ESSAYS ON HETEROGENEOUS TREATMENT EFFECTS IN THE LABOR MARKET

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by

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ABSTRACT

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This dissertation includes three chapters. The first two chapters mainly focus on the effects of job displacement on earnings. The first chapter analyzes how the effects of job displacement on earnings vary among natives and immigrants in the United States, while the second chapter studies the distributional effects of job displacement using quantile regression and distribution regression. The third chapter considers the distributional effects of having children on women's income. In particular, I apply the Changes in Changes approach and distribution regression to study how the motherhood penalty varies across different women.

Chapter 1, tilted THE HETEROGENEOUS EFFECTS OF JOB DISPLACEMENT ON IMMIGRANT AND NATIVE WORKERS, extends the previous literature by analyzing the consequences of job displacement on weekly earnings among natives and immigrants in the United States and also examines whether job displacement has different effects among native displaced workers residing in states with different share of immigrants' population. Although there are contradictory findings in the literature about how immigrants would respond to labor market shocks differently from natives, the existing literature agreed on the fact that workers who involuntarily lose their jobs experience long spells of unemployment after displacement. Fully understanding the consequences of job displacement among natives and immigrants, and the role of the share of immigrants' population on native displaced workers may help policy-makers to better formulate immigration policies as well as off-setting labor market policies for displaced workers. My results show that some groups of immigrants experience

slightly smaller earning losses following displacement compared to natives, and I did not find significant effects of the share of immigrants on the earning loss of native displaced workers.

Chapter 2, titled HETEROGENEOUS EFFECTS OF JOB DISPLACEMENT ON EARNINGS (with Brantly Callaway), considers how the effect of job displacement varies across different individuals. In particular, our interest centers on features of the distribution of the *individual-level* effect of job displacement. Identifying features of this distribution is particularly challenging – e.g., even if we could randomly assign workers to be displaced or not, many of the parameters that we consider would not be point identified. We exploit our access to panel data, and our approach relies on comparing outcomes of displaced workers to outcomes the same workers would have experienced if they had not been displaced and if they maintained the same rank in the distribution of earnings as they had before they were displaced. Using data from the Displaced Workers Survey, we find that displaced workers earn about $157 per week less than they would have earned if they had not been displaced. We also find that there is substantial heterogeneity. We estimate that 42% of workers have higher earnings than they would have had if they had not been displaced and that a large fraction of workers have substantially lower earnings than the average effect of displacement. Finally, we also document major differences in the distribution of the effect of job displacement across education levels, sex, age, and counterfactual earnings levels. Throughout the paper, we rely heavily on quantile regression. First, we use quantile regression as a flexible (yet feasible) first step estimator of conditional distributions and quantile functions that our main results build on. We also use quantile regression to study how covariates affect the distribution of the individual-level effect of job displacement.

Chapter 3, titled THE HETEROGENEOUS EFFECTS OF HAVING CHILDREN ON WOMEN'S INCOME, estimates the distributional effects of having children on women's annual income in the United States using the National Longitudinal Survey

of Youth from 1979 to 2016. Existing work on motherhood penalty shows that while the wage gap among men and women becomes smaller in the United States, the gap between mothers and childless women is increasing (Waldfogel 1998). After childbirth, women usually experience an immediate decrease in their earnings relative to what they would have earned if they had not become a mother. The gap closes somewhat over time though mothers never fully catch up to their counterfactuals. Previous work tried to explain the motherhood wage penalty by estimating the average treatment effect of children on women's earnings, but these effects can be quite heterogeneous across mothers with different observable characteristics. By utilizing the Changes-in-Changes model and distribution regression, I find that around 90% of mothers have lower income after having children. White, married, older, and highly educated mothers with two or more children experience a substantial drop in their income.

To my father, Eskandar Azadikhah Jahromi

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CHAPTER 1

THE EFFECTS OF JOB DISPLACEMENT ON IMMIGRANT AND NATIVE WORKERS

1.1 Introduction

The number of international immigrants is growing in the United States. The population of immigrants in the United States was approximately 44.5 million in 2017 which constitutes 13.5% of the total population. According to the latest International Migration Report, from 1990 to 2018 the total population of immigrants in the United States grew by 121.3%. North America has the third largest number of immigrants, around 58 million, in 2017[1]. Studying the economic impact of immigrants on the host country has drawn the attention of scholars and academics since the nature and population of immigrants have changed sharply during the past decades. While the literature on effects of immigrants on employment and the labor market of United States is well established, less attention has been paid on how immigrants respond to labor market shocks differently than natives.

One of the important labor market shocks that many individuals experience during their lifetime is job displacement. Job displacement refers to an involuntary job separation that is caused by changes in the firms' operations or in employers' decision rather than the lack of performance of the employee (Kletzer 1998). Involuntary separations usually cause long-lasting earning losses, but voluntary separation can cause an increase in earnings (von Wachter, Handwerker, and Hildreth 2009). Carrington

[1] 80 million international migrants are living in Asia and 78 million in Europe, see more in http://www.un.org/en/development/desa/population/migration/publications

and Fallick 2017 analyzed several reasons that would explain the earning losses of displaced workers. The main two reasons are based on the human capital and job matching theories. Based on human capital theories, workers accumulate firm-specific (location-specific) skills over time which could be devalued after displacement. These theories can explain why high-tenure displaced workers experience a larger wage penalty than those with short-tenure. Based on the job matching theories, high-tenure workers are more likely to be on high-quality matches. Losing high-quality matches are costly as displaced workers have to start new job-matching process which could end up to lower quality matches. Younger displaced workers usually experience smaller wage penalty than older displaced workers as they are less likely to be on high-quality matches.

The human capital and job matching theories can help with understanding why immigrants experience differential effects than native workers after losing jobs. On the one hand, immigrants have less country-specific human capital, face language barriers, and are less informed about the job opportunities and social assistance programs in the host country so they may experience severe negative effects on their earnings after displacement (Dustmann, Glitz, Schönberg, and Brücker 2015, Boman 2011). On the other hand, recent immigrants have smaller tenure than native workers, so one would expect a lower wage penalty for them after displacement. From this perspective, immigrants are less likely to have found the highest quality job match and have less location-specific human capital which makes them more mobile. If immigrants are more mobile than natives then they tend to have more efficient post-displacement job search and are more likely to find higher-quality matches after displacement.

I begin my analysis by asking whether there are substantially different effects of job displacement among natives and immigrants. Considering the fact that the number of immigrants is increasing over time, fully understanding the consequences of job displacement on natives and immigrants may help policy-makers to investigate the helps and harms of the immigrants on local labor markets in the United States and better formulate immigration policies as well as off-setting labor market policies. For example, policies that encourage the dispersion of immigrants around the country may minimize the economic threats due to large immigrants inflows on the native workers (Grossman 1982). I extend the previous work by analyzing the consequences of job displacement on weekly earnings among natives and immigrants in the United States and also examining whether job loss has different effects for native displaced workers who are residing in states with different share of immigrants in the population. I find some evidence that native displaced workers experience slightly more decline in their earnings compared to immigrants which can be related to the higher mobility of immigrants.

The Migration Policy Institute (MPI) refers to the term *immigrants* (also known as the foreign-born) as "people residing in the United States who were not U.S. citizens at birth"[2]. Although this definition has become standard, it is too broad as heterogeneity exists between different types of immigrants. Cortes 2004 categorized immigrants into two groups; economic immigrants and refugees who can obtain different levels of human capital and wages in the host country. Although I can not make a distinction between refugees and immigrants from the available data set, I separate immigrants into three categories; immigrants from OECD countries, immigrants from Latin-America, and immigrants from the rest of the world. A list of countries in each category is presented in Table 1.14. Bratsberg, Raaum, and Roed 2018 similarly distinguished between immigrants from developing countries and immigrants from western European countries in their analysis. Dustmann, Glitz, Schönberg, and Brücker 2015 also separated immigrants from OECD- and non-OECD countries. Natives are defined as those who were born in the United States to American parents.

[2]This population includes naturalized citizens, lawful permanent residents (LPRs), certain legal nonimmigrants (e.g., persons on student or work visas), those admitted under refugee or asylee status, and persons illegally residing in the United States"

This paper is related to two main topics in the literature; one refers to the impact of job displacement on earnings as well as different responses among immigrants and natives, and the other refers to the effects of flows of immigrants on native displaced workers. The empirical research on the effects of job displacement on earnings is well established and the majority of them agree that displaced workers earnings are, on average, 10-15% lower than non-displaced workers even a couple of years after separation (see more in Podgursky and Swaim 1987, Hamermesh 1989, Topel 1990, Ruhm 1991, Jacobson, LaLonde, and Sullivan 1993, Farber 1997, Fallick 1996, Stevens 1997, Kletzer 1998, Farber 2011, von Wachter, Handwerker, and Hildreth 2009). However, existing empirical studies do not provide a clear prediction of the effects of job displacement on immigrants in the United States. Some other studies analyze the effects of job loss among natives and immigrants in other countries. For example, Bratsberg, Raaum, and Roed 2018 study the effects of job loss among natives and immigrants in Norway and argue that immigrants have lower country-specific human capital, limited language skills, and smaller networks which can explain why they experience serious consequences after job loss.

The literature on the effects of flows of immigrants on the host country has been growing during the past decades while the majority of them have focused on the assimilation of immigrants in local labor markets of the host countries. While some find little impact of immigrants on the local labor markets (e.g., Card 2005, Chiswick 1986, Grossman 1982), others find increase use of government services and lower tax payment (e.g., Borjas 1995, Borjas 1985). For example, Card 2005 evaluated the impact of immigrants on the labor market opportunities of less-skilled natives and found no correlation between wages of native high school dropouts and increase in the supply of less-skilled workers through immigration. Grossman 1982 also found small negative effects on the wages of natives as a result of inflows of immigrants. However, Borjas 1995 discussed how unskilled immigrants are more likely to use government services and pay lower taxes which can negatively affect the host countries[3].

I link the data sets obtained from the Displaced Workers Survey (DWS) and the Current Population Survey (CPS) from 1999 to 2018 to evaluate the effects of job displacement on earnings among natives and immigrants as well as comparing these effects among natives residing in states with different share of immigrants in the population. My analyses are based on a sample of 142,252 individuals in which 13.25% of them are immigrants. Around 93% of immigrants have been in the United States for over 10 years. The mean of weekly earnings for displaced immigrants is around 8.2% lower than displaced natives in the pre-displacement period while this gap moderates after displacement such that the mean of weekly earnings for immigrants at their job after displacement is only 5.4% lower than natives. Among the three categories of immigrants, displaced immigrants from OECD countries earn 30% and 28% more than natives before and after displacement respectively. Approximately 15.22% of natives and 15.34% of immigrants are displaced in my sample.

I find no evidence that the impact of job displacement is different for OECD immigrants than it is for natives, but immigrants from Latin-America and other countries (except OECD and Latin-America) experience slightly smaller reductions in their earnings compared to natives. This can be due to the fact that natives and OECD immigrants are more likely to lose high paying jobs after displacement compared to the other immigrants and experience larger reductions in their earnings. However, based on the human capital migration theory, immigrants have less location-specific human capital and a higher propensity to move which would lead to more efficient post-displacement job search (Boman 2011). The linear probability model shows no statistically significant difference between the probability of employment among displaced immigrants of OECD countries and natives, while displaced immigrants of

[3]Other works that studied the impact of immigrants on the labor market of the host countries include; Chiswick 1978, Wilson and Portes 1980, Borjas 1987, Borjas 2003, LaLonde and Topel 1992, Bloch 1999.

Latin-America and other countries have 7% and 5% smaller chance of being employed compared to natives after job loss. The share of immigrants in the population in each state from for 1999, 2000, 2010, and 2017 are obtained from MPI to investigate whether the job displacement effects on weekly earnings for native workers differ based on the share of immigrants in the population. Linear regression analysis shows that job displacement effects on weekly earnings of native workers do not depend on the share of immigrants (i.e., the coefficient on the interaction term is not statistically significant).

The rest of this paper is organized as follows. In Section 2, I provide the literature review. In Section 3, I present the identification of job displacement. Section 4 contains the data description and summary statistics. Section 5 and 6 presents empirical results and robustness checks. Section 7 concludes. Figures and tables are collected in the Appendix.

1.2 Prior Literature

My study builds on two major areas of research. One is related to the empirical work on job displacement and its unwelcome consequences on earrings and the other is related to the effects of immigrants on the host country especially on the labor market. In this paper, I try to bring together these areas of research. The closest study to my work is Bratsberg, Raaum, and Roed 2018 who used a Norwegian administrative data to study the effects of job displacement on employment and earnings of immigrants and natives.

1.2.1 Job Displacement

The literature on job displacement has been growing during recent decades. Previous work shows that displaced workers experience long spells of unemployment and persistent changes in earnings. Farber 2017 argued that difficulty in finding full-time employment after displacement is the first cost of job loss. The chance of being employed after job loss is different among individuals with different characteristics. For example, younger workers are more likely to be employed than older workers and workers with higher education have a higher chance of employment. Herzog Jr and Schlottmann 1995 studied the likelihood of reemployment after job displacement. They believed that reemployment of displaced workers not only depends on the workers' characteristics but also varies with characteristics of the displaced job, characteristics of the search and location, and timing of displacement.

Other studies that focused on the effects of job displacement on earnings are as follows. Stevens 1997 examined the long-term wage and earning losses of displaced workers by using longitudinal data from the Panel Study of Income Dynamics. Her results showed that the effects of displacement are quite persistent, and she estimated average earning losses of approximately 9% six or more years after separation for displaced workers. She also showed persistent earning and wage losses could be related to multiple job losses. Ruhm 1991 investigated the permanent effects of job displacement on wages and employment, and concluded that displaced workers earn 10-13% less than non-displaced workers four years after displacement. Bognanno and Delgado 2008 studied the job displacement costs in Japan between 2000 and 2003 and estimated income penalty of around $1,110 per year of age. They also found more severe effects among older workers.

Some other studies have tried to combine administrative records (e.g., Social Security) with survey data sets to not only overcome the recall bias problem existed in the surveys but also identify the long-term effects of job displacement on earnings. In the DWS, respondents were asked to recall a job displacement that has happened in the past three years. This can lead to "Recall" bias. The recall bias refers to the errors that may happen when people are asked to recall an event that happened in the past (Evans and Leighton 1995). von Wachter, Handwerker, and Hildreth

2009 combined the DWS with administrative data from California to estimate the costs of job displacement and concluded that while the DWS suffers from the recall bias, administrative data overestimate the effects of job loss on earnings. Jacobson, LaLonde, and Sullivan 1993 combined records on workers' earnings from Pennsylvania administrative data with the information about their firms and created a longitudinal data set to analyze the earning losses of high-tenure workers. Their results showed that the average earning loss of displaced workers is about 25% per year. Couch and Placzek 2010 did similar work using Connecticut administrative data and estimated around 32-33% reduction in displaced workers' earnings after displacement.

Flaaen, Shapiro, and Sorkin 2017 linked the Survey of Income and Program Participation (SIPP) with the Longitudinal Employer-Household Dynamics (LEHD) and found significant heterogeneity in the consequences of job separation depending on different reasons of job displacement. Hyman 2018 developed a data set based on the Trade Adjustment Assistance (TAA) program and the LEHD to study the causal effects of the retraining program on earnings, education, employment, and mobility between 1990 and 2011. Topel 1990 used the DWS and the PSID and found empirical evidence on short-run and long-run effects of job displacement on annual earnings and wages. For instance, the estimates of the earnings of a typical, nonunion blue-collar worker who does not change his occupation four years after displacement are about 20% below his pre-displacement earnings.

Some other literature discusses the earning losses of displaced workers through the human capital theory. Workers invest in firm-specific human capital over time which would be devalued when they lose their jobs and result in lower wages and earnings (Carrington and Fallick 2017). Nedelkoska, Neffke, and Wiederhold 2015 suggest that skill mismatches (i.e., quality of job matching) can also explain the earning losses of displaced workers. They used a German administrative data and found the support of higher earning losses for industry switchers, occupational switchers, and those who switch skill portfolios. Riumallo-Herl et al. 2014 used the DWS to compare the earning losses of displaced workers who switch industries after displacement (switchers) with those who stay at their industries (stayers). His results showed that switchers with high-tenure at the pre-displacement suffer greater reductions in their earnings than similar stayers.

The consistent finding across all these studies is that displaced workers experience persistent negative changes in their earnings which can be followed by a series of ongoing problems on health, educational attainment, mortality, and divorce. Gallo, Bradley, Siegel, and Kasl 2000 used the Health and Retirement Survey of 1992 and 1994 to show that the late-life involuntary job separation is significantly associated with both physical disability and poorer mental health of displaced workers. Sullivan and von Wachter 2009 matched the administrative data on male workers in Pennsylvania with the Social Security Administration data to study the link between job displacement and mortality, and they estimated 10-15% increase in the annual death hazard among displaced workers. Stevens and Schaller 2011 showed that parental job loss affects children's academic achievement negatively and increases the probability that a child repeat a grade in school by 15%. Charles and Stephens 2004 explored the probability of divorce after two negative earning shocks, job displacement and disability. Their results showed that while disabilities experienced by spouses do not affect the divorced hazard, job displacement has increased the risk of divorce. Job displacement can also affect the retirement decision of displaced workers. Marmora and Ritter 2015 used the panel data from the Survey of Income and Program Participation (SIPP) and provided evidence that suggests job loss and the subsequent unemployment spell affect workers' retirement decisions and social security uptake, especially for those who have reached age 62.

1.2.2 Labor Market Effects of Immigrants

Prior studies have attempted to compare the performance of immigrants with natives and analyze the social and economic impacts of immigrants on the host country, especially on the labor market. While prior literature covered different aspects of job displacement, it leave open the question of whether the consequences of job displacement on earnings are different between natives and immigrants in the United States.

Cortes 2004 categorized immigrants into two groups; economic immigrants and refugees. *Economic immigrants* flee their country to access better economic opportunities or higher education and can return to their native country whenever they desire, *refugees*[4] flee their country to avoid war and persecution and usually cannot remigrate to their native country. Unlike economic immigrants who might return and spend their retirement in their motherland, refugees are unable or unwilling to return and this can result in different levels of human capital investment and the wage gap between refugees and immigrants. In this paper, I cannot make any distinction between refugees and immigrants due to the limitation of the data set so all the individuals are classified as being either natives or immigrants. Following the existing literature, I define natives as those who were born in the United States to American parents.

Immigrants can have positive or negative impacts on the supply of the labor market. On the one hand, immigrant labor can be considered as a substitute input for native-born labor as they are willing to work at lower wages due to their limited access to transfer payments and unemployment benefits. On the other hand, immigrant labor can be a complementary input as they could bring skills to the host country. If there is a shortage of skills in the host country this can result in an increase in labor

[4] The United States Department of State defines a refugee as "someone who has fled from his or her home country and cannot return because he or she has a well-founded fear of persecution based on religion, race, nationality, political opinion or membership in a particular social group".

demand and higher wages (Roy 1997). Some studies are optimistic about the role of immigrants on local labor market opportunities of natives. Card 2005 evaluated the impact of immigrants on the labor market opportunities of less-skilled natives and found no correlation between wages of native high school dropouts and the supply of less-skilled workers. Grossman 1982 used cross-sectional data set for 1970 and found small effects on the wages of natives as a result of inflows of immigrants. Ortega and Peri 2009 studied the effects of international immigrants on investment, employment and productivity in OECD countries and concluded that the inflow of immigrants not only increase the investment (i.e., GDP in the short run) and employment rate but also has no negative effects on average wages or income per worker in the short-run or in the long-run. Other studies are a little pessimistic, for example, Altonji and Card 1991 studied the less-skilled labor market outcomes of natives in 120 cities in the United States with a high immigrant density between 1970-1980. They found some degree of competition between immigrants and less-skilled natives and concluded that the inflow of immigrants affects the labor supply in industries in which low-skilled workers are mostly employed[5].

The earning gap between recent immigrants and natives can be explained by several reasons. Recent immigrants have lower country-specific human capital, are less informed about the job opportunities in the United States, and are less likely to be unionized or obtain occupational licenses. Chiswick 1978 showed that immigrants are more likely to have lower earnings than natives at the earlier years of arrival but the earning gap would close after about 10 to 15 years. He also implied that immigrants are more able or more highly motivated than natives which may explain why their earnings crossover native earnings over time. Borjas 1987 challenged this hypothesis about the higher ability of immigrants relative to natives and argued that the cohort differences in the quality of immigrants and assimilation could explain the positive

[5] Apparel, Leather, Agriculture, Furniture, Miscellaneous Manufacturing, Private households services, Hotels and motels, Transportation services, Restaurants and bars, Textile mills.

relationship between immigrant earnings and year-since-migration. His findings are explained in the form of the theory of immigration proposed by Sjaastad 1962 in which the emigration rate is a function of the mean income in the home country, mean income in the host country, and the cost of emigration.

Although much attention has been devoted to the earning gap among immigrants and natives in the host country, not that many investigate the differential effects of job displacement on earnings among immigrants and natives. Bratsberg, Raaum, and Roed 2018 analyzed the effects of job loss among natives and immigrants in Norway and argued that immigrants have lower country-specific human capital, limited language skills, and smaller networks which can explain why immigrants experience more severe consequences after job loss. Islam, Stillman, and Worswick 2018 examined how Australian-born workers and immigrants respond to two economic shocks - job displacement and serious health problem - differently. In terms of job displacement, they found that while job displacement has no different effects on either single Australian-born or immigrant men, it has larger long-term effects on single immigrant women than it has on single Australian-born women. They also compared Australian-born couples with immigrant couples and concluded that Australian-born couples face more severe consequences of job loss than immigrant couples, as husbands in Austrian-born couples usually lost relatively higher paying jobs. I extend the previous studies by analyzing the effects of job displacement on earnings of natives and immigrants in the United States.

1.3 Job Displacement Identification

I divide the population of workers in my sample into three categories of immigrants and natives; immigrants from OECD countries, immigrants from Latin-America, immigrants from the rest of the world, and natives who were born in the United States to American parents. The list of OECD countries and Latin-America are presented in

Table 1.14. There are 18,854 immigrants and 123,405 natives which contain 86.75% of the sample population in my data set. Around 15.22% of natives and 15.34% of total immigrants were displaced between 1999 and 2018. The total average weekly earning for native workers is \$859 while it is \$790 for immigrants. The average weekly earnings at the current and the lost job for native displaced workers are \$699 and \$778 respectively while these values are \$666 and \$719 for immigrants. According to the

Table 1.1: Reasons of Displacement among Immigrants and Natives

Job Displacement	OECD	Latin-America	Other	Natives
Job Displacement Rate	14.20%	16.37%	15.30%	15.22%
Reasons for Displacement				
Plant or company closed down or moved	35.05%	34.89%	33.57%	33.20%
Plant or company operating but lost/left job because of insufficient work	34.34%	49.60%	51.26%	37.41%
Plant or company operating but lost/left job because position or shift abolished	30.61%	15.50%	15.16%	29.38%

Source: The Displaced Workers Survey 1999-2018.

Displaced Workers Survey, displaced workers are defined as those who lost their jobs due to plant closure, insufficient work, or slack work. Table 1.1 presents the number of displaced workers as well as different reasons for separation in my data set. Among the three categories of immigrants, those from OECD countries are more similar to natives in terms of job loss rate and the reasons for separations. The first row of Table 1.1 shows the rates of job loss for each category of immigrants and natives. OECD immigrants have the lowest rate of job loss while immigrants from Latin-America have the largest rate among others. The rates of job loss due to company/plant closing down are approximately the same among immigrants and natives. One interesting finding is that the rate of job loss due to insufficient work is higher for immigrants from Latin-America and the rest of the world compared to natives and OECD immigrants.

Immigrants from OECD countries and natives are almost twice as likely to experience job loss due to position/shifts abolish compared to other immigrants.

Table 1.2: Probability of Employment among Displaced Immigrants and Natives

Immigration Status	Regression		
	(1)	(2)	(3)
OECD Immigrants	0.007 (0.017)	-0.016 (0.019)	-0.034 (0.021)
Latin-America Immigrants	-0.071* (0.013)	-0.043* (0.012)	-0.039* (0.012)
Other Immigrants	-0.046* (0.008)	-0.015 (0.014)	-0.009 (0.015)
Controls	No	Age, race, gender, education	Age, race, gender, education, industry, and year fixed effects

Note: Robust standard errors, are shown in parentheses. Significance level: * p<0.05

In Table 1.2, I investigate whether the probability of employment differs among displaced immigrants and native workers. The outcome of interest is an indicator of whether or not a displaced worker is employed at the time of the survey. As shown in the first column of Table 1.2, without controlling for any covariates, Latin-America and other displaced immigrants have lower probabilities of employment compared to native displaced workers. However, there is no statistically significant difference between the probability of employment among OECD immigrants and native displaced workers. I control for a set of covariates and year fixed effects and the results are presented in columns (2) and (3) of Table 1.2. Latin-America immigrants are estimated to have around 4% higher probability of employment compared to native displaced workers. I did not find any statistically significant differences between the probability of employment among OECD immigrants and native displaced workers even after controlling for a set of covariates and year fixed effects.

1.3.1 Job Displacement Endogeneity Problem

In the job displacement literature displaced workers are mostly considered to be displaced involuntary rather than because of lack of performance (Kletzer 1998). The asymmetric information model of layoffs (i.e., the "lemons" model) was introduced by Gibbons and Katz 1991. Using DWS data set from 1984 to 1986, their model suggests that the market usually treats white-collar workers, who got displaced due to slack work/layoffs, as low-quality workers and post-displacement wages for them are lower than those who lost their jobs due to plant closures. However, subsequent empirical studies test the asymmetric information model of layoffs and obtain different results. For example, Song 2007 showed that the difference between pre- and post-displacement wages, estimated by Gibbons and Katz 1991, are biased due to the long recall period in their sample[6].

Krashinsky 2002 also showed that the size of the pre-displacement firms could explain the difference between pre- and post-displacement wages. Laid-off workers experience a larger wage penalty after job displacement as they are most likely to be displaced from larger establishments with higher wages compared to other displaced workers. From another perspective, workers who are less attached to their firms and have more outside options are more likely to leave first before being laid off while those who stay with the failing firms until they completely shut down usually have poorer outside opportunities (Carrington and Fallick 2017). Oyer and Schaefer 2000 argue that firms' decisions on how to dismiss unproductive workers depend on costs associated with displacing them. Firms are more likely to face employment discrimination litigation when firing a small group of workers than when dismissing a large group as a mass layoff. These studies contradict the "lemons" model, so the reason of separation does not reveal any information about the quality of displaced

[6]Respondents were asked about job displacement that had happened in the last five years between 1984 and 1994 in the Displaced Workers Survey.

workers. In this paper, I overcome the endogeneity problem by using the fixed effects model which allows me to control for time-invariant unobservables.

1.4 Data and Key Variables

1.4.1 Data

I am using the Current Population Survey (CPS) for the years 1999 to 2018 along with the Displaced Workers Survey (DWS), which is a January supplement to CPS[7], to analyze the effects of job displacement on earnings across natives and immigrants[8]. CPS provides data on a set of covariates as well as the place of birth from which I can define the number of immigrants and also categorize immigrants into three groups; immigrants from OECD countries, immigrants from Latin-America countries, and all the other immigrants from other countries.

Although DWS provides useful information on job displacement status and the current weekly earnings of displaced workers and their weekly earnings on their lost job, it does not provide any detail on the earnings of non-displaced workers. Therefore, I need to find a way to obtain data on earnings for non-displaced workers as well. I generate my detailed data set by combining DWS with CPS. After identifying non-displaced workers in my sample, I use their identifications numbers (CPSID) to find those displaced workers who also show up in the CPS outgoing rotation groups[9].

[7] The DWS in 2000 was collected in February instead of January.

[8] Immigrants are defined as all the foreign-born population in the CPS sample. This includes documented and undocumented immigrants. Although CPS does not provide information on the legal stay of immigrants, previous studies tried to separate the population of documented immigrants from undocumented. Warren and Passel 1987 introduced the residual method in which they estimated the total number of undocumented foreign-born by calculating the gap between the total number of the foreign-born population counted in the census and legal foreign-born population counted by Immigration and Naturalization Services (INS). I do not make any distinction between documented and undocumented immigrants in this paper.

[9] Households in the CPS are usually interviewed for four consecutive months, then they will be ignored for 8 months, and then interviewed for another four months. After the eighth time participating, they will leave the sample. Households that are interviewed in the fourth or eighth month (which are called the outgoing rotation groups) are asked additional labor questions.

This matching process helps me to find information on the current weekly earnings as well as the lag of weekly earnings for non-displaced workers. My detailed data set is a short panel data in which everyone is observed for two periods; displaced workers are observed at pre- and post-displacement and non-displaced workers are observed at the time of the survey and a year before the survey.

My covariates include groups of age, gender, education, race, marital status, and industries. Education is divided into four categories: less than high school, high school, some college, and at least a college degree. Less than high school is my omitted group. Age is divided into three groups: 20 to 34 years old (young), 35 to 49 years old (middle-aged), and 50 to 65 years old (old). The omitted group is 50 to 65 years old. Marital status divided into three groups: single, divorced, and married and the omitted group is single. Gender is categorized into male and female with male being omitted. Race is categorized as white, black, and other races with other races being omitted. An indicator of whether an individual is displaced or not is also added to the model. The industry is classified based on the Standard Industrial Classification into ten groups[10]. The dependent variable is the change in the log of weekly earnings before and after displacement. I also include both the full-time and part-time employment in the model while I did not include workers with zero earnings[11].

In order to study the effects of job displacement on native workers residing in states with different share of immigrants in the population in the United States, I obtain the data of the share of immigrants in the population in different states from MPI for 1999, 2000, 2010, and 2017. According to the "Immigrant Population and Share of Total Population by State" report which is publicly available on the Migration Policy Institute data hub, California (26.2%), New York (20.4%), New Jersey (17.5%),

[10] "Agriculture, Forestry and Fishing", "Mining", "Construction", "Manufacturing", "Transportation, Communications, Electric, Gas, And Sanitary Services", "Wholesale Trade", "Retail Trade", "Finance", "Insurance and Real Estate", "Services", and "Administration"

[11] The standard practice in the displaced worker analysis is to exclude those who have zero earnings either before or after job displacement. Flaaen, Shapiro, and Sorkin 2017 included those zero earning displaced workers which doubles their estimate of earning losses.

Hawaii (17.5%), Florida (16.7%), Nevada (15.8%), Texas (13.9%), District of Columbia (12.9%), Arizona (12.8%), Illinois (12.3%), Massachusetts (12.2%), and Rhode Island (11.4%) have the largest share of immigrants in 2000. Connecticut, Maryland, and Washington were added to the list of states with higher share of immigrants over time. These 15 states made up the biggest share of immigrants in the population from 1999 till 2017. I define a dummy variable for the share of immigrants in the population which is equal to 1 if an individual is residing in any of states mentioned above and zero otherwise.

1.4.2 Sample Characteristics

In Table 1.3, I present the summary statistics for key variables used in my analysis calculated for natives, OECD immigrants, Latin-America immigrants, and immigrants from other countries. The first column contains the statistics for OECD immigrants. All three categories of immigrants have almost similar age distributions such that most of the workers are between 34 to 49 years old (middle-aged) while OECD immigrants are slightly older than the other two categories of immigrants. Although the proportion of female in the sample is smaller than males for immigrants from Latin-America and other countries, it is slightly larger for natives and OECD immigrants. The population of blacks is the largest between immigrants from Latin-America. Around 65% of workers are married in the sample. One interesting fact is that the share of natives and OECD immigrants with less than 12 years of schooling is much smaller compared to the other immigrants in my sample. Among different groups of immigrants, those from OECD countries have the highest level of education.

Table 1.4 contains the sample means only for displaced workers in various demographic groups. Job displacement is more common among the middle-aged (34 to 49 years old) workers than among younger and older workers. The rate of displacement is the largest among married workers. In terms of education, while the rate of job loss is the largest for OECD immigrants with at least a college degree, this rate is at the highest for other immigrants with lower levels of education. I also check the rates of job displacement among different industries; Services, Manufacturing, Construction, and Retail Trades have the highest number of displaced workers relative to the other industries.

1.5 Empirical Model and Results

The analysis of this study proceeds in two stages. First, I investigate the effects of job displacement on changes in the log of weekly earnings among natives and immigrants in the United States. Second, I examine how these effects vary among native displaced workers residing in states with different share of immigrants in the population. The analysis is restricted to individuals aged 20 to 64 who satisfied the following sample selection rules: (1) the individual was employed at the time of the survey after displacement; (2) the individual was not in the Army; (3) the individual loses his/her job due to plant closing, position abolished or slack work.

I start my analysis assuming changes in weekly earnings for displaced workers are defined as the difference between the log of weekly earnings at the current job (i.e., job held at the time of the survey) and the log of weekly earnings at the lost job, while the changes in the weekly earnings for non-displaced workers are determined as the gap between the log of weekly earnings at the time of the survey and the log of weekly earnings at the previous period. Although in the job displacement literature this is a common method to measure changes in weekly earnings, Jacobson, LaLonde, and Sullivan 1993 argue that this measure of the earning loss may not capture the true effects of job loss, especially for displaced workers, due to three reasons. First, the earning gap cannot capture the change in macroeconomic factors that might affect earnings. Second, displaced workers may have higher earnings if they have not been displaced, so comparing the earnings before and after displacement cannot account for the full earning losses. Lastly, for some workers, weekly earnings start to decline a couple of years prior to separation, therefore comparing earnings right after the separation with earnings right before would not capture the true earning declines.

I begin my analysis by estimating the effects of job displacement on the changes in the weekly earnings (ΔY_{it}) by estimating the following linear regression model.

$$Y_{it} = \lambda_i + \gamma_t + X_i^T \beta_t + \alpha D_{it} + \rho_t M_i + \eta(M_i * D_{it}) + \epsilon_{it},$$

$$\Longrightarrow \Delta Y_{it} = \Delta\gamma_t + X_i^T \beta + \alpha D_{it} + \rho M_i + \eta(M_i * D_{it}) + \Delta\epsilon_{it}, \tag{1.5.1}$$

ΔY_{it} is the change in the earning of the worker i in time t, λ_i is an individual fixed effect, γ_t is a year fixed effect and $\Delta\gamma_t = \gamma_t - \gamma_{t-1}$, X_i is a vector of observable characteristics which includes age, gender, race, marital status, education, and industry. β is equal to $\beta = \beta_t - \beta_{t-1}$ which captures the change in time-varying effects of covariates on ΔY_{it}. For example, weekly earnings of high-educated individuals grow at faster rate than low-educated ones over time. D_{it} denotes the job displacement status. M_i contains indicators for three immigrants categories (OECD, Latin-America, and other immigrants). ρ is equal to $\rho = \rho_t - \rho_{t-1}$ which represents different time-varying weekly earnings growth rate for each group of immigrants. For example, weekly earnings of OECD immigrants grow at faster rate than other groups of immigrants over time. ϵ_{it} is an error term. In my analysis immigrants will be compared to native, white, married, 35 to 49 years old men who are not college graduates and work in the manufacturing industry.

My interest lies in the coefficients α and η which represent the native effects of the job displacement and whether there are differential effects for immigrants. In Table 1.5, I provide the estimation results from the regression analysis of Equation 1.5.1. Results exhibit strong and negative impact of job displacement on changes in the weekly earnings of native workers. Each column of Table 1.5 shows a separate regression with a different set of covariates and different types of standard errors. The coefficients in the column (1) are obtained while controlling for age, education, gender, race, marital status, industry, and year fixed effects. The coefficient of job displacement shows that native displaced workers experience around 23 log point larger decline in their earnings than do non-displaced workers. Looking at the interaction terms, job displacement has slightly lower negative effects on weekly earnings of immigrants compared to native workers while these effects are only statistically significant for immigrants from other countries.

The coefficients in columns (2) to (5) of Table 1.5 are obtained while the standard errors are clustered over 51 states and 10 categories of industries respectively. Small standard errors with narrow confidence intervals, large t-statistics, and over rejection of the true null hypotheses are the results of ignoring the existence of within-cluster error correlation (Cameron and Miller 2015). I did not control for industries in columns (2) and (4) since Jacobson, LaLonde, and Sullivan 1993 argue that controlling for industries might be problematic and underestimate the effects of job displacement[12]. The results in column (4) suggest that Latin-America and other immigrants experience slightly smaller negative changes in their weekly earnings compared to natives. This could be related to the higher pre-displacement earnings of native-born workers and also the fact that immigrants have less location-specific human capital and are more mobile. If immigrants are more mobile than natives then one would expect more efficient post-displacement job search (Boman 2011).

Table 1.6 presents the effects of job displacement among high- and low-educated natives and immigrants. The high-educated dummy variable is equal to 1 if an individual has more than 12 years of schooling and 0 otherwise. The high-educated workers experience larger negative effects on their weekly earnings after displacement

[12]Workers who stayed in firms or industries that lay off other workers may also face some changes in their earnings. So, controlling for industries may result in smaller displacement effects.

compared to low-educated displaced workers. High-educated workers are more likely to lose high paying jobs and experience larger earning penalties after job loss. Although in Table 1.5 the effects of job displacement on weekly earnings were estimated to be more negative for natives than immigrants, these effects are reversed among high-educated groups. Looking at the effects of job displacement among high-educated natives and immigrants (the third row in Table 1.6), the high-educated native workers are estimated to experience slightly smaller negative effects after displacement compared to high-educated displaced immigrants.

To investigate the effects of job displacement on displaced native workers residing in states with different share of immigrants in the population, I add a dummy variable, S_i, which is equal to one if an individual is residing in a state with higher share of immigrants in the population and zero otherwise. Then, I estimate the following regression for native workers;

$$Y_{it} = \lambda_i + \gamma_t + X_i^T \beta_t + \alpha D_{it} + \rho_t S_i + \eta(S_i * D_{it}) + \epsilon_{it},$$

$$\Longrightarrow \Delta Y_{it} = \Delta\gamma_t + X_i^T \beta + \alpha D_{it} + \rho S_i + \eta(S_i * D_{it}) + \epsilon_{it}, \tag{1.5.2}$$

Where $\rho = \rho_t - \rho_{t-1}$ which captures the time-varying effects of residing in states with higher share of immigrants on ΔY_{it}. Such that native workers who are residing in states with higher share of immigrants have different earning growth path than those who are residing in states with lower share of immigrants. I present the estimates of the coefficients of Equation 1.5.2 in Table 1.7. Similar to the previous table, each column includes a different set of covariates and different types of standard errors. Although the effects of job displacement still exhibit significant negative effects on the changes in the weekly earnings, I find no evidence that the effects of job displacement on weekly earnings vary among natives residing in states with higher share of immigrants in the population than other natives.

1.6 Robustness Checks and Extensions

1.6.1 Alternative Dependent Variable

As a robustness check, additional models are also estimated to investigate the adverse effects of job displacement on weekly earnings of natives and immigrants. In the first model the dependent variable is log of weekly earnings at the time of the survey, $log(Y_{it})$ (post-displacement earnings for displaced workers), and the control variables are the same as the Equation 1.5.1 except I additionally control for the lag of weekly earnings. Using the lagged dependent variable model I can write:

$$Y_{it} = \gamma_t + X_{it}^T \beta + \alpha D_{it} + \zeta Y_{it-1} + \rho M_i + \eta(M_i * D_{it}) + \epsilon_{it}. \tag{1.6.1}$$

Where Y_{it} is log of weekly earnings for individual i at time t. γ_t is a year fixed effect, X_{it} is a vector of observable characteristics which includes age, gender, marital status, education, and industry. D_{it} denotes the job displacement status. Y_{it-1} is log of weekly earnings at time $t-1$. M_i contains indicators for three immigrants categories (OECD, Latin-America, and other immigrants). ϵ_{it} is the error term.

In Table 1.8, I present the estimates for alternative dependent variable. The results are consistent with the results in Table 1.5. Job displacement has significant negative effects on the log of weekly earnings. Although the coefficients on the interaction terms for OECD immigrants and Latin-America are not statistically different from zero, other immigrants experience slightly smaller reduction in their weekly earnings compared to natives after displacement. The analysis of the share of immigrants in the population is presented in Table 1.9. I did not find any different effects of job displacement among displaced native workers residing in states with higher share of immigrants in the population. However, the coefficients on the share of immigrants in the population imply that native workers residing in states with higher share of immigrants in the population experience around 7-8 log point increase in their weekly earnings over time compared to those living in states with lower share of immigrants. This is consistent with the idea that immigrants can be a complementary input for native workers as they can bring skills to the host country which can result in an increase in labor demand and higher wages (Roy 1997).

In the second model, zero earnings were included in the analysis. To be able to include the zero earnings, I use the $log(x+1)$ transformation where x is weekly earnings and lag of weekly earnings. The results are presented in the Tables 1.10 and 1.11. These results are consistent and similar with those from the main model in which I excluded the zero earnings. The number of zero earnings in my analysis are relatively small and inclusion of them do not affect my results..

1.6.2 Alternative Comparison Group Specification

Lastly, instead of separating immigrants into three categories I follow the work of Bratsberg, Raaum, and Roed 2018 and categorize the immigrants in two groups. One includes immigrants from less developed countries (LDC) and other consists of immigrants from the rest of the world (which I refer to as the non-LDC immigrants). To define the less developed countries I use the report of United Nations Committee for Development Policy[13] and present a table of these counties in Table 1.15. Then, I repeat my analysis for the separate groups of LDC and non-LDC immigrants. The results are presented in Tables 1.12 and 1.13. The results are quite similar for the group of LDC and non-LDC immigrants while the coefficients for LDC immigrants are not statistically different from zero. Non-LDC Immigrants are estimated to experience positive changes in their weekly earnings compared to natives after displacement.

[13]https://www.un.org/development/desa/dpad/least-developed-country-category.html

1.7 Conclusion

This paper extends the previous work by analyzing the consequences of job displacement on weekly earnings among natives and immigrants in the United States and also examining whether job displacement has different effects among native displaced workers residing in states with different share of immigrants in the population. My analysis distinguishes between immigrants from OECD countries, immigrants from Latin-America, and immigrants from other countries.

I combine the Current Population Survey with the Displaced Workers Survey from 1999 to 2018 to investigate the impact of job displacement on weekly earnings among immigrant and native workers in the United States. My results show no statistically significant difference in the effects of job displacement among immigrants from OECD countries and natives. However, immigrants from Latin-America and all the other countries experience smaller earnings losses compared to natives. This can be explained by the lower levels of earnings of these groups of immigrants or the human capital theory in which immigrants have lower location-specific human capital and are more mobile which would lead to more efficient post-displacement job search (Boman 2011). Immigrants from Latin-America and all the other countries (except OECD countries) in my sample are on average less educated compared to natives and OECD immigrants. From another perspective, due to the types of jobs that immigrants hold, they may have limited access to unemployment benefits after displacement and consequently have a higher job finding rate.

I find no evidence that job displacement has differential effects on displaced native workers residing in states with higher share of immigrants in the population. However, I find empirical evidence on the impact of the share of immigrants in the population on log of weekly earnings of natives. Natives who are residing in states with higher share of immigrants experience 7 to 8 log point increase in their weekly earnings compared to natives residing in states with smaller share of immigrants in the population. This is consistent with one of the theories of Roy 1997 in which immigrants are considered as a complementary input for native-born labor as they can bring skills to the host country which can result in an increase in labor demand and higher wages.

1.8 Tables

Table 1.3: Characteristics of Immigrants and US-Born Workers

Demographics	OECD Immigrants	Latin-America Immigrants	Other Immigrants	US-Born
Age				
20 to 34 years old	19.77%	26.72%	28.69%	26.40%
34 to 49 years old	43.12%	47.00%	45.87%	40.81%
50 to 64 years old	37.11%	26.28%	25.44%	32.80%
Gender				
Female	50.32%	46.89%	42.26%	50.41%
Male	49.68%	53.11%	57.74%	49.59%
Race				
White	84.31%	85.30%	53.06%	87.99%
Black	1.86%	11.53%	9.56%	8.41%
Other races	13.83 %	3.18%	37.38%	3.60%
Marital Status				
Married	68.82%	62.81%	70.44%	63.27%
Single	16.19%	21.29%	19.05%	21.59%
Divorced/Widowed	14.99%	15.91%	10.51%	15.13%
Education Level				
Less than high school	3.75%	23.82%	26.34%	4.04%
High school	20.13%	29.64%	23.30%	28.29%
Some college degree	27.76%	23.44%	17.39%	32.05%
At Least a college degree	48.36%	23.10%	32.97%	35.62%
Weekly Earnings	$1,012.4	$686.4	$768.7	$859.7

Source: Displaced Workers Survey, Current Population Survey.

Table 1.4: Characteristics of Immigrants and US-Born Displaced Workers

Demographics	OECD Immigrants	Latin-America Immigrants	Other Immigrants	US-Born
Age				
20 to 34 years old	25.93%	33.96%	36.94%	37.24%
34 to 49 years old	43.46%	48.04%	43.43%	38.33%
50 to 64 years old	30.61%	18.00%	17.63%	24.43%
Gender				
Female	39.49%	36.78%	32.64%	44.04%
Male	60.51%	63.22%	67.36%	55.96%
Race				
White	87.38%	85.60%	61.34%	87.57%
Black	1.64%	9.39%	8.82%	8.75 %
Other races	10.98%	5.01%	29.85%	3.68%
Marital Status				
Married	60.75%	59.31%	66.70%	53.93%
Single	19.86%	24.10%	21.63%	27.81%
Divorced/Widowed	19.39%	16.59%	11.66%	18.26%
Education Level				
Less than high school	2.34%	25.82%	30.67%	5.66%
High school	17.76%	29.42%	26.83%	29.97%
Some college degree	33.88%	23.63%	16.10%	34.80%
At Least a college degree	46.03%	21.13%	26.40%	29.57%
Weekly Earnings	$901.7	$549.7	$652.2	$699.7

Source: Displaced Workers Survey, Current Population Survey.

Table 1.5: Impact of Job Displacement on the Changes in the Weekly Earnings among Natives and Immigrants

Dependent variable: ΔY	(1)	(2)	(3)	(4)	(5)
Job Displacement	-0.233*	-0.234*	-0.233*	-0.234*	-0.233*
	(0.008)	(0.009)	(0.009)	(0.023)	(0.024)
OECD*Job Displacement	0.004	0.004	0.004	0.004	0.004
	(0.062)	(0.058)	(0.058)	(0.066)	(0.065)
Latin-America*Job Displacement	0.055	0.056	0.055	0.056*	0.055*
	(0.035)	(0.031)	(0.032)	(0.021)	(0.021)
Other Immigrants*Job Displacement	0.057*	0.059*	0.057*	0.059*	0.057*
	(0.022)	(0.021)	(0.021)	(0.026)	(0.026)
Controls	All	No industry	All	No industry	All
Cluster SE-State	No	Yes	Yes	No	No
Cluster SE-Industry	No	No	No	Yes	Yes
Observations	141,969	141,969	141,969	141,969	141,969

Note: The dependent variable is the change in the weekly earnings, the gap between $\log(Y_t)$ and $\log(Y_{t-1})$. "All" refers to the set of covariates which includes age, education, gender, race, marital status, industry, and year fixed effects. Standard errors are obtained under two clusters; over 10 categories of industry as well as 51 states.
* indicates statistically significant at the 5% level.

Table 1.6: Impact of Job Displacement on the Changes in the Weekly Earnings among High-Educated and Low-Educated Natives and Immigrants

Dependent variable: ΔY	(1)	(2)	(3)	(4)	(5)
Job Displacement	-0.134*	-0.134*	-0.134*	-0.134*	-0.134*
	(0.021)	(0.02)	(0.02)	(0.029)	(0.030)
High-Educated*Job Displacement	-0.10*	-0.10*	-0.10*	-0.10*	-0.10*
	(0.035)	(0.031)	(0.031)	(0.026)	(0.026)
High-Educated*Native* Job Displacement	0.096*	0.096*	0.096*	0.096*	0.096*
	(0.038)	(0.034)	(0.034)	(0.030)	(0.030)
Controls	All	No industry	All	No industry	All
Cluster SE-State	No	Yes	Yes	No	No
Cluster SE-Industry	No	No	No	Yes	Yes
Observations	141,969	141,969	141,969	141,969	141,969

Note: The dependent variable is the change in the weekly earnings, the gap between $\log(Y_t)$ and $\log(Y_{t-1})$. "All" refers to the set of covariates which includes age, gender, race, marital status, industry, and year fixed effects. High-educated dummy variable is equal to 1 if an individual has more than 12 years of schooling and 0 otherwise. Native dummy variable is equal to 1 if an individual was born in the United Stated to American parents and zero if an individual is foreign-born. Standard errors are obtained under two clusters; over 10 categories of industry as well as 51 states.
* indicates statistically significant at the 5% level.

Table 1.7: Impact of Job Displacement on Changes in the Weekly Earnings among Natives Residing in States with Different Share of Immigrations in the Population

Dependent variable: ΔY	(1)	(2)	(3)	(4)	(5)
High-Share*Job Displacement	-0.019	-0.019	-0.020	-0.019	-0.020
	(0.016)	(0.020)	(0.020)	(0.016)	(0.016)
High-Share	-0.0003	-0.003	-0.0003	-0.003	-0.0003
	(0.020)	(0.004)	(0.004)	(0.021)	(0.021)
Job Displacement	-0.219*	-0.221*	-0.219*	-0.221*	-0.219*
	(0.008)	(0.008)	(0.008)	(0.026)	(0.026)
Controls	All	No industry	All	No industry	All
Cluster SE-State	No	Yes	Yes	No	No
Cluster SE-Industry	No	No	No	Yes	Yes
Observations	141,969	141,969	141,969	141,969	141,969

Note: The dependent variable is the change in the weekly earnings, the gap between $\log(Y_t)$ and $\log(Y_{t-1})$. "All" refers to the set of covariates which includes age, education, gender, race, marital status, industry, year and state fixed effects. High-Share is equal to 1 for states with higher share of immigrants.
* indicates statistically significant at the 5% level.

Table 1.8: Impact of Job Displacement on the Log of Weekly Earnings

Dependent variable: Log(Y)	(1)	(2)	(3)	(4)	(5)
Job Displacement	-0.257*	-0.257*	-0.257*	-0.257*	-0.257*
	(0.007)	(0.009)	(0.009)	(0.021)	(0.020)
OECD*Job Displacement	0.005	0.003	0.005	0.003	0.005
	(0.056)	(0.0536)	(0.052)	(0.063)	(0.063)
Latin-America*Job Displacement	0.024	0.031	0.024	0.031	0.024
	(0.032)	(0.031)	(0.032)	(0.021)	(0.020)
Other Immigrants*Job Displacement	0.063*	0.069*	0.053*	0.069*	0.063*
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Controls	All	No industry	All	No industry	All
Cluster SE-State	No	Yes	Yes	No	No
Cluster SE-Industry	No	No	No	Yes	Yes
Observations	141,969	141,969	141,969	141,969	141,969

Note: The dependent variable is the log of weekly earnings at the time of the survey, $\log(Y_t)$. "All" refers to the set of covariates which includes age, education, gender, race, marital status, industry, lag of weekly earnings, and year fixed effects.
* indicates statistically significant at the 5% level.

Table 1.9: The Impact of Job Displacement on the Log of Weekly Earnings among Natives Residing in States with Different Share of Immigrations in the Population

Dependent variable: Log(Y)	(1)	(2)	(3)	(4)	(5)
High-Share*Job Displacement	-0.026	-0.024	-0.026	-0.024	-0.026
	(0.015)	(0.020)	(0.020)	(0.015)	(0.015)
High-Share	0.084*	0.074*	0.084*	0.074*	0.084*
	(0.018)	(0.005)	(0.005)	(0.017)	(0.016)
Job Displacement	-0.244*	-0.244*	-0.244*	-0.244*	-0.244*
	(0.007)	(0.007)	(0.007)	(0.024)	(0.023)
Controls	All	No industry	All	No industry	All
Cluster SE-State	No	Yes	Yes	No	No
Cluster SE-Industry	No	No	No	Yes	Yes
Observations	141,969	141,969	141,969	141,969	141,969

Note: The dependent variable is the log of weekly earnings at the time of the survey, $\log(Y_t)$. "All" refers to the set of covariates which includes age, education, gender, race, marital status, industry, lag of weekly earnings, and year and state fixed effects. High-Share is equal to 1 for states with higher share of immigrants.
* indicates statistically significant at the 5% level.

Table 1.10: Impact of Job Displacement on the Change in the Weekly Earnings

Dependent variable: Log(Y)	(1)	(2)	(3)	(4)	(5)
Job Displacement	-0.219*	-0.220*	-0.219*	-0.220*	-0.219*
	(0.007)	(0.009)	(0.009)	(0.024)	(0.024)
OECD*Job Displacement	0.009	0.009	0.009	0.009	0.009
	(0.053)	(0.05)	(0.05)	(0.052)	(0.052)
Latin-America*Job Displacement	0.051	0.052	0.051	0.052*	0.051*
	(0.033)	(0.033)	(0.033)	(0.018)	(0.018)
Other Immigrants*Job Displacement	0.05*	0.051*	0.05*	0.051	0.05*
	(0.022)	(0.019)	(0.019)	(0.024)	(0.024)
Controls	All	No industry	All	No industry	All
Cluster SE-State	No	Yes	Yes	No	No
Cluster SE-Industry	No	No	No	Yes	Yes
Observations	142,259	142,259	142,259	142,259	142,259

Note: The dependent variable is the change in the weekly earnings, the gap between $\log(Y_t{+}1)$ and $\log(Y_{t-1}{+}1)$. Zero earnings are included. "All" refers to the set of covariates which includes age, education, gender, race, marital status, industry, lag of weekly earnings, and year fixed effects.

* indicates statistically significant at the 5% level.

Table 1.11: The Impact of Job Displacement on the Change in the Weekly Earnings among Natives Residing in States with Different Share of Immigrations in the Population

Dependent variable: Log(Y)	(1)	(2)	(3)	(4)	(5)
High-Share*Job Displacement	-0.017	-0.017	-0.017	-0.017	-0.017
	(0.015)	(0.017)	(0.017)	(0.015)	(0.015)
High-Share	0.037	0.034*	0.037*	0.034	0.037
	(0.022)	(0.004)	(0.004)	(0.029)	(0.028)
Job Displacement	-0.21*	-0.21*	-0.21*	-0.21*	-0.21*
	(0.007)	(0.008)	(0.008)	(0.026)	(0.026)
Controls	All	No industry	All	No industry	All
Cluster SE-State	No	Yes	Yes	No	No
Cluster SE-Industry	No	No	No	Yes	Yes
Observations	142,259	142,259	142,259	142,259	142,259

Note: The dependent variable is the change in the weekly earnings, the gap between $\log(Y_t+1)$ and $\log(Y_{t-1}+1)$. Zero earnings are included. "All" refers to the set of covariates which includes age, education, gender, race, marital status, industry, lag of weekly earnings, and year and state fixed effects. High-Share is equal to 1 for states with higher share of immigrants.
* indicates statistically significant at the 5% level.

Table 1.12: Impact of Job Displacement on the Changes in the Weekly Earnings

Dependent variable: ΔY	(1)	(2)	(3)	(4)	(5)
Job Displacement	-0.223*	-0.225*	-0.223*	-0.225*	-0.223*
	(0.009)	(0.01)	(0.01)	(0.023)	(0.023)
LDC*Job Displacement	0.051	0.0514	0.0512	0.0514	0.0512
	(0.056)	(0.057)	(0.057)	(0.059)	(0.059)
Non-LDC*Job Displacement	0.059*	0.060*	0.059*	0.060*	0.059*
	(0.022)	(0.018)	(0.018)	(0.017)	(0.016)
Controls	All	No industry	All	No industry	All
Cluster SE-State	No	Yes	Yes	No	No
Cluster SE-Industry	No	No	No	Yes	Yes
Observations	120,487	120,487	120,487	120,487	120,487

Note: The dependent variable is the change in the weekly earnings, the gap between $\log(Y_t)$ and $\log(Y_{t-1})$. "All" refers to the set of covariates which includes age, education, gender, race, marital status, industry, and year fixed effects. Standard errors are obtained under two clusters; over 10 categories of industry as well as 51 states.
* indicates statistically significant at the 5% level.

Table 1.13: Impact of Job Displacement on the Log of Weekly Earnings

Dependent variable: Log(Y)	(1)	(2)	(3)	(4)	(5)
Job Displacement	-0.248*	-0.248*	-0.248*	-0.248*	-0.248*
	(0.0075)	(0.0097)	(0.0097)	(0.0208)	(0.0202)
LDC*Job Displacement	0.073	0.071	0.073	0.071	0.073
	(0.054)	(0.0537)	(0.052)	(0.051)	(0.052)
Non-LDC*Job Displacement	0.053*	0.056*	0.053*	0.056*	0.053*
	(0.0189)	(0.0163)	(0.0163)	(0.0144)	(0.0128)
Controls	All	No industry	All	No industry	All
Cluster SE-State	No	Yes	Yes	No	No
Cluster SE-Industry	No	No	No	Yes	Yes
Observations	120,487	120,487	120,487	120,487	120,487

Note: The dependent variable is the log of weekly earnings at the time of the survey, Y_t. "All" refers to the set of covariates which includes age, education, gender, race, marital status, industry, lag of weekly earnings, and year fixed effects.
* indicates statistically significant at the 5% level.

Table 1.14: List of OECD and Latin-America Countries

OECD Countries		Latin-America Countries
Australia	Poland	Argentina
Austria	Portugal	Bolivia
Belgium	Slovak	Brazil
Canada	Republic of Slovenia	Chile
Czech Republic	Spain	Costa Rica
Denmark	Sweden	Cuba
Estonia	Switzerland	Dominican Republic
Finland	Turkey	Ecuador
France	United Kingdom	El Salvador
Germany	United States	French Guiana
Greece		Guadeloupe
Hungary		Haiti
Iceland		Honduras
Ireland		Martinique
Israel		Mexico
Italy		Nicaragua
Japan		Panama
Korea		Paraguay
Latvia		Peru
Lithuania		Puerto Rico
Luxembourg		Saint Barthelemy
Netherlands		Saint Martin
New Zealand		Uruguay
Norway		Venezuela

Note: The list of OECD countries is obtained from OECD.org.

Table 1.15: List of Less Developed Countries

LDC	
Afghanistan	Malawi
Angola	Mali
Bangladesh	Mauritania
Benin	Mozambique
Bhutan	Myanmar
Burkina Faso	Nepal
Burundi	Niger
Cambodia	Rwanda
Central African Republic	Sao Tomeand Principe
Chad	Senegal
Comoros	Sierra Leone
Democratic Republic of the Congo	Solomon Islands4
Djibouti	Somalia
Eritrea	South Sudan
Ethiopia	Sudan
Gambia	Timor-Leste
Guinea	Togo
Guinea-Bissau	Tuvalu
Haiti	Uganda
Kiribati	United Republic of Tanzania
Lao People's Democratic Republic	Vanuatu
Lesotho	Yemen
Liberia	Zambia
Madagascar	

Source: United Nations Committee for Development Policy.

CHAPTER 2

THE HETEROGENEOUS EFFECTS OF JOB DISPLACEMENT ON EARNINGS

with Brantly Callaway

2.1 Introduction

A typical finding in the job displacement literature is that the "effect" of job displacement is large and persistent (see, for example, Jacobson, LaLonde, and Sullivan 1993 and much subsequent work). However, considering a single effect of job displacement ignores potential heterogeneity across individuals. Treatment effect heterogeneity is a general feature of many program evaluation problems in economics, but treatment effect heterogeneity is likely to be particularly important in the context of job displacement. In particular, some individuals may be able to find similar jobs quickly after they are displaced, but other individuals may experience larger losses due to decreased match quality with their employer (e.g., Jovanovic 1979), loss of firm-, occupation-, or industry-specific human capital (Topel 1991; Neal 1995), loss of a job at a high paying firm (Abowd, Kramarz, and Margolis 1999), or failure to find full time work (Farber 2017).[1] In the current paper, we are interested in trying to distinguish between the cases where all individuals experience roughly the same effect of job displacement or where the "effect" of job displacement mixes together individuals who have relatively small effects of job displacement with individuals who

[1]See Carrington and Fallick 2017 for a good discussion of reasons for why earnings may decline following job displacement.

experience large negative effects of job displacement. Distinguishing between these cases is important for our understanding of job displacement as well as being useful for targeting policy responses to job displacement.

Learning about treatment effect heterogeneity requires learning about the distribution of the treatment effect itself.[2] And the distribution of the treatment effect itself depends, in turn, on the joint distribution of treated and untreated potential outcomes. Learning about the joint distribution of treated and untreated potential outcomes is quite challenging – e.g., even if we could randomly assign workers' displacement status, this would only identify the marginal distributions not the joint – and has been the subject of a large literature in econometrics. This literature includes Heckman, Smith, and Clements 1997; Fan and Park 2010; Fan, Guerre, and Zhu 2017; Frandsen and Lefgren 2017; Callaway 2018, among others. This literature has largely focused on partial identification results under relatively weak assumptions or invoking stronger assumptions to obtain point identification. Here, we consider an assumption that results in point identification. The assumption is that, in the absence of job displacement, an individual would have maintained their rank in the distribution of earnings across time periods. This sort of assumption is a kind of rank-invariance assumption though it is a rank invariance over time assumption rather than rank invariance across treatment states which is more common. One can also think of this less as an assumption and more as trying to understand the question: What is an individual's earnings following job displacement relative to what they would have earned if they had not been displaced *and* had maintained their rank in the distribution of earnings? This interpretation, as we discuss below, does not require the aforementioned rank invariance assumption. Instead, it subtly changes the parameter

[2] A related (though distinct) idea is to compare the distribution of observed outcomes for displaced workers to their counterfactual distribution of outcomes if they had not been displaced. Researchers often compare the quantiles of these two distributions – this class of parameters is called quantile treatment effects. However, these types of parameters are likely to have severe limitations for understanding heterogeneous effects of job displacement which we discuss in more detail in the next section.

of interest.

The first step to understanding heterogeneous effects of job displacement is to estimate the (conditional) distributions of treated and untreated potential outcomes. This is a non-trivial task (though one we have largely abstracted from so far because these sorts of arguments are more standard). For identification, we use the approach of Callaway, Li, and Oka 2018. The two main identifying assumptions are that the distribution of the change in untreated potential outcomes over time is independent of treatment status after conditioning on covariates (this is a Difference in Differences-type assumption) and that the conditional copula for the change in untreated potential outcomes and initial level of untreated potential outcomes is the same for the treated group and untreated group.[3] What these assumptions mean in the context of job displacement is that (i) the path of earnings that displaced workers would have experienced had they not been displaced is the same as the path of earnings that non-displaced workers with the same characteristics did experience, and (ii) if, for example, the largest increases in earnings for non-displaced workers tended to go to individuals with the highest earnings in the previous period, the same would have occurred for displaced workers if they had not been displaced from their job. One challenge is that we cannot implement the estimation procedure proposed in Callaway, Li, and Oka 2018 because their approach is for the case with discrete covariates only and involves sample splitting. This is not feasible in the current application where there are some continuous covariates (e.g., age) and too many discrete covariates relative to the number of observations to split the sample on every possible combination of the discrete covariates. Instead, we use quantile regression (Koenker and Bassett Jr 1978; Koenker 2005) to estimate each required conditional distribution / quantile function. Quantile regression provides a flexible way to model the conditional distributions and

[3] We note that other approaches could be used for identification here; for example, one could assume that treatment is as good as randomly assigned after conditioning on covariates and lagged outcomes or one could assume a conditional version of the Change in Changes model (Athey and Imbens 2006) as in Melly and Santangelo 2015.

quantiles that we need to estimate in the first step.[4]

After the conditional distributions of treated and untreated potential outcomes have been obtained in the first step, we use the rank invariance assumption to construct the joint distribution of treated and untreated potential outcomes. In particular, under the assumption of rank invariance, an individual's counterfactual outcome can be obtained by taking their (conditional) rank in the first time period and evaluating the conditional quantile of counterfactual outcomes (which is identified) at that particular rank. This step results in every observed treated potential outcome being matched with an untreated potential outcome. Once this step has been completed, we can obtain any feature of the distribution of the treatment effect. This also implies that the distribution of the treatment effect conditional on covariates is identified as well. In a final step, we again use quantile regression to summarize how the effect of job displacement varies with covariates at different parts of the conditional distribution.

We use the Displaced Workers Survey (DWS) which is a supplement to the Current Population Survey (CPS) to estimate the entire distribution of the effect of job displacement on weekly earnings for displaced workers and evaluate how the effects of job displacement vary across individuals with different observable characteristics (e.g., age, sex, education, marital status) and as a function of what a displaced worker's earnings would have been in the absence of job displacement. Our main results use the 2016 DWS. We find that, on average, displaced workers lose about \$157 per week relative to what they would have earned if they had not been displaced (this corresponds to 18% of pre-displacement earnings). We also find that there is substantial heterogeneity. We estimate that 42% of displaced workers have *higher* earnings following displacement than they would have had if they had not been displaced. On the other hand, we also estimate that 21% of workers earn at least \$500 less per week than they would have earned if they had not been displaced. In a final

[4]Other papers that have used quantile regression as a first-step estimator include Melly and Santangelo 2015; Wüthrich 2019.

step, we use quantile regression to study the effect of covariates on the distribution of the individual-level effect of job displacement. We find substantial heterogeneity across observed characteristics. The largest effects of job displacement appear to be concentrated among older, male, college graduates as well as those who would have had high earnings if they had not been displaced. As an additional check, we compare our results to those obtained under the condition that a worker who is displaced from their job would have earnings that are exactly equal to their pre-displacement earnings.[5] We also extend our results to different time periods using the 2010 DWS (corresponding to workers displaced during the Great Recession) and the 1998 DWS (corresponding to workers displaced in a substantially earlier time period when the U.S. economy was strong). We find larger effects of job displacement during the Great Recession, but, interestingly, we continue to find a substantial fraction, 28%, that have higher earnings following displacement than they would have had if they had not been displaced.

2.2 Identification

In this section, we discuss the parameters that we are interested in identifying and the assumptions required to identify them. But first, we introduce some notation. We consider the case where a researcher has access to two periods of panel data which corresponds to the available data that we use from the Displaced Workers Survey. We denote the two periods by t and $t-1$. We define outcomes in each period by Y_t and Y_{t-1}. Individuals have treated and untreated potential outcomes, $Y_s(1)$ and $Y_s(0)$, for $s \in \{t, t-1\}$; these correspond to the outcomes that a particular individual would experience if they were displaced or not displaced at a particular point in time. We

[5] An alternative way to think about this is asking the question: What is an individual's earnings following job displacement relative to what they earned before they were displaced? This sort of question does not require any identifying assumption (both of these are observed outcomes), but it may not be the research question of interest.

consider the case where no one is treated in the first period and some individuals become treated in the second period.[6] Let the variable D denote whether or not an individual is treated. Thus, we observe $Y_t = DY_t(1) + (1 - D)Y_t(0)$ and $Y_{t-1} = Y_{t-1}(0)$. We also observe some covariates X.

Our interest centers on features of the distribution of $(Y_t(1) - Y_t(0)|D = 1)$. This is the distribution of the treatment effect conditional on being part of the treated group. In other words, it is the distribution of the difference between outcomes that displaced workers experienced and what they would have experienced if they had not been displaced. However, identifying features of this distribution is challenging – primarily because, for each individual, either $Y_t(1)$ or $Y_t(0)$ is observed but $Y_t(1) - Y_t(0)$ is not observed for any individual. Parameters that depend on this distribution include many treatment effect heterogeneity-type parameters: (i) the fraction of displaced workers that have higher earnings following displacement than they would have had if they had not been displaced, and (ii) how much lower earnings are for individuals who are most affected by job displacement, among others. We are also interested in features of the distribution of $(Y_t(1) - Y_t(0)|Y_t(0), D = 1)$. These sorts of parameters include the distribution of the treatment effect for displaced workers as a function of what a displaced worker's earnings would have been if they had not been displaced. Finally, we are interested in the distribution of the treatment effect for displaced workers as a function of covariates; i.e., features of the distribution of $(Y_t(1) - Y_t(0)|X, D = 1)$. In particular, these parameters allow us to think about what individual-level characteristics are associated with the largest effects of job displacement. Our approach allows one to look at different parts of the conditional

[6]This is essentially the setup in the case of our application on job displacement due to the Displaced Workers Survey being administered only every other year. In addition, part of our identification argument utilizes a Difference in Differences-type identification strategy. This sort of identification strategy identifies effects for individuals who move from being untreated to treated over time. It does not identify effects for individuals who are treated in the first period; moreover, this group is not used for identifying counterfactual outcome distributions (see discussion below) and therefore can be dropped from the analysis.

distribution of the effect of job displacement which is substantially more informative than looking at average effects across different values of covariates which is commonly done in applied work (see, for example, the discussion in Bitler, Gelbach, and Hoynes 2017).

What each of these parameters have in common is that they depend on the joint distribution $(Y_t(1), Y_t(0)|X, D = 1)$. By Sklar's Theorem (Sklar 1959), joint distributions can be written as the copula (which captures the dependence of two random variables) of the marginal distributions. Thus, we take a two step approach to identifying this joint distribution. In the first step, we identify the marginal distributions $F_{Y_t(1)|X,D=1}$ and $F_{Y_t(0)|X,D=1}$. In the second step, we impose additional assumptions to additionally identify their joint distribution.

2.2.1 Step 1: Identifying Marginal Distributions

The first step in our analysis is to show that $F_{Y_t(1)|X,D=1}$ and $F_{Y_t(0)|X,D=1}$ are identified. $F_{Y_t(1)|X,D=1}$ is immediately identified from the sampling process. It is just the conditional distribution of displaced potential earnings for the group of individuals that are displaced from their job – these are the earnings that are observed for displaced workers.

On the other hand, identifying $F_{Y_t(0)|X,D=1}$ is substantially more challenging. This is the distribution of earnings that displaced workers would have experienced if they had not been displaced. We make the following two assumptions:

Assumption 1 (Distributional Difference in Differences)**.**

$$\Delta Y_t(0) \perp\!\!\!\perp D|X$$

Assumption 2 (Copula Invariance Assumption)**.** *Let* $C_{\Delta Y_t(0), Y_{t-1}(0)|X,D=d}$ *denote the copula between the change in untreated potential outcomes and the initial level of* *untreated potential outcomes conditional on covariates and treatment status. Then,*

$$C_{\Delta Y_t(0), Y_{t-1}(0)|X, D=1} = C_{\Delta Y_t(0), Y_{t-1}(0)|X, D=0} \quad a.s.$$

Assumptions 1 and 2 are the assumptions made in Callaway, Li, and Oka 2018 to identify the counterfactual distribution $F_{Y_t(0)|X,D=1}$. The first is a Difference in Differences assumption. It says that the distribution of the path of earnings that individuals who are displaced from their job would have experienced if they had not been displaced from their job is the same as the distribution of the path of earnings that individuals that were not displaced from their job actually experienced (after conditioning on covariates). The second assumption is a Copula Invariance assumption. It says that if, for example, we observe the largest earnings increases over time going to those at the top of the earnings distribution for the group of non-displaced workers, then we would have observed the same thing for displaced workers if they had not been displaced (again, this is conditional on covariates). Callaway, Li, and Oka 2018 show that, under these conditions

$$F_{Y_t(0)|X,D=1}(y|x) = P(\Delta Y_t + F^{-1}_{Y_{t-1}|X,D=1}(F_{Y_{t-1}|X,D=0}(Y_{t-1}|x)|x) \leq y|x, D = 0) \qquad (2.2.1)$$

where every term in Equation (2.2.1) is identified. This implies that both marginal distributions of interest are identified.

2.2.2 Step 2: Identifying the Joint Distribution

Identifying the joint distribution of treated and untreated potential outcomes is a notoriously difficult challenge in the microeconometrics literature. Standard identifying assumptions (or even random assignment of the treatment) do not identify this joint distribution. This section describes our approach to identifying this distribution.

Define

$$\widetilde{Y}_t(0) = Q_{Y_t(0)|X,D=1}(F_{Y_{t-1}|X,D=1}(Y_{t-1}|X)|X) \tag{2.2.2}$$

Notice that $F_{Y_{t-1}|X,D=1}(Y_{t-1}|X)$ is a displaced worker's rank in the conditional earnings distribution in the period before anyone is displaced. Thus, conditional on our identification arguments in the previous section for the counterfactual distribution, the transformation on the right hand side of Equation (2.2.2) is available and $\widetilde{Y}_t(0)$ is the outcome that a displaced worker would experience if they had not been displaced and if they had the same rank in the earnings distribution as they did in the previous period.

Immediately, Equation (2.2.2) implies that the joint distribution $(Y_t(1), \widetilde{Y}_t(0)|X, D = 1)$ is identified. One idea is to just call features of this joint distribution as the parameters of interest. In other words, without any additional assumptions, we can identify parameters such as the fraction of displaced individuals that have higher earnings than they would have had if they had not been displaced and if they had maintained the same rank in the earnings distribution as they did in the pre-treatment period.

Alternatively, we can make the additional assumption:

Assumption 3 (Rank Invariance Over Time (RIOT))**.**

$$F_{Y_t(0)|X,D=1}(Y_t(0)|X) = F_{Y_{t-1}|X,D=1}(Y_{t-1}|X)$$

Assumption 3 says that, in the absence of job displacement, displaced workers would have maintained their rank in the distribution of earnings over time. Assumption 3 implies that $Y_t(0) = \widetilde{Y}_t(0)$ and, thus, that the joint distribution $(Y_t(1), Y_t(0)|X, D = 1)$ is identified, and we can obtain any features of this joint distribution. For comparison, identifying this distribution allows one to ask: What fraction of displaced workers actually benefit from job displacement without the additional caveat mentioned above.

At any rate, employing Assumption 3 leads to exactly the same results as focusing on features of the joint distribution $(Y_t(1), \widetilde{Y}_t(0)|X, D = 1)$ – only the interpretation is different. We use the notation $Y_t(0)$ rather than $\widetilde{Y}_t(0)$ throughout the rest of the paper for simplicity but we interpret our results carefully in the application as the caveat of being conditional on having the same rank over time likely applies.

Remark 2.1. An alternative approach is to define $\widetilde{Y}_t(0) = Y_{t-1}$ for individuals in the treated group.[7] In this case, one can immediately identify the joint distribution $(Y_t(1), \widetilde{Y}_t(0)|X, D = 1)$. Relative to the previous case, there are some advantages and disadvantages to this approach. The main advantage is that it does not require Assumptions 1 and 2 as the distribution of $(Y_t(1), \widetilde{Y}_t(0)|X, D = 1)$ is identified from the sampling process rather than requiring identification assumptions. Without further assumptions, one can immediately identify things like: the fraction of displaced workers who have higher earnings following job displacement than they had before they were displaced, etc. The main disadvantage is that, in this case, $Y_t(0) = \widetilde{Y}_t(0)$ seems less plausible. But, at any rate, this approach serves as a useful comparison.

Remark 2.2. Assumption 3 is distinct from the more commonly invoked "cross-sectional" rank invariance assumption – namely, that a displaced worker's rank in the distribution of $Y_t(1)$ (which is observed) is the same as their rank would be in the distribution of $Y_t(0)$ if they had not been displaced. This is a very strong (and implausible) assumption in the current context as it rules out things like individuals not finding work following displacement.

2.2.3 Comparison to Quantile Treatment Effects

A common alternative strategy for trying to analyze heterogeneous effects of experiencing some treatment is to compare the distribution of treated potential outcomes to the distribution of untreated potential outcomes; in our case, this would mean

[7] Farber 2017, Section 7 considers the fraction of displaced workers who have higher earnings following displacement than they did in their previous job which is similar to what we propose here.

comparing the distribution of outcomes that displaced workers actually experienced to the distribution of outcomes that they would have experienced in the absence of job displacement.[8] Unlike our main parameters of interest, comparing these two distributions does not require identifying the joint distribution of treated and untreated potential outcomes – in fact, these are the two distributions that are identified using only Step 1 of our identification argument above. Often researchers invert these two distributions and take their difference. The result of this transformation is a class of parameters called quantile treatment effects which are important parameters in the program evaluation literature.

In the context of job displacement, these types of parameters may have severe limitations. These limitations stem from the fact that individuals may have substantial differences in their "rank" if they were displaced relative to if they were not displaced.[9] This means that, although it is tempting to interpret differences between, for example, the 10th percentile of displaced potential earnings and non-displaced potential earnings as the effect of job displacement for individuals at the lower part of the earnings distribution, this sort of interpretation is unlikely to be correct. The reason is that some individuals in the lower part of the earnings distribution might have had relatively high earnings if they had not been displaced. When some individuals remain unemployed or have substantially reduced hours following job displacement, this

[8] Identifying these distributions typically either requires experimental data or some identifying argument. However, there are many approaches that are available in the econometrics literature for identifying these distributions; e.g., Firpo 2007 under selection on observables, Abadie 2003; Frolich and Melly 2013; Wüthrich 2019 when a researcher has access to an instrument, Athey and Imbens 2006; Bonhomme and Sauder 2011; Melly and Santangelo 2015; Callaway, Li, and Oka 2018; Callaway and Li 2019 when a researcher has access to more than one period of data. The latter set of papers is most relevant in the current application.

[9] This sort of issue is well-known in the econometrics literature (e.g., Heckman, Smith, and Clements 1997). In other cases, comparing these two distributions may be very useful for policy evaluation or to perform social welfare calculations (Sen 1997; Carneiro, Hansen, and Heckman 2001). However, this does not apply for job displacement primarily because job displacement is not a "policy" (if it were, our results would indicate that it is a really bad policy). Instead, the reason why researchers would be interested in distributions here is to examine heterogeneous effects as we do in the current paper, and simply comparing the marginal distributions is of limited usefulness in this case.

can systematically lead to larger differences between the observed distribution and counterfactual distribution occurring in the lower part of the distribution irrespective of whether the effect of job displacement is bigger or smaller for low or high earnings workers. Instead, to think about these sorts of effects, we propose to estimate the distribution of the individual-level effect of job displacement for displaced workers conditional on what earnings would have been in the absence of displacement. Because of our above results on identifying the joint distribution of treated and untreated potential outcomes, these sorts of parameters are identified and estimable in our framework.

2.3 Estimation

Given the identification results above, determining the best estimation strategy is still challenging. Like many applications in economics, we have a relatively large number of covariates that we would like to condition on to make the identifying assumptions more plausible and only a moderate number of observations. The first step is to estimate the counterfactual distribution of untreated potential outcomes conditional on covariates for the treated group as in Equation (2.2.1). This requires estimating three conditional distribution/quantile functions. These are challenging. Distributional assumptions about the distribution of outcomes conditional on covariates are likely to be invalid. On the other hand, implementing fully nonparametric estimators is infeasible in our case. Callaway, Li, and Oka 2018 consider the case with only discrete covariates and propose nonparametric estimators based on splitting the sample according to each possible combination of discrete covariates. This is infeasible in our case because we have some continuous covariates and the number of discrete covariates is too large relative to the number of observations to split the sample. Instead, we propose to estimate these conditional distributions/quantiles using quantile regression. In particular, we suppose that

Assumption 4 (Quantile Regression)**.** *For all* $(s,d) \in \{t, t-1\} \times \{0,1\}$,

$$Y_s(d) = Q_{Y_s|X,D=d}(U_{sd}|X) = P_{sd}(X)'\beta_{sd}(U_{sd})$$

where $U_{sd}|X, D \sim U[0,1]$ *and* $P_{sd}(X)$ *are some transformations of the vector of covariates* X*.*

Assumption 4 suggests that we can estimate each of the terms in Equation (2.2.1) using QR. In particular, consider the following algorithm to estimate the counterfactual distribution in Equation (2.2.1) and obtain $Y_t(0)$ for each individual in our sample.

Algorithm 1. *Let* τ *denote a fine grid of equally spaced elements between 0 and 1,*

1. *Estimate* $\hat{Q}_{Y_{t-1}|X,D=1}(\tau)$ *by quantile regression of* Y_{t-1} *on* X *for individuals in the treated group*

2. *Estimate* $\hat{Q}_{Y_{t-1}|X,D=0}(\tau)$ *by quantile regression and invert to obtain* $\hat{F}_{Y_{t-1}|X,D=0}$*.*

3. *Set* $\widetilde{Y}_{t-1} \coloneqq \hat{Q}_{Y_{t-1}|X,D=1}(\hat{F}_{Y_{t-1}|X,D=0}(Y_{t-1}|X)|X)$

4. *Estimate* $\hat{Q}_{Y_t(0)|X,D=1}(\tau)$ *by quantile regression of* $\Delta Y_t + \widetilde{Y}_{t-1}$ *on* X *for individuals in the untreated group*

5. *Set* $Y_t(0) \coloneqq \hat{Q}_{Y_t(0)|X,D=1}(\hat{F}_{Y_{t-1}|X,D=1}(Y_{t-1}|X)|X)$

Algorithm 1 provides $Y_{it}(0)$ for all individuals in the treated group. Thus, we can immediately obtain features of the unconditional distribution of $(Y_t(1), Y_t(0)|D=1)$ using the draws of $(Y_{it}, Y_{it}(0))$ for displaced workers. For example, we can estimate the fraction of individuals that have higher earnings following job displacement by

$$\frac{1}{n_1} \sum_{i \in \mathcal{D}} \mathbb{1}\{Y_{it} - Y_{it}(0) > 0\}$$

where n_1 denotes the number of displaced workers and $\mathcal{D}$ denotes the set of displaced workers. We are also interested in how features of the distribution of $Y_t(1) - Y_t(0)$ vary with covariates. It is straightforward to run OLS regressions of $Y_t(1) - Y_t(0)$ on covariates. These results are interesting, and we report them in the application. However, our identification results allow us to go substantially further than this. In particular, we can also estimate how covariates affect the entire distribution of the effect of job displacement. To estimate this, we again can use quantile regression. In particular, we suppose that

$$Q_{(Y_t(1)-Y_t(0))|X,D=1}(\tau|x) = P_{\Delta_X}(x)'\beta_{\Delta_X}(\tau)$$

where P_{Δ_X} denotes transformations of the covariates. The estimates of these conditional quantiles and quantile regression parameters are of interest. For example, if we set τ to be small (e.g., 0.05), $\beta_{\Delta_X}(\tau)$ provides the effect of covariates on the individual-level effect of job displacement for individuals who are most negatively affected by job displacement.

Finally, we are interested in features of the distribution of $Y_t(1) - Y_t(0)$ across different values of $Y_t(0)$; that is, how the effect of job displacement changes across different values of what a displaced worker's earnings would have been if they had not been displaced. Once again, one can estimate OLS regressions of $Y_t(1) - Y_t(0)$ on $Y_t(0)$; however, we also use quantile regression to study how the entire distribution of the effect of job displacement is related to non-displaced potential earnings. Here we suppose that

$$Q_{(Y_t(1)-Y_t(0))|Y_t(0),D=1}(\tau|y) = P_{\Delta_Y}(y)'\beta_{\Delta_Y}(\tau)$$

where $P_{\Delta Y}(y)$ are transformations of non-displaced potential earnings, and β_{Δ_Y} gives the effect of non-displaced potential earnings on particular quantiles of the individual-level effect of job displacement.

2.4 Results

Our work is related to a large literature on job displacement. We only briefly survey the most relevant research here. Almost all work on job displacement considers a single "effect" of job displacement. This effect usually corresponds to an estimated parameter in an OLS regression. Much of the literature (e.g., Jacobson, LaLonde, and Sullivan 1993) has been interested in the long-term effect of job displacement. This is particularly true for papers that have used widely available panel data sets such as the NLSY and PSID (e.g., Ruhm 1991; Stevens 1997; Kletzer and Fairlie 2003) as well as administrative data (e.g., von Wachter, Song, and Manchester 2009; Couch and Placzek 2010). On the other hand, research that has used the Displaced Workers Survey has focused on short run effects of job displacement because the DWS does not contain follow up interviews with displaced workers. Thus, here, we consider only short-run heterogeneous effects of job displacement though it would be interesting to extend our approach to the long-term case though this would require an alternative data source.

Two strands of the literature are most related to the current paper. First, in a series of work (Farber 1997; Farber 2005; Farber 2017), Henry Farber has used the DWS to study the effect of job displacement. Broadly, the main concerns of these papers are the employment and earnings effects of job displacement and how these have varied over time. As mentioned above, Farber 2017 also considers the fraction of displaced workers whose earnings are higher following job displacement than they were in their pre-displacement job. A primary difference between our main results and those in Farber 2017 is that we try to adjust for how earnings for displaced workers would have evolved over time in the absence of job displacement. Our results that also use raw changes in earnings over time are also somewhat different; in particular, Farber 2017, Section 7 focuses mostly on displaced workers that have moved back into full time work while we focus on all displaced workers that are still in the labor force. In addition, we think about the entire distribution of the effect of job displacement and how the distribution of the effect of job displacement varies across covariates and varies for individuals who would have had high or low earnings if they had not been displaced from their job. On the other hand, Farber 2017 carefully considers the role of measurement error and shows that this may somewhat reduce the fraction of displaced workers who actually have higher earnings following job displacement than they had in their pre-displacement job. Second, the application in Callaway 2018 also considers heterogeneous effects of job displacement. The assumptions in that paper are weaker than in the current paper (particularly in the case when we invoke Assumption 3) though his approach leads to bounds on heterogeneous treatment effect parameters rather than point identification which implies that we are able to learn more about the heterogeneous effect of job displacement. In addition, using our approach, we are also able to relate heterogeneous effects to covariates as well as to what earnings would have been in the absence of job displacement.

2.4.1 Data

We use data from the Displaced Workers Survey (DWS). The DWS has been administrated every two years since 1984 as a supplement to the Current Population Survey (CPS) (Flood, King, Ruggles, and Warren 2017) and contains information on earnings and employment status for displaced workers. Respondents are asked whether they lost their jobs at any point in the last three years due to: (i) plant closing or moving, (ii) insufficient work, or (iii) position being abolished. These are sometimes referred to as the "big three" reasons for job displacement (Farber 2017). Our main results below focus on 2015-2016 which is the most recent period when DWS data is available. We also provide similar results for 2009-2010 (during the Great Recession) and 1997-1998 (an earlier period when the U.S. economy was quite strong).

Although the DWS provides useful information on job displacement status and the current weekly earnings of displaced workers and their weekly earnings on their lost job, it does not provide any information about the earnings of non-displaced workers (though non-displaced workers can be identified). After identifying non-displaced workers using the DWS, we obtain their earnings by matching the DWS to CPS outgoing rotation group data using individual-level identification numbers which are available from IPUMS.[10] This matching process helps us to find information on the current weekly earnings as well as weekly earnings in the previous year for non-displaced workers. Effectively, this turns our untreated group into a two period panel dataset. We also drop all non-displaced workers whose earnings are missing in either period. For workers that are displaced, we drop those that are not currently in the labor force and assign 0 earnings to those in the labor force but who are not currently employed. The timing for available earnings is also somewhat different for displaced and non-displaced workers. For displaced workers, the DWS asks for their earnings in their pre-displacement job. But displacement could have occurred any time in the last three years. On the other hand, for non-displaced workers, we only have their current weekly earnings and their earnings one year ago.

The summary statistics for key variables of displaced and non-displaced workers in 2016 are presented in Table 2.1. Weekly earnings are about $74 dollars higher for non-displaced workers than displaced workers in the period before they are displaced. Following job displacement, this gap in average weekly earnings increases to $313. This occurs due to a combination of average weekly earnings increasing by $44 for non-displaced workers and decreasing by $195 for displaced workers (on top of the

[10] Households in the CPS are usually interviewed for four consecutive months, are out of the sample for the next eight months, and then interviewed for another four months. After the eighth time participating, they leave the sample. Households that are interviewed in the fourth or eighth month (which are called outgoing rotation groups) are asked additional labor market questions. Thus, using our approach, we observe earnings for the untreated group in two consecutive years, but they can be different months for different individuals.

Table 2.1: Summary Statistics

	Displaced	Non-Displaced	Difference	P-val on Difference
Earnings				
2016 Earnings	687.82	1000.43	-312.604	0.00
2015 Earnings	882.56	956.43	-73.875	0.00
Change Earnings	-194.74	43.99	-238.729	0.00
Covariates				
Male	0.57	0.51	0.057	0.00
White	0.81	0.82	-0.008	0.48
Married	0.51	0.63	-0.113	0.00
College	0.33	0.40	-0.066	0.00
Less HS	0.07	0.06	0.009	0.21
Age	42.11	45.4	-3.294	0.00
N	1633	3915		

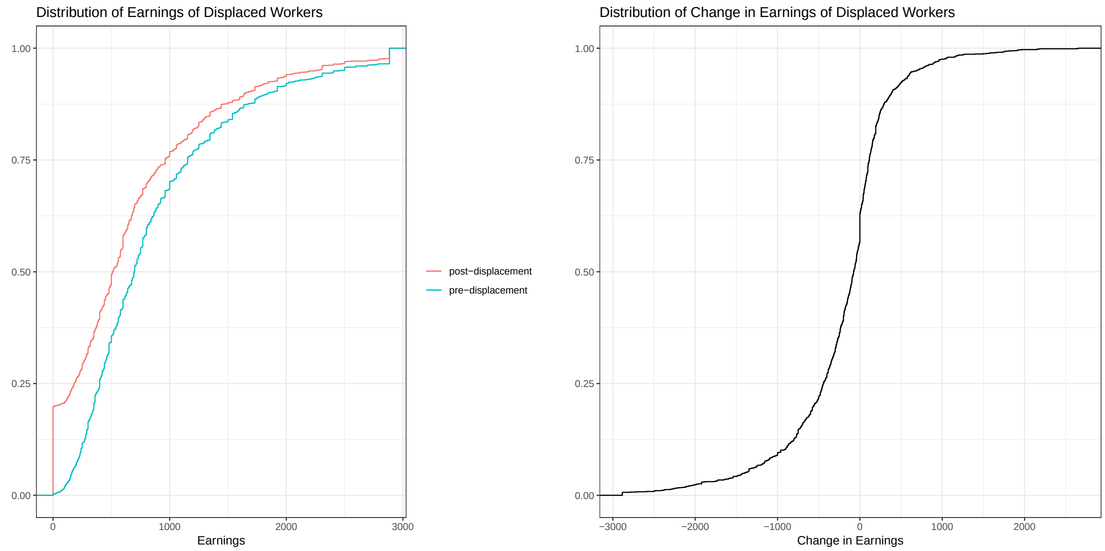
Sources: CPS and Displaced Workers Survey

existing gap in the previous period). For covariates, we include an individual's sex, race, marital status, age, and education. Displaced workers are more likely to be male, not married, younger, and not have a college degree; both groups are roughly equally likely to be white or have less than a high school education.

2.4.2 Results

As a first step, Figure 2.1 plots the distributions of weekly earnings for displaced workers pre- and post-displacement. Clearly, the distribution of post-displacement earnings is shifted to the left. Notably, there is also a mass point in the distribution of post-displacement earnings – about 20% of displaced workers have 0 earnings. These distributions also exhibit a pattern that is consistent with very heterogeneous effects – namely, the difference between the distributions is largest in the lower part of the earnings distributions. The right panel of Figure 2.1 plots the distribution of the change in weekly earnings over time for displaced workers. Recall that this distribution

Figure 2.1: Distributions of Earnings of Displaced Workers

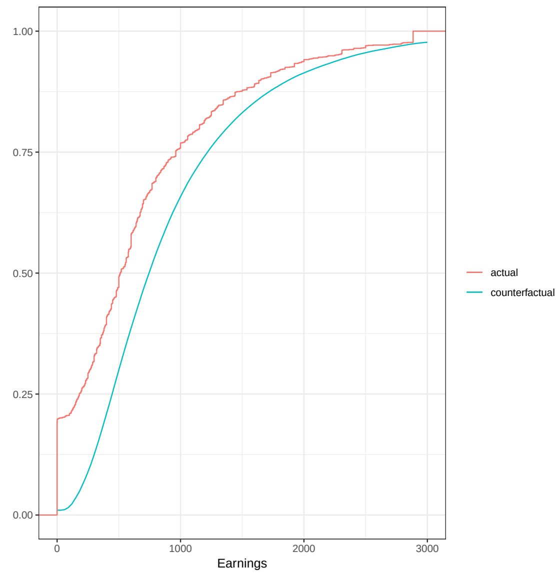


Notes: The left panel contains plots of the distributions of earnings before and after displacement for displaced workers. The right panel plots the distribution of the change in weekly earnings for workers who were displaced from their jobs.

does not require any identifying assumption; it is just the difference in earnings over time. Interestingly, our estimates in the figure indicate that about 37% of displaced workers have higher weekly earnings than they had before they were displaced. This number may overstate how many workers actually have higher earnings than they *would have had* if they had not been displaced especially in the case where earnings would have increased in the absence of job displacement. On the other hand, around 22% of workers earn at least $500 less per week than before displacement and about 10% earn at least $1000 less per week than before displacement – these are substantial differences. Thus, it is immediately clear that the difference in the average decrease in earnings for displaced workers relative to non-displaced workers of $239 per week (see Table 2.1) masks substantial heterogeneity.

The next step in our analysis is to construct the counterfactual distribution of weekly earnings that displaced workers would have experienced if they had not been displaced from their jobs. We do this using the approach of Callaway, Li, and Oka 2018

Figure 2.2: Counterfactual Distribution of Earnings for Displaced Workers



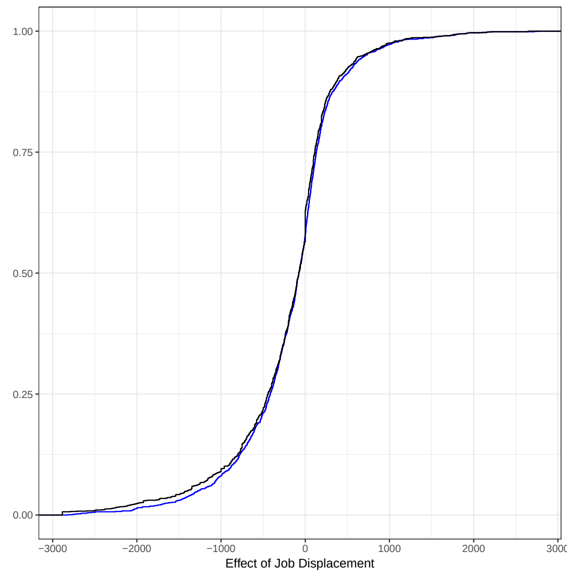
Notes: Plots of the actual and counterfactual distribution of earnings for displaced workers.

as described in Equation (2.2.1) and Algorithm 1. Figure 2.2 plots this counterfactual distribution.[11] Notice that the counterfactual distribution sits to the right of the actual distribution of earnings. Also, the biggest difference between the two distributions occurs in the lower part of the distribution. Once the counterfactual distribution is identified, this also implies that the average effect of job displacement is identified. We estimate that job displacement reduces earnings by \$157 per week on average, which is about 18% of pre-displacement earnings.

Remark 2.3. The results in Figure 2.2 are also closely related to quantile treatment effects. The Quantile Treatment Effect on the Treated (QTT) is the difference between the quantiles of observed outcomes for displaced workers and the quantiles of the counterfactual distribution of outcomes that they would have experienced if they had not been displaced. The QTT comes from inverting the distributions in Figure 2.2. Because the difference between these two distributions is largest at the lower part

[11]The distribution depends on the value of the covariates; the figure reports the average of the distribution over all values of covariates for displaced workers in the dataset. This is a standard way of converting conditional effects into unconditional effects.

Figure 2.3: Distribution of the Treatment Effect under Assumption 3



Notes: The black line plots the distribution of the change in outcomes over time for displaced workers. The blue line plots the distribution of the treatment effect for displaced workers.

of the distribution, it is tempting to interpret this as saying that the effect of job displacement is largest for workers in the lower part of the earnings distribution. This is an incorrect interpretation though because it ignores the possibility that workers change their ranks in the distribution of earnings due to job displacement (e.g., workers who would have had high earnings if they had not been displaced remaining unemployed following displacement). In fact, we find evidence (discussed below) that points in exactly the opposite direction – that the effect of job displacement appears to be larger for high earnings workers than low earnings workers.

Under Assumption 3, we can map each value of $Y_t(1)$ to $Y_t(0)$ and create a pair of $(Y_t(1), Y_t(0))$ for each displaced worker. Figure 2.3 plots the distribution of $Y_t(1) - Y_t(0)$ for displaced workers under Assumption 3. Interestingly, the distribution of the treatment effect is remarkably close to the distribution of the change in outcomes over time.[12] This implies that our results for the distribution of the actual effect of

[12]It is worth explaining more why the distributions are so similar. The black line is the distribution of $Y_t - Y_{t-1}$ for displaced workers. The blue line plots the distribution of $Y_t - Y_t(0)$ for displaced workers. Thus, any differences between the two distributions comes from differences between Y_{t-1} and

job displacement are essentially identical to our earlier estimates that instead use the change in earnings over time for displaced workers.[13] Here, we estimate that (i) 42% of displaced workers would have had higher earnings if they had not been displaced,[14] (ii) 21% have at least \$500 less in weekly earnings than they would have had if they had not been displaced, and (iii) 8% have at least \$1000 less weekly earnings than they would have had if they had not been displaced.

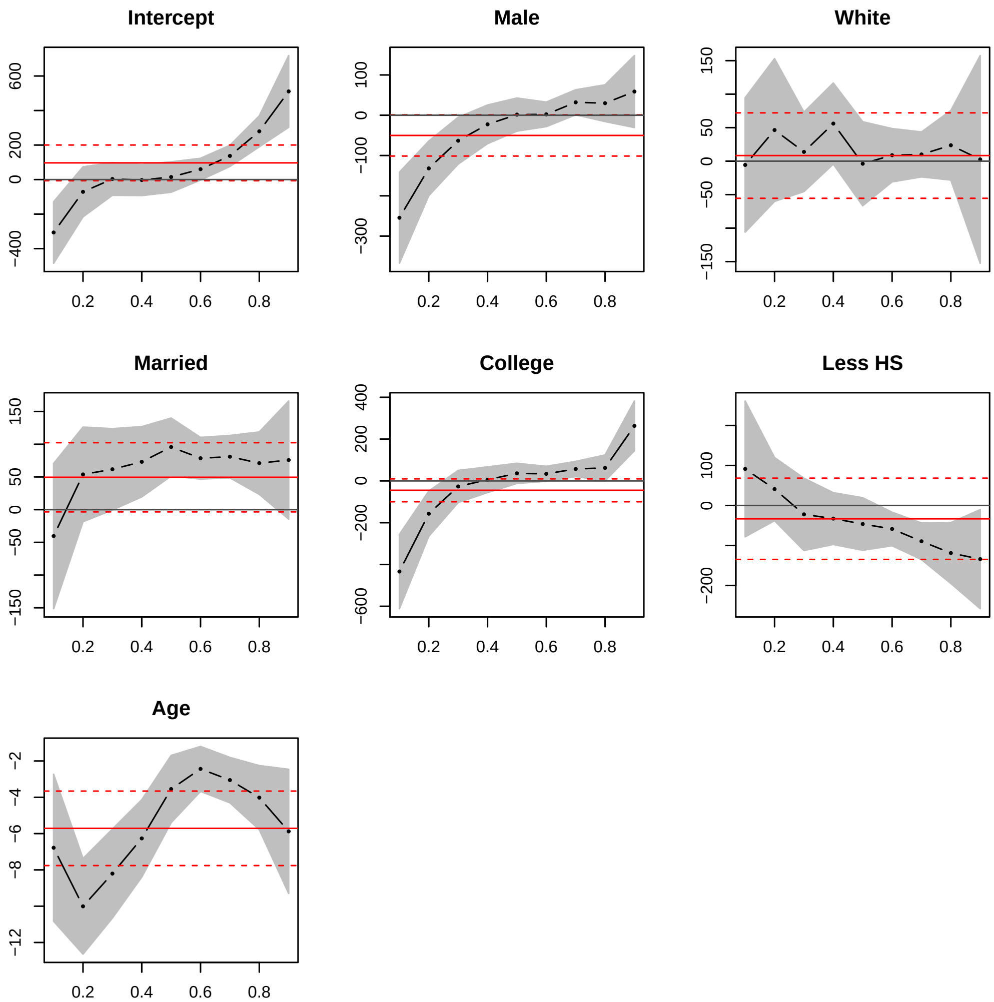
Taken together, our results so far indicate that there is substantial heterogeneity in the effect of job displacement. Even though there is a fairly large average effect of job displacement, a substantial fraction of workers appear to be no worse off due to job displacement than they would have been if they had not been displaced. On the other hand, the negative effects of job displacement seem to be largely concentrated among 10-20% of displaced workers who experience large negative effects.

$Y_t(0)$. From Equation (2.2.2), we generate $Y_t(0) = Q_{Y_t(0)|X,D=1}(F_{Y_{t-1}|X,D=1}(Y_{t-1}|X)|X)$; in practice, this amounts to finding an individuals rank in the conditional distribution of outcomes in time period $t-1$ and applying the conditional quantile function for untreated potential outcomes to that rank. Also, notice that $Y_{t-1} = Q_{Y_{t-1}|X,D=1}(F_{Y_{t-1}|X,D=1}(Y_{t-1}|X)|X)$; in other words, an alternative way to obtain an individual's outcome in period $t-1$ is to find their rank in the conditional distribution in time period $t-1$ and apply the conditional quantile for outcomes in period $t-1$ to that rank. Thus, any difference between the distribution of $Y_t - Y_{t-1}$ and $Y_t - Y_t(0)$ for displaced workers comes down to differences in the conditional quantiles (or distributions) of $Y_t(0)$ and Y_{t-1}. In our case, these two distributions are very similar – suggestive evidence of this can be found in Figures 2.1 and 2.2 – and this explains the reason for the high degree of similarity in Figure 2.3.

[13]As discussed earlier, the appropriate interpretation of these results depends on whether or not one believes Assumption 3. Under Assumption 3, the results that we report should be considered as differences between actual earnings of displaced workers and what they would have earned had they not been displaced. Otherwise, these are differences between actual outcomes for displaced workers and the outcome that they would have experienced if (i) they had not been displaced from their job and (ii) they maintained the same rank in the conditional earnings distribution over time.

[14] It is perhaps surprising that such a large fraction of displaced workers appear to have higher earnings than they would have had if they had not been displaced. Comparing pre- and post-displacement earnings of displaced workers, Farber 2017 similarly finds large percentages of workers earning more in their post-displacement job than in their pre-displacement job. Some possible explanations for this finding that he offers (and that likely apply to our case as well) are: (i) some displaced workers may have higher earnings but lower utility in their new job, (ii) search costs may keep workers from looking for new jobs that could potentially offer higher earnings, and (iii) risk aversion in the sense that workers have more information about their current job and may be reluctant to leave it for the possibility of a higher paying job when they have less information about other aspects of the new job. Another possible explanation is measurement error in earnings though Farber 2017 considers a sensitivity analysis related to measurement error and finds only fairly moderate changes to these results even under conservative assumptions on measurement error in earnings.

Figure 2.4: Quantile Regression Estimates of the Treatment Effect on Covariates



Notes: QR estimates of the effect of covariates on the quantiles of the treatment effect. The solid horizontal line provides OLS estimates of the effect of each covariate, and the horizontal dashed line contains a 90% confidence interval. The dotted line provides QR estimates of the effect of each covariate at particular quantiles from 0.1, 0.2, . . . , 0.9. The shaded area contains pointwise 90% confidence intervals from the quantile regressions.

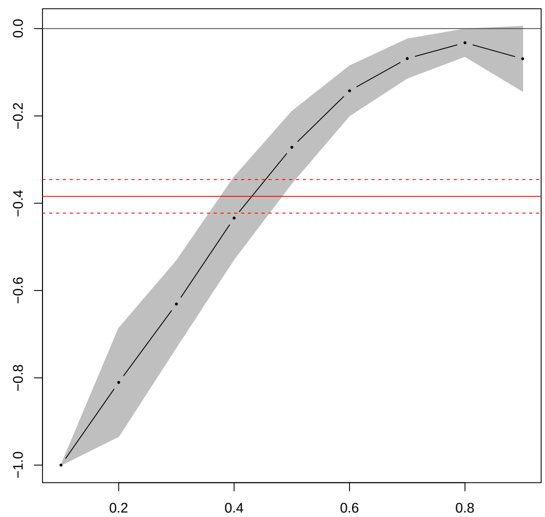
Next, we use quantile regression to see how covariates affect the distribution of the treatment effect for displaced workers. Figure 2.4 provides estimates of the effect of each covariate across different quantiles of the conditional distribution of the treatment effect. There are some quite interesting patterns here. On average, the effect of job displacement is somewhat more negative for men than for women, but the effect is very heterogeneous. For individuals that experience large negative effects, the effects for men are much more negative than the effects for women.[15] On the other hand, for individuals that are towards the top of the distribution of the treatment effect (i.e., experience small or positive effects of job displacement), men tend to have larger benefits than women. Differences in the effect of job displacement by race tend to be small and do not exhibit strong patterns at different parts of the conditional distribution. The effect of job displacement for college graduates is also very heterogeneous. Some college graduates experience tremendously large negative effects from job displacement, while college graduates (conditional on doing well following job displacement) tend to do better than high school graduates. For individuals with less than a high school education, these results are almost exactly reversed. Finally, our results indicate that being older increases the negative effects of job displacement among those most affected by job displacement. Once again, though, the effects are quite heterogeneous – the effect of age for those less affected by job displacement appears to be either positive or close to 0.

These results suggest that the largest negative effects of job displacement tend to be for older, male, college graduates. This should probably not be a surprising result as the effect of job displacement depends on both earnings following job displacement, Y_t, as well as what earnings *would have been* in the absence of job displacement, $Y_t(0)$. Older, male, college graduates may have large earnings losses due to job displacement particularly in the case where if, had they not been displaced, they would have had high earnings.

Finally, in this section, we investigate how the effect of job displacement varies with $Y_t(0)$ – i.e., how the effect varies across individuals as a function of what their earnings would have been if they had not been displaced. We plot quantile regression estimates from regressing $(Y_t - Y_t(0))$ on $Y_t(0)$ in Figure 2.5. On average, the effect of job displacement is larger for individuals who would have had higher earnings in

[15] Recall that the outcome in these quantile regressions is $Y_t - Y_t(0)$ for displaced individuals. This means that "large negative effects" occur at the lower quantiles in Figure 2.4.

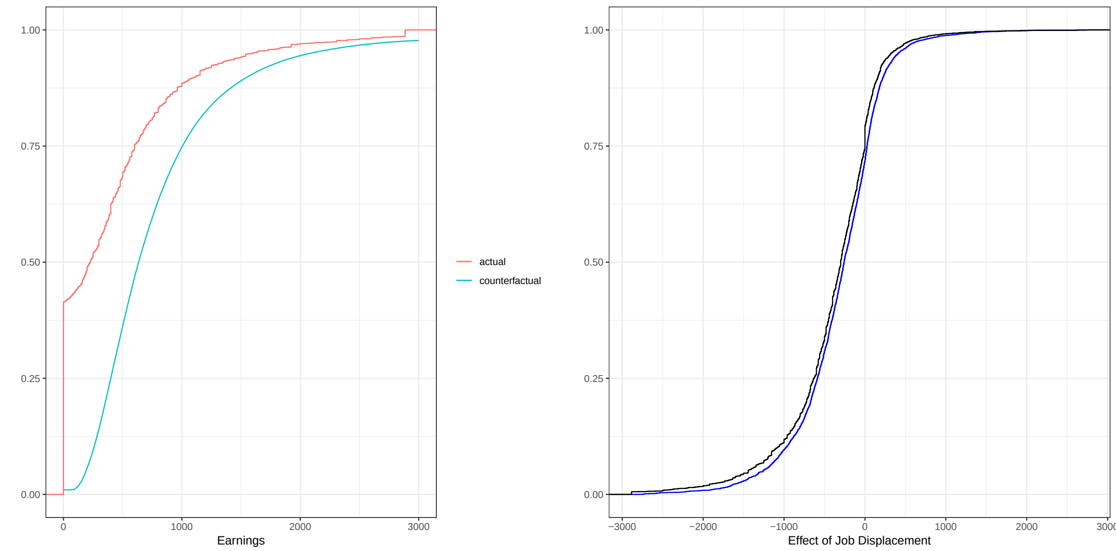
Figure 2.5: Quantile Regression Estimates of the Treatment Effect on $Y_t(0)$



Notes: QR estimates of the effect of what earnings would have been in the absence of job displacement, $Y_t(0)$, on the quantiles of the treatment effect. The solid horizontal line provides OLS estimates of the effect of non-displaced potential earnings, and the horizontal dashed line contains a 90% confidence interval. The dotted line provides QR estimates of the effect of non-displaced potential earnings at particular quantiles from 0.1, 0.2, …, 0.9. The shaded area contains pointwise 90% confidence intervals from the quantile regressions.

the absence of job displacement. Notably, this is exactly the opposite interpretation as one would be tempted to make if one only compared the marginal distributions (see discussion in Theorem 2.3). In addition, there is substantial heterogeneity. For individuals that experience the largest negative effects of job displacement, these negative effects appear to be substantially larger for those who would have had high earnings in the absence of job displacement. On the other hand, for those who are less affected by job displacement, the difference between the effect for those who would have had high earnings relative to those who would have had low earnings in the absence of job displacement is small.

Figure 2.6: Main Results for 2009-2010



Notes: The left panel contains plots of the distributions of earnings for displaced workers as well as their counterfactual distribution of earnings if they had not been displaced. The right panel contains the distribution of the treatment effect for displaced workers.

2.4.3 Results in Other Time Periods

To conclude this section, we provide analogous results for 2009-2010 (corresponding to the Great Recession) and for 1997-1998 (an earlier time period when the U.S. economy was quite strong). We briefly discuss the main results from these other periods below and provide additional details in the appendix (in particular, we provide summary statistics and quantile regression estimates of the effect of covariates and the effect of non-displaced potential earnings on the distribution of the treatment effect). All of the results in this section use the same identification arguments and estimation approaches that were applied to the 2015-2016 data above.

Results for 2009-2010

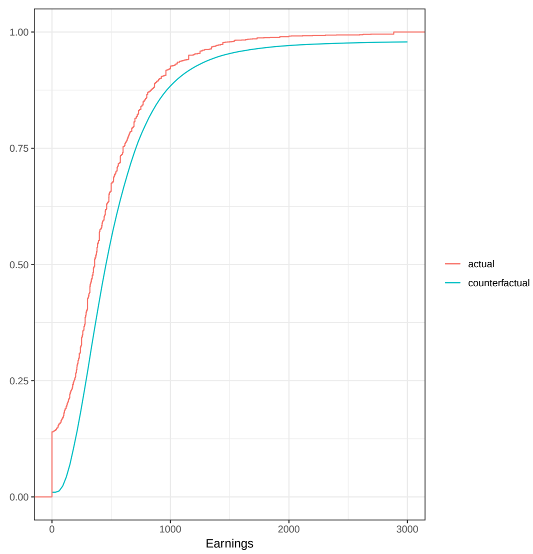
These results are for workers who were displaced at the height of the Great Recession. Figure 2.6 plots the observed and counterfactual distributions of outcomes for displaced workers. The effects of job displacement are much larger, on average, than in 2015-2016. We estimate that workers who were displaced from their job earn \$308 less on average than they would have earned if they had not been displaced; this corresponds to 39% lower earnings on average relative to pre-displacement earnings. Much of these effects appear to be driven by not being able to find work. 41% of displaced workers continued to be unemployed in 2010 (for 2015-2016, only 20% of displaced workers continued to be unemployed in 2016). Interestingly, we estimate that 28% of displaced workers have higher earnings than they would have had if they had not been displaced. Even when we just compare current earnings to pre-displacement earnings, we find that 21% of displaced workers had higher earnings following displacement than they had in their pre-displacement job. This indicates that even though the effects of job displacement appear to be substantially larger during the Great Recession, there is still substantial heterogeneity and a non-trivial fraction of displaced workers seem to have higher earnings than they otherwise would have had.

The effect of covariates on the distribution of the treatment effect in 2009-2010 is broadly similar to the effect in 2015-2016 (see Figure 2.8 in the appendix). In particular, among those most negatively affected by job displacement, older, male, college graduates tend to be the most affected (here, there is also some evidence that among those most affected by job displacement, married displaced workers are also more negatively affected by job displacement). Finally, similarly to 2015-2016, the effect of job displacement tends to be negatively related to earnings that individuals would have experienced if they had not been displaced (see Figure 2.9 in the appendix). These effects tend to be biggest for individuals who experience the largest negative effects of job displacement.

Results for 1997-1998

Finally, we examine the effect of job displacement in 1997-1998. This is an earlier period than the ones we have considered so far, and the U.S. economy was quite strong during this period.

Figure 2.7: Main Results for 1997-1998



Notes: The left panel contains plots of the distributions of earnings for displaced workers as well as their counterfactual distribution of earnings if they had not been displaced. The right panel contains the distribution of the treatment effect for displaced workers.

Figure 2.7 contains our main estimates from this period. We estimate that displaced workers earned on average \$68 less per week than they would have earned if they had not been displaced. This estimate corresponds to 13% lower earnings on average relative to pre-displacement earnings. This estimate is somewhat smaller than the corresponding estimate for 2015-2016 and much smaller than the corresponding estimate for 2009-2010. Part of the reason for the smaller effects here is that only 14% of displaced workers are unemployed in 1998. In addition, we estimate that 45% of displaced workers have higher earnings than they would have had if they had not been displaced. Similarly, 42% of displaced workers had higher earnings in their current job than they did in their pre-displacement job. Finally, our estimates for the effect of covariates and untreated potential outcomes are broadly the same as in the other periods (see Figures 2.10 and 2.11 in the appendix). For displaced workers that are most affected by job displacement, these effects once again tend to be largest for older, male, college graduates and for those who would have had high earnings if they had not been displaced. Given our results in earlier sections, these results seem to indicate that the finding that the largest effects of job displacement are concentrated among these groups is robust across time periods and across various states of the macroeconomy.

2.5 Conclusion

In this paper, we have studied how the effect of job displacement varies across different individuals. To do this, we compared the earnings of displaced workers to what their earnings would have been if they had maintained their rank in the (conditional) distribution of weekly earnings (though we allow for the distribution of weekly earnings to change over time). A key requirement of our approach was to be able to estimate several conditional distributions in a first step in a way that was both not too restrictive while also being feasible with a moderate sized data set and a fairly large number of covariates. To do this, we relied heavily on first-step quantile regression estimators.

We found that displaced workers lose about \$157 per week due to job displacement, on average. However, that average effect masks a tremendous amount of heterogeneity. A large fraction of workers appear to be no worse off (or even better off) in terms of their earnings following job displacement than they would have been if they had not been displaced. On the other hand, another large fraction loses substantially more than the average earnings loss, 21% have at least \$500 and 8% have at least \$1000 less in weekly earnings than they would have had if they had not been displaced. Once we had obtained the distribution of the effect of job displacement, we used quantile regression to study how the distribution of the effect of job displacement depends on covariates. We also showed that this distribution varies substantially across sex, education levels, and age, as well as across different amounts of earnings that displaced workers would have had if they had not been displaced.

2.6 Additional Tables and Figures

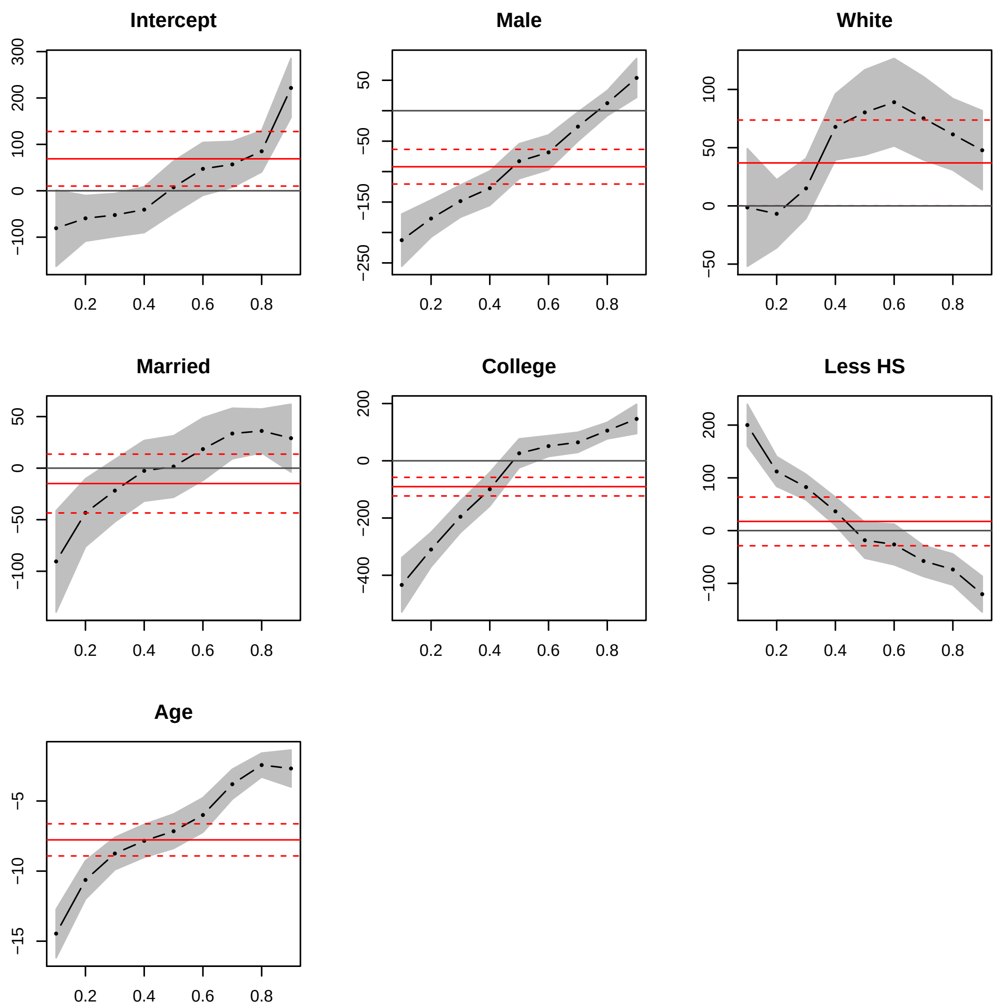
2.6.1 2009-2010 Results

Table 2.2: 2009-2010 Summary Statistics

	Displaced	Non-Displaced	Difference	P-val on Difference
Earnings				
2010 Earnings	418.37	887.54	-469.169	0.00
2009 Earnings	797.24	876.66	-79.424	0.00
Change Earnings	-378.87	10.87	-389.745	0.00
Covariates				
Male	0.60	0.48	0.122	0.00
White	0.83	0.85	-0.017	0.04
Married	0.53	0.62	-0.096	0.00
College	0.25	0.38	-0.123	0.00
Less HS	0.10	0.06	0.044	0.00
Age	41.50	44.87	-3.371	0.00
N	4088	4000		

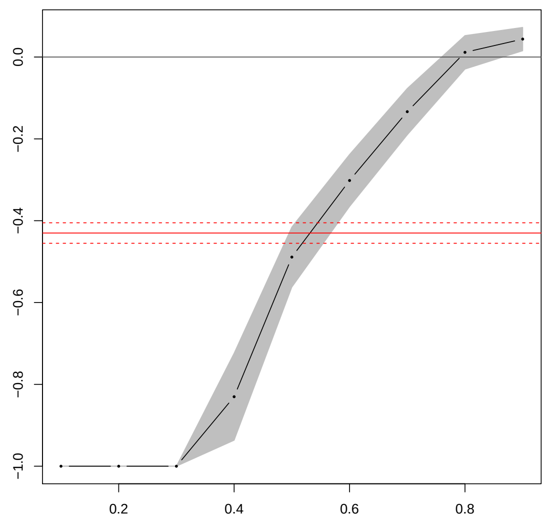
Sources: CPS and Displaced Workers Survey

Figure 2.8: 2009-2010 Quantile Regression Estimates of the Treatment Effect on Covariates



Notes: QR estimates of the effect of covariates on the quantiles of the treatment effect in 2009-2010. The solid horizontal line provides OLS estimates of the effect of each covariate, and the horizontal dashed line contains a 90% confidence interval. The dotted line provides QR estimates of the effect of each covariate at particular quantiles from 0.1, 0.2, . . . , 0.9. The shaded area contains pointwise 90% confidence intervals from the quantile regressions.

Figure 2.9: 2009-2010 Quantile Regression Estimates of the Treatment Effect on $Y_t(0)$



Notes: QR estimates of the effect of what earnings would have been in the absence of job displacement, $Y_t(0)$, on the quantiles of the treatment effect in 2009-2010. The solid horizontal line provides OLS estimates of the effect of non-displaced potential earnings, and the horizontal dashed line contains a 90% confidence interval. The dotted line provides QR estimates of the effect of non-displaced potential earnings at particular quantiles from 0.1, 0.2, . . . , 0.9. The shaded area contains pointwise 90% confidence intervals from the quantile regressions.

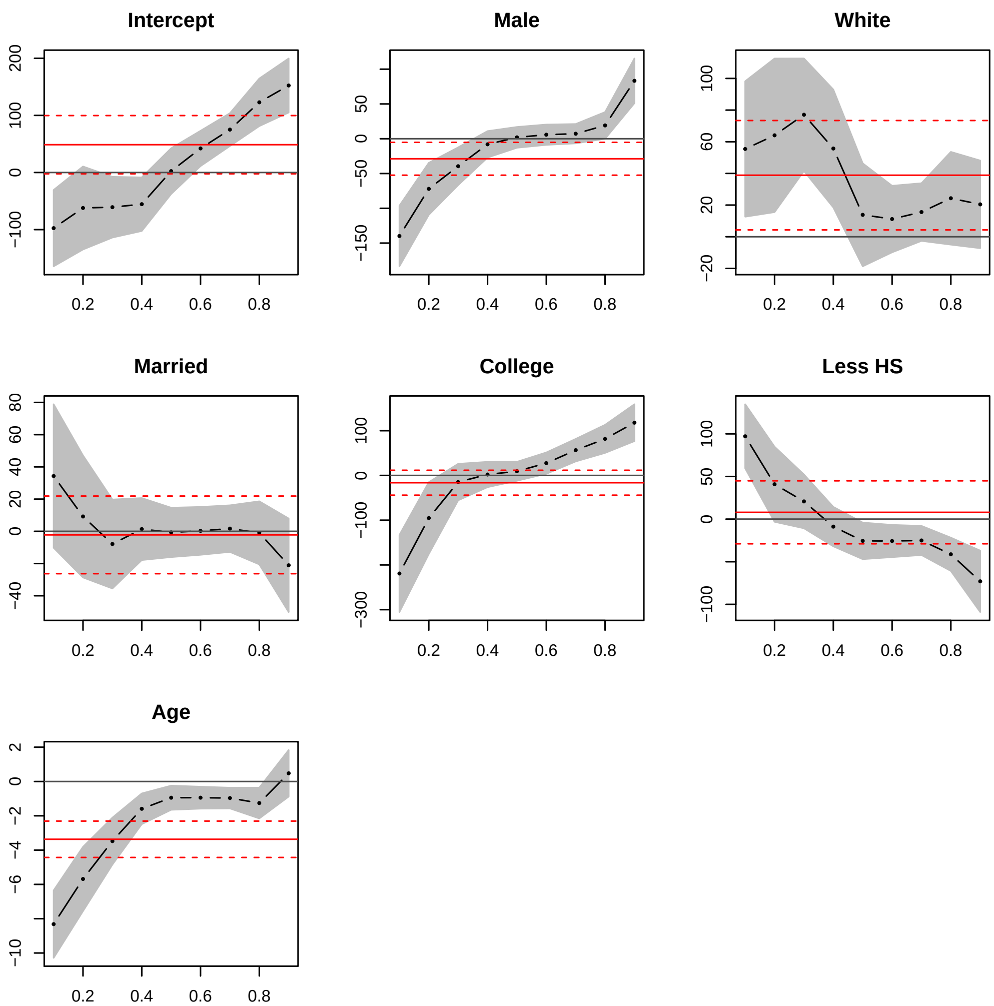
2.6.2 1997-1998 Results

Table 2.3: 1997-1998 Summary Statistics

	Displaced	Non-Displaced	Difference	P-val on Difference
Earnings				
1998 Earnings	443.38	621.07	-177.692	0.00
1997 Earnings	526.52	582.40	-55.886	0.00
Change Earnings	-83.14	38.67	-121.806	0.00
Covariates				
Male	0.56	0.50	0.057	0.00
White	0.87	0.88	-0.008	0.34
Married	0.55	0.66	-0.108	0.00
College	0.24	0.31	-0.069	0.00
Less HS	0.12	0.08	0.038	0.00
Age	38.69	42.03	-3.34	0.00
N	2387	3842		

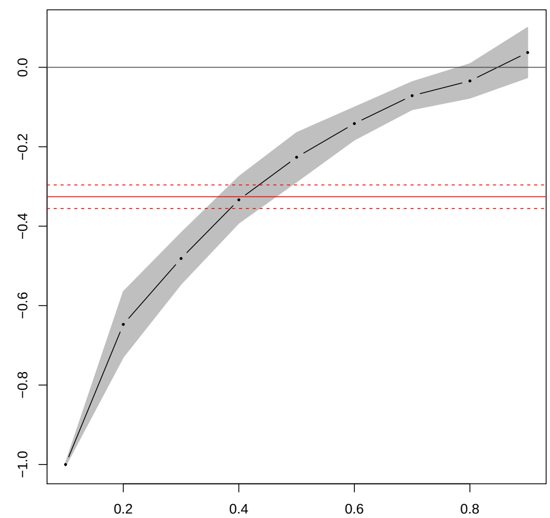
Sources: CPS and Displaced Workers Survey

Figure 2.10: 1997-1998 Quantile Regression Estimates of the Treatment Effect on Covariates



Notes: QR estimates of the effect of covariates on the quantiles of the treatment effect in 1997-1998. The solid horizontal line provides OLS estimates of the effect of each covariate, and the horizontal dashed line contains a 90% confidence interval. The dotted line provides QR estimates of the effect of each covariate at particular quantiles from 0.1, 0.2, . . . , 0.9. The shaded area contains pointwise 90% confidence intervals from the quantile regressions.

Figure 2.11: 1997-1998 Quantile Regression Estimates of the Treatment Effect on $Y_t(0)$



Notes: QR estimates of the effect of what earnings would have been in the absence of job displacement, $Y_t(0)$, on the quantiles of the treatment effect in 1997-1998. The solid horizontal line provides OLS estimates of the effect of non-displaced potential earnings, and the horizontal dashed line contains a 90% confidence interval. The dotted line provides QR estimates of the effect of non-displaced potential earnings at particular quantiles from 0.1, 0.2, . . . , 0.9. The shaded area contains pointwise 90% confidence intervals from the quantile regressions.

CHAPTER 3

THE HETEROGENEOUS EFFECTS OF HAVING CHILDREN ON WOMEN'S INCOME

3.1 Introduction

Prior work on gender inequality has mainly focused on the role of human capital accumulation and discrimination (Altonji and Blank 1999). Despite anti-discrimination policies and the disappearance of the education gap among men and women, gender inequality is still substantial which may be due to having children (Kleven, Landais, and Sogaard 2018). After childbirth, women usually experience an immediate drop in their earnings relative to what they would have earned if they had not become a mother. The gap closes somewhat over time though mothers never fully catch up to their counterfactuals. This paper studies the distributional effects of having children on women's income and shows how the motherhood income penalty varies across women with different observable characteristics and levels of income.

The knowledge of the heterogeneous effects of childbirth on mothers' income is important due to several reasons. First, most of the family policies (e.g., maternity leave, childcare assistance) are based on the average effect which may not be applicable in many cases. Having children which on average may affect the women's income negatively, may have no effects or even some beneficial effects on other mothers. Some women may return to full-time employment relatively quickly while they do not experience substantial income penalty, some may move to part-time employment or switch to more family-friendly jobs which usually have lower pay, and others may drop out of the labor force and experience larger drops in their wages. If career interruptions to take care of children lead to lower wages for some mothers, then perhaps policies like more generous maternity leave or childcare assistance to those who experience the largest wage penalty would reduce the income gap among mothers and childless women (Waldfogel 1998, Carrasco 2001). Second, focusing on the distribution of the motherhood penalty can help scholars and policy-makers with a better understanding of parents' decisions about the size of the family (Zhang 2009). Lastly, for noisy outcomes like earnings and income the distributional analysis is more suitable as it accounts for outliers.

Estimating the distributional effects of childbirth on mothers' income is challenging for two reasons. First, I need to compare the income distribution of mothers with their income distribution had they not had any children which is not observable. So, I need to define the counterfactual distribution of annual income for women with children. Following Athey and Imbens 2006, I use the Changes-in-Changes (CIC) analysis to estimate the counterfactual distribution of annual income for mothers from three known and observable potential outcome distributions; the potential outcome distribution of the treated group before treatment, and two potential outcome distributions of the untreated group before and after treatment. To estimate these three marginal distributions, I utilize the distribution regression method following the work of Chernozhukov, Fernandez-Val, and Melly 2013.

Identifying the counterfactual distribution is not enough for the distributional analysis of the motherhood penalty. The second main challenge is that my parameter of interest for capturing the heterogeneous effects of childbirth depends on the joint distribution of annual income for mothers and their counterfactuals. Therefore, additional information is required to associate the annual income of mothers with their counterfactuals. Women may keep or change their ranks in the income distribution after having children which are not observable even under standard identification assumptions like the selection on observables. If I assume that in the absence of children, women will possess the same ranks in the income distribution as their ranks before entering motherhood, then I can identify the joint distribution and consequently the distributional effects of child penalty on income. This is called the rank invariance assumption over time.

My work is conceptually related to Budig and Hodges 2010 in which they estimate the size of the wage disadvantages of mothers over the full distribution of earnings. They used the quantile regression with a fixed effects approach and concluded that high-earning women experience smaller penalties compared to low-earning while some factors like family resources (e.g., husbands' income) and work effort (e.g., number of annual weeks worked and weekly work hours) play an important role in explaining the earning losses of mothers. They showed most of the motherhood penalty for low-earning mothers could be explained by work effort. Low-paying jobs usually have limited benefits and flexibility, so women at these jobs are more likely to decrease their working hours or even quit their jobs to provide childcare. While for high-earning mothers losing job experience and human capital are the main factors that explain the wage reductions.

Working mothers may change their labor market behavior and make some adjustments in their jobs after having children. These career interruptions to take care of children usually lead to lower wages for mothers. Existing research provides several channels that can explain the lower wages and earnings for women with children. Kleven et al. 2019 argued that changes in the employment status, working hours, or wage rates are the three main channels that cause the earning penalty for both men and women with children. Some women may decide to leave the labor force after having a child. Others may reduce their working hours, which could disrupt their human capital accumulation and lead to a drop in their productivity. Finally, some may change their jobs to more family-friendly occupations that usually have lower wage rates.

Other mechanisms like the number of children in the household can explain the motherhood penalty as well. For example, Zhang 2009 estimated 9% wage gap among women with one child and childless women in Canada between 1993 and 2004, while this gap increased to 12% and 20% for women with two and three children respectively. Besides the number of children, the ages of children can also affect the earnings of working mothers. Some studies show that women with younger children experience a more substantial penalty compared to their counterfactuals. These penalties diminish somewhat as children become older. For example, Budig 2003 showed that preschool children increase the likelihood of mothers leaving the labor force while older children encourage women to enter full-time employment.

Looking at the distributional effect of having children on women's income, I find that around 90% of mothers have experienced lower income after having children and these effects are quite heterogeneous among mothers. For instance, at the 5th quantile, annual income of mothers are estimated to be \$29,261 lower than what they would have been without having children, while at the 95th quantile, annual income of mothers are estimated to be \$4,843 higher than their counterfactuals. To investigate how the income penalty varies across mothers with different observable characteristics, I regress the gap between the current income of mothers and their counterfactuals on a range of human capital and demographic characteristics. I find that white, married, older, and highly educated mothers with two or more children have substantially lower income after having children. The estimated sign of coefficients suggests that black mothers experience lower income penalty, if they bear any, compared to white mothers which is consistent with the existing literature. Married mothers may tend to have access to other family resources (e.g., husband's income) to share the direct care of children and be more likely to quit their jobs or switch to more family-friendly jobs, leading to higher penalties compared to those that never married. The income penalty is larger for high school graduates and college graduates while high school dropouts bear smaller penalty in their income, which may be related to lower human capital, compared to those with some college level of education.

I am also interested in considering the effects of the number of children and children aged less than six years old on the motherhood penalty. The employment status of women confirms the negative relationship between labor market participation and fertility. As the number of children increases, the share of employment falls among mothers. The linear regression analysis also provides evidence of larger income penalty for mothers with two or more children. I also find that existence of preschool children in the household positively affect the income of mothers. This may be due to the reason that mothers increase their labor supply to compensate the fall in the family income due to childcare costs. In another word, the income effect of having younger children outweighs the substitution effect.

The rest of this paper is organized as follows. In Section 2, I provide a review of the existing work on motherhood and the labor market. In Section 3, I propose the model and the estimators of marginal distributions and the parameters of interest for analyzing the heterogeneous effects of child penalty on women's income. Section 4 contains the data description and key variables. Section 5 concludes. The robustness checks are presented in section 6. Figures and tables are collected in the Appendix.

3.2 Motherhood and Labor Market

The role of discrimination against mothers is well documented among economics and sociology scholars. Changes in the working behavior and labor market participation of women in recent decades may suggest smaller wage gap among men and women, but existing work finds no significant decline in motherhood penalty over time (Avellar and Smock 2003). Working mothers may respond to childbirth differently. Several mechanisms may play a role in the reduction in their earnings; mothers may quit their jobs, change their employment status, lower their working hours, or even switch to more family-friendly occupations with more flexibility. Each of these factors can have heterogeneous effects on mothers with different levels of earnings and different observable characteristics.

The income penalty for mothers could result from a change in their employment status. Some mothers may drop out of the labor force or become part-time employees who usually have lower wage rates after having children. The amount of time that mothers stay out of the labor force also matters, and the income penalty increases if mothers have long-lasting interruptions in their careers, which can result in skill reduction. For example, Zhang 2009 estimated average hourly earnings gap of 30% at the age of 40 between women without children and mothers with more than three years of career interruption. Budig and Hodges 2010 investigated how family resources, time allocation to work, and switching to more family-friendly jobs among other factors can affect the motherhood penalty differently across the earnings distribution. For example, work effort (e.g., number of annual weeks worked and weekly work hours) accounts for one-third of the motherhood penalty for low-earner women and almost none for high-earners and job experience losses can explain a large portion of the wage reduction for high-earner women and almost none for low-earners.

Human capital accumulation of mothers may drop due to a reduction in working hours after childbirth. According to Becker 1985, household responsibilities and childcare reduce women's energy compared to other "nonmarket" uses of time by men. Therefore, women are expected to decrease their working hours which discourage them from investing in their human capital. Lundberg and Rose 2000 mainly focused on differences between the labor supply decision of married parents and married non-parents and their findings indicate that there is a reallocation of time and effort in households with children. Mothers who experience an interruption in the labor marker activities experience around 23% decline in their wages due to reductions in market time and effort in the form of reduced hours, experiences, and tenure.

The effects of education levels on motherhood penalty are not clear in the literature. Some studies found no evidence on the effects of education on the wage penalty (e.g., Budig, England, et al. 2001), while others found contradictory results. For example, Anderson, Binder, and Krause 2002 focused on the non-Hispanic white women between 14 and 44 years old in the 1968-88 National Longitudinal Survey of Labor Market and measured the total motherhood wage gap of 15% per child. They found the largest wage penalty among high school graduate and college graduate mothers while low skilled mothers experience no penalty in earnings due to lower human capital. Conversely, Todd 2001 studied whether the wage gaps between women with and without children vary with the levels of educational attainments in five industrialized countries, and found that the educational attainment, in Canada and the United States, helps with offsetting the wage penalty for women. Anderson, Binder, and Krause 2003 estimated, on average, a 10% maternal wage penalty and suggested that while the wage penalty for high school graduates with older children is around 4-6%, high school dropouts and college graduates do not experience any penalties. This can be related to the types of jobs that high school graduates hold which often require more presents in the office and have lower flexibility.

The potential endogeneity of fertility variables has been discussed in prior existing work[1]. One approach to overcome the endogeneity problem is to use natural experiments. For example, Rosenzweig and Wolpin 1980 used the multiple birth events in the first pregnancy to trace the impact of an unexpected extra child on the labor supply decisions of mothers. The problem with this approach is that the sample size is likely to be very small. If one assumes that the labor supply model is linear then the endogeneity that comes from time-invariant unobservables can be captured by using the fixed effects model, but for non-linear labor supply equations, other solutions are

[1]See more in Browning 1992, Stanca 2012, Kalwij 2000, Bernhardt 1993, Shapiro and Mott 1979, Carrasco 2001, Ahn and Mira 2002, among others.

proposed. The majority of prior studies have used the instrumental variables to take into account the endogeneity of fertility. Finding a valid instrument variable is difficult, and previously used IVs (e.g., sex mix of the first two children) can mainly provide insights on the local effect of having more children rather than the total effects of children and specifically the first child which limits the usefulness of such approaches for my analysis.

Browning 1992 provides a comprehensive summary of modeling choices for a number of prior literature that has allowed for the endogeneity of fertility. Different instruments have been used in these studies and the results vary considerably but most of them found that fertility either has no effect[2] or sometimes has small but significant positive effects[3] on female labor supply. In some studies allowing for endogeneity changes the direction of the effects of having more children on female labor market supply. For example, assuming fertility is exogenous, Iacovou 2001 found that having a third child is associated with a reduction in women's labor supply while with the endogeneity of fertility having a third child is associated with no effect or possibly positive effect on the labor market participation of women. This may be due to the reason that the income effect of having children outweighs the substitution effect[4].

Some existing work analyzes the motherhood penalty in other countries. Gangl and Ziefle 2009 compared the wage penalty in three advanced economies; Germany, Britain, and United States and estimated 10% to 18% wage penalty per child while these effects are larger for mothers in Germany compared to American and British mothers. They also showed that changing the employer and losing accumulated tenure after parental leave can increase the motherhood wage penalty. Molina and Montuenga 2009 studied the motherhood wage penalty for Spanish women between 1994 and 2001

[2] For example, see Cramer 1980.

[3] For example, see Cain and Dooley 1976.

[4] After a childbirth working mothers may increase their labor supply to compensate the fall in the family income due to childcare costs (i.e., income effect), or work less or even stop working to avoid the childcare costs (i.e., substitution effect).

and measured wage losses of 6%, 14%, and 15% for women with one, two, and three or more children. Kleven et al. 2019 used Danish administrative data from 1980 to 2013 to examine the maternal wage penalty persistence over time. They concluded that the motherhood penalty is about 20% in the long run and the wage gap between women with children and without children increases over time substantially.

3.3 Model and Estimation

I am interested in the heterogeneous effects of a binary treatment D, whether a woman has any children or not, on the outcome Y which is the annual income from wages and salary. The effect of having children on women's income may be quite heterogeneous across women. Having children which, on average, may affect the women's income negatively, may have no effects or even some beneficial effects on the income of other mothers. Some mothers may move back to full-time employment or even increase their working hours after having children, some may become part-time employees or switch to jobs with more flexibility, and others may leave the labor force.

The parameter of interest to estimate the distribution of the treatment effect is the Quantile of the Treatment Effect on the Treated ($QoTT$) which is obtained in two steps. First, I need to compare the outcome distribution of the treated group with their outcome distributions if they had not been treated (i.e., the counterfactual distribution). Second, I need to associate each treated potential outcomes with untreated potential outcomes for the treated group to identify the distributional treatment effect.

An individual is either treated (women with children) or untreated (childless women). So, each individual belongs to group $G \in \{0, 1\}$ and is observed in period $T \in \{0, 1\}$. I assume those who have their first children between 1986 and 2000[5] are

[5] Women are at the highest reproductive age (21 to 44 years old) between 1986 and 2000 in my sample. I assume age 44 is the latest age at which women have their first child (as only small fractions of women have their first child after that age in my sample).

treated and belong to $G = 1$ and all childless women belong to $G = 0$. The time of the treatment is defined based on the birth of the first child. $T = 1$ refers to the 2 years after the time of the birth of the first child and $T = 0$ refers to 2 years before the birth of the first child. The vector of covariates, X, includes age, three dummies for each marital status and race, and four dummies for education. Let Y^N be the untreated potential outcome and Y^I be the treated potential outcome. The observed outcome is defined as

$$Y = Y^N \cdot (1 - D) + Y^I \cdot D.$$

Most of the previous literature has focused on identifying the Average Treatment Effect (ATE) and Average Treatment Effect on the Treated (ATT) which are defined as

$$ATE = E[Y_t^I - Y_t^N],$$
$$ATT = E[Y_t^I - Y_t^N | D_t = 1].$$

In this paper, I focus on the distributional treatment effects on the treated rather than the average which is given by

$$F_{Y_t^I - Y_t^N | X, D_t = 1}(\delta | x) = P(Y_t^I - Y_t^N \leq \delta | X, D_t = 1) = E[1\{Y_t^I - Y_t^N \leq \delta | X, D_t = 1\}]. \quad (3.3.1)$$

Identifying the distribution of the treatment effect is challenging as it depends on the joint distribution of treated and untreated potential outcomes for the treated group which is not observable even under standard identification assumptions. In the following section I explain how to obtain the distribution of the effects of having children on women's income.

3.3.1 Model

In the first step, I estimate the counterfactual distribution of income for mothers using the Changes-in-Changes (CIC) method that was initially introduced by Athey and Imbens 2006 (AI hereafter) and then extended by Melly and Santangelo 2015 with adding covariates to the original model. See other methods in Melly 2006, Albrecht, Van Vuuren, and Vroman 2009, Chernozhukov, Fernandez-Val, and Melly 2013, Athey and Imbens 2006, Callaway and Li 2019, Callaway, Li, and Oka 2018. The CIC approach is designed for the case with two groups and two periods in which the counterfactual distribution of potential outcomes is obtained from three known and observed outcome distributions; the potential outcome distribution of treated group at period $T = 0$, and two potential outcome distributions of the untreated group at period $T = 0$ and $T = 1$. Following AI's work, I need to introduce a set of assumptions to identify the CIC model. For simplicity I use similar notations as AI. The shorthanded notations in this paper are as follow:

$$Y^N_{gtx} \overset{d}{\sim} Y^N|G = g, T = t, X = x, Y^I_{gtx} \overset{d}{\sim} Y^I|G = g, T = t, X = x,$$
$$Y_{gtx} \overset{d}{\sim} Y|G = g, T = t, X = x, U_{gt} \overset{d}{\sim} U|G = g, T = t.$$

where $\overset{d}{\sim}$ stands for "is distributed as." The corresponding conditional distribution functions are $F_{Y^N|gtx}, F_{Y^I|gtx}, F_{Y|gtx}, F_{U|gtx}$. AI analyzed sets of assumptions that identify the counterfactual distribution for the treated group. The three main assumptions are as below.

Assumption 1. (potential outcomes) The outcome for an individual in the absence of the treatment is defined as $Y^N = h(X, T, U)$.

Where $h(.)$ is a non-restricted function and U captures the unobservable variation in Y that can not be explained by X.

Assumption 2. (strict monotonicity) The production function $h(X, T, U)$ is strictly increasing in U for t $\in$ {0, 1} and $\forall x \in \mathcal{X}$.

Assumption 3. (time invariance) We have $U \perp T|G, X$

Assumption 1 implies that all the unobservables are captured by U and the random variable Y^N for an individual with $U = u$ is the same in a given time, and do not depend on the group indicator. Assumption 2 requires that higher U is associated with higher outcomes. Assumption 3 is the main assumption which implies that the conditional distributions of unobservables are the same over time within each group (treated and control). This assumption is called rank similarity by Chernozhukov and Hansen 2005 and is less restrictive than the rank invariance assumption in which the ranks are assumed to be identical in all treatment states.

Suppose that Assumptions 1-3 hold and $0 < \tau < 1$. Then $F_{Y^N_{11x}}$ is identified for $\forall x \in \mathcal{X}$ with;

$$F_{Y^N|11x}(y) = F_{Y|10x}(F^{-1}_{Y|00x}(F_{Y|01x}(y))). \tag{3.3.2}$$

The proof of Equation 3.3.2 is presented in Melly and Santangelo 2015.

Using Equation 3.3.2, I can identify the unobserved distribution of untreated potential outcomes for the treated group, $F_{Y^N|11x}$, with the knowledge of three observed distributions $F_{Y|10x}$, $F_{Y|00x}$, $F_{Y|01x}$. I use the distribution regression method by Chernozhukov, Fernandez-Val, and Melly 2013 to estimate two conditional marginal distributions of $F_{Y|10x}$ and $F_{Y|01x}$ and following Koenker and Bassett Jr 1978, I use quantile regression method to obtain the conditional quantile $F^{-1}_{Y|00x}$.

The distribution regression estimators for $F_{Y|10x}$ and $F_{Y|01x}$ are obtained by running the repeated binary choice model on a range of Y values,

$$F_{Y_{gt}|gtx}(y) = \Lambda(X'_{gt}\beta_{gt}(y)), \tag{3.3.3}$$

where $\Lambda(\cdot)$ is a logistic link function equals to $\Lambda(v) = \frac{exp(v)}{1+exp(v)}$, X' is a transformation vector of covariates, and $\beta(\cdot)$ is an unknown parameter that I am going to estimate.

Using the conditional linear quantile regression of Koenker and Bassett Jr 1978, I can obtain the quantile regression from known outcomes and get the $F^{-1}_{Y|00x}$.

$$Q_{Y|gtx}(\tau) = F^{-1}_{Y|gtx}(Y) = X'_{gt}\beta_{gt}(\tau). \tag{3.3.4}$$

The knowledge of marginal distributions of income and the counterfactual distribution of untreated potential outcomes for the treated group are not enough for distributional analysis. Estimating the distribution of the treatment effects is more challenging than the Average Treatment Effect as it depends on the joint distribution of treated and untreated potential outcomes. Women may keep or change their ranks in the income distribution after having children which is not observable even under standard identification assumptions like the selection on observables. Therefore, additional information is required to associate the income of mothers with their counterfactuals and compute the gap.

To point identify the distribution of the effects of having children on women's income, I use the rank invariance assumption over time. Intuitively, rank invariance over time means that if treated individuals with the same characteristics had not been treated, then they should have the same ranks in the potential outcome distribution as their ranks at the pre-treatment period. In other words, after controlling for a set of covariates, if mothers have not had any children then they would possess the same ranks in the income distribution as their ranks before entering motherhood.

Assumption 4. (Rank Invariance Over Time)

$$F_{Y^N|11x}(Y^N_{11x}) = F_{Y^I|10x}(Y^I_{10x})$$

$$\Rightarrow Y^N_{11x} = F^{-1}_{Y^N|11x}(F_{Y^I|10x}(Y^I_{10x})). \tag{3.3.5}$$

Under Assumption 4, I can associate the income of mothers (Y^I) with their counterfactuals (Y^N), obtain the pair of (Y^N, Y^I), and estimate the joint distribution of treated and untreated potential outcomes for mothers. The parameter of interest to measure the distributional effects of having children on women's annual income is the quantile of the difference between these two values. Define

$$QoTT(\tau) = F^{-1}_{Y^I - Y^N|11x}(\tau) = Q_{Y^I - Y^N|11x}(\tau),$$

$QoTT$ is useful to understand the heterogeneity of a treatment among individuals and is the main parameter of interest in this paper.

3.3.2 Estimation

In this section, I explain how to estimate the conditional distributions of annual income for mothers and their counterfactuals as well as the distributional effects of having children on women's income. To estimate the counterfactual distribution of income for mothers, I need to estimate $F_{Y|01x}$, $F^{-1}_{Y|00x}$, and $F_{Y|10x}$ respectively. In the first step, I apply the distribution regression method to estimate the conditional distribution of observed income for $G = 0$ at $T = 1$ such that,

$$\hat{F}_{Y|01x}(y) = \Lambda(X'_{gt}\hat{\beta}_{01}(y)). \tag{3.3.6}$$

After computing $\hat{F}_{Y_{01}|01x}(y)$, in the next step I estimate the quantiles of income for childless women, $G = 0$, at time $T = 0$ at particular values of $u = \hat{F}_{Y_{01}|01x}(y)$ such that,

$$F^{-1}_{Y|00x}(u) = \hat{Q}_{Y|00x}(u) = X'_{gt}\hat{\beta}_{00}(u). \tag{3.3.7}$$

In the final step, I use the distribution regression to estimate the conditional distribution of annual income for mothers, $G = 1$, at $T = 0$ and particular values of $\hat{F}^{-1}_{Y|00x}(u)$. So, the estimator for counterfactual distribution is defined by,

$$\hat{F}_{Y^N|11x}(y) = \hat{F}_{Y|10x}(\hat{F^{-1}}_{Y|00x}(\hat{F}_{Y|01x}(y))),$$

With the knowledge of $\hat{F}_{Y^N|11x}(y)$ and Equation 3.3.5, I can estimate the untreated potential outcomes for the mothers such that,

$$\hat{Y}^N_{11x} = \hat{F}^{-1}_{Y^N|11x}(\hat{F}_{Y^I|10x}(Y^I_{10x})).$$

Eventually, create the pair of $(\hat{Y}^I, \hat{Y}^N)$ and calculate the $QoTT$ by,

$$\widehat{QoTT}(\tau) = \hat{F^{-1}}_{Y^I - Y^N|11x}(\tau) = \hat{Q}_{Y^I - Y^N|11x}(\tau).$$

3.4 Data and Key Variables

Studying the impacts of having children on the annual income of mothers requires panel data with information on labor market outcomes and children. I pooled the 1979 to 2016 waves of the National Longitudinal Survey of Youth (NLSY), which is a national sample of 12,686 young men and women aged between 14 to 22 years old in 1979 (50% of the sample consists of women). Respondents were interviewed annually till 1998 and biannually thereafter. The NLSY is well suited for my analysis as it follows women up until their mid-50s. It includes details about the number of children, year of the first child birth, age of the respondent at the first pregnancy, annual income as well as a set of key variables that were collected repeatedly.

The dependent variable is the real income on the current job (in 2000 US dollars). The independent variables include dummy variables for age, education, marital status, and race. I include age to control for life-cycle trends and add education and race to capture other factors that affect human capital accumulation. I am also interested in considering the effects of the number of children in the household which is specified by a dummy variable that equals 1 if there are two or more children in the house. Two points are worth noting here. First, while my analysis is based on the impact of the first child, long-term studies will include the impact of other children as well. Second, I did not make any difference between biological, step, or adopted children in the data set as I do not have the information on that for some years[6].

Table 3.1 presents the summary statistics of the sample for women with and without children. There are some substantial differences between women without children and women with children in the sample. On average, women with children are about 10 years older. More than 70% of the sample of childless women is between 14 to 28 years while this rate is around 32% for mothers. Around 59% of women with children are married while this rate for childless women is around 24%. The share of childless women with at least twelve years of education is higher than mothers. Around 57% of mothers are employed while this rate is around 70% for childless women. The average annual income and hourly wages of mothers are about 24% lower than for childless women.

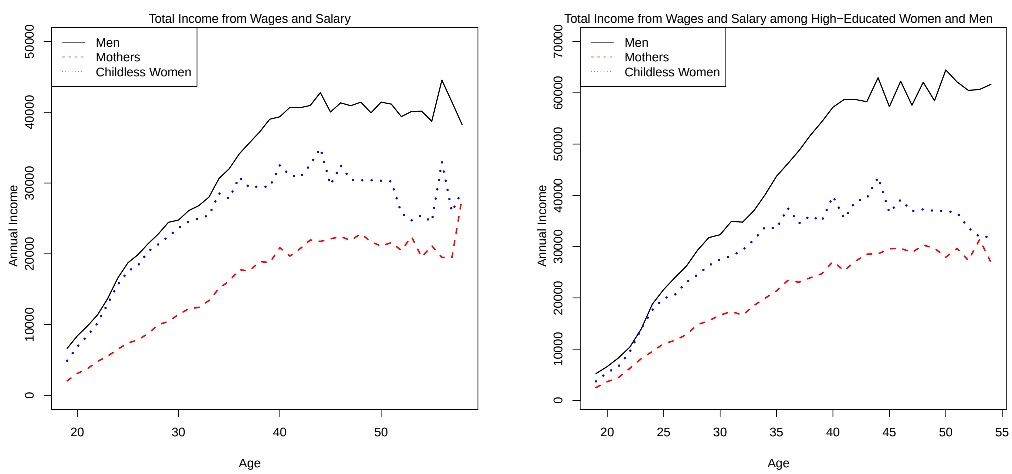


Figure 3.1: Annual Income among Mothers, Childless Women, and Men

Notes: The left panel plots the annual income over a range of age among all mothers, childless women, and men and the left pannel plots the annual income over ages among highly educated (more than 12 years of schooling) mothers, childless women, and men.

[6]The NLSY in 1979 till 1998 did not make any difference between biological children from step or adopted children.

The income gap between women with children and without children has been rising with age. The left panel in Figure 3.1 presents average annual income for mothers, childless women, and men aged 18 to 55 between 1979 and 2016. The graph shows that mothers earn less than childless women at almost all levels of age. For example, at the age of 25, the average annual income of women with children were \$7,432 while for childless women and men were \$17,189 and \$19,913, respectively (2000 dollars). The income gap between mothers and childless women shrinks at the higher levels of age while the gap between mothers and men opens up. I also present the annual income of the highly educated[7] mothers, childless women, and men over a range of age in the right panel of Figure 3.1. The graphs are similar to the left panel while the gap among highly educated men and mothers are wider than before.

Table 3.3 presents the labor market participation of women by the number of children. The employment status of mothers and childless women confirm the negative relationship between labor market participation and fertility. The shares of employment fall while the shares of those not in the labor force rise as the number of children increases. Around 70% of women with no children are employed and this share falls to 59.62% with the birth of the first child, to 59.53% with the second child, to 52.73% with the third child, and to 42.85% with the fourth and more child.

3.5 Results

Considering childless women as the control group, I present the estimated conditional distributions of income for mothers and their counterfactuals in Figure 3.2. The distribution of annual income for mothers is on the left side of the distribution of annual income for childless women, which presents the lower levels of income for mothers. There is also a mass point at zero earnings which shows a large group of women lose their jobs after having children.

[7] More than 12 years of schooling.

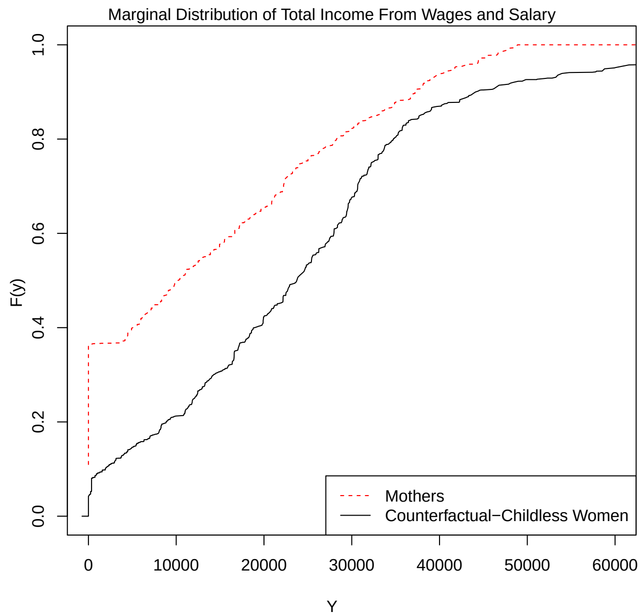


Figure 3.2: Marginal Distribution of Total Income From Wages and Salary

After obtaining the counterfactual income distribution for mothers, I can estimate the pair of (Y^N, Y^I) and compute the gap. Figure 3.3 plots the heterogeneous distributional effects of having children on mothers' income as it is measured by the quantile of the difference between Y^I and Y^N in this paper. The horizontal line represents the ATE. On average, women lose around \$11,695 of their annual income after having children but these effects are quite heterogeneous across women. Looking at the $QoTT$, around 90% of women experience lower levels of income after having children, while small percentage of mothers experience positive changes in their income. At the 5th quantile, mothers are estimated to lose around \$29,261 of their income. At the 50th quantile, median, the income of mothers drop by \$11,997 and at the 95th quantile, the women's incomes are \$4,843 higher after having children.

The first column of Table 3.2 shows how the effects of having children on annual income vary among mothers with different observable characteristics. The dependent variable in the first column of Table 3.2 is $(Y^I - Y^N)$. The standard errors of the

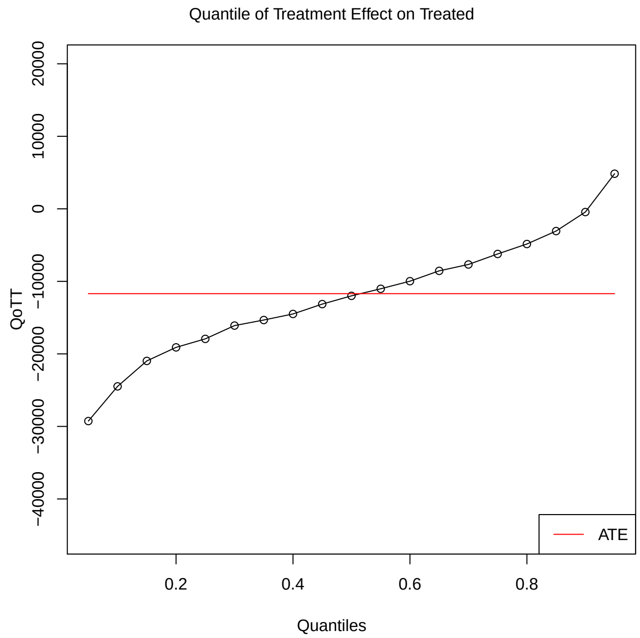


Figure 3.3: The Quantile of the Treatment Effect on the Treated Group

coefficients in Table 3.2 are adjusted for the first step of estimation of conditional marginal distributions by bootstrapping[8]. Older mothers experience a slightly smaller reduction in their annual income after having children compared to younger ones. White mothers are experiencing larger income penalty after having children compared to Hispanic mothers while there is smaller income penalty for blacks. Most of the prior studies (e.g., Waldfogel 1997, Anderson, Binder, and Krause 2003) reported smaller wage penalty for black mothers, if they bear any. The signs of coefficients on the race dummy variables suggest that white women may trade off higher paid jobs to take care of their children and experience larger penalties in their income.

Higher levels of education are associated with higher income penalty such that mothers with at least a college degree experience the largest drop in their income after having children. This is consistent with Anderson, Binder, and Krause 2003 work in which they also found higher wage penalty for high school graduates and college

[8]I estimate the standard errors by bootstrapping and sampling my data with replacement, each the same size as the original sample with 100 iterations.

graduates. Married mothers experience income penalty compared to single mothers. Married mothers may tend to have access to other family resources (e.g., husband's income) to share the cost of childcare and may be more likely to quit their jobs or switch to more family-friendly jobs, leading to income penalty in comparison to those that never married. The number of children in the household also affects the income penalty of mothers. Those with more than two children in the household experience more substantial drop in their income.

I also analyze the probability of labor force participation among mothers. The age of children is also included in the probability of labor force participation analysis which is specified by a dummy variable that equals 1 if the youngest child in the household is less than six years old. Table 3.4 presents the marginal effects of the linear probability model with different covariates. Higher levels of age are associated with lower probability of labor force participation. White mothers are around 3% less likely to participate in the labor force compared to Hispanic mothers. Divorced and married mothers are more likely to participate in the labor market than single mothers. Higher levels of education imply a higher probability of labor market participation such that high school drop-out mothers are around 20% less likely to participate. Those mothers with more than two children or younger children in the household have a lower probability of participation in the labor force.

Figure 3.4 shows the income impacts of the first child births among mothers, childless women, and fathers. The vertical line presents the event of the birth of the first child. To consider childless women in this graph, I need to assign the placebo births to them which are drawn from the observed conditional distribution of the age of pregnancy among mothers. The conditional distribution of the observed age at the first pregnancy among mothers is approximated by the distribution regression, and then I draw random pregnancy ages from the estimated conditional distribution for childless women. The gap between the income of men and women with children opens

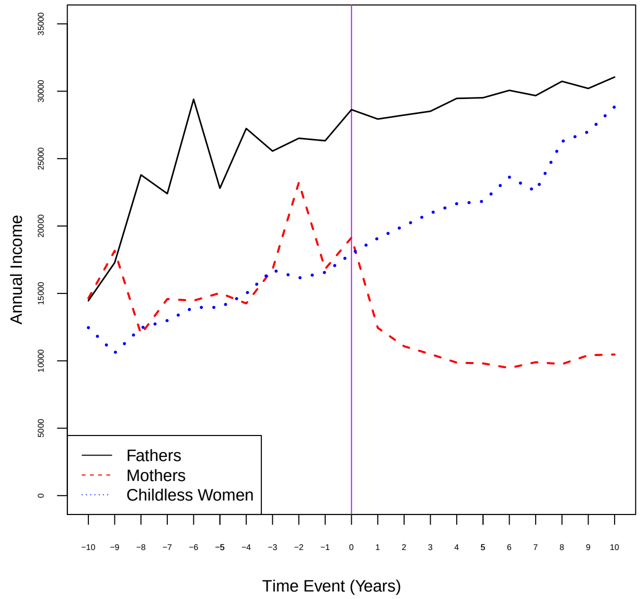


Figure 3.4: Impacts of First Child Birth on Annual Income

up after the first child birth, while the the gap between the income of fathers and childless women closes over time. The gap between income of fathers and mothers is pretty stable after the birth of the first child over time such that after ten years the average annual income of men with children is around \$31,000 while it is around \$10,500 for women with children. The graph of fathers shows that men's income is unaffected by the childbirth and has an upward trend over time. Mothers and childless women have almost similar levels of income before the first child birth, however, the gap among their income opens up after the first child birth. The graph of childless women shows the upward trend of income over time as well. Mothers experience the highest drop in their income one year after the first child birth and then their income become stable at lower levels of income afterwards.

Figure 3.5 investigates the effect of the first child birth on annual income among mothers with two or more children, mothers with one child, and childless women. Mothers with two or more children experience a larger drop in their income compared

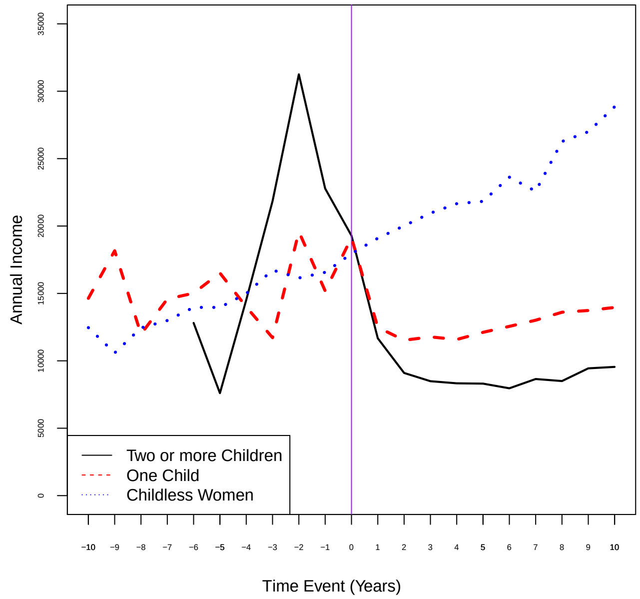


Figure 3.5: Impacts of First Child Birth on Annual Income Among Mothers and Childless Women

to women with one child. This is consistent with existing literature and the regression analysis in this paper that also find larger income penalty for mothers with two or more children.

The trends of residuals from regressing the annual income over a set of covariates (i.e., age, age squared, age cube, and dummy variables for education, race, and marital status) for men and women with children and childless women against the time event are presented in the Figure 3.6. I exclude those individuals with zero earnings in the right panel. Looking at the graph of mothers, some part of the effects disappears after taking out zeros, which is related to changes in the employment status before having children. Some women may decide to leave the labor force a few years before having children. Some parts of the earning losses of mothers can also be explained by changes in the occupation/industry that happened before having children. Mothers usually switch to more family-friendly jobs with flexible working hours before the first childbirth.

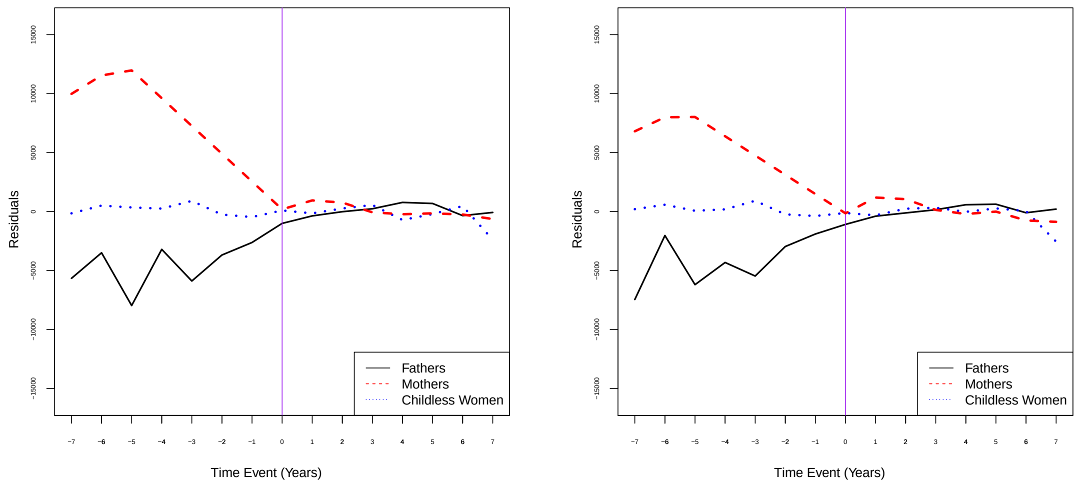


Figure 3.6: Residual Trends among Fathers, Mothers, and Childless Women

Notes: The residuals are obtained from regressing annual income over a set of covariates (i.e., age, age squared, age cube, and dummy variables for education, race, and marital status). Individuals with zero earnings are included in the left panel and excluded in the right panel.

3.6 Extensions

3.6.1 Alternative Dependent Variables

I repeat my analysis by looking at the changes in hourly wages and daily working hours of mothers after having children. I top-code the adjusted hourly wages (2000 dollars) at $150 and daily working hours at 10 hours. The left panel of Figure 3.7 shows the trend of hourly wages over the range of ages among men, mothers, and childless women. The gap between hourly wages of mothers and childless women are small, however it opens up at some point and closes again. The right panel plots the daily working hours over the range of ages among men, mothers, and childless women. Childless women and fathers have similar trends, such that daily hourly wages slightly start to decline at their mid-40s as they are getting closer to their retirements. Mothers have lower daily working hours compared to men and childless women, however their working hours rises with age. This can be related to the career interruption that mothers experience after having children which delays mothers' exits from the labor force[9]

The estimated conditional distributions of hourly wages and working hours for mothers and their counterfactuals are presented in Figure 3.8. The actual and counterfactual conditional distributions of hourly wages are plotted in the left panel. The graph shows a small gap between the actual and counterfactuals conditional distribution of hourly wages for mothers. The conditional distributions of daily working hours are shown in the right panel. The mother's actual conditional distribution of working hours is at the left side of their counterfactuals which shows a substantial lower working hours for mothers. The gap between the actual conditional distributions of mothers' working hours and their counterfactuals is larger at the lower levels of working hours and closes up at the higher levels.

The left panel of Figure 3.9 plots the distributional treatment effects of having children on women's hourly wages. Around 30% of mothers experience a raise in their hourly wages after having children while others experience negative changes. The horizontal line represents the average treatment effect of having children on women's hourly wages. On average, women lose around \$0.97 of their hourly wages after having children. The distributional effects of having children on hourly wages does not show that much of the heterogeneity among mothers. At the 5th quantile, women are estimated to lose around \$10.66 of their hourly wages after having children while at the 95th quantile, women are estimated to experience \$7.55 increase in their hourly wages.

The right panel of Figure 3.9 plots the distributional treatment effects of having children on women's daily working hours. On average, the daily working hours of mothers drop by 3.06 hours after having children. However, these changes are not the

[9] Hank 2004 studied the effects of having children on women's late life labor market behavior in Germany from 1984 through 2000. His findings showed that having children postpone the women's exits from the labor market and the effects are stronger for those who experience first child birth relatively late.

same for everyone. Looking at the $QoTT$, around 10% of women either increase or do not change their daily working hours while others work less. At the 5th quantile, mothers are estimated to work 7.4 hours less than their daily working hours if they have not had any children. This is a large drop which can be related to those working women who become unemployed after having children. At the 95th quantile, women increase their daily working hours by 0.53 hours after having children. The distributional treatment effects analysis of hourly wages and daily working hours suggest that the changes in the income of mothers are mainly due to changes in the daily working hours rather than changes in hourly wages.

The results of the linear regression analysis are presented in the second and third columns of Table 3.2. White women experience lower hourly wage and working hours after having children compared to Hispanic mothers. Married women experience no hourly wages penalty and lower working hours after entering motherhood in comparison to single mothers. Married mothers are more likely to become part-time employee after having children since they have access to other family resources to share the childcare costs. The less educated women face no penalty in their hourly wages and large drop in their working hours. The less educated mothers are more likely to be employed at low-paying jobs which have no benefits and lower flexibility so they are more likely to quit their jobs after having children. Mothers with at lease a college degree experience larger drop in their hourly wages than those who are not college graduates, since they are more likely to lose high paying jobs after having children. The hourly wages and working hours of mothers with two or more children drop compared to those with one child. Women with more than two children are more likely to switch to jobs with more flexibility and lower wage rates. Mothers with younger children in the households

I also look at the trends of hourly wages and daily working hours 10 years before and 10 years after the first child birth. The left panel in Figure 3.10 shows that the hourly wages of mothers start to drop a year prior to the first child birth and continue to fall up until 5 years after and then stay steady. Childless women and fathers overall experience an upward trend in their hourly wages. The right panel of Figure 3.10 shows that the working hours of fathers and mothers have similar trend before while mothers' working hours start to fall two years prior to the first child birth and become steady after that.

3.7 Conclusion

The distributional effect of a policy is more helpful to policy-makers than the average treatment effect. A treatment which is beneficial on average may have adverse effects on some individuals. This paper studies the heterogeneous effects of having children on women's income using the National Longitudinal Survey of Youth for the period of 1979 to 2016. I estimate the entire distribution of the child penalty by the Quantile of the Treatment Effect on the Treated group ($QoTT$). To obtain the $QoTT$, I use the Changes-in-Changes method to identify the conditional distribution of income for mothers if they had not had any children. I find that the effects of motherhood on women's income are heterogeneous. Around 90% of mothers experience income penalty after having children while the rest of mothers experience positive effects. I also show that child penalty varies among mothers with different observable characteristics. White, married, older, and highly educated mothers with two or more children experience a substantial drop in their income.

3.8 Tables and Figures

Tables

Table 3.1: Summary Statistics

Demographics	Women with Children	Women without Children
Age		
14 to 28 years old	31.52%	72.40%
29 to 43 years old	42.96%	20.29%
44 to 60 years old	25.52%	7.31%
Race		
White	57.99%	61.77%
Black	25.53%	23.14%
Hispanic	16.48%	15.09%
Marital Status		
Married	58.63%	24.17%
Never Married	16.84%	66.26%
Divorced/Widowed	24.53%	9.57%
Education Level		
Less than high school	17.18%	19.43%
High school	46.41%	33.97%
Some college degree	22.82%	26.23%
At Least a College degree	13.60%	20.37%
Employment		
Employed	57.30%	69.33%
Unemployed	8.06%	9.56%
Not in the labor force	34.64%	21.11%
Annual Income	$13,413	$16,583
Hourly Wages	$13.46	$16.71
Daily Working Hours	5.63	6.34
Observations	117,353	50,304

Source: The National Longitudinal Survey of Youth 1979-2016.

Table 3.2: Summary of Results

Covariates	Dependent Variables		
	$Y_{11}^I - Y_{11}^N$	$W_{11}^I - W_{11}^N$	$Hr_{11}^I - Hr_{11}^N$
Age	281.2	0.161	−0.051
	(363.6)	(0.26)	(0.04)
Black	1,210.3	−0.546	0.213
	(2143.1)	(1.41)	(0.58)
White	−3,742.4**	−1.63	−1.350**
	(1855.3)	(1.27)	(0.39)
Divorced/Separated/Widowed	727.7	1.738	0.251
	(2441.6)	(0.99)	(0.7)
Married	−2,498.5	1.220	−0.423
	(1872.4)	(0.71)	(0.53)
Less than High School	−880.5	0.986	−2.333**
	(1906.5)	(1.59)	(0.89)
High School	−2,799.6**	−0.035	−0.737
	(1301.1)	(0.71)	(0.39)
At Least A College Degree	−2,918.2	−0.470	−0.262
	(3485.7)	(2.35)	(0.4)
More than Two Children in the Household	−2,507.3 **	−2.112	−1.083**
	(1005.9)	(1.13)	(0.26)
Constant	−12,531.16	−5.336	0.357
	(10394.2)	(7.69)	(1.32)
Observations	984	778	942
R^2	0.06	0.02	0.14
Adjusted R^2	0.05	0.004	0.14
Residual Std. Error	13,972.590 (df = 974)	9.853 (df = 768)	2.567 (df = 932)
F Statistic	6.421**(df = 9; 974)	1.313 (df = 9; 768)	17.360* (df = 9; 932)

Notes: The dependent variable is changes in the annual income in the first column, changes in hourly wages in the second column, and changes in daily working hours in the third column. The excluded group is not-married, Hispanic mothers with some college levels of education. Bootstrapped standard errors are in parentheses. Significance levels: *p<0.1, **p<0.05

Table 3.3: Labor Market Participation by Number of Children: All Women

Number of Children	0	1	2	3	4 or more
Employed (Full-time/Part-time)	69.33%	59.62%	59.53%	52.73%	42.85%
Unemployed	9.56%	9.40%	6.94%	7.40%	8.03%
Not in the Labor Force	21.11%	30.98%	33.52%	39.87%	49.11%

Notes: Source: The National Longitudinal Survey of Youth 1979-2016.

Table 3.4: Probability of the Labor force Participation: Women with Children

Covariates	Coefficients
Age	−0.0272*
	(0.0005)
Black	0.0218*
	(0.0101)
White	−0.0317*
	(0.009)
Divorced/Separated/Widowed	0.065*
	(0.013)
Married	0.032*
	(0.012)
Less than High School	−0.208*
	(0.017)
High School	-0.045*
	(0.008)
At Least a College Degree	0.0112
	(0.008)
More than two Children in the Household	-0.061*
	(0.006)
Youngest Child Under Six Years Old	-0.028*
	(0.009)

Notes: The dependent variable is a dummy variable for the labor force participation among women with children. The excluded group is single, Hispanic mothers with some college level of education. Standard errors are in parentheses. Significance levels: *p<0.05.

Additional Figures

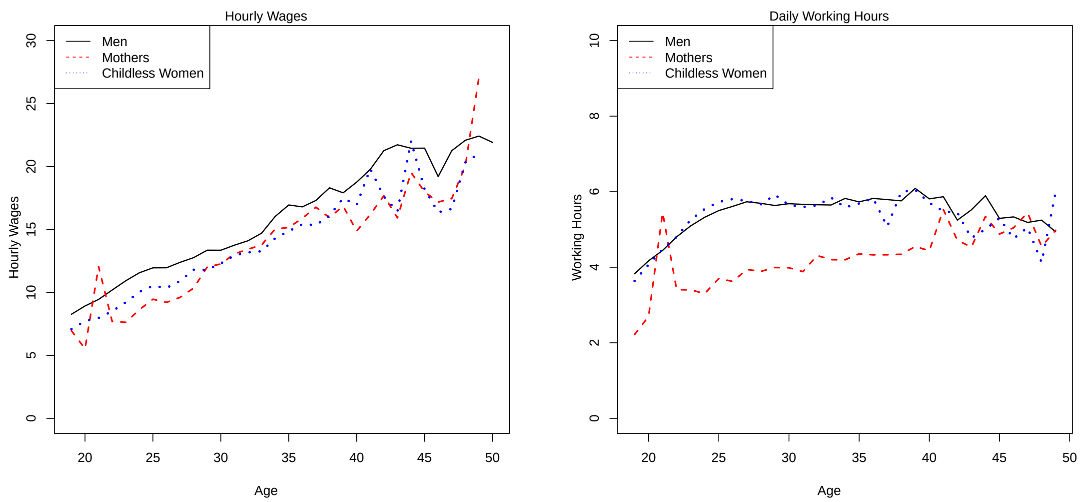


Figure 3.7: Hourly Wages and Daily Working Hours among Mothers, Childless Women, and Men

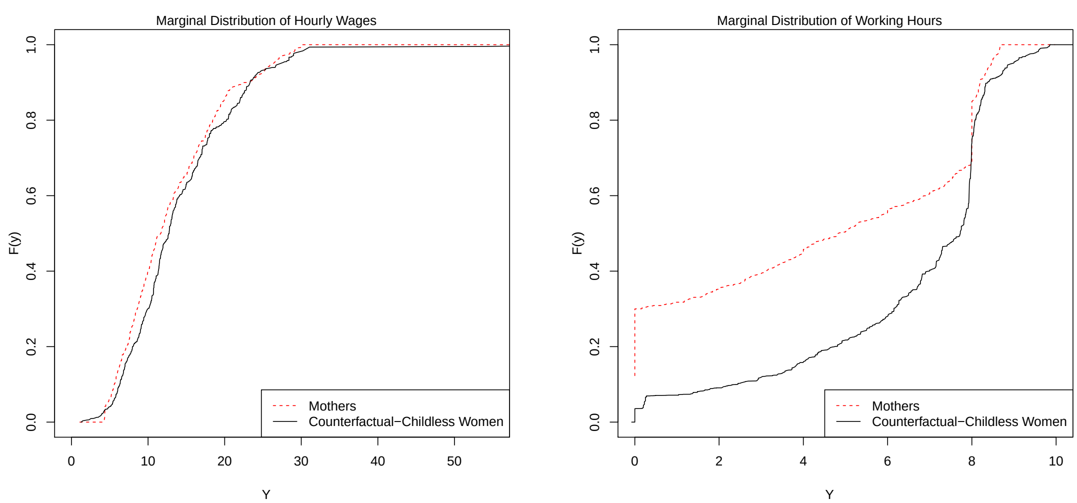


Figure 3.8: Conditional Marginal Distributions

Notes: The left panel plots the conditional distribution of hourly wages and the right panel plots the conditional distribution of daily working hours for mothers and their counterfactuals.

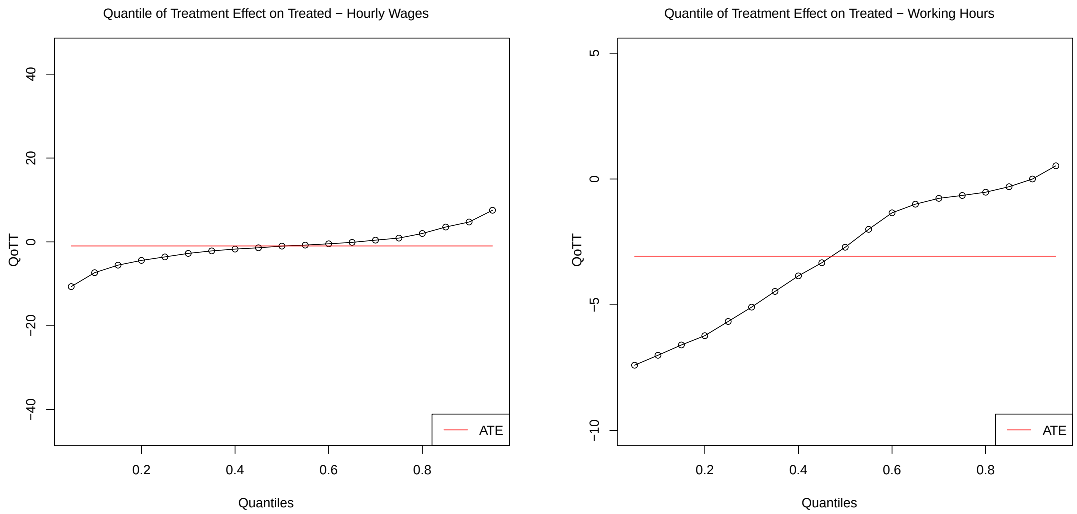


Figure 3.9: The Quantile of the Treatment Effect on the Treated - Hourly Wages and Daily Working Hours

Notes: The left panel plots the $QoTT$ when the hourly wages is the dependent variable and the right panel plots the $QoTT$ when the daily working hours is the dependent variable.

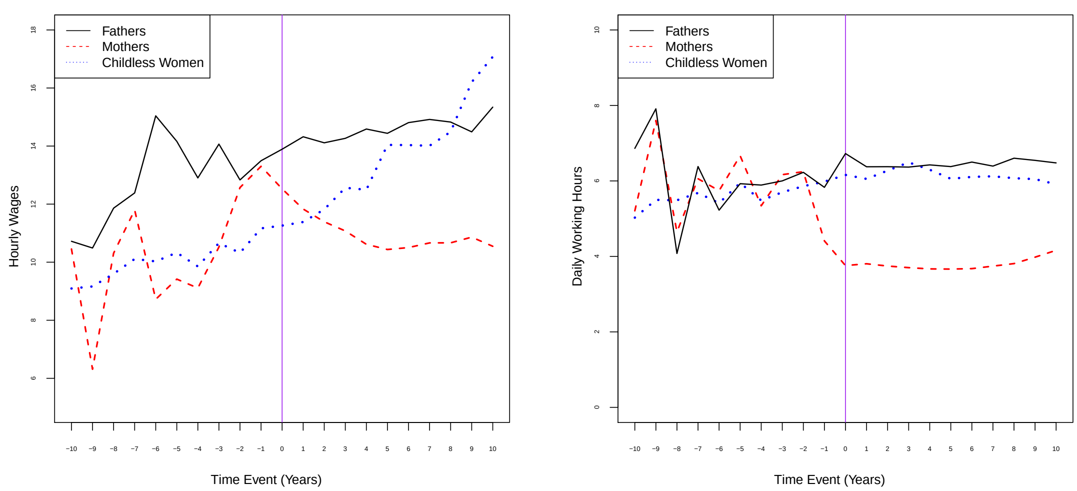


Figure 3.10: Impacts of the First Child Birth on Hourly Wages and Daily Working Hours

Notes: The left panel presents the trend of hourly wages ten years before and after the first child birth among men and women with children and childless women. The right panel presents the trend of daily working hours ten years before and after the first child birth among men and women with children and childless women.

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