remedial programs. They were meant to be offered for people that hadn't finished high school or other remedial problems. The selection problems became very severe. So Card and I started working again. We did another paper later with a different cohort of trainees. Card's innovation, what he added to what I was doing in our joint paper, was he figured out that we could also have selection on growth path. Basically that literature is really about the following. There's two things that are going on: One is there's a time series earnings process for the training group and there's also a selection process for who gets into the program. That's made pretty clear in the paper. And David suggested that we shouldn't just have a selection on the level. We could also use selection on the trend. We could imagine that people were heterogeneous both in level = that is what I have done - and in the growth in earnings. [Selection gets] a lot of possibilities. And from that, we tried to generate selection models that could account for what I had accounted for in this ashenfelter dip. Jim Heckman became interested in that problem. He had a very clever solution to the problem, which was symmetric differences and differences. The idea was that: let's say you had a first order zero correlation process for earnings. If you thought that the training program was the period of the selection and took the symmetric difference going back as far as you went forward - He wrote this up in a paper with [?], that was extremely clever - that was another form of difference in difference that could correct for selection also. We tried lots of these things. Frankly, my conclusion from that was that I thought we had to have randomized trials, basically. I think both David and I figured out that the earnings process was complex enough. And then the selection problems were complex, too. And we didn't know how the selection worked exactly. So we couldn't really model it very well. I had made that case earlier in a very obscure paper in the industrial relations research association - now called Labor Employment Relations Association. They had a meeting - I forget where it was - and I wrote a paper for the meeting, which was "A case for randomized trials". It was published in the Proceedings of the Industrial Relations Research Association around 1975 or so. It makes the argument for randomized trials because the selection is a problem. It also gives a little story because it shows elementary analysis of sample design. There are two aspects to it. One is how big a sample you need. And the other is, do you have to balance it, make it 50 percent of each type. Of course, you don't have to, but it will change the power of the test if you do. So I made those points. But, there was also a very serious problem about randomized trials, a moral problem really, which is some people were simply opposed to the randomized trial idea on the grounds it withholds the treatment from somebody. Now, the fact of that matter was that - and I pointed out in this paper - we wouldn't have enough money to service all the trainees in the first place. So a couple of papers were written about this. In other words, we had to turn some people down. Well, if you have to turn people down, why not use [randomization]. And even if you knew that some people would benefit more than others, you could still randomize within those groups so you could take people that are going to benefit greatly and pick in that group a very high fraction, say 90 percent go to the training program and then reduce that fraction as it goes down. You can still make the comparison within each group. Then the average across the groups is an unbiased estimator of the average weighted by the appropriate fraction of people in each group. So I tried to make those points. Actually Art Goldberger had written a paper in which he showed that, basically, if treatment data points are more expensive than controls, than it makes sense to unbalance the sample and have more controls than treatments. So, there was kind of a lively discussion about that. Some of that's come back. Now we see again, people are starting to talk again about sample design. I think they've begun to realize it's expensive. And so that's got started. And ultimately, this guy, Howard Rosen, did have this support work program run as a randomized trial. On the point of view of training programs, that's probably the first one that was ever done. I think Jim Heckman was on the board of the group that did it. I forgot who did it. They deliberately kept me out of it, I think, because I had become quite controversial with the fact that I proposed the whole thing in the first place. I wasn't quite so welcome. Well, it didn't matter to me because I was onto doing something else anyway [laughs].

Arthur: [00:27:45] So. So you started to write the paper in the early 70s at the Office of Evaluation. It seems you had already those ideas, but the paper came out only in 78. Why it took so long?

Ashenfelter: [00:28:00] Well, I was in the government. Two reasons, really. I was in the government from 72 to 73. So I was reporting those results in 73. I think the earliest version of the results is in this other paper I mentioned, IRA preceedings, that's probably about 75. So that would have been work that was done maybe in 74. Would have been just after I left the government. But there was a second reason. It wasn't easy to get a published. [laughs] It was rejected. I've forgotten how many... I know it was rejected by the American Economic Review. I don't remember the other ones, but eventually I sent to the RESTAT and it got public. I would have published earlier except for that. I got rejected. That's life. [laughs]

Arthur: [00:28:55] That's normally a problem. Yeah. So. Right. If you had to say the paper was a paper that you wrote with ideas from the Office of Economic Evaluation or from Princeton or a mix of both?

Ashenfelter: [00:29:15] I mean, the primary work on the data and the analysis was really done in the government. It would have been done in 73 mainlly. That was the primary work. But at the time, we were supposed to be an evaluation office for all of the Labor Department's programs. So we also had a program on occupational safety and health. We did some work with the Fair Labor Standards Administration that who does the minimum wage and stuff like that. There was a training program. There's several other agencies there. We didn't have any connection with labor management, according to disclosure administration, which sort of regulates unions. But there were two other programs we were dealing with. And I had people [doing work]. Bob Smith was doing work on occupational safety and health. He wrote a whole book about the organization of that.

[00:30:18] [Off-topic]

Ashenfelter: [00:33:03] Well, so I was doing more. I wrote a paper about the minimum wage. It was an evaluation of how strongly it was enforced. Most people don't know this, but in the US there's actually no penalty for violating the minimum wage. If they catch you, then what happens is you have to pay what you should've paid. [laughs] So actually, you would think everybody would violate it, right? Because all you do is you pay back what you would have. So that's made me suspicious that maybe the minimum wage shouldn't have quite as big an effect as people thought, partly because people didn't pay it. So I wrote a paper about that. It's at the JPE, I did it with Bob Smith. Then Bob himself wrote several papers on that. We also did an evaluation of something called the Black Lung Compensation Act, which is very amusing. So the office was really directing other evaluations too. Black Lung Compensation Act was a law. Black lung is a disease that coal miners get. They'll get it. And there is a government program to pay their health benefits and also to pinch them off. And we try to figure out what the employment effect of that was going to be when it was adopted because it was adopted with a tax on coal. I think that program still exists, by the way. But all the coal industry is disappearing. So we're doing a lot of things there. We were trying to do some work on evaluating the employment service, which is the system where we match workers and jobs. We had lots of... If you had a list of the taps... That probably exist, by the way, that would tell you all of the things we're doing. The taps... Where would you get them? The Bureau of Labor Statistics has a library, a very good library, and it undoubtedly has all that material in it. In the old days, when I was there with the agency, Labor Department was at 14th and Constitution and the Labor Department's library was actually at 14th and Constitution. It was really a lovely old operation. It must be somewhere else now. I don't know where it would be. I'd be surprised if the taps... Must be copies of them there. I haven't thought about those really in years and years. I don't know how many of them we had there. I think you'd find some of them are quite historic. I mean, Ron Ehrenberg wrote one. Dale Mortensen wrote one. The goal was really to get applied labor research... I don't have a list of those...

Technical Analysis Papers (TAPS)

Arthur: [00:36:33] Those were different from the working papers of the department. Right?

Ashenfelter: [00:36:40] They were technical analysis papers, they were called. So they were working papers, but they were called technical analysis papers.

Arthur: [00:36:48] Do you think I would find them as a working paper or...

Ashenfelter: [00:36:54] Look, let's just do this. Let's go online, see what we find.

[00:37:00] [Searching information on the internet]

Ashenfelter: [00:43:41] The librarians are a fabled[?] crowd, actually. When I was there as a student, there was a woman named Hazel Benjamin. She's dead now, but she goes back maybe to the 30s. The library has always been a big deal. For example, union publications have historically been sent there for archiving. We do a lot of it. There's a there's an organization of industrial nations section librarians. There are several of them. There's one at Cornell. I think there's one at Michigan State. There's one at Berkeley. There are various around the country. And Princeton has one of those. The goal was always to try to have copies of union contracts and things like that, other documents from the government that were especially relevant for Labor. And it would serve as an archive. It's a public good, really. It's not just for us. It's meant to be something useful in general. And there's been a series of librarians. But, you know, typically they last for a while and then they retire. And we're looking for one right now in fact. A new one. And sometimes they go on to other places. The Cornell people might hire our librarian away or something. You know, that's fine. Just get another person that can keep work until you get somebody young. So he's the director. The industrial realtions section library is now a part of the social science reference group. So it's a social sciences and it's a part of that. And he's the guy who runs it. And he knows industrial relations. He does a lot more things...

Early Self-Selection (and Heckman…)

Arthur: [00:45:52] Great. So what can you say about the term self-selection? Because for what I understand, it was not usual to talk about self-selection in the 70s, was it?

Ashenfelter: [00:46:08] No. Well, like that was the problem for program evaluation. There were two things, really. Well, we thought about who would into the program. It was always a little ambiguous as to who would go on the program. It's partly because program operators could select people, but partly also you are always relying on the fact that people would have to self select to apply for them. So these are voluntary programs and the idea was that if they volunteered, now you could see the problem in that. That's because you would ask the question: "Well, who would volunteer?" And of course, the answer is somebody who is having a temporary earnings loss. So let's say you could become unemployed and you didn't have a job. That's a perfect time to get some additional training. If you're wise. For example, during the financial crisis, we would've been smart to build infrastructure. We weren't smart. We didn't self-select correctly. And we should have done that. Maybe the dam in Michigan wouldn't have broken. You saw that? 100 year old dam, it's busted. You know, it's probably a wakeup call for other people there. Fifteen thousand Dams in the Unitesd states and a lot of them were built 100 years ago. They are in bad shape. That was a way of thinking about how do you model why people come into the training program. And the notion is, if you can figure out a way to determine how they come to the training program, it'll solve the problem. So a good example is but let's say that a person has a permanent level of income and a transitory level. A good model of income is they have this permanent level, but there's movements around it. Ok? Now, let's ask ourselves: "which people would be more likely to go with the training program? somebody who had a transitory up or someone who had a transitory down?" Well, obviously transitory down. Because, if you have a year, that's not going to work out very well for you, then that's the perfect time to take time off and go get some more training. So the idea was to try to find a way to model it. Now, it can get complicated, right? Because once there's self selection, if you don't know how people are doing it... Suppose that the earnings process is one where there's a trend and a level. So then now people would have transitory dips would want to get in the program, but also the ones that have lower trends will want to get into the program. So that was kind of what Card that I tried to work on, figuring out a way of dealing with selection in that later paper. But you're right, I think the selection problem probably became best known, really, with this work of Jim Hackman's on labor supply of women. Because the concern here was that the relationship between wages and how much women work could be obscured by the fact that you would expect only the women who had, you know, who are going to get decent jobs to take a job. So all the people that have low wages are left out of your analysis, if you [?] this thing with people who are working. That word self selection, I think came into common use during the study of women's labor supply. And the issue there was important because... Another thing that I did - Jim and I did together - we were interested what labor supply was like, because we were concerned about the incentive effects of things like a negative income tax. Today, negative income tax is basically what they call a universal income brand. That's the current name for it. But back in the day, it was called negative income tax. Actually, Milton Friedman favored it. A lot of economists like the idea, but I think Henry Simon may have been the first person to talk about it. I once saw Friedman discuss it, - They said, you know, a ice cube wouldn't melt in his mouth. He was such a good talker - trying to explain to a couple. You know, there are [NIT?] Caps Tax credited as kind of like it. It's just not the same because if you're not working, you don't get anything. This guaranteed you something even if you didn't work. Anyway, so a lot of us worked on labor supply problems because we were interested in the incentive effects associated with those kind of programs. And there was a belief that labor supply effects would be better for women than they would be for men. And so then the question of what the effects would be could be a very serious consequence. There were a lot of papers trying to estimate labor supply. But this is really hard to do.

Arthur: [00:51:54] So what you're saying is that it seems that both you and Heckman were concerned with the same problem, self selection, but in different applications. So what happened was that you found a way to deal with it in what you were concerned with. And he found his way of dealing with it, modeling the bias out.

Ashenfelter: [00:52:23] Yeah, yeah, that's right. I tried to stall by going to longitudinal data. That was my idea. It was to going longitudinal data and then try to model in a simple way the process by which people are selecting. But it's a combination of things. But the longitudinal data give you a huge plus. You know, even when you have randomized trials longitudinal data is very helpful because it can make your treatment effect more precise. Suppose you did a randomized trial. You can compare the treatment and the control group, and you'll get a difference. And that's a treatment effect. And it has a sampling error. But if you had data on the same people prior, then you could actually take a difference in differences which would still be unbiased estimate, but If there's persistence in the measure that you're getting, could have a lower sampling error.

Arthur: [00:53:30] Ok, ok. This question may sound strange, but nowadays we have this clash between randomisation and structural models. So the way you are telling me this story, it seems that you and Heckmann were not in any way competing. It was different solutions for a similar problem.

Ashenfelter: [00:53:51] Yeah. No, that's right. And he actually worked on her Randomize trial. He was brought in to work on this manpowere and development training program. He did work with a randomized activity. He got into randomisation too for training programs. I always thought it depends a little bit on the problem. So, for example, this work that Bob LaLonde did. I always thought it was important because it could be that a simple econometric device would substitute for randomized trials. I mean, it could be. If there were no terrible selection problems. It could very well work. And there are some examples where it does work. But occasionally because of this Ashenfelter Dip especially, because of the way the selection was so complex and we didn't know what the selection method was, I just came to the conclusion that we couldn't really do it with a model, we needed more. I think Jim probably came to the same conclusion, because he worked on these. He was the one doing the randomized trial with the training programs, I never actually did it [laughs].

Research Discontinuity, Rubin, Oaxaca-Blinder…

Arthur: [00:55:12] And what you remember about other experimental methods or natural experimental methods, because in the 60s there was the research discontinuity design being done in psychology. Have you heard about that?

Ashenfelter: [00:55:33] It started in sociology, I think, the RD's. It didn't take off. It was pushed by this guy, Campbell. There was a guy in sociology and Campbell. I'll never forget, I talked with him once. When he saw the results that I had in that paper at 1978, he was shocked. He said: "well, this is so straightforward! Is quite apparent that there's a training effect here!" I said: "Yes. It does look like there is one doesn't it?" [laughs]. And so he was fascinated by that method, but he had actually proposed regression discontinuity methods earlier. They caught on in economics in a strange way, I think. It was this guy who published a paper about admission to NYU or someplace like that - whose name I forget. That particular paper, which is not about a very important problem. I mean, it's not even economics, really. And that but that paper kind of caught people's eyes. Suddenly, regression discontinuity became a hot topic. These randomized field trials, they are partly attractive because a lot of people who did experiments in the laboratory... Well they didn't really worry about randomization very much because they had a lot of control. But a lot of those were familiar with the idea of the randomization and some of them have kind of moved over to field trials. And when they did that, I think that was an attractive feature. The reality is, I think that the right way to think about it is that you use a method that's really appropriate for your problem. And some problems are just too difficult... There's two attractions to randomized trials if they're done correctly. One attraction is you get done by its treatment effect. The other attraction is everybody can really understand why that's sort of a treatment effect, they can appreciate it. The disadvantage is it takes a long time and it's expensive. So right now, for example, people are working on the COVID. I just saw a talk of Josh Angrist earlier this week. So everybody's working on the COVID and he's actually trying to do some work on improving Treatment. He's convinced that - and I don't know why he is convinced he certainly doesn't know anything about it - some doctors have been giving people ventilators too early. That the bill later actually makes things worse rather than better. And that the ventilator should be more of a last-ditch effort. You know, they're attracted by the fact that you could put a little pulse meter on your finger and you can measure the oxidation in your blood. When it starts to go down low, people are thinking, well, you're not getting enough oxygen. So what he wants to do is to try to measure what the treatment effect of using the ventilator on people and survivorship is. He's actually trying to do this right now as we speak. And he thinks he has some instrumental variables to solve that problem. So basically, the natural experiment idea is instrumental variables. It's a way to try to find credible instruments. What it really means is: "I have an instrumental variable and I really believe in it" [laughs]. A few papers, early papers, had the word natural experiment. You know, really had natural experiments. Well, there was one that involved, I think in Greece maybe, a volcano and What the effect of the volcano was on things. But the guy who kind of broke that barrier was Angrist because he actually had an example of a natural experiment that was a real experiment. That's very unusual [laughs]. There's not many like that.

Arthur: [01:00:16] [laughs] So, you said you’ve talked to Campbell. And did you talk to Dunald Rubin during the 60s or 70s?

Ashenfelter: [01:00:25] I met him, but I didn't really discuss things with him much you know.

Arthur: [01:00:29] Yeah? He was at Princeton. Right?

Ashenfelter: [01:00:35] He may have visited it. He's been peripatetic. He's been all over the place. I always associated him with Harvard. He's not there anymore either. I don't know where he is.

Arthur: [01:00:45] So you don't remember discussing about his papers at the time?

Ashenfelter: [01:00:53] No. No. I know he's involved in this area and a lot of people are interested in some of the matching techniques. I was always leery of those frankly. I would rather to see a plain old regression. If you've got a variable to control for, let's just do it. I think that, you know, the matching idea or there are several other versions of it are kind of a naming. Like a difference in differences is a way of saying "I'm going to show you a regression. You can understand". They're kind of a way of showing you a simple difference that you can understand. Very simple idea.

Arthur: [01:01:43] And what about the Oaxaca-Blinder decomposition?

Ashenfelter: [01:01:58] Oaxaca-Blinder. Is hard to pronounce that. [Actually, Oaxaca is a you know, it's it's a it's not a Spanish name. It's a it's a state. Mexico. Okay. For people. For Indigenous people. OK to learn. So how they got you know, I'm sure the Spanish oh. A X ACA. You don't see X very often in Spanish. Yeah. So I mispronounced warhawk. I think he was a student] He was my first graduate student. Al Rees got him on to doing male-female differences. Then he came to see me and the next thing you know, I suggested we do this linear decomposition. Great idea. And Blinder was actually... I think Blinder's got it from Oaxaca. I'm not really sure if anybody's ever asked him that. But the fact of the matter is that Oaxaca was working on its dissertation and Blinder hadn't finished his dissertation at MIT. He was on our faculty, but he hadn't finished it. So I think that maybe he picked up that from Oaxaca. I mean, it's often called Oaxaca-Blinder, but I think maybe... You could ask Oaxaca himself. He's Still around. He's emeritus now. He's a very interesting background. He's older than I am, even though he is my student. He went to the military. He's from Fresno, California, which is a working class area. It's a agricultural area with many races. So it has Japanese has a bunch of these Monks[?], you know, these people that have worked with us in Vietnam, who come to the US. He's father was Mexican and his mother's Armenian. So he's a cross between a Mexican and Armenian. The Armenians who came to the U.S. either went to New York or they went to Fresno and became agricultural. That's why Armenians in the US are really almost all over California now. There's been a governor of california who was a Armenian. There's a town called Glendale. It's near Los Angeles and is basically all of Armenians. They are Armenian Americans, not really Armenians. And he maried a Japanese American, actually [laughs]. He had been at the military and then he came to graduate school. And he's a very meticulous guy. Everything he does is very meticulous. There's another decompositions. He did work with Mike Ransom on a similar problem of trying to allocate money if you thought the women were discriminated against. Suppose you wanted to give the money back, how do you actually allocate that out? There's a paper about that. So, yeah, that's his work. And he had access to an unusual thing. He had microdata. So his special [?] really was that he had access to microdata. He had one of the first microdata analysis of male-female differences, and then he had this way of attributing it to the... There are different ways of describing it, but it's because of linear progression, it makes it easy to discuss it. And I think that's why Blinder's thesis... And they both have publications around the same time. I think Blinder's is in the journal Human Resources and in Oaxaca is in the International Economic Review. They were around the same time. He was an industrial relations section guy, one of the earliest ones. Then he went to University of Arizona. Stayed there his whole career.

Arthur: [01:06:23] So my point is that it seems that we have these solutions. Your solution, Jim Hackman's solution, Oaxaca-Blinder Solution for dealing with observational data, problems in your observational data. And you all come from Princeton. So do you think this is a coincidence? How do you see this?

Ashenfelter: [01:06:46] No, it's probably not a coincidence. I think it was a combination. Most of us were interested in - and what we were instilled to be interested in because of our teachers - public policy problems. So Al Rees was always interested in that. His friend George Shultz was. Bill Bowen who was also there was. Richard Lester, who was a more traditional labor economist, was also interested in public policy problems. They were all interested in solving public policy problems. And it happened at that moment that econometrics was becoming a well taught and an important subject at Princeton. Dick Quandt was behind that. He's now 90 years old, but still going strong. Quandt was really, I think, a prime mover in that kind of teaching economics. He's a very precise person. He's a good teacher, very good at teaching things, technical things. And he was very helpful. I think there were a few others that we're all kind of at that moment. They didn't have the training in labor economics or so much interest in labor. Stan Black was there at the time. He's really a trade economist. Phil [Howey?] who was really a macroeconomist... They all had some interest in technical things. And some of that was due to a man named Oskar Morgenstern. The original econometrics research project, which still exists, was his. So he was the one who was always bringing econometricians visiting, if not otherwise, to the campus. And he was always interested in new methods. You know, he was often thought of as a facilitator. I don't know if he was German or Austrian, one or the other. He migrated during the thirties and I'm not sure of exactly why, in fact. And Von Neumann was at the Institute for Advanced Study. The only book he really wrote by himself, is called "The accuracy of economic observations". It's about measurement. Very unusual thing. So he was a very difficult man. I can't say that I liked him very much. But he still had this ERP. We still have econometric research. And, you know, the person right now who's probably the key member of it is Bo Honore. Bo Honoré has done program evaluation work of his own and is a student of Jim Hackman. In some ways, they are similar people. My colleague David Lee really sees himself more as an econometrician now and he doesn't comes out of that the same school. Now, David was a Princeton Ph.D., but he kind of comes out of that. So I think it was a combination of everybody being pretty well trained technically. But the problems were the labor economics problems that seemed important. So the question we always ask is: "so why? Why, what do you do? Why do we care? Why are we doing this? What's the reason for it?" And that I think is probably the Princeton tradition in the labor economics area. You should answer the question: "Why are you doing this?"

Mathematica

Arthur: [01:11:01] Yeah. Did you know that Morgenstern was the founder of Mathematica?

Ashenfelter: [01:11:09] Yeah, I think he was one of them. Maybe there was someone else. Two or three people. Yeah. I think Dick Quandt was a shareholder. Harold Kuhn was involved in it. Well, so Mathematica is an unusual enterprise. It originally consisted of several companies, one of which I think might still exist called math tech. But it was originally meant to be kind of like solving linear programming problems, to do technical work for industry. And they had Al Rees got involved with that and that's when this Mathematica policy research took off. I think now that the MPR [not understandable] is probably maybe the only thing that remains. The other parts, all got sold off it. But it was originally a company that had something like three parts to it. Fairly big. The original thing itself was technical work. I think Quandt a partner in it, too. And he did a lot of work on discrete choice. He was interested in transportation mode. So there was a lot of work back when things like the [barkwood?] was being built here in San Francisco, the subway. There was a lot of work on, on modal choice, which is really the origin. And if you think about it, there's a famous paper of McFadden's on conditional logit analysis and the rationalization of it as a model with utilities. That was designed for studying transportation modes. McFadden was deeply involved in the consulting for the Bay Area Transit company. And Quandt was involved, I don't think there, but in other transportation problems. So, discrete choice Models were really designed for a specific purpose.

Microeconometrics vs. Macroeconometrics in the 80s

Arthur: [01:13:17] Ok, that's interesting. So I guess this is one of my last questions about the paper or what happened to the paper. Right after, I mean, not exactly right after, but in the next two years after the publication of the difference and differences and the selection method of Heckman, some critiques of econometrics, mainly macroeconometrics, came out like Sims, Lucas, Hendry, Leamer. So how do you see the development of micro econometrics in face of all those problems in macro?

Ashenfelter: [01:14:07] That's interesting. I mean, we all knew about these other papers, but most of us didn't really see that as affecting what we're doing. Leamer's Paper "Let's take the Con out of Econometrics". Great title. But, you know, his solution is... He's a Bayesian, right? So his solution has never really been popular with economists. And Lucas, as I always thought of, as merely a theoretical critique as well. So I never saw its connection to what I was doing. Although I did a little work in macroeconomics in the early 80s, some with Joe Altonji, someone we haven mentioned. We have a paper that was in Economica. And I did another one with Card in Review of Economic Studies where we tried to, a little bit inspired by the fact that we were familiar with doing these auto regressions, see whether you could get some macro models. The thing about Bob Lucas, he always wrote down crystal clear models that you could actually take to data. I was what I admired about his work. In that regard. He was very old fashioned. For example, the only person that wrote in Macro that was like him, I thought, was Paul Samuelson. You know what? Paul Samuelson wrote down the multiplier accelerator, which is still probably a core abuse of macro [laughs]. He wrote that down in the 30s and 40s. And you could see right away he is thinking as he's writing down that model: "I want a second order difference equation to come out of that". Right? That you could fit that to data. Samuelsen was a very knowledgeable about empirical material. I talked with him for many years in a program for judges. He was well read and up to date. He was not by any means esoteric. He wasn't really an econometrician. He was before that. But I always thought since when I first read that paper of submissions: "What a clever guy. This guy knew what he wanted to come out at the end". It was going to be a second order Linear regression. And Lucas is similar. You could see that he knew what he wanted to get out at the end. So I always like that one with him, too. I thought he was such a good economist. I think he's wrong yet about a lot of issues. And in some ways, a lot of what he did has kind of disappeared. But I don't think much of that had to do with us. You know, in a way by our discussion or you can see the commonality. Right? So there was McFadden working on transportation choice, using micro data and trying to work out alternative choices in the conditional logit setup. Heckman's got his self selection thing. I'm trying to work with longitudinal data to try to solve the problem. In a way, they're kind of related. It was kind of a revolutionary period in some ways of bringing data into micro economics and using the techniques we had. And it started in the randomized trials, which are now very common. Those were started long ago. There's no new idea there. I even did one just for the fun of it. I had one of on a student program on unemployment insurance, had to do with a [pre?]-Check system that they had. Probably relevant for right now because we've extended unemployment benefits so far. But it was funny because it was a little tiny program, not a very interesting treatment, but I got to do actual randomized trials all by myself [laughs]. It was published in the journal Econometrics. We did it with David Ashmore and Olivier

Deschenes. And that paper was, for me, kind of a treat because when you do these really big projects, you can't really do it yourself, right? You lose control. And so I was actually involved with the randomization. I could do everything myself. It's kind of a very nice feeling, even if the problem was not really very important.

Ending – Center of Gravity

Arthur: [01:18:57] Ok, so finally, could you describe for me the main movements in the center of gravity of labor economics and micro econometrics during the 70s and 80s? I don't know. What was the place of Princeton? Chicago, Columbia, M.I.T.? And how things changed during the 70s and 80s.

Ashenfelter: [01:19:23] Well, I think by the time of the 70s and early 80s, we were producing many of the students that were going to create strong labor groups in other departments. So, you know, my students have been John Altonji, who's at Yale, Angrist at M.I.T.. Heckmann... Mike Greenstone now is a big wheel at Chicago. David Card moved to Berkeley. There have been bursts of activity in different places. For example, for a while, Janet Currie was at UCLA. And that was a fairly active area, too. I mean, they have a group there, but it kind of comes and goes. They have this guy, [not undestandable] there now. I think he's doing a good job. He may bring it back. I think he's a student from Berkeley or Columbia. I can't remember which, but he's been around. So it was also the case that we had the funding to. So we had a lot of visitors. I mean, there were a huge number of people. Anybody you could think of almost in the labor economics field had come through there at one time or another. Anybody all the way from... I don't know how many different visitors we had... We had the funding for it, too. Two things were going for us. We had the right ideas, very facilitating administration; and money. And so it was all the right things in the right place at the right time.

Arthur: [01:21:26] That's great. And when do you think that M.I.T. started to became become bigger and the fields?

Ashenfelter: [01:21:35] They really built up... They tried, but we hired Farber back away from them. I think they're now, you know, with with David [?], Joshua Angrist, Acemoglu. Acemoglu does this too, by the way. They've all been at Princeton at one time or another. Mary [?Bertrand] who's in Chicago, she was at Princeton actually for her first job. I think that MIT is a really strong group now. The thing about M.I.T. is that they've always had one advantage over us, which is - and Harvard, too - that it's easier to convince students to go there. So we did have some very good students that come to us. And that's been helpful. But a lot of our stuff has value added. And at MIT and Harvard, I think, they get their choice basically. If they miss a good student is a mistake, right? [laughs] Whereas we have to really fight to get students. It's very difficult. So they have an advantage. And I always said to me:once they had the people, they were going to have the best students. Just because it's, you know, high quality and high quality out. And the other thing is they do a very good job in running their program. I think M.I.T. is exceptional that way. They are people that want to be primadonnas, basically learn to not be prima donnas when they go there. And they have a lot of prima donnas that behave in a very cooperative way. You throw Acemoglu in the same room with Josh Angrist, I could see sparks [laughs]. [Josh gave me a call. I don't know if you know this. He wrote this. Like I told you, Angus had written this memorial, Alan Krueger, and I think he was getting nostalgic. So he just called me up out of the blue one under par. And so he was being dissed out. But I said to somebody, you know, Josh doesn't do this, Daljeet very well. Not really is not really the best thing. But you know what?] The point is that they try to operate as a team and I think they do pretty well. So, you know, the only thing they were missing is they didn't have the right approach to the problem. They didn't have their modern labor economists. Once they got them, then I think they've done very well. Chicago always had had a strong empirical group. You know, they a insisted on getting Heckman even at a time when, you know, when Heckman was always slightly at odds - He can be like a bull in a china shop too - with some of the people there. Berkeley always had good people. They're stressed out, especially with the current financial crisis. But then there have been, you know, other places. Michigan has had a strong group, very strong group, and they've trained good students. I would say that it's spread out to be a bigger deal. But even, you know, even in Michigan, one of the early people who were there was Gary Solon - was one of our students. So it was kind of a lot of things came together at one point.

[01:25:39] [Closure, farewell and discussion about possible interviewees]

Appendix 10 – Interview with Gary Solon: 01-06-2020

Interviewer: Arthur Brackman Netto
Platform: Zoom
Recorder: Easy Recorder
Transcripts: Sonix.ai and minor revisions

Introduction

Arthur: [00:00:01] Ok, very good. So I'm a Ph.D At the University Sao Paulo and I'm a fellow of the Hope Center at Duke and my research concerns the history of micro econometrics, policy evaluation, applied labor economics and experiments, natural experiments. Some institutions that I'm concerned with are the Wisconsin Institution for Research on Poverty, Princeton's industrial relations section, Mathematica, Rand Corporation, the Office of Economic Evaluation, Michigan Survey Research Center. And some people that I'm researching are Greg Lewis, Albert Rees, Orley Ashenfelter, James Heckman, Card, Kruger, Angrist. And some people from Michigan, like James Morgan, from Wisconsin, like Robert Lampman or Harold Harold Watts. And the idea is to put all those guys and those institutions together and say how micro econometrics and labor economics became a thing in the 70s and 80s and 90s. And behind that tell how experiments and natural experiments became such a big movement in economics. So mostly, I'll have some broad questions about labor economics and micro econometrics and then some minor questions about Princeton and the industrial relations action. That's mostly what I'm looking for here. Is there something that you would like to say before I start some questions?

Solon: [00:01:57] Yeah, I find that I'm having a bit of an impulse to react against… Well, you probably feel like the list you gave was very broad, but in some ways it seems narrow to me. Like, if you were to say what you just said to me to a Chicago Ph.D., they'd go nuts. And, of course, I spent 24 years at the University of Michigan, and I feel what was important about the Survey Research Center (and you were right to name Jim Morgan in this) was the Panel Study of Income Dynamics. But in terms of research methodology, to my mind the important center was not the Survey Research Center, but the Department of Economics, and in particular my colleagues John Bound and Charlie Brown along with myself. I mean, I want to give all due credit to the Survey Research Center for what important data resources the PSID and, after it, the Health and Retirement Study have been. But in terms of methodology for first-rate causal inference, it was the Economics Department people I named who were way ahead of the Survey Research Center people. And of course, it's not just Chicago, I mean, you know, the people at M.I.T. and Harvard would also be taking umbrage. And partly because I worked so long at Michigan and other places that weren't in the top, you know, five or six departments, so-called, I do rebel against the idea that all the good ideas come from only two or three places. For example, I'm crediting Charlie Brown and John Bound a great deal. I think they are two of the greatest labor economists ever. And it was a great privilege for me to get to work with them so long. But they were both alumni of the Harvard program and were proteges of Richard Freeman and Zvi Griliches, who were both very important figures in the development of modern labor economics. And while I'm ranting about this (I hope you don't mind!), I hope you don't lose sight of the importance of Columbia University. In the late 1950s, Jacob Mincer and Gary Becker were the most important pioneers of human capital economics, working together at Columbia and having lots of very important proteges from there. Actually, one you could fruitfully talk with, I think… A lot of the people, as you know, have passed away, you can't talk with them anymore, but George Borjas is still alive and active. And he came out of that Columbia program. So anyway, that's my speech, that these movements are more broadly based than you might think. And in many cases, some of the people you named were important in some ways, but might have actually been years behind other people.

Arthur: [00:05:32] That's exactly the kind of problem I'm having here, because this history has really brought really, really broad. I'm trying to pick up the guys behind the natural experiments movement. But if I'm going to see labor economics and micro econometrics in a really broader way, I know this goes to Columbia, to Chicago, to lots of places. I mean, one of my papers is about Orley and Heckman in the 70s. And we know that Heckmann wrote his self selection model in the seventies. And we know that he had been to Columbia and then to Chicago. So this story about the self selection model is not a Princeton story, but Columbia and Chicago history, although it starts at Princeton.

Solon: [00:06:26] That's another example. Did you know that, at the same time Heckman did this, the same discoveries were made by Lung-Fei Lee while he was a graduate student at the University of Rochester?

Arthur: [00:06:37] No, I didn't.

Solon: [00:06:38] And one place you can see that is if you read the classic Willis and Rosen model about education and self-selection, where they do that so-called lambda-metrics, they give all credit to Lung-Fei because he was their student there. This is sort of an example of what I'm talking about. When I taught that for many, many years, mostly in graduate econometrics classes, I always called it the Heckman-Lee estimator because I knew they both concurrently developed it. But because Heckman is more famous than Lung-Fei, he tends to get all the credit, but it's not right. They actually both did it at the same time independently.

Arthur: [00:07:18] And what happens is that I found lots of references to the word self selection in two methods of dealing with self selection in sociology and lots of places before Heckman. But I mean, it's a very complex history.

Solon: [00:07:33] Yeah, but what they came up with is saying they could treat the expected value of the error term, given endogenous selection into the sample, as essentially an omitted variable, and then they could treat it by including an estimated version of that omitted variable. That was truly an intellectual breakthrough. I'm just saying two people made that breakthrough at the same time.

Princeton as a Grad Student

Arthur: [00:08:03] Yeah, yeah, I know and that's very interesting. I didn't know about the Lee paper. I had to look that. So let's talk about how was being a graduate student in the 70s with all of these happening. So how it was? I mean, labor and microeconometrics were not, like, clear at that time, right?

Solon: [00:08:31] I was at Princeton 1979 to 83. One way to place this is that, among the remarkable stream of Princeton graduate students at that time, I was one year behind Dave Card, which was quite an experience. I always felt like I was coming up short because he and Orley were my main reference points. It turns out I'm pretty good at what I do, but not as good as they are. So I felt like sort of a fool for a while. I was two years behind a guy named Mark Plant, who was actually a very good researcher who co-wrote some with Orley, but then left academia after several years at UCLA. I was three years behind Joe Altonji and David Bloom. Actually, a classmate of David Card's who was also very good and a good role model for me was Mike Ransom. Before I arrived at Princeton, there were people like John Ham and Hank Farber. So I was surprised when you emailed me that I was the first one to have econometrics in the title of my dissertation, but the reason I had it was because one of my three chapters was entirely an econometrics chapter. So I couldn't call it three essays in labor economics because one of the three wasn't. It was really an econometrics paper, which had to do with fixed effects models, but it was about the econometrics, not about the labor market. By the time I was there, there was already a very well-established tradition that Orley's students whose first field was empirical labor economics were quite regularly treating econometrics as their second field, and that was a strong tradition. When you said I was the first one with econometrics in the title, in that dimension I was not breaking any new ground. People like Farber and Ham and, for that matter, Heckman a few years before were already quite regularly taking econometrics very seriously following Orley's example in that. And actually, I think, a fairly important influence for some of these people, I know Heckman would… Have you talked with Heckman?

Arthur: [00:11:14] I mean, I'm trying to, but I'm having some problems with his schedule.

Solon: [00:11:20] Yeah, he's a busy man. He would be very interesting to talk with if you can get him to. I expect he would give a huge amount of credit to a senior econometrician named Richard Quandt, who was on the faculty at Princeton and was a pioneer in endogenous switching regressions models. And I think Jim would credit him, he has credited him for that in the past. And I think Jim would say the reason he came to some of his own path-breaking work, including the structural model of women's labor force participation and the associated econometrics (And that led in turn to his famous paper on sample selection bias.), I think he would say he was working on that because he had learned the endogenous switching regressions model from Quandt. So Quandt was quite often a committee member for those of us who were studying with Orley, including that he was an important member of my dissertation committee. In particular, on that econometrics chapter that was one of the three chapters I wrote, he was a big help to me on that.

Arthur: [00:12:38] He taught the econometrics course. Right?

Solon: [00:12:46] Actually, by the time I was there, he wasn't teaching graduate courses very much anymore. He was the director of the Ph.D. program, so he'd taken on that administrative work. He was very good at that, too. I have very fond memories of him. But I think some of the other people who were ahead of me probably did take graduate courses from him, and probably many of them did have him on their dissertation committee. Another important figure then who was a classmate of Joe Altonji and David Bloom was Rob Porter, one of the pioneers of modern empirical industrial organization. And I know that Dave Card, who was his classmate, was a huge fan of his. In a way, he was doing what we were doing, except it was a little more original for him. He was applying modern microeconomics and econometrics to industrial organization. And I think Quandt was a very important adviser for him. And Rob also was applying the endogenous switching regressions model, to doing structural models in industrial organization, and made actually quite a great career out of it.

Arthur: [00:14:14] Yeah, he did. So what do you think is that there was alreayd a very structured way of doing labor economics in the 80s at Princeton?

Solon: [00:14:36] At the time I arrived in 1979, I was very purposefully going to Princeton to study with Orley. And I understood what that meant. It meant, you know, kind of following in his footsteps in how to go about doing empirical micro. That path was already trodden by many other people by 1979. What was not well trodden at Princeton was natural experiments and what I kind of like to call unnatural experiments (by which I mean, you know, purposeful randomized experiments), which was what Orley was becoming enthusiastic about while I was in graduate school. He was not particularly engaged in natural experiments. I was. And he was supportive of my doing that, but that was not what he was focused on. That's part of why I sent you that e-mail exchange with Steve Pischke. Just as I was reacting to which particular people you were naming and which particular institutions, and saying, "No, it was much broader than that." When Steve first emailed me, he was seemingly wanting to credit Angrist, Krueger, and Imbens almost with inventing natural experiments. And I said, "Well, of course not." I mean, the way I put it, I said, "Well, look, in your book *Mostly Harmless Econometrics*, you point out that John Snow was practicing this in the mid- 1800s." And I said that's an example of how this in a way is just common sense that many researchers came up with – and mostly not in economics if you go far enough back. And Steve agrees with this now. We had a little more e-mail recently. But in terms of natural experiments, as I pointed out in the email, what kind of turned me on to it first was this UCLA dissertation in the 1970s by Kathleen Classen, which I believe was written under the supervision of Finis Welch, who is more or less a contemporary of Orley. I believe he's still alive and you can talk with him. I would recommend that. I'm glad you talked with Orley, because I recall that years ago at a meeting of the Society of Labor Economists, Finis Welch was being honored and Orley was sort of the keynote speaker who was introducing Finis' speech. And Orley was reminiscing about the camaraderie among the small cohort of young labor economists, including him and Finis, but also Sherwin Rosen, George Johnson... Well, a bunch of people. It was sort of a small group, mostly not coming from Princeton. Probably more of them were coming from Chicago than anywhere else, including Finis. And Orley was just talking about how exciting it was that they were all embarking on this new mission to apply modern micro theory and econometric methods to labor markets. And of course, they didn't invent this. Gregg Lewis, Jacob Mincer, and Gary Becker were a bit ahead of them. To some extent, they were disciples of those people. Probably people coming out of Chicago like Finis were directly advised by Gregg Lewis. I guess I'd make a distinction here. You're sort of wanting to look at microeconometrics, and those earlier people like Orley and Sherwin and many others were all about using what they were learning in their economics Ph.D. programs about economic theory and econometrics. They had this very exciting idea that the labor market was amenable to that kind of analysis. But natural experiments were another matter. I mean, in a way, it's an earlier development in the sense of...

[00:19:58] [internet failure]

[00:20:02] Oh, okay, now you're just back. Oh, I'm going to have a gap of a few seconds there. Yeah.

[00:20:17] [internet failure]

Solon: [00:20:27] ...can point to cases here and there, like that UCLA dissertation, and in the e-mail I mentioned how inspired I was by Charlie Brown's minimum wage research, which you can view in the framework of natural experiments, although it's not usually described that way. But then the fad about it – “Oh, everyone's talking about natural experiments" – actually came later on. All of my empirical work was of that ilk while I was at Princeton, but I was a bit ahead of the curve at Princeton. I was kind of out of step with the other people. It was not what the other people were doing at the time. The one thing that was kind of like it, where Orley and I were somewhat on the same wavelength, was that he was very interested in putting resources into randomized experiments. And then that leads to Bob LaLonde's dissertation, which Orley advised and he probably gave Bob the topic. Unfortunately, you can't talk with Bob because he died a few years ago. He was my office mate in my last year at Princeton. So then let me give you another anecdote. Some years later – I don't know when, late 1980s or around 1990 – I'm back at Princeton just to give a labor seminar there. And for the first time, I meet Josh Angrist, who's a grad student there at the time. And you know how when you do these things, you meet a person every half hour. So I'm in my half hour with Josh Angrist meeting him for the first time, and he's haranguing me about natural experiments. And I don't really say anything, I just keep nodding and saying, "Yeah, yeah, I know, I know." What I'm thinking, if you could read the bubble above my head is, "Why are you lecturing me about this? I was doing this years ago. You're preaching to the choir. I already know all about this." So I have that one reaction when people start to act like Josh Angrist invented this stuff: "Well, he is a late-comer. He didn't." He demonstrably did not. At the same time, I would say Josh made two really important contributions. One is that he wrote some very important papers, usually in collaboration with Guido Imbens, about properties of instrumental variables estimation. The other is that, by haranguing everybody about the value of natural experiments, he got people's attention. So he deserves a lot of credit for getting labor economists and other empirical economists to check identification more seriously. And many of us, including Orley and me and lots of others, were already thinking along those lines and saying, "People need to be more serious about the credibility of identifying assumptions." And that's exactly why Orley was becoming enamored of randomized experiments during the time I was his student. I think he was having probably sort of a mid-life professional crisis. He was at the top of the profession, he was understood to be one of the very top people in the field, he was quickly a full professor at Princeton, he was publishing in the top journals. But, if you go back to those early papers, including his work and Heckman's and their joint work, by today's standards, the exclusion restrictions that they were basing their identification on were ridiculous. You couldn't publish that work now. And it's brilliant work, basically in how it's combining economic theory and econometrics – basically structural work of an early type. And I was still assigning many of those papers when I was still teaching. But you'd have to be crazy to believe the results because the identifying assumptions are completely incredible – in the negative sense, not when you praise something as incredible. And I think Orley turned 40 while I was in grad school, and I think he was reflecting on it. What was the real value of his work? And I think he had doubts about it and was changing course and saying that he along with the rest of the profession, if we are actually going to find truth, we're going to have to get serious about the credibility of the identifying assumptions. So I think he was very much thinking along those lines while I was there. But where it was leading him mainly was that he wanted to run controlled experiments so that the variation would be assured of being exogenous. And what some other people, including me, were doing, which became kind of a movement, as you say, later on was to seek the natural experiments, to say, "Well, can we look around and find data from settings where the variation we need is just kind of happening coincidentally? And we can get data from those episodes to get credible identification." What Classen was using in that work about unemployment insurance that caught my attention before I went to grad school was situations in Pennsylvania and Arizona. If you want to look this up, it's in a short paper in the *Industrial and Labor Relations Review* in 1977, a special issue about the economics of unemployment insurance. Orley was a discussant of one of the papers at that conference, by the way. And Finis Welch has a very nice sort of survey article in there where he describes Classen's work as essentially being a regression kink design, which wasn't called that at that time, but that's actually, in a way of looking at her study, what it really was. And he pointed that out. But anyway, what she was doing was she was looking at episodes in Pennsylvania and Arizona where suddenly the state legislature had instituted a big increase in the maximum weekly benefit amount in the unemployment insurance program. So she was doing a before-and-after study. You could think of it as a diffs-in-diffs study. She was saying, "Well, let's look at people who became unemployed a bit after the change and people who became unemployed a bit before the change. They faced dramatically different benefit levels." And luckily for her, there wasn't that much else changing in the macroeconomic environment at the time. So the before-and-after comparison was a reasonable way of trying to tease out the effects of benefit changes on unemployment duration. And there were lots of other studies of the same question around the same time, but they were cross-sectional studies. They'd do things like compare unemployment duration in a high-benefit state to unemployment duration in a low-benefit state, as if the two states were otherwise comparable, but they obviously weren't. And I was reading that literature while I worked in the Unemployment Insurance Service in the Labor Department, and I was just thinking most of these studies are completely implausible. And Classen's study stuck out to me: "Well, that makes sense. That seems like a pretty sensible way of trying to find the causal effect of the benefit change." I don't know if she called it a natural experiment or not. But that's what she meant.

Arthur: [00:29:21] Yeah. Yeah. I mean, using this nomenclature was not usual. It became more common in the 90s. Probably in the 70s she wasn't using the term natural experiments...

Solon: [00:29:35] She may or may not have. And I think I used it some. I don't know if I actually used the phrase in my *Econometrica* paper in 1985 or my other dissertation chapter, which was a diffs-in-diffs study of effects of changes in unemployment insurance eligibility rules on quit rates. I know that verbally sometimes I called it that, and I probably sometimes called it a quasi-experiment. So I think the language was sort of around some. But I don't know that it was regularly being used.

Labor Department at the 70s

Arthur: [00:30:14] So you had your first contact with this kind of literature was at the Labor Department. That's it?

Solon: [00:30:23] Yeah, I had been a statistics major in college, then I worked a few jobs in Washington, D.C. after I graduated college in 75, and then kind of discovered that I wanted to be a labor economist. So in 79, I went to Princeton to study with Orley. And the second of the three jobs I had in Washington was working as a statistician in the Unemployment Insurance Service in the U.S. Department of Labor. They didn't actually have that much work for me to do, so I had a lot of time to just follow my own curiosity. So I started reading the literature about work incentive effects of unemployment insurance. It was a very hot topic in the 70s because Martin Feldstein was writing one paper after another, sort of angrily denouncing the perverse incentives in the U.S. unemployment insurance program. So he was viewed as the devil in the Unemployment Insurance Service, even though what he was saying was exactly what you'd expect a public finance economist to say. Of course, there was some truth to what he was saying, although I know people at the Unemployment Insurance Service didn't like to say so. They liked to pretend there was no such thing as work disincentive effects from unemployment insurance, which seemed to me obviously wrong [laughs]. All you had to do was talk to people who had been on unemployment insurance to find out that some of them were gaming the system and so forth. But anyway, because I had sort of a low-level background as a statistician, I just thought it was really cool to look at what is the statistical work that economists are doing on this. And I thought that most of it was preposterous: "Well, this doesn't make any sense. This is not a plausible way of finding out what's the causal effect of unemployment benefit level on unemployment duration." In many cases, the causation could easily be in the opposite direction, but then that Classen paper stood out to me: "Well, that's a reasonable way to try to sort it out." And my first dissertation chapter – the main one, the one I used as my job talk – was taking a similar approach to look at, when you reduce the take-home benefit level by subjecting unemployment benefits to the income tax, how does that affect unemployment duration? So I was doing a fairly similar sort of thing at another level of differencing. I was looking at Georgia before and after unemployment benefits became taxable, but they were taxable only for people with higher family income. The benefit taxation applied only if your gross income was above a certain level. So my idea was we could treat them as the treatment group, and then the majority of people whose income wasn't that high were a sort of control group. So compared to Classen, there was another level of differencing. I guess she was just doing differences. I was doing differences in differences [laughs], although we didn't call it that then. It just seemed to me to be an obvious way to try to solve the identification problem. And as I said in the e-mail trail I sent you, one of the highest compliments I ever got was, somewhere along the way, when Dave Card said that when he saw me doing that a year behind him in grad school, it caught his interest and he said that was one of the first things that got him interested in the natural experiments idea. So I guess the meta-message I'm saying is ultimately lots and lots of people were involved. I'm not claiming to have invented it at all. It was something that people here and there were thinking about. And then it picked up steam over time.

IRS Descriptions

Arthur: [00:35:15] Yeah, that's exactly the kind of started that I'm trying to tell, because that's what I'm feeling when I'm looking into this literature. Things were pretty much there during the 70s and later on it started to become bigger and get some attention and then it became the movement that we know nowadays. So something that has a name and has their own methods with their own names. But they were pretty much there in the 70s, even in the 60s, maybe, but they didn't have a name. And it was kind of someone doing that there and in somewhere else, but nothing that was structured yet. And so I'm trying to see how those things got together and became a reality. And so I have a different kind of question to you: If you go back to your years as a grad student, what is the picture of the industrial relations section that comes to your mind? I mean, Dave Card said he remembers Orley smoking his cigar. So what is that you remember?

Solon: [00:36:37] Yes, at that time, the Industrial Relations Section was housed in the basement of the university library called Firestone Library. And so we sort of had our own little community down there. You may be surprised to hear that Orley was the only senior faculty member in labor economics at that time. That made me a bit nervous. I was going there to study with him, and if he had left, I would have had a major problem because he was the whole deal, really. There were two assistant professors, Jim Brown and Cordelia Reimers. And I actually never got to know Cordelia very well. I think she was mostly at the Woodrow Wilson School, the public affairs school. But Jim had an office in the Industrial Relations Section, and we actually became quite good friends. And he wasn't officially on my committee, but he was an unofficial adviser, a very smart guy. He came out of Chicago, by the way. Who would have been his adviser? Maybe Sherwin Rosen, I don't know. Jim is still alive. He would be an interesting person to talk to. He was at the Section the same time I was, but as a junior faculty member. It sounds like what Dave was describing to you was this scene where... Well, here's the thing about being advised by Orley. There was no point in making an appointment with him because, if you did, he wouldn't show up. He was just on his own trip and he, you know, just did what he wanted. But he sort of led by example, in a way. What would happen would be sometimes he would just kind of amble into the Section whenever he did and he would hold forth. Everyone would come out of their offices to hear him doing his thing. And sometimes it was just a comedy routine, basically. But he was also talking a lot about economics. I remember at the time Dave saying, "Well, what you get from Orley is philosophy, the way he thinks about economics," and you kind of got it on the fly. I mean, there wasn't even a full labor economics sequence at Princeton then. There was just one semester taught by Orley, and that was it. There wasn't even a second course. So we would take one semester from Orley. Most of his lectures were about areas he had worked in himself. So there were these huge gaps, which turned out to be the things that Sherwin Rosen had concentrated on. So when I was a new assistant professor in Michigan having to teach labor classes, that was sort of when I discovered this goldmine of work by Sherwin on human capital investment and equilibration of the labor market – which Orley had hardly lectured about at all because it wasn't what he had concentrated in. I'd been wondering all along, "Well, what's such a big deal about Sherwin Rosen? What did he do?" And then I started reading it when I needed to round out my own education in labor economics so I could teach it properly. So what we had was this very limited curriculum. We had a wonderful and active weekly seminar in labor economics. Dave probably talked about that. I think it was Friday afternoons and it was in Dickinson Hall where the Economics Department resided at that time. And it was great. It was a workshop in the best sense of the word. We, the grad students, could pretty much schedule ourselves into seminars whenever we wanted to. (And I'll come back to that in a few minutes, about how I had to do that a lot just to get feedback from Orley about my own work. He wouldn't read my papers or meet with me otherwise. He just wasn't organized like that.) But every week we were all gathered together, more often with an outside speaker than an internal one (but we had both), and we'd have this very serious and intense 90-minute conversation about whatever research was being presented. Orley, of course, was very vocal. He was kind of the leader of the audience in that. But by listening to his reactions to what was being presented, we were kind of learning at his feet about his way of thinking about research. And by that time, he was very interested in, "Should we actually believe the answers this person is offering?" So there was always a focus on, actually, the research design and was the conclusion being offered really what we ought to believe, or were there alternative interpretations or better ways to get more convincing evidence. So that was a terrific way to be learning our craft, in that workshop which was meeting pretty much every week. The story I was telling, and probably everybody was experiencing this, is that if you weren't co-writing with Orley (which I wasn't or mostly wasn't, but Dave and John Ham and Joe Altonji and some other people were), it was very hard to get him to actually read your draft and get him to react to it. What I discovered was that a very good way to get feedback was just to schedule myself to give a seminar on it. And I think usually he hadn't read it ahead of time, but he was so brilliant that I would get first-rate feedback from him on the spot at the seminar. So that's how I would get feedback on my papers from him. I would schedule myself into the labor seminar and get the feedback from him at the seminar. I have a recollection of one of the times I did that (It might have been about the econometrics, no, no, it was about my second chapter, the diffs-in-diffs study about quit rates and unemployment insurance.), but I discovered this econometric puzzle, which with Orley's help led to my third chapter, and Dick Quandt helped on that as well. I was just sort of presenting some stuff, very weird stuff that was showing up in my serial correlation diagnostics. But anyway, so Orley sort of gets into it. He wasn't treating me as a student whose feelings had to be respected. He was doing the same thing he did if Jacob Mincer came and gave a seminar, he was just going at it. You know, "What's wrong with this study?" and "How could we improve this?" And it was very constructive in that way, but not in a way of being tactful with me. So one of the grad students, who was a couple of years ahead of me, was like very concerned, and she came to me, almost like I needed a hug, saying, "Don't take it personally. That's just the way Orley is." And I said, "I'm not. I'm very glad to have gotten the feedback. That's why I scheduled myself in this seminar." But it was like that.

You did have to be thick-skinned because he didn't worry about his grad students' feelings at all. If he had criticism of our work, he just let it flow. I value that. The hard part was getting him to read or hear, just getting the opportunities to get the criticism. But there were ways to do it. And giving seminars was a very good way to do it. You just had to go in realizing you're going to get a lot of criticism. But that was the point for me. I wanted the criticism.

Arthur: [00:45:34] And you had an office at the basement? grad students had offices there?

Solon: [00:45:39] Oh, well, it happened [sighs]... Usually students did get an office, but they had a lot of visitors and so forth, so I didn't really get a proper office till my very last year. I had a tiny carrel in the library in a space near the Industrial Relations Section. So it wasn't till that last year when I shared the office with Bob LaLonde, who was a year behind me, that I had a proper office in the Section. But I was kind of next to the Section and I spent a lot of time in there. And of course, if Orley was in and holding forth, it was not to be missed. I mean, it was entertaining, but it was also educational for us. So it sounds like you already heard about that from Dave Card, but I had that same experience, and anybody else from around that time, Mike Ransom, Joe Altonji, Bob LaLonde, and so forth, would have the same story.

Arthur: [00:46:47] And what do you remember about having contact with other departments? I don't know, statistics, sociology. You guys had contact?

Solon: [00:47:04] Generally, no, really not at all. I mean, there must have been occasions where somebody knew somebody and might have had intellectual conversations, but that was not... No, I'd say to a first approximation, that just wasn't happening. Because we were isolated, we weren't even in the Economics Department. The Industrial Relations Section was in a separate space. So, you know, in my first two years in taking coursework, of course, I was in the Department building a lot because of being there for classes. After that, well, I would be there for an hour and a half Friday afternoon for the labor seminar because that was held in the Department space; there wasn't a seminar room in the Industrial Relations Section. I would go over there occasionally to meet individual faculty members, like if I needed some advice from Richard Quandt about why my programs weren't running [laughs] or whatever I needed help from him on. But no, after the coursework, we were fairly removed from the Economics Department, never mind other departments. In Michigan, I had more interdisciplinary contact because, in my first many years there, part of my appointment was in the public policy school. So I was influenced a lot by a colleague who was more a sociologist than anything else named Mary Corcoran, who kind of showed me the ropes of studying intergenerational mobility, which was one of my own main research areas over the course of my career after grad school. But at Princeton I don't recall there being much interdisciplinary conversation at all.

Arthur: [00:49:24] And what about contact with people from outside? Like NBER, Chicago. Columbia...

Solon: [00:49:32] You mean while we were in grad school or later on?

Arthur: [00:49:37] Yes.

Solon: [00:49:48] I would say only through the seminar series that we would be exposed. Orley would bring people he thought were interesting in to talk about their current research, and they were from all over the place. Well, one other thing was that the Industrial Relations

Section was lavishly financed by their endowment. So there was money for bringing in visitors as well. So, for example, I just talked about how Orley was able to give me some guidance about this econometric problem I wrote my third chapter about. Well, Steve Nickell had been visiting from England, I think the year before I arrived as a grad student. And it happened that he was working on that. He published an *Econometrica* paper in 1981 that was an abridged version of that work. So Orley knew all about the issue and Nickell's work on it. And so he was able to point me to that, which was tremendously helpful, and it really led to my third dissertation chapter. It was an extension of Nickell's 1981 *Econometrica* paper. So there was this stream of people, like Nickell the year before I arrived. My last year in grad school, there were two visiting people from elsewhere. One of them is my friend now in Tucson, Ron Oaxaca, who is a Princeton Ph.D. You really ought to talk to Ron, because Orley was one of his advisers while Orley was still an assistant professor, I think. Ron was a grad student at Princeton a bit after Orley was – probably more or less a contemporary of Heckman, I suppose. So Ron would give you an earlier perspective, but Ron came back for a year as a visitor during my last year of grad school, and we became friends then. And it was good to have him there. He was a bit of an unofficial advisor and was a bit more avuncular, more touchy-feely than Orley was. Which was helpful to have my year on the job market. So we became friends then and are friends to this day. Well, I was actually his replacement at the University of Arizona when he retired. I filled the position that he vacated by retiring, and he and his wife still live in Tucson. My wife and I are friends with them now. We'd go to dinner with them till the pandemic. Now we just do occasional email with them. Someday we'll be able to see them again. He would be a good person to talk with from that era.

Arthur: [00:52:52] Yeah. I mean, he was contemporary to Jim Heckman, Orley Ashenfelter and John Pencavel.

Solon: [00:53:01] Yeah, I think more or less. He was probably a little bit junior to John and Orley. He probably overlapped with them, and then Orley was still there as a faculty member. He was probably one of Ron's dissertation committee members. I would guess Al Rees was probably his chair, but I'm not sure. Quite likely he had Dick Quandt on his committee, but I don't know. But anyway, I was just saying, we would have visitors. I would say the main outside influence was the people that would come in as seminar speakers. By that time, Gregg Lewis had retired from Chicago, but he was on the faculty at Duke. Orley brought him once to talk about, I guess, the book he was writing about unions and wage rates. I guess I briefly met Gregg for a couple minutes at that point and I attended his seminar. In a way, he was our grandfather, right? He didn't publish very much. But by way of the people he trained at Chicago, he was definitely one of the main pioneers of modern labor economics, and we knew that. Orley revered him as such. So did my generation of grad students, because we were learning what Gregg Lewis had done and what his influence was. I noticed that later generations have no idea who he was. It's kind of nice that you're doing this historical work. One of my favorite colleagues when I was at Michigan State was Todd Elder, who is a protege of Joe Altonji from when Joe was on the Northwestern faculty and Todd was a grad student there. So Todd is sort of my intellectual nephew. If Joe Altonji is my older brother and Todd is his son, that makes Todd my nephew. In terms of methodology, Todd and I were very much on the same page. We are sort of part of the same extended family. But Todd had heard of Gregg Lewis, but he really had no idea what he did because he was in the next generation. So it's very cool to me that you're doing this work because I do feel that Gregg Lewis's generation is probably not very much appreciated by the generations that came after me. My generation was sort of taught by the Orley Ashenfelters and Sherwin Rosens and so forth who had learned from Gregg Lewis and did tell us that they had. But I think the next generation after me didn't get that, and probably aren't as aware as they should be of what

Jacob Mincer and Gary Becker did. Both of them are major heroes to me, even though I met each of them only on a couple of occasions. But from afar, I studied their work and learned a tremendous amount from it. They're among my biggest professional heroes.

Arthur: [00:56:56] Gregg Lewis is very interesting. He didn't publish much, but it seems that he was a very good adviser. People liked him a lot. And what's interesting is that after he died, he left his archives at Duke. So we have access to all his archives there.

Solon: [00:57:21] Well, that's awesome. Did you discover some interesting things there?

Arthur: [00:57:24] So the thing is that because of the virus, I had to come back to Brazil. So now I'm in Brazil. And I only looked at Albert Rees' archives that are there also. I didn't have time to look into Greg Lewis archives...

Solon: [00:57:44] I hope you'll have a chance to go back.

The Bigger Network of Labor Economics in the 70s and 80s

Arthur: [00:57:47] I will go back. I'm planning to go back. Yeah. So the idea is having a paper about this generation, especially about Gregg Lewis and maybe his connection with Albert Rees, because it seems that things start pretty much with this generation and then their students went and made it broader. I mean there's a Greg Lewis - Albert Rees - Orley connection and then your generation. And then it's really big because each one of them had lots of students and things get big. But we can trace...

Solon: [00:58:29] Right. I feel like in Michigan during the years that John Bound, Charlie Brown, and I worked together, we kind of had our own variation on that, which I like to think of as Michigan labor economics. But really the fundamental principle is just, "Let's follow the evidence. Let's be serious about what the evidence has to show us, even if it's contrary to what we went in thinking or maybe even especially if it is." Because to us that was the main point of empirical work, to shift our priors. If you're going to stick to what you thought in the first place no matter what, why even bother looking at the data? And I would say, after my leaving and John and Charlie kind of getting past their prime as researchers, that sort of labor economics has declined at Michigan. At first I was grieving about that, and then I realized, no, what happened is it's not dead. We exported it all over the world. I got an e-mail this morning from one of my first and best students, Michael Baker, who practices this sort of labor economics at the University of Toronto. I have former colleagues and students who are doing this work at the University of Edinburgh, in Scotland. I was just in touch with Marianne Page, a Michigan student from the 90s, who pretty much built another center of this style of work at the University of California at Davis. And on and on and on... It's very well and alive at Michigan State with my former student Steven Haider and Joe Altonji's former student Todd Elder. Another excellent younger researcher in our lineage is Jessamyn Schaller at Claremont McKenna. Jessamyn is the protégé of Ann Huff Stevens, who was my advisee at Michigan. So just as you said, it spread and it's all over the place now. And actually, it's much more decentralized. But if I were going to list sort of who the best practitioners of it are now and where they are, it would be a somewhat different set of places than where it grew up. Well, probably the best place now, to pick one, would be Berkeley, which is because Dave Card moved there and then he brought in Pat Kline, who was our student at Michigan. Just as you said, it spread. And where it's well and alive tends to kind of vary over time according to who locates where, and there's an ebb and flow. Northwestern was a major center of it for some years because of Joe Altonji, Bruce Meyer, and Chris Taber.

Then once Joe moved to Yale, especially with Costas Meghir joining in there, Yale has become a pretty leading place for labor economics research. And Northwestern almost completely collapsed for labor research. In a way, what they did there, whether it was consciously or just played out this way, is now they've got a lot of people doing work somewhat of this sort in development economics, even though there's not much going on there in labor economics. So there's kind of an ebb and flow of these coincidences of, you know, who dies and who retires and who moves in and where new centers are built up just because of who got a job where.

Arthur: [01:02:37] So you think during the 70s and 80s... Who were the major players in labor economics, Princeton, Chicago, Columbia?

Solon: [01:02:50] Of course, I was very conscious of it in choosing where to study... I got to know Orley a little bit. My last job in Washington was at a temporary national commission that was reviewing the system of labor force statistics. And Orley was a friend of the chair of the commission, a Washington labor economist named Sar Levitan. He was probably a little more of a politician than an economist, really, but he was chair of this commission that was reviewing the work mainly of the Bureau of Labor Statistics and the Census Bureau. And I got hired to be sort of a junior statistician there. But then it turned out I could make a bigger contribution by just working alongside the economists there. And so I actually drafted many sections of the commission's report despite being so junior and undertrained for it. It was empowering to me. I thought, "Oh, I can do what the economists are doing. How about if I just go get a proper training and do it for the rest of my life?" Anyway, Orley was on a statistical advisory panel to the commission. So he visited on a few occasions and I got a little acquainted with him there. And as I just sort of looked around at, well, among the younger labor economics researchers (mostly in academia), where did they get their training, it was so striking what a successful training program Princeton was at that time. And I had some personal reasons to locate not too far from Philadelphia. A few minutes ago, my wife went past. That was a time when I was falling in love with her, and she lived in Philadelphia at the time. There are many reasons, personal and professional, why Princeton was the place for me to go study. So I was kind of cognizant and it helped that I wasn't just like a kid in college. I was someone working among economists and getting interested in labor economics. I had more resources than the typical grad student applicant would have for figuring out where do I want to get a proper training as an economist. Chicago was certainly a major center at that time. But it had a reputation. Their policy at that time was to admit a huge entering class and then flunk out about maybe 70 percent of them at the end of the first year. And I was cocky enough that I probably would have assumed I wouldn't be among the ones flunking out. But I understood that it was a terrible environment for students to be competing against each other for scarce spots in the second-year class. It was pretty much the opposite at Princeton. The students worked together. Actually, I want to come back to that in a few if you have time, and I'm going on a long time. But what was the best part about the Industrial Relations Section... At that time, there weren't very many faculty and Orley was around when he felt like it and not when he didn't. So the community, above all, was among the grad students. We were in it together. I'll come back to that in a bit and fill out that picture a bit. Dave Card probably told you about that, too. It was probably great for him to be working in a place where he was the little brother of Joe Altonji and David Bloom and so forth, I would think. But where were the centers? Well, certainly Chicago was a major one at the time I was applying to grad school. But I did not apply there because I was so put off by the culture of the place. The other places I applied that I was pretty serious about were... I applied to a couple places in the Washington, D.C. area because that's where I lived, but I wanted to go to kind of a higher-level program, get a better training. So the places I was serious about applying to were Princeton, M.I.T., Harvard, and Penn. But Penn was not as much of a training ground in labor as the others, although it did have stuff going. Paul Taubman was quite active at that time at Penn and made some important contributions. Probably that was on my list partly because of the location in Philadelphia, and then Harvard and M.I.T were... M.I.T at that time was viewed as the number-one economics department. In the economics profession at large, M.I.T. was the most successful training program. And it was pretty clearly understood to be at that time. But it was an odd place in labor economics. The senior labor person there was Michael Piore, who was not doing the kind of labor economics that Orley and Sherwin and others were pioneering. He was really from a previous generation that didn't buy into that. Hank Farber was there, but he was an assistant professor. So it wasn't the same. In a way, it was the best place to study if you could get there, except it wasn't that good in labor economics. So I wasn't accepted there. But had I been, I probably would have gone to Princeton anyway. And I was accepted at Harvard, but they also at that time did not have a very good reputation for how they were treating their grad students. So I turned them down to go to Princeton instead. It was a little unusual because Harvard just has this aura. And people probably just would have thought, "Oh, if you get into Harvard, you go to Harvard." But I correctly figured that Princeton was probably a better place for me to study given how well-formed my interests were already. Having said that, Charlie Brown, who is some years ahead of me, and John Bound, who is a bit junior to me in terms of professional seniority, who became cherished colleagues at Michigan, did both study at Harvard. Harvard did well by the two of them because they both studied with both Richard Freeman and Zvi Griliches. And between the two of them, they got what they needed and got a very good training. They, by the way, are people you could talk with if you want to get the Harvard story. Charlie and John are both actually still on the Michigan faculty and they're from fairly different eras. Charlie, I think, was a classmate of Eddie Lazear, Gary Chamberlain, and Jim Medoff. So there was quite a community at Harvard. And Charlie for many years co-wrote very productively with Jim Medoff. It helped a lot that Charlie, unlike Jim, had seriously studied with Zvi Griliches too. So Charlie more than Jim knew his econometrics and took our brand of economics more seriously than Medoff did. Medoff, to some extent, was a throwback to that earlier generation. Maybe he was somewhat closer to Michael Piore than to what we were doing. That's sort of showing my age now, I've been rambling. So we were talking about where were the centers of modern labor economics. So Harvard was an important one. Richard Freeman was a very important person in that generation, along with Orley and Sherwin and others. If you could talk with Richard, you should. As I recall he did some kind of postdoc or something. He spent a year in Chicago, I think, maybe before he became an assistant prof at Harvard, and I remember him saying that that was very important in his own intellectual development. He probably got to rub elbows a bit with Gregg Lewis by spending a little time in Chicago. Becker might not have been back there yet, but I don't know. He probably was. Becker was back there by the 60s, wasn't he?

Arthur: [01:12:53] Yeah. Yeah. Yeah.

Solon: [01:12:55] So he probably did hang out a bit with Lewis and Becker and that probably kind of got him a little bit more with the program of what you're calling modern labor economics. So Freeman was quite an important figure in the 70s, and folks like Charlie Brown and Eddie Lazear were students of his, among others.

Arthur: [01:13:26] But do you feel like Freeman was doing the same thing that you guys were doing at Princeton? It was the same kind of econometrics or it was different?

Solon: [01:13:40] I think it was similar. He was doing quantitative work on labor market questions. Richard never studied econometrics as seriously as we did. And I think it had an adverse effect on his later career. I think after the mid 80s, he was kind of out of date. He kept writing, but he'd kind of fallen behind because he hadn't studied econometrics as much as we had. I mean, to this day, you know, he's always been a very intellectually curious man and is a great role model in that way. But I think he wasn't as good at formal analysis as those of us who were trained at Princeton. Actually, this was a problem of people who went through Harvard. Students of Richard's who did not also study with Zvi Griliches or Gary Chamberlain were handicapped by not having enough formal training. So a lot of the Harvard students were like that. They just didn't think analytically as well as I feel they should have. But then there were some like Charlie and John, who did also study with Zvi, or later on Gary Chamberlain or maybe later on Guido Imbens, and get a proper training in methodology, theoretical and econometric. So I think the Harvard thing was... There was also this wave towards modern empirical work at Harvard, but Richard wasn't as teched up as some of the Princeton and Chicago people. And then Columbia was kind of a whole other story. It was probably as exciting a place as anywhere in the late 50s when Mincer and Becker were both there. But then Becker went back to Chicago. Mincer was a really important pioneer, but he wasn't that teched up either. He was really from an earlier generation. He wasn't that teched up, and I think Columbia kind of fell behind in that way. Jacob himself wasn't that teched up. The econometrics staffing at Columbia was not good. So I think Columbia kind of fell behind some. Jacob still had students. It would be really interesting for you to talk with George Borjas because I think he also did some kind of postdoc at Chicago after he studied at Columbia. He actually co-wrote with Heckman. One thing they worked on was duration models. People don't cite Borjas and Heckman that much anymore, but Borjas was actually working on that with Heckman, I guess while Borjas was visiting in Chicago. So he'd be a little bit like Richard at a later time. Borjas got some very important training at Columbia, must have been hugely trained in the economics of human capital. But I suspect he got a lot of his econometrics training afterwards at Chicago and then just teaching himself after that.

Arthur: [01:17:38] Ok, that sounds interesting. I guess I'll go. I'll have to make some calls. Or put all you guys in a room and make you talk, and then.

Solon: [01:17:47] Or have a Zoom session. It would be very interesting to me to compare notes. You know, I occasionally cross paths with George, and then we usually do the conversation where these two geezer labor economists remember when there was still economics in labor economics. So we're a little bit a part of this... I mean, I understand the point of natural experiments and I feel like I was ahead of the curve on them. But when people like George or Dan Hamermesh and I get together, one thing we tend to talk about is we tend to complain about how many younger so-called labor economists today don't take economics seriously.

Arthur: [01:18:41] Yeah. It's all about the method, right?

Solon: [01:18:45] That's kind of what I talk with George about. But George actually, like me, has also been very serious about methodology. He's definitely part of that wave as well, maybe more interested in econometrics of the Heckman variety, and probably less enamored than I am of natural experiments.

Arthur: [01:19:15] You said you wanted to talk about the community at Princeton.

More IRS descriptions!

Solon: [01:19:19] Oh, yeah. There was one other thing about life in the Section. The people who were just there all the time were the grad students. And also, like the secretarial staff, who were like our unofficial aunts. They were kind of motherly to us.

Arthur: [01:19:39] Do you remember their names?

Solon: [01:19:41] Irene Rowe and Edna Lloyd. Actually, do you know what they would do sometimes? In the same spirit of Orley not keeping appointments… Actually, I do want to give Orley due credit. I'll come back to that, too, because I'm so grateful to have studied with him and I would do it all over again. I actually just e-mailed with him recently – well, partly because, you know, you told me that he had suggested getting in touch with me. So that was a good occasion for me to renew contact with him. We'd been out of touch for about a year or so. Every now and then, there'd be this episode where Orley was not getting his recommendation letters out for those of us who were on the job market, and the secretaries would go on strike on our behalf [laughs]. If Orley wasn't getting his letters out, they basically just refused to do any other work for him. This was back when there were still typewriters. They'd put the covers on their typewriters and just sit there and say, "We're not typing anything for you, Orley, until we get these letters out." So they were kind of our mother hens in a way. They really took care of us. The grad students, we were sort of a band of mostly brothers because there weren't very many female economics students back in those days, even much fewer than today. So it was a community. It was kind of unusual that no one else within my exact cohort – my year -- did labor. I was the only one who did labor economics. The most successful students in my cohort were pioneers of game theory, who were students of Hugo Sonnenschein. And I was good friends with them because we took classes together and we liked each other. I played tennis with some of those folks. But in labor economics, I was all alone in my class, but that was unusual. Dave Card and Mike Ransom were in the class one year ahead of me, and they were close colleagues to each other. They were people I could talk with. Mark Plant was two years ahead of me, and he was very present in the Section and was a good person to talk with. Joe Altonji and David Bloom were there when I was a first-year student. I didn't have as much connection with them because in the first year of an economics Ph.D. program you don't get to sit around and talk about research a lot. So they were good big brothers to the extent they could be. But actually, Joe was the teaching assistant in the second semester of the first-year econometrics sequence. And he was a very earnest, hard-working teaching assistant. When I was in my first year, Dave Card was in his second year. He was probably getting more out of those upper-class colleagues than I was able to at first. Actually, Jim Brown as an assistant professor was sort of a part of that community, too. He was in his office most of the time and liked to hang out with us. So it was a critical mass of people at similar career stages who were in it together and were all committed to doing empirical labor economics in the Orley fashion in which we were relying heavily on modern econometric methods. So that was very much a community and I think there were things like that going on in some other places, too. I'm sure that the Chicago students working with Heckman were probably working together a lot and talking with each other and so forth. We had that going at Michigan. People you could talk to, our students there, would describe a similar thing. Their offices tended to be at the Population Studies Center at the University of Michigan, because that's where they could get fellowship support. So people who were being advised by John and Charlie and me then tended to have offices together at the Population Studies Center. Then, by that time, John was working part-time at the Population Studies Center, too. So when I talk to reminisce with former students, they'll talk about how great it was that they had that community of grad students clustered together. So that's a wonderful thing when you get it. And we really did have that going in the Section at Princeton back in those days.

Arthur: [01:25:00] Yeah. We have that at Duke. And that's really great... Being together.

Solon: [01:25:08] Yes. You know what I'm talking about.

Arthur: [01:25:11] ...with your fellows and people with the same kind of idea, it's great. And having the opportunity to talk to the professors when they come out, it's a great environment for learning.

Solon: [01:25:26] So that was a very special thing about the Section in those days and for all I know… It obviously continued for at least some time, maybe to this day. That cohort that had Josh Angrist, Dwayne Benjamin, Thomas Lemieux, John DiNardo, that was quite extraordinary, and they worked together a lot and learned a lot from each other. So I would hear about it from them. And I remember Orley talking about that group once. He said, "Oh, you might think from the outside that it was some kind of magic wand that the faculty waved." And he said, "You know, it just kind of just happened. We lucked into having this group of students at the same time who loved working together and were really inspired." And I think the faculty helped, too. That was when Card was still there. Probably even more than Orley, he was a really crucial adviser for them. But also, there was a magic that just came from the coincidence of having this group of students there at the same time. John DiNardo passed away a couple of years ago, but the rest of them are still alive and mostly quite active. You would have a great time talking with Thomas Lemieux, if you could. Very friendly guy.

Arthur: [01:26:57] OK. I talked to Josh...

Solon: [01:27:00] That's good. I wish you could talk with John DiNardo, but it's no longer possible. Dwayne Benjamin might be interesting. He ended up becoming a development economist, and I think he's now a university administrator at the University of Toronto. But that could be an interesting perspective to get from him. I think he hung out at the Section a lot, but he was also a student of Angus Deaton.

Arthur: [01:27:36] Deaton taught at Princeton, right, during the 80s?

Solon: [01:27:41] He actually joined the faculty the year after I left. And so I just missed him. But he did visit at Princeton some of the time while I was there. I should have taken his classes probably, but I didn't. My friends who did said those were terrific classes for learning econometrics in a way that was useful to practitioners.

Possible Interviews

Arthur: [01:28:09] So you think that I should interview Thomas Lemieux, George Borjas, someone else?

Solon: [01:28:16] My goodness. Of course, at some point you just have to stop talking to people and get to writing. It depends on what you want to ask people, about which era and what perspective. But it's good that you already talked with Josh. If you want to get another take about that era, Thomas is probably as good as anyone to talk with. When I thought about who are the people to talk with, I was a little startled by how many of the people have passed away.

Arthur: [01:29:00] That's why we have to make these talks.

[01:29:03] [Off-topic]

Beyond Princeton

Solon: [01:33:32] We haven't even talked about the Wisconsin people, except you mentioning the Poverty Institute and the negative income tax experiments. Of course, there was a whole community there too for a while. I was actually pretty friendly with Glen Cain, who was one of the senior labor people at that time, because he was a member of that statistics commission I worked for. He was sort of the token academic on that commission, and I got to know him in those days. He was very nice about talking with me about where to apply to grad school and all that. I would guess Glen is not alive anymore, but I'm actually not certain. I think he probably died sometime in the last few years.

Arthur: [01:34:26] I'm really looking now into Princeton and that's my main goal. Now it's learning about the Albert Rees - Ashenfelter - your cohort, and then Josh's cohort. But I already talked to my supervisors, they know this is an ongoing project, probably for a lifetime and the idea is always putting more into this history. And certainly I'm going to talk about Harvard and all those other places at some point because it doesn't make sense to write this history only above Princeton because we know it was not only Princeton.

Solon: [01:35:16] I'm glad I spoke up, though, because there's a tendency in academia for people to talk about their own sect as the important one. And I'm a bit of an outlier in that for various reasons, partly just my iconoclastic nature, but partly also working with Charlie and John, who are Harvard people, in Michigan and feeling so much kinship with them. If you talk only to Princeton people, you're going to get an exaggerated notion of Princeton's part in this. Princeton is one of the main players in this, that's true. But it's far from the only one, and somewhat I'm being a bit of a traitor to my sect in saying this, but that's just kind of the way I am. There were other major centers then and ever since. On the other hand, I want to say on the other side of that that Princeton was truly a major center then. That was the professional part of why I went there to study instead of the other places I might have. And although I've talked about how you had to have a bit of a thick skin to thrive as a student of Orley... And by the way, John Bound and Charlie Brown said the exact same thing about working with Zvi, that he just told you what he thought without sugar-coating it at all. All of us, they with respect to Zvi and me with respect to Orley, felt very grateful for having followed the path we did in grad school. We studied with the right people and we had to have a thick skin sometimes. Of course, people say this about Heckman as well. Jeff Smith, who's at Wisconsin now and was my colleague for a while at Michigan, surely would have stories to tell about being Heckman's student. I'm very grateful for what I got from Orley, including that when the chips were down and when I needed a job my last year in grad school, I believe he was the key person for persuading his contact at Michigan, George Johnson (who was a good mentor to me in my early Michigan years), that I was the person he should hire. I think I probably do owe my 24 years at Michigan to Orley persuading George that he should hire me. And I'm very grateful for the experience. There are other aspects of the experience at Princeton I haven't talked as much about, but Princeton took very good care of its students. That's part of why I went there instead of Harvard. There were some teachers there like Hugo Sonnenschein, Alan Blinder, and some others who were really conscientious and inspired and charismatic teachers. I had less to do with them when my coursework was done. But they were very important for me in that part of my studies. Actually, so was another person there that you probably won't hear about otherwise – Cheng Hsiao, you know about him? He's an econometric theorist. He's now at the University of Southern California, longtime editor of the *Journal of Econometrics*. In the early 80s, he was writing a book called *Analysis of Panel Data*, which was a very important textbook on fixed effects models and the econometric methods for them and related models. So Cheng was a real pioneer in econometric methods for panel data. Princeton was very understaffed in econometric theory at that time. They hadn't hired Angus Deaton yet, they hadn't hired Whitney Newey yet, who was an assistant prof there before he went back to M.I.T., and Dick Quandt was sort of preoccupied with other things, such as being director of the Ph.D. program. So the econometrics curriculum in the grad program was not good. But in my second year of grad school, Cheng Hsiao visited Princeton. At that time, he was on the University of Toronto faculty. So they knew they couldn't cover the courses, so they brought Cheng in. And the main thing I did in my second year, other than sit in on Orley's class (I actually sat in on Orley's class when I was in my first semester, but then I took the course seriously my second year.), Cheng basically took over the whole econometrics curriculum my second year. I basically just took every course Cheng was teaching that year. And mostly, other than studying labor economics, I mostly devoted my second year to learning econometrics from Cheng Hsiao. The other labor economists from Princeton got their training from other ways. People ahead of me probably got it mainly from Dick Quandt. People after me, like the Angrist generation, probably learned GMM from Whitney Newey and applied econometrics from Angus Deaton. So in a way, I was a cohort of one who had Cheng Hsiao to thank for my econometrics training. In my cohort, there was really no other way to get it except because Cheng visited that year, and it kind of saved me. I should be grateful to whoever at Princeton – I don't know who it was – who had the bright idea of getting Cheng to visit that year.

Arthur: [01:42:07] Professor...

[01:42:08] [Closure and Farewell]

Appendix 11 – Interview with Daniel Hamermesh: 01-07-2020

Interviewer: Arthur Brackman Netto
Platform: Zoom
Recorder: Easy Recorder
Transcripts: sonix.ai and minor revisions

[00:03:53] [Greetings and presentations]

Introduction – The Office of Evaluation

Arthur: [00:04:00] OK. So my research concerns mostly the history of micro econometrics and policy evaluation. So that's mostly about labor economics in the 60s and 70s and 80s. And I'm researching some institutions like the Wisconsin Institutes for Research on Poverty, Princeton's industrial relations section, Mathematica, the Office of Economic Evaluation and Michigan Survey Research Center.

Hamermesh: [00:04:33] Let me stop you there right now. One other place you might look at it's not really a place, but we did an awful lot of stuff. There were a group of us who sequentially you fill up a position in the US Department of Labor called ASPER. Are you familiar with that?

Arthur: [00:04:53] Yes, I'm familiar with that. I mean, what I've read is that at some point the Office of Economic Evaluation was discontinued and then everything went to the Department of Labor and one of the departments inside the Department of Labor was the ASPER.

Hamermesh: [00:05:16] Ok, back up a second in that. The Office of Evaluation: Is that the thing that was involved with the poverty program?

Arthur: [00:05:26] Yeah.

Hamermesh: [00:05:30] That's totally different. That became... I'm sorry to do this to you. That became part of what is now the Department of Health and Human Services called ASPE. Totally different from ASPER which was in Department of Labor. Two totally different things, different people. ASPER in the Department of Labor did only labor programs. Ok?

Arthur: [00:05:58] OK.

Hamermesh: [00:06:02] The person who was the economist who was involved in it initially was Ashenfelter. He did the first year. George Johnson, that he come across his name.

Arthur: [00:06:13] Yes. Yes. George Johnson from Michigan right?

Hamermesh: [00:06:17] Yes, Yes. He did the second year. I did the third year, which was seventy four or seventy five. The fourth year was done by Frank Starford, who is also from Michigan, and the fifth year was by Alan Gustman who was from Dartmouth during his entire career. But he was a Michigan Ph.D. We were all very young at that time. What we did was introduce and spend money on serious evaluation of labor programs. For me this was a wonderful experience.

[00:07:22] [internet failure]

Hamermesh: [00:07:28] Anyway, so for me, this was a great experience because I've done work on labor demand in my Phd thesis, but questions about labor demand came up and it got me thinking about a lot more and eventually led to my book in 1993. And I think this experience certainly affected Ashenfelter's work. He's probably most well known for his evaluation stuff, right? And it clearly came out of the work that we did in the Department of Labor. Indeed, Orley just is trying to collect all the papers written in Asper, which were called Technical Analysis Paper. And if you ask him, he can give you the entire list because he was just asking me about it. Yes.

Arthur: [00:08:14] Yeah! I'm involved with that. I mean, we were talking some weeks ago and then he remembered about those papers and he started to ask all you guys about those papers. And I know he has a wide list now. And he's sent me.

Hamermesh: [00:08:32] I may still have that list. After we're done talking, I'll mail you the spreadsheet. OK?

Arthur: [00:08:37] OK.

Hamermesh: [00:08:38] I mean, I've known Orley for 50 plus years now. He is not the most organized person at all as you probably noticed. He and I are opposites. I'm incredibly organized as you've probably gathered from my setting up this meeting. Anyway, so ASPER was not the same thing You're thinking of at all, but it did lead to all these pure labor evaluations and certainly... I think that... who was most affected among the five of us? Alan Gustman had gotten involved studying retirement. Everything he did after that has been on retirement and etc. So it defined his entire career. Orley has certainly defined much of his career, not all. Not the stuff on wine [laughs]. In my own case, it certainly defined all the stuff I've done on labor demand. Indeed, if you look at my 90s report Labor Demand, you'll see I thank a guy who had been a high government official. He eventually became secretary of Treasury. He asked the question. George Johnson, that's certainly affected his career also. Frank Staford not at all. But you can see this time in Washington led to a lot of stuff that we did later on and a lot of people paid attention to. So I would think about Asper being this sort of breeding ground, if you will, for an awful lot of researchers in labor economics in that and subsequent periods. So that's an aside. I didn't answer your question at all, but I sent you off on another path that may be somewhat relevant.

Arthur: [00:10:15] You know, I I've already heard some of that from Orley, but you are the second person that I'm talking to that actually lived at this moment and known these places. So it's great to hear from someone else about that. I mean, it's really important in fact. And there is lots of things missing there that it's hard to get for someone that's not even American. So it's good to hear that.

Gregg Lewis

Hamermesh: [00:10:44] I can even help you in more ways than you know. Firstly, as I mentioned, for some strange reason, I'm writing a thing for a book on the Chicago school, and I'm writing a piece about H. Gregg Lewis. I will send you a draft. I'll send you my second draft. This has been a tremendous amount of work to write. It's just because... I don't know if you read anything about Greg Lewis's have you?

Arthur: [00:11:17] One of my ideas for the thesis was writing a paper about Gregg Lewis and I've been to Duke and Duke, in its archives, has the papers from Gregg Lewis. And I've looked into some of that, but there's not much and so it became kind of hard writing a single paper about him. So my idea was to write about him and maybe his connection with Albert Rees and the Chicago-Princeton connection. But there's still not much there.

Hamermesh: [00:11:52] Ok, first of all, I would urge you not to do this. I think for several reasons. One, my former colleague and many times co-author Jeff Biddle. Have you come across this name? He's a Dukey. He's a Duke Ph.D. I think, from Crawfurd Goodwin in 1985. He did a piece on Lewis, which I have not read for a reason: because I don't want to influence what I write. I will send you my piece probably in August. I've got to get Biddle's comments on it. Let me just tell you, Gregg Lewis was the most obscure writer. He makes Jim Heckman look clear [laughs]. He really is just unbelievable. It's also the case that Gregg Lewis taught me undergraduate labor economics in 1963. And then my last two years at the university (63-65) he employed me as his research assistant. So in some sense, I have some bona fide to write about him and I'm trying to write about him in the context of what he actually did, nothing personal. So I'll send you that. OK? In a very real sense, the more I read of Gregg's stuff, the more I understand the extent to which he at least foreshadowed a lot of the fundamental contributions you're familiar with and maybe even actually suggested that when people read his stuff. For example, let's think, Selectivity bias. Heckmann selectivity bias. I call it Gronau-Heckman selectivity bias because Reuben did the same thing in a paper in 74, upon which Greg Lewis commented. But Greg, had actually talked about it in some notes two years earlier - unpublished notes. That's certainly one thing. In terms of things on labor demand, his paper - I'm sure you're fluent in Spanish, right? You could read Spanish.

Arthur: [00:14:04] I can read Spanish. But I'm not fluent.

Hamermesh: [00:14:08] Do you know his paper "Interés del Empleador en las Horas de Trabajo del Empleado"[?]. Have you read that?

Arthur: [00:14:19] No. Just a glimpse.

Hamermesh: [00:14:21] Ok, a glimpse. OK, check it out. I mean, it's incredibly dense. I can read the Spanish. I can read a little bit. I mean, you know, I live in Texas. I call this place then northern Mexico [laughs]. It's what it really is. But I mean, there's an English version of that unpublished, which I got a manuscript. And there are an awful lot of ideas there that underlay and inspired subsequent work. So this is a guy, if you're talking about applied micro, a large fraction of the ideas that we now take as gospel, which involves economics, not just measured a la angrist-pischke - which as you probably know I'm not very fond of. You probably know that already. You can probably tell that. I mean, this is real economics. OK? And the people from the Chicago school, I think were almost all inspired by Gregg. There's a paper by Rosen and Willis, called "education and self selection", which I'm sure you know. The suggestion on that is very clearly... The idea on selectivity is very clear in what Lewis's comments on Gronau's paper. George Borjas's paper on self selection and immigration, which I'm sure you know. Fundamental paper. That idea also: The germ of that is contained in Gregg Lewis's short comments on Gronau's selectivity. So this is a guy... I mean, it's easy to say ex post that some work inspired everything. I mean, on the other and an awful lot of it is in there. Anyway, that's my steal on Lewis. I just dictated to you the summary of the start of my final section, but there's much more to it than that. So let me answer your questions rather than be raving about Lewis.

Arthur: [00:16:13] Gregg Lewis is really obscure [laughs].

Hamermesh: [00:16:17] I know. I mean, I'll prove that to you. I did a survey which is being completed now asking all research fellows of the IZA. You're familiar with IZA?

Arthur: [00:16:30] Yeah.

Hamermesh: [00:16:31] Of course. Asking them, did they know the name of Greg Lewis? And if you do, what do you know about him? What did he do? It's very clear that most of the responses - I got about a 50 percent response rate for an e mail survey, which is terrific. You may not know that, but 50 percent response is really good. Only 40 percent have ever heard of him. These are labor economists, less than half have heard of him. The best story on this is one extremely famous person - guy won a Nobel Prize - wrote back to me very sheepishly that he answered the survey and said: "yes, I've heard of him". And then he realized he was answering about Carl Lewis. You know, Carl Lewis? Anyway, I thought this was just very amusing. I mean, to be mistaken for a runner is just ridiculous. Well, people don't know that we was at all, very clearly. Young people don't. Also, if you run a regression - a probit - on characteristics of the respondents, in particular their age, people who have got their phds after 2000 have never heard of him.

Arthur: [00:17:55] Yeah. The thing is he didn't publish much, right? So it's hard for people to know him.

Hamermesh: [00:18:04] No [laughs]. He didn't publish much. And what he published was incredibly difficult to read. So it's not surprising at all. Anyway, let me answer your questions now. You must have a list of questions. Why dont you fire away.

Early Labor Economics

Arthur: [00:18:20] Yeah, I have some, but... I would start with some broad questions... I guess I still have this question because it's good to hear. So how would you describe the field of labor economics and micro econometrics during your Phd and early career? I mean, it was not like what we have today, right? Today we have this distinction between the angrist thing and the structural thing. And things were not this way back there. Right?

Hamermesh: [00:18:58] I don't think either of those things existed then. I mean, certainly not... I know your family name is German. Do you know any German at all?

Arthur: [00:19:12] Yes, a little bit.

Hamermesh: [00:19:14] Ok, fine. I mean, today, I view these guys: their motto is exogeneity 'ubber alles'. Yeah, I'm fluent in German for strange reasons, OK? And, you know, I find almost no economics there. I mean, it is just: "does X really precede Y?" But the mechanism and the behavior that might generate that relationship between X and Y, these guys don't think about. They don't care about it. And to me, it's that mechanism which in fact is the economics. What's the behavior? What's the maximizing behavior of the agents whose behavior is generating the X and Y? I have nothing wrong with thinking about causality, but I'd like to know in order to operate in the system what the mechanism is that generates that causality. So we never thought about that in that way at all. We would about endogeneity. We were taught immense amounts of econometrics about simultaneous equation systems. So people worried about it. There's nothing new in that. The other stuff today, structural stuff, I view that as a different kind of cult. These are all cults to me, OK? That cult at least has some economics in it, but it's so structured that it depends upon how you write down the functions embodying the behavior rather than thinking about what the behavior might imply itself. I'm "a plague on both your houses" guy. And I'm happy to bet that if you ask Ashenfelter, deep down, he would say exactly the same thing as we all would. Have you talked to George Borjas at all?

Arthur: [00:21:09] No, no, no, I haven't.

Hamermesh: [00:21:11] You might. Well, he's nice. He's a Columbia phd. He was a student of Jacob Mincer. He probably knew knew Jacob better than anybody else you could talk to. And he also spent a postdoc (75-77) at Chicago. And therefore he was extremely heavily influenced by Chicago as well. And I know that. He's one of my closest friends. In fact, we're talking after I'm done with you. You know, we're talking about something. Nothing to do with this, believe me. And he, too, is extremely heavily affected by this. He didn't do the asper thing. He a little bit too young for that. But he certainly was affected by Chicago, as you can tell, by looking at his work. In a sense it's not Angrist - although he worried a little bit about that - it certainly isn't the structural stuff - which I view as being extremely narrow - rather, it's using economic theory to think about predictions and then try to test those predictions as best as possible. Angrist and company - What I'm sure to say - we didn't test them properly, we didn't worry enough about causality. And I'm quite sure a structural type - of which there really are not very many. That's a small cult - would say: "look, you may have thought you were testing things, but you didn't specify the structure sufficiently enough. You didn't characterize the structure in sufficient detail to draw the inferences that you want to take." So I would say we worried about economics much more than either of these groups does or did. Perhaps not worrying about causality enough, perhaps not worrying about the underlying structure of functions enough, although certainly in the case of labor demand - I mean, the stuff I've done and the stuff I summarized in my book - was incredibly detailed in terms of structure. It wouldn't satisfy the exogeneity purists, but we certainly get structure and Lord knows. And the people estimating labor supply based on utility functions and trying to infer from the formal utility functions what the underlying parameters were, were also, I would argue, structural - although probably not sufficiently so to satisfy... I'm sure, Jim Heckman, for example - I dont know if you want to talk to Jim.

Arthur: [00:23:43] I'm trying to, but...

Hamermesh: [00:23:44] Good luck [laughs]. Good luck with that one, sir. I would give you about a .01 chance Okay? But I know Jim. I've known Jim since 1969. I was the extra reader on his Ph.d. Thesis, in fact. I was a year ahead of him. I had started at Princeton in his last year there. And I know he would say exactly the same thing, he would say: "a plague on both their houses". Keep that watchword in mind. I think that characterizes all the people of my generation. I don't want to put words into the mouth of the dead. But Sherwin Rosen, who I view as probably the top labor economist of roughly my generation who passed away quite young at age 63, I know he felt the same way about this. He and I had a talk at a conference - would have been about 1997, four years before he died - about methodology. And he was just shaking his head in disbelief at the natural experiment people. He Just thought this was just a total lack of economics. Rosen's paper, are youfamiliar with Rosen's paper in 74, the hedonic prices paper?

Arthur: [00:25:05] I don't think so.

Hamermesh: [00:25:07] Oh, please! You must read this! "Hedonic Prices and implicit markets" JPE January 74. It is, by the way, I did this paper in the JEL two years ago on citations. You probably saw that one. I had data from 73 to 77. It was the single most cited empirical paper - or applied paper - in those five years. In my view, this was one of the five most important papers published since I started studying economics in the early 60s. Truly fundamental piece, and it embodies all the things that we're talking about: new theory - although in some sense one might argue the basic theory goes back that Adam Smith or perhaps to Gregg Lewis - talked about exactly the same thing anyway. I think all of us of that generation would say the same thing. My watchword, as I said, is a plague on both your houses, which comes from Romeo and Juliet, by the way, as you probably know. But I think it's true in this case as well. What else? What else? What else can I tell you?

Years at Princeton

Arthur: [00:26:29] Oh, you can tell me about when you arrived at Princeton. What did you find there? How was Princeton in the early seventies?

Hamermesh: [00:26:41] Late sixties. Late sixties. A lot of the stuff that you think about... Let me tell you a line, in fact, that one of my... A Chicago Phd student when I was an undergraduate, I've no longer than anybody else in the profession since 1962, his name was Finis Welch, you know the name? Finis was a Chicago Phd who was very much a Chicago economist: "Use theory, Try to measure things well." I remember he was fighting with Alan Krueger during the early 1990s about the minimum wage stuff. He coined the phrase "PAIN" (Princeton approved natural experiment) [laughs]. In other words: "did it get the stamp of approval from Card and Krueger?" Although even David Card... I don't know If you're going to talk to David at all.

Arthur: [00:27:51] Yes. I've talked to him.

Hamermesh: [00:27:53] OK. You have talked to him. I mean, he and I have talked about this over the years and he's told me several times he finds the absence of theory in a lot of these people do to be very disturbing. And my line on this is: "If a sociologist could do it. We shouldn't be doing it." They can run STATA there.

Arthur: [00:28:20] There's an interesting thing there. I'm working with bibliometrics and if we're looking into Dave Card's papers and Orley's papers, they are totally different in what concerns the words they are using and methods they are using then people after them.

Hamermesh: [00:28:39] That's correct. David still does [?] Economics and [?] Economics. And you can see this. You've read that paper I sent you with Bittle about the revolution, right? And you can see this in these people's papers. And David said to me, maybe he didn't tell you, but he feels he'd like to see more theory underlying this. I know if he made that clear to you. He probably did. He's quite consistent. And yet, I think because referees who are mostly often younger are so concerned with exogeneity that they'll just simply say: "well, I don't like your instrument and therefore I don't care about the idea". They don't say this, but they implicitly don't care about the underlying economic idea that's going up. This is almost the same point Jim Heckman is making in his top five journals paper recently. Although he's complaining about his own work not getting accepted. Obviously, as we all do [laughs]. But there's a substantive point there, not just a personal point - which with Jim is always personal. As you probably noticed, every economist you deal with, he's picked a fight with at some point. You know that already.

Arthur: [00:30:03] It's not completely clear for me, but I kind of noticed he is not an easy guy.

Hamermesh: [00:30:10] Not an easy guy. He and I got along from 1969 to 1998 and since then, because of something he did and something that happened he wasn't happy about, I've been following George Borjas advice. George worked for him briefly. George said: "don't write, don't call, don't contact him. You will regret it." [laughs] Don't quote me on this. But certainly I'm not unique. I know it's not me. It's a person fixed effect on his side. It's not a match effect at all. Nor is it a person fixed effect on my own side. Anyway, Princeton at that time - in 1960 was nowhere - I spent four years there. The chief leading labor economist there was Albert Rees. And he had been my graduate teacher at Chicago, in fact. And then he went to Princeton. He actually hired me. He was department chair. He was department chair briefly before I got there. He hired me and then somebody else became chair.

Arthur: [00:31:35] Yeah. I think that maybe he became the department chair... Because he became director of the industrial relations section in 71, I guess. Before that it was...

Hamermesh: [00:31:47] No, no, no. He was director of industrial relations from the time he got there, which had been 66. Certainly not 71 because I was director in 71 [laughs]. I know that for absolutely sure. But I mean being director of the section doesn't mean much. You have a very nice budget, a few faculty and bunches of people. This is quite cute actually. It was housed in part of Princeton main library: Firestone Library. And it was housed in the section that was surrounded by fencing. There was a fence in special section of the library. I rememeber once we hadBob Hall, the macroeconomists down giving a paper. He was at M.I.T. at the time and he gave a paper and he was heavily attacked. And he made this wonderful comment that he hopes they'll keep all these Princeton economists in their cage in the library. Very, very cute line. It wasn't what you think of today at all. It consisted of Al Rees, who's very much a Chicago economist, albeit not very mathematical. And the other senior person was a man named Frederick Harbison. He passed away in like 77 or so, who was an institutional economist. He wasn't a Chicago economist. He wasn't mathematical. He really didn't do what I would consider modern labor economics. These were the two senior people there. Orley had just started there in 68, a year ahead of me.

Albert Rees

Arthur: [00:33:46] I have a question on this, because this is kind of hard for me. It seems that Princeton went through a change from Frank Harbisson to Albert Rees. It was really institutional and became more empirical. And yes, this is kind of hard to understand how it's happened. It seems that Harbisson was behind that, right?

Hamermesh: [00:34:08] These things depend upon people. Yeah, but you have to remember, I mean, Reese was not really a human capital person, his most famous work I would argue was Rees & Schultz - this is George Schultz, who was secretary of state in the United States - a labor market survey. And Rees also did this wonderful paper on information and labor markets, empirical stuff on [search and asking?] wages. So he was a modern labor economist, but Fis - Frederick Harbison - was certainly not. I remember I had the office in the I.R.S. next to a man who had been Provost to Princeton or Dean to the faculty named J. Douglas Brown, who you've never heard of, I'm quite sure, who is an institutional labor economist. And at one point, I was sitting back in my office and I overheard Brown and Harbisson talking and this complaining: "Do you think that this mathematical non sense will ever stop?" And you know the answer is - I think I put this in the paper with Bibble from two years ago - The answer is: it did stop, we stoped being so theoretical because there's no economics anymore. So since Harbisson got his way, although not quite in the way he would have imagined. Oh, so yes, there was this switch. But the switch came because basically Al was there and Al was a modern labor economist, not a very theoretical one. In fact, I just learned this Al Rees' Phd advisor in Chicago was Frederick Harbison.

Arthur: [00:35:50] Yeah?

Hamermesh: [00:35:51] I did not know this. Absolutely correct. Yeah.

Arthur: [00:35:54] I thought it was Greg Lewis.

Hamermesh: [00:35:57] I thought so too. I was making a list in this paper I'm writing of Lewis's students, which is a phenomenal list. And I was going to write Rees and. I looked at Rees' thesis paper and no: Greg was on the committee but Harbison was the chairman. [not understandable] If you go back and look, Rees had a paper in the JPE, I think in 51, in which he definitely says "from my thesis. And I thank the chairman, Frederick H. Harbison." Anyway, the point is that the change clearly resulted because they hired this man who was a modern labor economist, and he basically took over. Harbison I think still had one more Phd student while I was there, but that didn't go very well.

Arthur: [00:36:53] He was at the end of his career. Right?

Hamermesh: [00:36:58] Yeah, well, he wasn't very healthy. I mean, he wasn't that much older than Rees. Maybe 10 years older than Rees at most. But he wasn't very healthy. Also all the young people realized that what he was doing was very out of date. So I look at people who've got Phds in labor in the late 60s to mid 70s when both rees and Harbison were there and certainly Heckmann - who was actually a student of Richard Quandt the econometrician, although I think Rees was on the committee and so as I at the end. I'm trying to think who else were labor people from that time, there were a few others, but they're all Rees' students. And then by the late 70s, they became Orley's students. Certainly, and there's a man who got started in 74 there and got his Phd in 79 Randy (Randall) Filer who you probably don't know of. He was certainly a Rees student and did a very Rees type of thesis. But it was a change because of that one man getting there. It didn't affect [?] Princeton is today quite the contrary. But Al was really only there from 66 to maybe 77. And even then, for the last couple of years, he was in Washington. After that, he became provost at Princeton. And then you wound up the rest of his career at the Sloan Foundation in New York. So I mean, his influence is very short-lived. But it did mark the change, the first change in Princeton, I would argue, yeah. The question to ask me if I were a historian of thought, which I'm not, this Lewis paper is my second last effort in that. Never again. It's too much work. I know people will be angry at me for what I write. But the question for me is: "why the change from the Princeton that I knew that Al Rees created and that Orley helped create. Why the change to the Princeton that you're familiar with and that you're talking about, which happened in the 90s? And I'm not sure why. I mean, I think David Card... Despite his professed interest in theory, I think, is the major person responsible for this, and in particular... What do you think is the first paper in this terrible distraction of natural experiments and exogeneity.

Natural Experiments

Arthur: [00:39:40] I mean, they tend to talk a lot about..

Hamermesh: [00:39:43] One paper! You get only one paper, remember?

Arthur: [00:39:45] One paper... One paper is hard. I mean, we have all these early 90s papers. But what they always talk about Is the Angrist thesis with the Vietnam guys. They always talk about that one. No?

Hamermesh: [00:40:02] No, no. I would... That is random. But in terms of natural experiments, there's no question in my mind the first one was Mariel boatlift paper (Card 89 and 90).

Arthur: [00:40:13] Oh yeah! That one is great. Yeah!

Hamermesh: [00:40:17] And it turns out to be all wrong. You got to read George Borjas' work on it. But in my view, it was the first of these papers trying to look at a natural experiment. They didn't rely on the randomisation that josh's stuff on the vietnam lottery did. That's Josh's contribution. Which is why, by the way, there's no doubt in my mind that Josh and David will share a Nobel Prize someday. No doubt in my mind whatsoever.

Arthur: [00:40:48] Yeah. It was kind of worried that they would want it before I started interviewing them because it would become really hard to reach them [laughs].

Hamermesh: [00:41:04] Both Josh and David are happy to talk to people. They're very vocal. And I mean, David is much shyer than Josh. But they're both very serious guys about these things. I'm not surprised they talk to you at all. But within the next five years, they'll win Nobel Prize together. No question at all. So I think it was the idea of natural experiments and then just as the randomization were about causality, that was the novelty. The one thing that's in David's paper, which wasn't novel at all - in fact, I think it goes back to Greg Lewis again. Sorry about that - was the idea of a double difference. I mean, Lewis was supervising Masters and Ph.D. thesis in the 50s. There were four of them I found, which essentially did double differences, looking at the current difference compared to a previous difference where the current difference as a treatment and a control and both groups were not treated beforehand. That's a double difference. It's classic and that goes back to the 50s, maybe further, but that's the first time i've seen it in economics. So that's not the contribution. It's rather the contribution is in David's case, and certainly in Josh's case, circumventing issues of non-randomness and selectivity by saying something was a shock. that was exogenous. That's their contribution. Doubled difference: Not at all. Nothing new there. So I think it was those ideas that changed Princeton and the fact that Al was there. It wasn't Orley at all, I don't think.

Arthur: [00:42:47] But don't you think that in this paper, the difference in Difference Paper in 79 was important in this?

Hamermesh: [00:42:58] I like to think when I think about this Nobel Prize, I always like to think they'll find a third person who was a precursor. So for example, there was a Nobel Prize in mechanism design for - What's his name? Oh, my mind is shot - for two guys. Eric - forget his surname - and then the other guy from Chicago that was given also to Leo Hurwicz. It was very clear Hurwicz' work was a precursor of these other two guys stuff. He was 90 when he got this Nobel Prize. He died a few months later. In a very real sense, I view Orley's stuff as a precursor. But in terms of the essential idea of the true exogeneity of something be it a natural experiment or a randomisation which have to be thrown up naturally, that's not in his work. His work was in worrying about: "here's the behavior before the shock truly unaffected by the shock". The shock in the case that Orley identified was the application of a training program. And he pointed out basically that there was selectivity into the training program. Those who were in the program had been doing unusually badly before that in a way that was not explained by their demographics. That's his contribution. That's not the same fundamental contribution, although I believe it stimulated the other. And certainly it wasn't the first double difference. It's pointing out the double differences you need to be careful with because the pre may not be so pre. That's a way to think about that seventy-nine paper and the Ashenfelter dip.

Heckman x Ashenfelter

Arthur: [00:44:55] Because what I'm writing or trying to explain is that something happened in Princeton, that they were worried with self-selection and selection problems and that Orley went into a road that puts him into the difference and difference and Heckman went to the other side and wrote the self-selection paper. So it's quite interesting that they were colleagues, they were worried with a similar problem.

Hamermesh: [00:45:25] No! They weren't colleagues ever. They happened to be Ph.D. students...

Arthur: [00:45:32] At the same time. Yeah. And Ashenfelter was even his advisor...

Hamermesh: [00:45:37] But that was well before Orley did anything on this. This stuff came out of his time in the Department of Labor and 72 and 73. Whereas Jim's thesis paper was worried about labor supply and they did by the way write the paper any in Econometrica in seventy 74, which you must be familiar with which very much grew out of Jim's thesis. But even then the selectivity idea was not well spelled out. It was spelled out in Gronau 74 and Lewis 74. And then the econometrics of handling that in a pretty simple way is what Jim is most well-known for, the 79 econometrica paper. So I think you're putting a bit more weight on a collaboration that really didn't happen. I think these were independent ideas that coalesced in Jim's paper in 79, and that in turn led in the early 90s, late 80s to people thinking more deeply about exogeneity - which is in fact the card and angrist stuff. So these are very separate developments, in my view. All of them are separate, but they all fit together. Look, knowledge doesn't just happen. I mean, as the old line goes: "there's nothing new under the sun." And yet a very real sense there is. We make these little steps. Look at Newton, the most famous line; "If I have seen further than others, it's by standing on the shoulders of giants." I think in that when writing about myself: "If I've seen maybe further than others is just by standing on the hips of midgets". [laughs] Anyway. Is there anything else? How'd you get involved with Gary Solon of all people.

Princeton Cohorts

Arthur: [00:47:26] Gary Solon. I mean, Orley said that I should talk to him.

Hamermesh: [00:47:31] Ok. It's interesting. I would have thought he'd be the first person to talk to. He was at Princeton, 78 to 82 I think or 83. And the first thing of Gary is as I know of is nothing like any of this stuff. I mean, this first paper... Well, there's one paper he and Orley wrote for a session that I put together at the AEA, which was published in the papers and proceedings in 83. But he had this paper in the 80s, which he got out of this work in the Department of Labor on the impact of taxing unemployment insurance on labor supply. But that certainly not fitting into any of this stuff at all. And certainly Gary's most well known for is intergenerational income differences. And that's certainly doesn't come out of any of this at all.

Arthur: [00:48:25] Yeah, the idea of talking to Gary was more to hearing from him about life as a graduate student at Princeton. And that coming from someone that's not exactly the guys that we are talking about, but that was there at the same time - or almost the same time. The idea was hearing different things from Princeton. That's something that I would like to hear from you, is that you've met probably Heckmann, Ashenfelter, John Pancevel, Ron Oaxaca? During that time. So it was a pretty interesting cohort of students. What do you remember about it?

Hamermesh: [00:49:08] Let's be careful about this, okay? John Pencavel and I are exactly the same vintage. We are both PhD 69, in fact, and Pencavel took a job at Stanford, as you know, that I had turned down.

Arthur: [00:49:22] Oh, yeah?

Hamermesh: [00:49:26] Which was probably, in my view, the worst mistake of my life. I should have taken that job. I really should have. And there are reasons I didn't. My wife... That's a cute story if you have a second. At Princeton in those days, you didn't have to make much of an effort to get a job [was?] Very much inside. Princeton called me and Al Rees said to come down and talk about your thesis at lunch and we'll make you an offer. That was it. The other four offers I got were over the phone. In other words, the person from the school made tours of a few eastern schools picked up people they like made me an offer. That was true for Stanford and North Carolina and UCLA. The guy from Stanford called up and said: "we want to make you an offer". Me: "I'm not interested." "But would you like us to fly you out to see the campus?" "No". I mean, the stupidity on my part was just simply astronomical. But John, in fact, and I never overlap although we know each other a little bit over the years. Ron Oaxaca, on the other hand, was certainly there. He's my age, but he spent two years in the Navy. So I think Ron got his Phd in 71 or 72 and he and I overlapped for two, maybe three years. I know for two for sure. Ron is most well known for, of course, the decomposition, as you well know. But that idea came really from the discussion he had with Orley. I mean, it's called Blinder-Oaxaca and yet I always tell people about I call it Ashenfelter Blinded Oaxaca. And nobody else says this, but it really came out of a discussion with Orley, who was on his comittee - well, they were basically almost classmates, but I think Orley was on his committee. And Orley was the one I heard the discussion in the other office next to me. And he was heavily involved in that. So there's a case where he doesn't get all the credit he deserved. Whereas in the self selection stuff, he might get more credit. But in the end, this things all work out. But who else would have been there then? Have you talked to Joe Altonji?

Arthur: [00:51:59] Yes. Yes, I've talked to him.

Hamermesh: [00:52:01] Ok. You did good. He was at Princeton, his Phd is 80 or 81. He was there in the late 70s and he was very certain a student or Orley. I do think he might have been there 77 to 81 or so. And again, his work is totally different. He really is an economist. He is not in any one of these schools. He thinks about the economics. Indeed of all these people we're talking about, I've just read Altonji & Pierret, QJE, 2001. I was teaching a class yesterday [soun failure] of all places, online. And Joe's stuff more than anybody else really is old-style Chicago. In other words, there's an awful lot of economics in it and trying to think about how to measurement without worrying too much about exogeneity. I mean, [failure] that too. We just mentioned this. I think he is the most Chicagoesc old-style economist I know. In that sense, if you were talking about Chicago school, he's a person to talk to. He wouldn't write anything. He's impossible to get to do anything. He's very much like Jim without quite as much energy or craziness. Joe is very easy to deal with. He's just incredibly disorganized. But he really is the old style Chicago, he's not the princeton... He's the style Chicago that pervaded Princeton until the late 80s. And he's really Al, Orley, and those people - more than anybody else - taken to the next level. Without worrying about structure, without worrying about exogeneity. Worrying instead about the underlying behavior and what it implies for [?].

Arthur: [00:53:55] Ok, that's interesting, too, to get this picture because there's a transition there,

Hamermesh: [00:54:01] It's a transition, but there's still leftover people, like Joe. He's 10 years younger than me. I still like to think [that if it's hard to do] and can't get published anywhere than you shouldn't be doing that stuff. All the profession wants is exogeneity.

Arthur: [00:54:22] Yeah. I'm not sure, because that's not exactly what I studied, but it seems that this trend is passing a little bit. It was more in the... ten years ago or something.

Hamermesh: [00:54:39] From your mouth to God's ears! [laughs] I hope you're correct. The sad fact - and there's nothing wrong with this - is that the old guys like me are not gonna take advantage of its passing. I think you're right. I think that stuff, like any other fad in economics or any discipline, is sort of declining. What will replace it? I like to think it's that plus also thinking about the economics. But who knows? We're just starting out on this new path. But this branch off previous existing [?] is the way I described what we were talking about.

George Shultz

Arthur: [00:55:22] Yeah. I have a question about the Department of Labor. You worked there in the early 70s. And do you remember something about George Shultz?

Hamermesh: [00:55:40] George Shultz held three cabinet positions. He was secretary of labor. He was secretary of Treasury. And later on he was secretary of state. Now, I'm almost certain. I know for sure he wasn't secretary of labor when I was there, nor was he secretary of labor when Orley was there. The secretary, I'm quite certain of this, was some union guy named Peter Brennan, who was most famous for carrying a pistol around the Department of Labor. He quit at the end of 74, halfway through my year at the Department of Labor. But what he had done was hire as the assistant secretary for policy evaluation and research, a man named Abraham Weiss, who was my boss. I don't know if he was Orley's boos, but certainly my boss: he hired me. And he was a guy who had been research director of the Teamsters Union. He knew no economics whatsoever. But he had worked at BLS (Bureau of Labor Statistics) in the thirties. So he was very interested in data. He hired me, hired the other two people. Halfway through my year there, Brennan quit as secretary and Gerald Ford, who had become president by then, appointed a man named John Dunlop. Have you ever heard of him?

Arthur: [00:57:28] I think so. I'm not sure if it's the same Dunlop, but I think so.

Hamermesh: [00:57:32] I'm sure it is. He was a Harvard professor. Was Richard Freeman's Phd adviser - and every other Harvard phd from the 40s, 50s and 60s. He was an incredibly smart man. He wasn't the modern labor economist, but he understood data and he was very interested in economics. And so if there is any economic influence from the top down at the DOL (department of labor), it was at that point onwards from Dr Dunlop. But the real thing is that Weiss's predecessor, I don't think there was any. Somebody hired Orley to work in this division and to try put some economics in it, and that carried on for five years. And when it was picked up again in the 90s under Clinton, when people like Alan Krueger, Larry Katz, [?] took a similar position, although a different title, trying to again put economic analysis into the study of labor within the government. That was the political basis for it.

Arthur: [00:58:47] Yeah. Orley said to me that George Shultz's had hired him to the Department of Labor. That's why I was asking you.

Hamermesh: [00:58:57] Hold on. Don't go away. Let me look at the Web and see when George Shultz was secretary of labor. See if I can find. OK. Hold on a second here. [...] That is absolutely wrong, absolutely wrong. Schultz became secretary of Treasury... hold on. George Shultz was secretary of labor from 1969 to 1970. He then moved forward to become the director of the Office of Management and Budget for two years. In 72, he was appointed secretary of the Treasury. So that is absolutely not correct at all. I don't see how Orley could have been hired by George Schultz because George Schultz left the Labor Department two years before Orley started. So Orley's recollection is just got to be wrong, OK?

Arthur: [01:00:11] Yes. The story that he told was something that before Shultz there were no technical economists in the Department of Labor and Shultz began this thing of Technical People but maybe he was not the one who hired him.

Hamermesh: [01:00:29] Go look up! Go look up George and see. He was there from 72, 73. Look up George Shultz's Wikipedia. That can't be correct. That's just wrong. Shultz, by the way, is an amazing individual. He's an Al Rees labor economist. He was not a human capital modern person. He still with us, he's 99, you know. He's still alive and still functioning more or less, which is pretty impressive. He also was most famous for having a tattoo. He was a Princeton undergraduate. He has a tattoo of a tiger on his rear end.

Arthur: [01:01:12] Really?

Hamermesh: [01:01:12] This came out at one point when he was secretary of state, which I just find terribly, terribly amusing. So he was secretary treasury72 to 74 and then managed the Department of Labor through July 1970... I'm not sure how much real economic stuff done, but I've got a great memory for this nonsense, this useless memory. My mind is stuffed with useless crap.

Mathematica

Arthur: [01:01:45] And do you remember something about the Mathematica? Do you remember something about that? I know Rees was involved...

Hamermesh: [01:01:54] Rees was very heavily involved in that. He was, I think, one of the principal investigators of the, I think, New Jersey negative income tax experiment. The person at Mathematica who was most involved was a man named David Kershaw. He died very young., probably in the late 70s. Orley did a little bit of consulting there I think. They were not doing labor per se. They were doing simply the impacts of that negative income tax experiments, of which there was: New Jersey, Seattle, Denver. That may have been also Gary-Indiana. And these were all very much coming out of money from the Department of Health, Education and Welfare from the ASPCE that you mentioned. And these were all coming out of a Nixon initiative. Nixon was very big on the negative income tax. In my view, I wish it had happened. We'd be much better off today, but politically in some sense Nixon was too liberal, of all things, by pushing that - which I find very amusing. ...I hated Nixon. He certainly wasn't Trump. He was a serious individual who cared about policy, which Trump doesn't give a damn about obviously. But I mean, all that stuff Mathematica had to do with one specific thing at that time. Obviously [fialure] because they have to make a living and get money from the government. But at that point all it had to do with was one thing naming: what was the labor supply responses to a negative income tax. Which is a labor economics question, but it's one question only. They were not the version the least. Nothing wrong with that. So who is the book? Who was the other? There's a book of studies of the negative income tax experiment by a man named Harold Watts. Is that Cain and Watts? I think there is a co-author on the edited volume, it may have been Glen Cain.

Arthur: [01:04:11] Yeah, it may have been. I know that Harold wrote a book about that. He's from Wisconsin, right?

Hamermesh: [01:04:17] Yes, well, he did. But the book of studies that they edited is from the late 70s. Hold on a second... Let me check that out in the web to see how I'm doing on this one [laughs].[...] Anyway, we're certainly Harold Watts edited this book. I don't know, maybe Glenn Cain was not a co-editor, but there was a book of studies of negative income tax, which hold on: do I have it still on my shelf? I think I threw it out. I moved and had get out of my office in Texas. And I got an awful lot of books, that was one that I could get at the library. I mean, if a library was open..

Arthur: [01:05:08] Yeah. No libraries, no libraries now... That's really sad.

Hamermesh: [01:05:12] It's depressing... There's one paper I know just where it is that I can get in the library and it doesn't seem to be available anywhere else. So I know what I'm going to do. What else can I do for you? What else can I answer?

Ending – Center of Gravity

Arthur: [01:05:29] I guess the last question is a more broad question about other institutions, because I've been researching a lot about Princeton, but I'm not sure what was the place of Harvard, M.I.T., Chicago, Columbia, Wisconsin in this network of labor economics.

Hamermesh: [01:05:53] The question that I would, I think, rephrase it is saying: "if you would just send me to one other school to talk about the essence of labor economics. Where would you send me?" Is that a way of rephrasing your question?

Arthur: [01:06:10] Yes. Yes. Yes.

Hamermesh: [01:06:16] Well, I would send you to one city. I wouldn't send you to one school, I'd send you to two schools, OK? I would send you to Harvard. Not because they had a senior labor economist then, but rather because by the late 70s, Richard Freeman was heavily involved with students. And more important, from the late 60s, early 70s until the mid 1990s, Zvi Grilleches was there. And while he wasn't a labor economist, he was certainly very much an Applied Micro person. He supervised a large number of labor related people. You can't talk to him. He died about 20 years ago now. But I would talk to Richard Freeman. Have you talked to Richard yet?

Arthur: [01:07:07] No, I haven't. But mostly because I'm not in touch with this side of the literature. So it's kind of strange to talk to someone where you don't understand much. I know a lot about this stuff that we've been talking, about these institutions, but I don't know much about this on our side. So I would have to study a bit to talk to Freeman. But I'm sure that this is the other side of the story that's really important.

Hamermesh: [01:07:37] Ok. So let me send you to somebody else, not at Harvard. Somebody who was at Princeton, who's a Princeton phd, a student of Al Reeses from 1977, who spent 20 years at M.I.T. and supervised and immense numbers of students and that's Hank Farber. Haven't talked to him?

Arthur: [01:07:57] No, I haven't.

Hamermesh: [01:07:57] He would be perfect because Bobby supervised more... Well, no, no. Because Richard supervised. I taught one semester at Harvard in 1981 because Richard was on leave. I had in my class eight students. Let's see: [Jonathan Leonard?]. You may have heard of him. Yes. [Harry Holzer?]. You know the Hoser. Yes. Holmes dad. [Peter Kuhn]. Probably the best known of them, right? Although the best known of them is John Bound. You know that name? These were all students in my class. They were harvard Ph.D. Students of Freeman or Grilleches in the early 80s. There were two other people there who you haven't heard of. But they published well and both of whom have had great fancy positions. In fact, one of them is now my boss. She is the provost at Barnard College. The other is a woman named Erica Groshen, who was commissioner of Bureau of Labor Statistics under Obama. So it's a phenomenal class. And Richard was involved with all of them. So if you want feel what Harvard was like for labor in the late 70s, up through like the early 90s, talk to Richard. If you want to feel how it was like. He was like in the same period as the late 70s, up through middle 90s. Hank Farber's the person. This is an era before Angrist is there. And before Harvard was all taken over by Larry Katz. For that 50 year period these are the two people they talk to. If you want to talk to anybody from another institution, other Princeton.

Arthur: [01:09:51] Yeah, because I am gradually becoming more aware that this is not only a Princeton story, and I don't want to make it seem that it's only Princeton. But as you may know by writing, history is not easy to talk about everything we need to focus otherwise..

Hamermesh: [01:10:14] My job is easy. All I have to do is think about Greg Lewis and what he did and what its influence is. Once you start thinking about all the interconnections, the connectivity of these things. I mean, there's no end to it. And where do you draw the line? Yeah, I think if you go any further afield than Princeton and Chicago talk to those two guys about those two schools at those times. That is the one extension I would do. Otherwise you could drive yourself crazy.

Arthur: [01:10:44] Yeah, I know that in the 80s MIT tried to hire ashenfelter, but he refused. And during that time when they tought that Ashenfelter would leave to MIT, they hired Krueger to Princeton. And Ashenfelter ended up staying there.

Hamermesh: [01:11:05] Some of these schools are so stupid. I'm a Yale PhD, as you probably know. It would have been probably in the early 90s. David Card was still at

Princeton. And I got a request from the Yale Economics Department to write a letter recommending him for an appointment at Yale. Yale wanted to hire him. So I called somebody else, a guy who was then chairman and became president of Yale and served as president for a long time. I called him up and I said: "what are you thinking of offering him and how much money?" And he said: "Well, I think we'll awesomes 40 thousand." I laughed and said: "You're not even in the right ballpark. Who do you think you are? You're going to get this guy that cheaply" That is typical Yale. They thought if we make you an offer, no matter who you are, you'll come. I mean, it was just stupid. And Yale, I'm very sad to say. I mean, that's my home. That's where I went to graduate school. It has never been what it should be in labor economics. They've had good people like Joe Altonji and some good students. But it has never been a major contributor in this at all. Which is very depressing because in some other areas, econometrics for instance, it really is top-notch.

Arthur: [01:12:28] So, if you had to choose like the five top schools in the labor economics in the 70s and 80s, which would you choose?

Hamermesh: [01:12:38] Chicago. Princeton. Harvard. In pure labor probably Stanford. I'll put Chicago, Princeton, Harvard. I wouldn't go beyond three. The rest were cut below.

Arthur: [01:12:56] What about Columbia? With Jacob Mincer and Gary Becker in the 70s.

Hamermesh: [01:13:01] I'm sorry. Through the 70s, put Columbia in there also because even though Gary had left Jacob supervised people like George Borjas and there were other ones, too. So, yes, I put Columbia in there and they certainly were very active. My first seminar off campus was in my fourth year in graduate school. Becker invited me to get my thesis paper, not for a job, just to give a paper at this seminar in 68. He was very much still there. Actually, Becker was there probably untill 73 or 74. I know he was there at the fall of 72 because he came down to Princeton and gave a lecture and I had lunch with him.

Arthur: [01:13:51] Do you know when Heckmann went to Columbia, because...

Hamermesh: [01:13:57] Heckman started at Columbia in 70. I'm absolutely certain of that.

Arthur: [01:14:01] So Becker left that year

Hamermesh: [01:14:05] Oh no no no no no no no no, no. Let me look and see. hold on, OK? Gary Becker let me look him on Wikipedia here.

Arthur: [01:14:14] Because I know that Heckman and Becker didn't overlap.

Hamermesh: [01:14:18] Not true at all. Can't be true.

Arthur: [01:14:20] Do you think they overlapped? Because Heckman wrote the paper saying that they didn't and he really wished they had overlapped at Columbia.

Hamermesh: [01:14:31] Hold on. No, you're quite right! I'm sorry. He must have been coming from Chicago. You're right. In 1970, Becker return to the university Chicago. You're actually right. [failure] They did not overlap at all. They overlap. certainly the time Jim went back there. You know, Becker's wonderful line about... Becker was approached by Harvard in the 70s. You know this story.

Arthur: [01:15:14] No, I dont.

Hamermesh: [01:15:16] Ok. And they said: "we'd like you to come out and spend a year here and let us look you over". Becker's response was basically: "you know, Grilleches didn't serve an apprenticeship when he went there. I don't want to be an apprentice." He told them to go to hell. Which, again, is a wonderful illustration of Harvard arrogance. He might well have gone because he would have been able to establish a whole new school there. But typical Harvard... That's so snooty that they lost that chance. But you're right. They did not overlap. I'm wrong. OK.

Arthur: [01:15:54] Ok, ok. It's interesting. I guess it was pretty much what I have for today. But if I have any. But if I have any more questions. Do you mind if I contact you?

Hamermesh: [01:16:05] Not at all. Send me an e-mail. As you probably noticed, I'm pretty good with the mail, right?

Arthur: [01:16:10] Yeah.

Hamermesh: [01:16:12] And I'm to inflict my mate, my Greg Lewis paper upon you. I'll be in August, as I said.

[01:16:18] Ok. And when when I have something more reboost with my papers, I'll send you to, OK.

[01:16:24] Please do. OK. And I look forward to. Thanks a lot, Arthur. Take care. Thank you very much. Have a nice day.

[01:16:29] You too. Bye bye. Bye.