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Job Loss: Consequences and Labor Market Policy

Jonas Cederlöf

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UPPSALA
UNIVERSITET

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Abstract
Cederlöf, J. 2020. Job Loss: Consequences and Labor Market Policy. *Economic studies* 184. 213 pp. Uppsala: Department of Economics, Uppsala University. ISBN 978-91-506-2818-0.

Essay I: This paper takes a novel approach to estimating the effects of involuntary job loss on future earnings, wages and employment. Whereas the previous literature has relied on mass layoffs and plant closures for exogenous variation in displacement, I use the fact that who is laid off is often determined by a seniority rule, specifically the last-in-first-out (LIFO) rule. This feature enables me to study also smaller sized layoffs affecting a broader set of workers. Using matched employer-employee data from Sweden, in combination with detailed individual-level data on layoff notifications, I rank workers according to relative seniority and identify establishment/occupation specific discontinuities in the probability of displacement which I exploit in a regression discontinuity framework. I find that displaced workers on average suffer large initial earnings losses of about 38 percent, but in contrast to previous studies, earnings recover fully within 7 years. I then exploit the heterogeneity across layoffs to examine when, and under what circumstances, the cost of displacement are most persistent. I show that persistent earnings losses are mainly associated with very large layoff events and that a substantive share of these losses are attributable to general equilibrium effects.

Essay II: Layoff rules are often criticized for creating an inefficient allocation of labor. However, such rules also provide insurance for workers. This paper examines the effects of advance notice of job loss for workers. Empirically, we use unique administrative data from Sweden on the exact dates of layoff notification as well as contracted notice periods, all at the individual level. Discontinuities in notificationtimes generated by collective bargaining agreements provide exogenous variation. Our regression-discontinuity estimates indicate that longer notice periods reduce the probability of non-employment and increase annual earnings during the first year after layoff notification. Workers who get longer notification periods experience smaller falls in their reemployment wages. We also show that firms make – and workers accept – severance payments in order to reduce the notice period. Workerswho are eligible for higher UI get lower severance payments.

Essay III: This paper studies which features of a caseworker that are important for job seeker outcomes, caseworker value-added and to what extent job seeker-caseworker matching matter. To break non-random sorting of job seekers to caseworkers we exploit that many local employment offices in Sweden assign job seekers to caseworkers based on date-of-birth. This as-if random allocation is coupled with detailed data on caseworkers. Our findings shows that female caseworkers perform better than male caseworker, in particular when they are paired with female job seekers. We also see that caseworkers with higher wages perform better. Many other observed caseworker characteristics, such as cognitive ability, personal experience of unemployment and educational background, are not related to caseworker performance. Based on the actions taken by the caseworkers, we find that caseworkers who have a preference for meetings are more successful. We also find that caseworkers who share the same labor market experience or educational level as the job seeker are more successful in mediating jobs to the unemployed. Finally, we document large and important differences in overall caseworker value-added.

Essay IV: Previous studies estimating the effect of generosity of unemployment insurance (UI) on unemployment duration has found that as job-seekers approach benefit exhaustion the probability of leaving unemployment increases sharply. Such "spikes" in the hazard rate has generally been interpreted as job-seekers timing their employment to coincide with benefit exhaustion. Card, Chetty and Weber (2007b) argue that such spikes rather reflect flight out of the labor force as benefits run out. This paper revisits this debate by studying a 30 week UI benefit extension in Sweden and its effects on unemployment duration, duration on UI, as well as the timing of employment. As the UI extension is predicated upon a job-seeker having a child below the age of 18 at the time of regular UI exhaustion this provides quasi-experimental variation which I exploit using a regression discontinuity design. I find that although increasing potential UI duration by 30 weeks increases actual take up by about 2.7 weeks, overall duration in unemployment and the probability of employment is largely unaffected. Moreover, I find no evidence of job-seekers manipulating the hazard to employment such that it coincides with UI benefit exhaustion. This result is attributed to generous replacement rates offered in other assistance programs available to job seekers who exhaust their benefits.

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To Ingela Cederlöf
1955 – 2012

Acknowledgments

At the time of writing these acknowledgments, I am a firm believer in the lack of free will. To me, it is without a doubt so that once actions and perceived choices are preceded by a chain of events which all led up to the end result. Albeit that we most of the time are unable to make sense of and pinpoint the long road that made us take the actions we did.[1] In rare cases, however, one may be able to pin down integral parts of what preceded ones 'choices'. Me starting and now finishing a PhD in economics is, I believe, one of these rare cases.

During my bachelor, I had been awarded a scholarship to study at Oklahoma State University for a semester. However, as my mother (to whom this thesis is dedicated) was diagnosed with cancer, I ended up staying in Sweden and instead taking a course in empirical methods in economics. This was a game changer and I remember thinking to myself; so this is how research is supposed to be done! Although, the professor on numerous occasions during the lectures interrupted himself saying "Jonas, you look puzzled?!", the material was presented in an inspiring, understandable and relatable way. Had it not been for this professor, I strongly doubt this thesis would have ever existed. The professor was Peter Fredriksson who later, thankfully, accepted the request to be my main supervisor during my PhD.

I'm am greatly indebted to Peter for his guidance during the process of writing this thesis. While others (myself included) tend to get bog-

[1] Digression: The absence of free is best illustrated with asking oneself: If I was able to go back in time where every circumstance, every particle in the universe, everything there is, was identical to how it was before: could you have made a different 'choice' or taken a different action? To put it in the jargon of an econometrican, is the potential outcome that did not occur even defined? The obvious answer to me is a blatant no, and if you reading these acknowledgments happen to believe otherwise come talk to me. I will even buy the beers. Anyhow, the absence of free will may imply that the world is deterministic, i.e that the universe is indeed pre-determined. Although, determinism is only a sufficient (albeit not necessary) condition for the absence of free will. If so, the whole concept of randomness goes out the window which in turn would invalidate most of the casual claims made in this thesis as they rely on variation mimicking randomness. However, after much pondering, I believe that the casual claims can be rationalized even thou the contra factual state may be undefined. I argue that one may still make casual claims based on an argument of orthogonality. Thus, I apologize for the use of the word random in this thesis and instruct supporters of determinism to read randomness as exogenous or orthogonal to all other heterogeneity directly influencing the outcome.

gled down in details and fail to see the forest for the trees, Peter has the amazing ability to find practical solutions to complex problems. It also never ceases to amaze me how broad, and yet still detailed, knowledge he possesses in economics. Peter has been extremely generous with his time, patiently listening and giving feedback to both good and not so good ideas. Furthermore, he has always reviewed my material extremely thoroughly. Even in the most sloppily crafted research proposals, he cannot help himself from correcting typos in footnotes. On a more personal level, Peter has always treated me (and certainly others) with the utmost respect and in our joint project he has made me feel like a co-author and a valuable member of the team whose ideas and insights matter. Even thou this surely is second nature to him, it meant a lot to me. A great deal of what I know about writing, executing and thinking about research in economics I owe to you Peter. Thank you!

I would also like to extend my gratitude to David Seim who acted as my co-supervisor. First off, at least two of these chapters would not have been written without David gracefully providing accesses to his data. David has pushed me to work hard and towards becoming a better researcher through his support and advice in e.g. the art of data handling. My co-author, Arash Nekeoi also deserves a thank you. Although we have had our differences, I have learned a lot about the virtue of being really thorough when it comes to all matters of research. A special thank you also to my co-authors Martin Söderström and Johan Vikström for reminding me when I needed it the most how rewarding research can be when you work with fun and intelligent people.

Speaking of fun and intelligent people, there is one of my fellow peers who deserves an honorable mentioning and that is Niklas Blomqvist. We have followed each other way back since we started our bachelor studies where we both were more interested in political science but gradually started leaning towards economics. I cannot have asked for better company along the way. Not only have I benefited tremendously from Niklas interest and profound knowledge in economics and politics, but I have also been granted the pleasure of enjoying one of the weirdest (yet somehow also funniest) sense of humor. Thank you for your friendship and for all the interesting discussions relating to economics, econometrics and all what else there is.

I have had the great privilege of getting to know many new friends during my PhD studies. Before transferring to Uppsala University, I spent several years at Stockholm University. First and foremost, I want to thank again Niklas Blomqvist but also Kasper Kragh-Søresensen and Fredrik Paues who helped me through the first year. Even though it is a cliché it is equally true that I would not have been able to manage it without you. Thank you also to my fellow 'cohorters' Markus Karlman, Roza Khoban, Karin Kinnerud, Erik Lindgren and Louise Lorentzon. To Jon Olofsson, sorry for slowing you down with my endless chatter. Thank you also to Danny Kessel and my fellow Stata nerd Elisabet Olme for great amounts of laughter, inspiration and fruitful discussions on how to write Stata code in the neatest and nicest way.

The final two years of my PhD I spent at Uppsala University. I remember being, and still am, amazed how welcoming everyone were. A great thank you to everyone and in particular Daniel Bougt, Sebastian Jävervall, Lilit Ottosson, Mohammad Sepahvand, Arnaldur Stefansson, Anna Thoresson, Lucas Tilly for your kindness and friendship and Sofia Hernäs with whom I also had the great pleasure of sharing an office with during the last year. I also had the great benefit of going on the European Job Market for Economists with Dagmar Müller, Maria Olsson, André Reslow and Tamas Vasi. You guys made the trip and the experience much more fun and rewarding than what I believe would have otherwise been the case. My stay at Uppsala University would not have been as complete as it now was without Mathias von Buxhoeveden. I had the pleasure of getting to know Mathias during my visit at University of California Berkeley where we accidentally bumped into each other. Thank you Mathias for all the interesting conversations and for being so keen on drinking beer and whiskey at Tupper&Reed.

Several members of faculty at the Department of Economics at Uppsala University, and elsewhere, have also contributed to this thesis. In particular, I want to thank Erik Öberg who showed genuine interest in my work and whose comments and suggestions improved the first chapter of this thesis greatly. I have also enjoyed and greatly benefited from my to/from/on/off train conversations with Björn Öckert where several ideas and thoughts about econometrics and identification have come in handy not only in these chapters but in research in general.

Last but not least, a tremendous thank you to my fiancée Madelene for the emotional support and putting up with my, to put it mildly, shortcomings. You have encouraged and enabled me to work hard and to seize opportunities that I might have otherwise dodged from. Most of all, you are a fantastic mother to our son Noah whom I also should thank for dragging me up at 5AM to go to work early during the last year of my PhD. I love you both!

Contents

Introduction

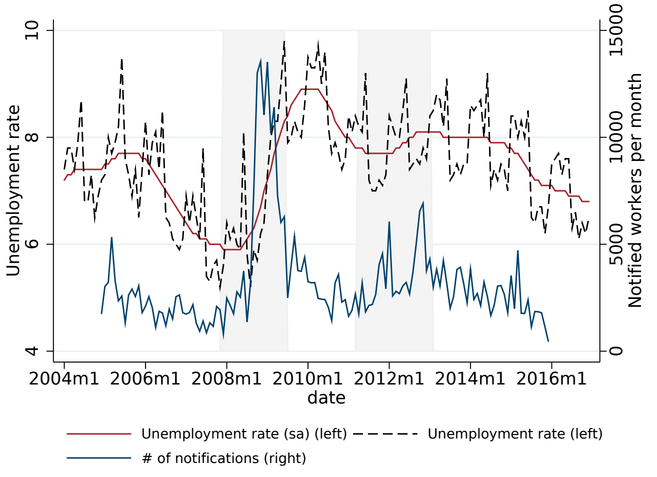
Early 2008, the Great Recession had began affecting the United States economy. During a couple of months, unemployment rates rose rapidly and had eventually doubled, reaching its peak at 10 percent in October 2009. Following the Lehman Brothers collapse in September 2008 the, until then, fairly local recession had turned into a global economic crisis. As seen in Figure 1, the Swedish economy was not exempted but unemployment rates rose with about 50 percent and nearly 200,000 workers were notified of their displacement within a year.

Becoming displaced can have detrimental effects on individual workers. Not only could displacement be a traumatic event in and by itself but it has been shown to lead to significant and even permanent losses in terms of future earnings, wages and employment (see Davis and von Wachter, 2011, for a summary of the litterature). Moreover, becoming unemployed through displacement may also negatively affect individuals health and well-being (Kuhn, Lalive and Zweimüller, 2009). Some evidence even suggest that getting displaced from a long-term job increases mortality and may reduce life-expectancy by up to 1.5 years (Eliason and Storrie, 2009, Sullivan and von Wachter, 2009). As if this was not enough, the negative consequences of unemployment also have the potential to be transmitted to younger generations as some evidence indicate that children of displaced workers perform worse in school and experience worse physical and mental health, particularly among children in low–socioeconomic status families (Stevens and Schaller, 2011, Schaller and Stevens, 2015).[1]

The large and potentially permanent negative consequences from job loss obviously puts a massive strain on public expenditures in terms of providing unemployment benefits, health care and social assistance. Particularly if the existence of welfare cultures are present. For example,

[1]There are some mixed evidence on the effects on health effects of displaced workers and the health and school performance of their children which appear to vary by the country of study.

Figure 1. Unemployment and layoff notifications in Sweden



Notes: The figure show the seasonally adjusted (red line) and unadjusted (black line) monthly unemployment rate in Sweden 2004:1–2016:12 which is read on the left axis. The blue line (read on the right axis) show the number of advance layoff notifications in a given month between 2005:1–2015:12.
Source: Statistics Sweden and Swedish Public Employment Services

Dahl, Kostøl and Mogstad (2014) show quite convincingly that children of parents having been granted social insurance – *ceteris paribus* – increase their likelihood of themselves participating in similar insurance schemes as adults. Not surprisingly then, governments around the world struggle with getting individuals back to work by various policy measures aimed towards lowering unemployment duration, increasing job-finding rates and labor force participation. This in order to uphold tax revenues to be able to provide the fundamental services of a state. While some level of unemployment will be natural in an imperfect market with frictions, the level is a function of what is done to aid and incentivize individuals to find new jobs and prevent them from becoming long-term unemployed.

This thesis consists of four self-contained chapters all addressing questions related to job loss, subsequent unemployment and labor supply. Specifically, I study the consequences of displacement for individual workers and what kind of incentive schemes and policy measures can be used to improve their subsequent outcomes and ease transition into new employment. While all chapters empirically studies workers in Sweden, I believe that the policies, incentives and the mechanisms which I seek do describe, can be thought of as being applicable to a broader more general context and setting.

The first chapter is titled **Saved by Seniority - The Effects of Displacement for Workers at the Margin of Layoff** and studies the short and long-run consequences of involuntary job loss for workers. This is done using variation spurred out of Swedish labor law which generates discontinuities in the likelihood of layoff whereby I can estimate the casual effect of job loss on future earnings, employment and wages. Previous literature studying this question has found large and permanent negative effects on future earnings and wages, but these estimates pertain primarily to high tenured workers laid off due to mass layoff or plant closures. I show in this chapter that when one studies less drastic and more regular adjustments to employment, the consequences of job loss are less severe and earnings losses appear to be transitory rather than persistent. Nevertheless, when focusing on large layoffs, I am able to replicate the standard fining or persistent earnings losses. I continue to show that these permanent losses can to a large extent be attributed to general equilibrium effects. When layoffs are large in relation to the local labor market, a large portion of workers having the same skill, experience and networks search for the same type of jobs which causes labor congestion on local labor markets rendering income losses to become more persistent.

The second chapter is written together with Peter Fredriksson, David Seim and Arash Nekoei and is titled **How Does Advance Layoff Notice Affect the Labor Market Prospects of Workers?**. In this chapter we characterize how workers adjust when facing job loss and investigate how this process is affected by a workers notification time. We use rich administrative data on layoff notifications coupled with quasi experimental variation generated by collective bargaining agreements stating that workers above the age of 55 (at notification) get longer notice periods. Using a regression discontinuity design we find that longer notice periods reduce the probability of non-employment and increase annual earnings during the first year after layoff notification. Moreover, workers who get longer notification periods experience smaller falls in their reemployment wages. We also see substantial amounts of severance payments made by firms – which workers are willing to accept – in order to reduce the notice period. Workers who are eligible for higher levels of unemployment insurance (UI) get lower severance payments.

The third chapter is joint with Martin Söderström and Johan Viktröm and is titled **What makes a good caseworker?**. Here we study the importance of caseworkers assigned to job seekers when registering at local public employment offices upon unemployment. Specifically, we examine which features of a caseworker that are important for job seeker outcomes and to what extent job seeker–caseworker matching matter. To that, we also estimate caseworker overall impact as measured by value-added. While the question itself is rather straight forward, answering it is complicated by the fact that job seekers are generally not randomly assigned to caseworkers. For example, the most productive caseworkers are often assigned the most disadvantaged job seekers, thus making a mere comparison across caseworkers possibly very misleading or at best uninformative. However, we are able to break this non-random sorting of job seekers to caseworkers by exploiting that many local employment offices in Sweden assign job seekers to caseworkers based which day of the month they are born. As the exact date-of-birth is uncorrelated with individual characteristics of the job seekers, this creates an as-if random allocation where caseworkers within a local office will have job seekers with similar observed and unobserved characteristics. Using an IV-framework we are also able to handle exemptions from the date-of-birth assignment rule.

We document large and important differences in overall caseworker value-added and when studying what characteristics among caseworkers are predictive for job seekers' successes we find that female caseworkers perform better than male caseworker, in particular when they are paired with female job seekers. However, many other observed caseworker characteristics, such as cognitive ability, personal experience of unemployment and educational background, are not related to caseworker performance. This result is also consistent with results from the teacher literature, which finds little evidence of a relationship between teacher quality and observed teacher characteristics (Rockoff, 2004, Rivkin, Hanushek and Kain, 2005, Rockoff et al., 2011). Nevertheless, we do find that caseworkers who share the same labor market experience or educational level as the job seeker are more successful in mediating jobs to the unemployed, Moreover, caseworkers that have a preference for meetings appear to be more successful in shortening job seekers unemployment duration

The fourth and last chapter in this thesis is titled **Extended Unemployment Benefits and the Hazard to Employment** and studies how the generosity of UI affect the hazard to employment and job seekers unemployment duration. To do this, I exploit a feature in the Swedish UI system which grants a 30 week UI extension to job seekers having a child below the age of 18 at the time of (regular) UI exhaustion. Surprisingly, and in contrast to many previous studies, I find no evidence of the extension having prolonged unemployment durations for eligible job seekers; although actual take up of UI did increase.[2] The absence of an effect on unemployment duration is likely attributable to access to fairly generous replacement rates offered in other programs that become available to job seekers who exhaust their unemployment benefits. I also investigate and test if the employment decision is timed such that it coincides with UI exhaustion. The results show no evidence of job seekers manipulating or postponing employment. Moreover, job seekers do not appear to lower their search intensity during the unemployment spell due to being aware of being entitled to longer benefit duration.

[2]See section 4.2 in Chapter 4 for a brief overview of the literature.

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Chapter 1

Saved by Seniority –

Effects of Displacement for Workers at the Margin of Layoff*

*I am deeply indebted to my advisor Peter Fredriksson whose comments have benefited this paper greatly. A special thanks also to David Seim for, in addition to feedback, providing accesses to the data. I also thank Niklas Blomqvist, Marcus Eliasson, Ines Helm, Camille Landais, Eva Mörk, Arash Nekoei, Erik Öberg, Björn Öckert as well as seminar participants at Stockholm University, Research Institute for Industrial Economic, Uppsala Centre for Labor Studies, Uppsala University, 31st EALE Conference, Institute for International Economic Studies, The Institute for Evaluation of Labour Market and Education Policy, Norwegian School of Economics, University of Bristol and University of Edinburgh for valuable comments and suggestions. Funding from Handelsbanken is gratefully acknowledged. All remaining errors are my own.

1.1 Introduction

A large literature documents that displaced workers suffer significant and even permanent losses in terms of their future earnings, employment and wages.[1] The underlying causes of this phenomenon is still, however, vividly debated and standard models of the labor market have trouble generating the magnitude and persistence of empirically observed losses (Davis and von Wachter, 2011) or disagree upon it sources (Carrington and Fallick, 2017). Moreover, the current empirical evidence pertain to, primarily male, high tenured workers experiencing mass layoffs or plant closures.

This paper studies the short and long-run consequences of job loss for workers, and explores why and under what circumstances the cost of displacement are most persistent. This is done empirically by exploiting discontinuities in the likelihood of lay off generated by a seniority rule used at layoffs in Sweden, specifically the last-in-first-out (LIFO) rule. The novel source of identification enables me to study earnings, employment and wage losses upon displacement and characterize the main drivers of its persistence for a broader and more representative population of workers, laid off due to less drastic and more regular adjustments to employment.

Understanding why and under what circumstances the costs of displacement are most persistent is important not only for our theoretical understanding of the labor market but also for public policy. Whereas short-term losses may call for policy measures such as intensified job search assistance or extend unemployment benefits, persistent costs and of displacement brings additional concern over the long-run labor market prospects of workers. In light of previous evidence, several economists have recommended policies to abate these long-run losses. Policies such as subsidizing reallocation and retraining of displaced workers; even sug-

[1] For results on subsequent labor market outcomes (see e.g. Davis and von Wachter, 2011, Eliason and Storrie, 2006, Hijzen, Upward and Wright, 2010, Jacobson, Lalonde and Sullivan, 1993, Kletzer and Fairlie, 2003, Lachowska, Mas and Woodbury, 2018, Ruhm, 1991, Schmieder, von Wachter and Heining, 2018, Song and von Wachter, 2014). For effects of displacement on health and morality (see e.g. Browning, Dano and Heinesen, 2006, Sullivan and von Wachter, 2009, Eliason and Storrie, 2009, Schmitz, 2011, Black, Devereux and Salvanes, 2015, Jolly and Phelan, 2017, Schaller and Stevens, 2015). Reviews of the literature can be found in Davis and von Wachter (2011), Couch and Placzek (2010), Fallick (1996).

gesting a government financed wage insurance which subsidizes earnings for workers whose new job pay less than that of their old job (United States Congress, 2010).

The key challenge in obtaining credible estimates of earnings losses upon job loss is that displacement is a non-random event. For instance, it is widely recognized that displaced workers may be adversely selected (see Gibbons and Katz, 1991, Pfann and Hamermesh, 2001, Lengerman and Vilhuber, 2002, von Wachter and Bender, 2006, Abowd, Vilhuber and McKinnon, 2009, Couch and Placzek, 2010, Schwerdt, 2011, Davis and von Wachter, 2011, Seim, 2019). If employers are able to select which workers to displace, whereas others leave the firm early in expectation of future layoffs, workers remaining at the time of displacement may be of lower quality. Since the seminal study by Jacobson, Lalonde and Sullivan (1993) the literature has relied on comparisons of displaced vis-à-vis non-displaced workers across firms, using mass layoffs as an exogenous source of variation. To distinguish between voluntary and involuntary quits in data, focus has primarily been on male high tenured workers with a strong attachment to the labor market where the separation is less likely to be voluntary. Estimates of earnings losses using mass layoffs therefore pertain to a particular subset of workers, laid off under very particular circumstances. And to the extent that low productivity firms attract low productivity workers (Abowd, Kramarz and Margolis, 1999) estimates reflect the causal effect of job loss for workers with less favorable characteristics.[2] Mass layoffs are also quite rare and extraordinary events constituting only a fraction of all involuntary separations. Strikingly, only about 7 percent of all reported layoffs and discharges in the United States in 2012 where due to mass layoffs.[3] Meanwhile, evidence is scarce on how job loss due to less drastic and more regular adjustments to employment affect workers, and to what extent focus-

[2]Previous research has shown that firms executing mass layoffs tend to be larger firms, concentrated to particular industries with overall higher turnover rates (Krashinsky, 2002, Fallick, 1996, von Wachter and Bender, 2006, Sullivan and von Wachter, 2009)

[3]Calculations are based upon data from the Bureau of Labor Statistics by combining data from the Mass Layoff Statistics program (which ended in March 2013) with the Job Openings and Labor Turnover Survey (JOLTS) reporting the total number of layoffs and discharges which is made up of all involuntary separations initiated by the employer. Both these data sources can be accessed at http://www.bls.gov.

ing on mass layoff events renders exceptionally negative outcomes for workers.

The LIFO rule is written into Swedish labor law and mandates that workers should be laid off in inverse order of seniority, whereby more recent hires ought to be let go before workers with higher tenure. Using detailed matched employer-employee data, containing information on job start and end dates, I rank workers according to their relative seniority (tenure) within an establishment which, by the LIFO rule, renders variation in the probability of displacement. Combining these data with wage registers and a unique individual register dataset containing all layoff notifications involving at least 5 workers during 2005–2015, I identify occupation specific cut-offs in downsizing establishments where the probability of displacement jumps discontinuously. This generates quasi-experimental variation which lends itself to a (fuzzy) regression discontinuity (RD) design. The key threat to a causal interpretation of these estimates is that firms selectively displace workers by choosing, not who but rather, how many workers to lay off. Although such manipulation is unlikely due to priority of recall for the last displaced worker, I carefully address this concern through a series of tests and find no evidence of selective firing based on observable characteristics or earnings prior to the displacement event.[4]

The main finding of the paper is that both the composition of workers and the size of the layoff, have important consequences for how workers are affected by job loss, particularly in the long run. In the first part of the paper, I estimate earnings losses of displaced workers and find that they on average suffer initial earnings losses of about 38 percent compared to their non-displaced coworkers. While not being fully comparable, the size of these initial losses are close to what has been observed for displaced workers in the United States who are laid off during recessions (Davis and von Wachter, 2011). As time progresses, however, the earnings gap between displaced and non-displaced workers shrink and is fully closed 7 years after displacement. Crucially, this is not driven by the non-displaced workers getting laid off at a later point

[4] To the extent that there are imbalances in unobserved worker productivity due to employers being able to selectively displace workers, estimates should be downward biased. Nevertheless, in light of the finding that earnings losses are transitory rather then persistent, this would suggest that displaced workers recover even faster.

in time. I then decompose average cumulated earnings losses into different margins of adjustment and show that these losses are primarily driven by lower wages and less employment, whereas the hours responses are of lesser importance.

As the finding of earnings losses being transitory, rather then persistent, stands in contrast to the previous literature which find long run earnings losses ranging between 10-20 percent of previous earnings (see Table A-1.1 for a summary), the second part of the paper exploits the large heterogeneity across layoffs in order to understand the main drivers of long run earnings losses. I begin by estimating earnings losses of displaced workers using mass layoffs following the standard event study approach. I find large and highly persistent effects of displacement thus ruling out that the transitory pattern observed in the RD analysis is context or time specific. I proceed by producing separate RD estimates for each layoff. I then correlate the short and the long run losses with characteristics of the workers, occupation and establishment involved in the layoff as well as economic conditions at the time of notification. While I find that older workers are more negatively affected by job loss, the key driver of persistent earnings losses turn out to be the relative size of the layoff. In fact, significant persistence can only be found among establishments executing mass layoffs, i.e, displace more then 30 percent of their workforce. This pattern remains even when controlling for worker characteristics as well as economic conditions. Going further, I exploit the fact that there is variation in the size of layoff relative to the local labor market, holding constant the size of the layoff in relation to the establishment. These estimates indicate that the key determinant of persistent earnings losses is the size of the layoff in relation to the local labor market suggesting that negative spillovers and general equilibrium effects play an important role for workers future labor market outcomes.

Relative to the previous literature estimating earnings losses upon displacement, this is the first paper to exploit seniority rules as an exogenous source of variation to involuntary job loss. By doing so, I provide new evidence of the the consequences of job loss for a broader and more representative population of workers. Moreover, the novel identification strategy allows me to study more common and less drastic adjustments to employment, in contrast to relying on much larger and exceptional layoffs events. This distinction also turns out to have significant implications for our view on how workers are affected by involuntary job loss, as persistent earnings losses are only found among workers experiencing large layoffs, which have been the focus of the literature so far.

The results of this paper also speak to the theoretical literature explaining the observed earnings losses of displaced workers in models featuring search frictions, unemployment fluctuations and job ladders (see e.g. Ljungqvist and Sargent, 1998, Davis and von Wachter, 2011, Krolikowski, 2017, Kuhn and Jung, 2019). My findings suggest that standard search-matching models of the labor market may in fact be able to account for both the magnitude and the persistence of earnings losses when considering more representative set of laid-off workers. Finally, the paper adds to the literature on how seniority rules are used at layoff (see section 1.2 for a brief overview).

The rest of the paper proceeds as follows. Section 1.2 provides a brief description of the overall usage of seniority rules at layoff, the Swedish labor market and gives a more detailed description of the Swedish LIFO principle that is used for identification. Section 1.3 describes the data and defines the relevant variables used to identify workers' relative seniority within an establishment. The empirical strategy is laid out in Section 1.4, together with a discussion and multiple tests of the identifying assumptions needed for causal inference. The section ends with examining the empirical relationship between workers' relative seniority and layoff, i.e. the first stage. Section 1.5 presents the results on workers subsequent labor market outcomes and decomposes the overall earnings effect into various margins of adjustment. In Section 1.6, I investigate the main drivers of earnings losses upon displacement and evaluate the relative importance of the general equilibrium effects created by larger layoffs. Finally, Section 1.7 concludes.

1.2 Seniority rules

The use of a seniority rule at layoff implies that more recent hires should be displaced before workers with longer tenure. Thus a workers' relative tenure ranking within a firm or establishment is predictive, albeit not perfectly, of whether he or she will become displaced in the event of an establishment downsizing. Seniority rules are part of the broader concept of employment protection as it provides insurance and protects tenured workers against unjust termination (Pissarides, 2001). While being largely beneficial for the incumbent worker, high employment protection is generally thought to increase firms firing costs which in turn may hamper job creation and generate inefficiently low labor turnover (see e.g. Lazear, 1990, Mortensen and Pissarides, 1994). Indeed, some studies find that relaxing employment protection, specifically exceptions from the seniority rule at small firms, renders increased labor flexibility and labor productivity (Bjuggren, 2018, von Below and Thoursie, 2010).

Seniority rules are commonly used at layoffs although with considerable differences across sectors and countries. Buhai et al. (2014) empirically documents the use of seniority rankings in layoff decisions in Denmark and Portugal, although it is unclear whether any formal rules are the cause of these findings. Abraham and Medoff (1984) survey about 200 firms in the United States and find that seniority rules are commonly used at layoff, particularly among unionized firms. Sorensen (2018) provides suggestive evidence of seniority rules being used during mass layoffs among German establishments although the use of such rules appear to have declined. Böckerman, Skedinger and Uusitalo (2018) and Landais et al. (2018) documents empirical patterns consistent with the use of seniority rules in Sweden, which together with the Netherlands, is one of few countries who explicitly refer to a seniority rule in the Employment Protection Act as the main criteria for prioritizing among workers in the event of downsizing (Böckerman, Skedinger and Uusitalo, 2018). However, none of the aforementioned paper have been able to pin down the usage of a strict seniority rule (e.g., LIFO rule) by establishing a discontinuity in the seniority ranking.

The Swedish labor market The Swedish labor market is characterized by high union involvement. There is, for instance, no legislated minimum wages in Sweden but instead wage floors are set in industry or even occupation specific collective bargaining agreements (CBA's) which by law cover all employees (also non union members) at a firm who has signed such an agreement. Moreover, there are always separate CBA's for white- and blue-collar workers. The wage setting system thus rely on high CBA coverage which in 2017 was about 90 percent of the Swedish workforce whereas the union membership rate was around 69 percent (Kjellberg, 2017).

Workers that are laid off due to no-fault individual dismissals are entitled to advance notice where the length of the notice period varies with tenure by law and sometimes by age according to local CBA's. The length of notice periods follows a stepwise pattern where the minimum notice period is 1 month for workers with less then 2 years of tenure. Workers with at least 2 but less then 4 years of tenure have 2 months of notice and the maximum statutory notice period is 6 months which is given to workers having worked at least 10 years with the same employer. For white-collar workers, most CBA's grant an additional 6 months of notice for workers above the age of 55 at dismissal.

The Swedish LIFO rule The Swedish Employment Protection Act (EPA:22§) stipulates that when a firm needs to downsize due to "shortage of work" it should follow a LIFO principle which mandates that workers should be laid off in inverse order of seniority.[5] In the event of a tie in tenure, priority should be given to the older worker. Formally, the LIFO rule applies at the establishment level. In the event of multiple layoffs, employers should divide workers into groups based on workers CBA affiliation and list workers according to the length of employment.[6] These groups form so called order of termination circuits (*turordningskrets*) (henceforth refereed to as an order circuit or circuit, for short). Importantly, labor law also stipulates a "last-out-first-in" principle (EPA:26§) where the displaced worker with the highest tenure within the circuit has priority of recall if the firm needs to start hiring within 9 months of the displacement. Priority of recall applies to workers with at least 12 months of tenure, who is deemed sufficiently qualified for the new job and had expressed a wish for recall to the employer prior to layoff.

[5]The term "shortage of work" is somewhat misleading as legal practice has come to interpret this as all lay-offs not related to personal behavior of an individual worker.

[6]Whereas the LIFO rule applies at the establishment level, a worker's tenure – on which he is ranked upon – is based on total time at the firm, irrespective of whether the worker has worked sporadically, part-time or full-time. During e.g. firm acquisitions or mergers tenure is not reset but the start date of employment is that of the initial employer.

Some parts of Swedish labor law consists of semi optional paragraphs, meaning that these could be bypassed by employee and employer organizations through CBA's or local agreements. One such paragraph is the LIFO rule. An employer may deviate from the LIFO principle by agreeing on a different order of priority with local union representatives in a negotiation. However, if the employer and the union are unable to strike a deal the LIFO rule as written in law should be applied. As such, the Swedish LIFO principle is a "soft" seniority rule, functioning as a default or starting point for negotiations between the local union and the employer. Unfortunately, little is known about how frequently agreements of deviations from the LIFO principle are made in practice.[7] Hence, it is ambiguous whether employer compliance with the LIFO principle at layoff is voluntary or at the demand of the local union. Finally, firms with less than 10 employees are allowed to exempt two workers that are of particular importance for the firm. Also, workers in managerial positions or part of the employers' family may be exempted from the LIFO rule.

1.3 Data

I have data on layoff notifications from 2005 to 2015. By law, any firm that intends to displace more than 5 workers within a 90 day period must notify the Public Employment Service (PES). In a first stage, the firm reports to the PES the number of intended layoffs and the reason for downsizing. In a second stage, on average 70 days after the first, the firm submits a list of names of the workers affected by the displacement. By law, the list should be sent in within a month of the first worker becoming laid off. Typically, all workers are notified on the same date whereas the date of displacement differs due to differences in statutory notification times as described above.

These data are then matched with a data set containing the universe of employer-employee matches between 1985 to 2016, which contains information on both firm and individual characteristics such as age, level of education and annual earnings. The data is annual, and along with

[7]Deviations from the LIFO principle should, however, not contravene "good practice in the labor market" or violate the Discrimination Act (EPA:22§).

the annual income statement, the employer reports the first and last month worked for each employee. These monthly markers make it possible to calculate firm specific tenure as well as to determine the current workforce at an establishment at the monthly level. One issue with the monthly markers is that employers sometimes routinely report workers as having worked the entire year so that January is too often reported as being the start of the employment spell which in turn may generate measurement error in tenure. Moreover, a common feature of matched employer-employee data are so called false firm deaths where firms for other reasons than shut-down change identification number. Such occurrences would lead to erroneously reseting workers tenure, thereby creating large amount of inaccurate ties in tenure within a firm. As these data shortcomings in measuring tenure will map directly onto the forcing variable I try to minimize its influence by dividing workers starting in January into quartiles of annual earnings in the first year of employment where lower quartiles are assumed to have started employment later. I also exclude circuits where more than 2/3 of workers have tenure equal to the mode of tenure within the circuit.[8] In Appendix C, I explain in detail the procedure for calculating tenure and address the potential sources of measurement error in the forcing variable and its consequences for identification.

As described in Section 1.2, the LIFO rule applies at the establishment $\times$CBA level. Ideally, one would like to have accesses to which workers are covered by which CBA. As data do not exist on workers CBA affiliation, I proxy this by (the Swedish version of) 2-digit level ISCO-88 (International Standard Classification of Occupations 1988) occupational codes provided in the wage register collected by Statistics Sweden each year. The register also contain information on (full-time equivalent) wages and is available for a very large sample of establishments covering almost 50 percent of all private sector workers and all public sector workers from 2000 to 2015. The sampling of private sector workers is done by firm

[8]An alternative approach, suggested by Hethey-Maier and Schmieder (2013), is to correct false firm/establishment deaths using worker flows. This involves categorizing last appearances of establishment identifiers as closures, mergers, spin-offs, etcetera, by placing restrictions on observed worker flows. As this approach requires more than one, possibly arbitrary, restrictions I find that placing only one restriction is more transparent.

(stratified by size) which again enables me to classify occupation for the entire workforce at each (sampled) establishment.[9]

Through these data, I determine the order of termination implied by the LIFO rule, for all establishments having sent a lay off notification to the PES and for which I have data on workers' occupation. Within circuits, I rank individuals according to seniority (SR_{ic}), adapting the convention of 1 being the highest tenured worker. Individual i's relative ranking within an order circuit c could then be written as

$$RR_{ic} = SR_{ic} - (\max_{i \in c}(SR_i) - N_c) \tag{1.1}$$

where SR_{ic} is the seniority ranking and N_c is the number of notified workers reported in the list submitted by the employer to the PES.[10] RR_{ic} is the forcing variable defining the relative tenure ranking normalized to zero for the worker who, by the LIFO rule, should be the last worker to *remain* employed. Figure 1.1 illustrates RR for two occupations (pink and gray) within a downsizing establishment in a given year. These form two separate circuits where workers are ranked according to tenure (and age in case of a tie).[11] In the upper row $N_c = 3$ and the lower $N_c = 2$. Thus, workers to the right of the cut-off (with $RR > 0$) would get displaced if the establishment fully applied the LIFO rule. Note that the number of notified workers (N_c) is set endogenously by the firm. This may be problematic if firms select N_c based on worker characteristics as it would cause selective firing. I address this concern thoroughly in Section 1.4.2.

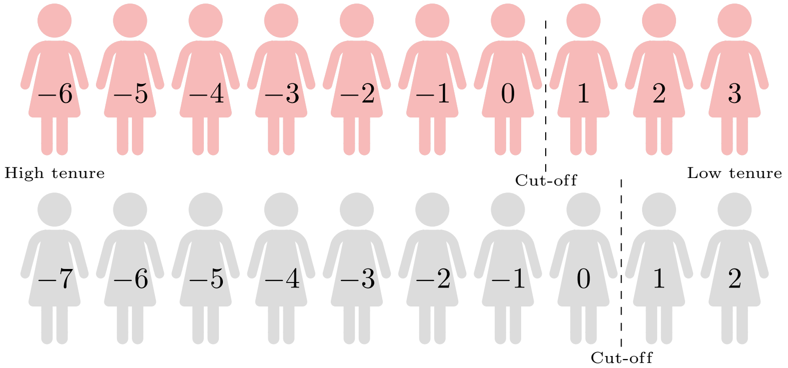
I have imposed some further restrictions on the data. First, I exclude layoff notifications where plant closures and bankruptcies are reported as the cause of displacement, as the threshold within circuits in such

[9] The survey is carried out in September and November and thus, strictly speaking, information on the composition of the workforce, occupation and wages corresponds to these months. This implies that order circuits should be better approximated for notifications that occur around September and November. Indeed, the precision of the first stage is much better for notifications made in the month of September to January than other months.

[10] I thus use the number of notified workers provided in the second stage of the reporting process. The reason is that firms have an incentive to over report the initial number of intended layoffs as they are prohibited from going beyond this number when finalizing the list of workers getting displaced.

[11] Ties in relative ranking could still exist if workers start their job in the same year and month and also being born in the same year and month.

FIGURE 1.1. GRAPHICAL ILLUSTRATION OF TWO ORDER CIRCUITS



Notes: The figure illustrates workers relative tenure/seniority ranking, normalized to zero at cut-off, for two different occupations (pink and gray) within an establishment in a given year which together forms two order circuits. Workers right of the cut-off, with positive relative ranking are those who, according to the LIFO rule, ought to be displaced when a firm downsizes due to shortage of work.

establishments are undefined since everyone is laid off. I also discard notification due to an establishment moving as it may be endogenous whether the worker chooses to reallocate with the establishment. Second, I restrict the analysis to industries dominated by blue-collar workers as the LIFO rule to a greater extent applies among blue-collar workers. From this restriction it also follows that almost all establishments operate in the private sector. Finally, I condition on layoff notifications affecting at least 10 workers within an order circuit which is restricted to contain at most 100 workers.[12]

1.3.1 Mass layoff vs. LIFO sample

As mentioned in section 1.1, the canonical way of estimating earnings losses upon job loss has been to identify instances where establishments layoff a large share of their workforce or shut down altogether. Since the seminal study by Jacobson, Lalonde and Sullivan (1993), a mass layoff

[12] The lack of a one-to-one mapping between CBA's and occupation codes makes it difficult to precisely define the relevant workforce subject to the notification. Thus, the full tenure distribution within the order circuit may be obscured by erroneously including workers in an order circuit which they do not belong to. Misallocating just one worker will render the circuit too large or too small, thereby leading me to place the discontinuity in the wrong place in the tenure distribution. Thus placing restrictions on the maximum size of the order circuit increases precision of the first stage as the probability of including the "wrong" workers decreases.

Table 1.1. Sample characteristics

	All Displaced Workers		LIFO Sample		Mass Layoff Sample	
	Mean	SD	Mean	SD	Mean	SD
Age	40.66	12.74	38.58	12.02	42.47	8.19
Female	0.35	0.48	0.21	0.41	0.00	0.00
Tenure	4.60	5.13	6.47	5.80	11.02	6.01
Annual Earnings (t-1)	26.65	13.75	25.24	9.95	29.66	11.80
Highest attained education						
Primary school	0.44	0.50	0.50	0.50	0.58	0.49
High school	0.43	0.50	0.45	0.50	0.35	0.48
College	0.12	0.33	0.05	0.23	0.07	0.26
N	425,890		16,747		22,880	

Notes: The table shows summary statistics for workers notified of their displacement between 2005-2015. The first column includes all workers notified in layoffs where more than 5 workers are involved and hence reported to the PES. The second column shows sample characteristics for workers used in the main analysis of this paper. The third column shows worker characteristics when following the standard restrictions imposed in the literature using mass layoffs. For details see section 1.6.1.

has been defined in the literature as observing at least 30 percent of the current workforce leaving the plant within a year. One limitation, however, is the inability to separate between voluntary and involuntary quits which introduces upward bias if the former is erroneously interpreted as the latter. To account for this, most studies focus on workers with strong attachment to the firm as voluntary quits could be considered less likely. In Table A-1.1, I summarize some of the most influential or recent studies estimating earnings losses upon displacement, all of which use mass layoff for identification. As can be seen in column (6)-(8), the typical study considers large layoffs, focusing on male workers with at least 6 years of tenure.

Even though studies exploiting mass layoffs may be internally valid, the external validity for the population of laid-off workers or the working population in general is not immediate. Indeed, Table A-3.1 illustrates that external validity may be an issue. The table presents descriptive statistics for all Swedish workers being part of a layoff notification consisting of 5 workers or more as well as for the sample of workers fulfilling the sample restrictions standard within the mass layoff literature. It is clear that these (male) workers are on average both older and have higher tenure and higher annual earnings then the average laid off worker in Sweden. Note that these dissimilarities may be even more pronounced for mass layoff samples outside Sweden as the LIFO rule may restrict employers from selecting its least productive workers. The middle two columns of Table A-3.1 show the same statistics but for the sample of laid off workers used in this study. When not being forced to condition on tenure, workers in my sample are more similar to the average notified worker.[13]

1.4 The LIFO rule and layoff

1.4.1 Empirical strategy

As seniority within an establishment will be positively correlated with worker ability and productivity, correlating workers relative ranking with future earnings will inevitably be biased due to omitted variables. Similarly, a mere comparison of displaced vis-à-vis non-displaced workers will render biased estimates as firms could selectively displace workers with an ex ante lower earnings trajectory (due to e.g. low productivity). The LIFO rule, however, imposes restrictions on the employer in choosing between two workers working at the same establishment who performs similar tasks.

Following the definition of relative ranking (RR) in equation (1.1), I define the instrument as $Z_{ic} = \mathbf{1}[RR_{ic} > 0]$ where $\mathbf{1}[\cdot]$ is the indicator function. Further, I define a control function for relative ranking

$$h(RR_{ic}) = [h_0(RR_{ic}) + h_1(\mathbf{1}[RR_{ic} > 0] \times RR_{ic})] \tag{1.2}$$

which allows for different slopes on each side of the threshold. Since tenure is discrete and measured in months, I rely on a parametric control function varying the functional form in contrast to more non-parametric estimation techniques suggested by Calonico, Cattaneo and Titiunik

[13] As the identifying variation comes from the compliers just at the threshold Table A-1.2 characterizes the complier population following Abadie, Angrist and Imbens (2002), Abadie (2003). In general, the overall estimation sample is very similar to the complier population.

(2014). The first stage equation can then be written as

$$D_{ic} = \alpha + \gamma Z_{ic} + h(RR_{ic}) + \phi_c + \rho X_i' + \varepsilon_{ic} \tag{1.3}$$

where γ is the first stage effect on the probability of being displaced (D_{ic}). X_i' is a vector of baseline covariates included in some specifications to increase efficiency and ε_{ic} an error-term. ϕ_c is an order circuit fixed effect which consists of unique combinations of a firm, establishment, occupation and notification year fixed effects. The corresponding outcome equation is

$$y_{ict} = \pi + \beta D_{ic} + h(RR_{ic}) + \phi_c + \delta X_i' + u_{ict}. \tag{1.4}$$

Substituting equation (1.3) into (1.4) yields the reduced form equation. As order circuits are proxied and the LIFO rule semi optional, assignment to displacement will not be a fully deterministic function of a workers relative ranking (i.e., $\gamma < 1$). Hence, in order to estimate the cost of displacement, I instrument D_{ic} with Z_{ic}, rendering a fuzzy RD-design. The resulting instrumental variable (IV) estimate may then be interpreted as the local average treatment effect (LATE) for workers just at the margin of lay off within establishments complying with the LIFO rule. Notice that equation (1.3) and (1.4) exploit variation within order circuits (establishment$\times$occupation$\times$year combinations) thereby avoiding any potential bias stemming from initial sorting of different types of workers into different types of firms.

Excludability of the instrument hinges upon the assumption that being just above the (proxied) threshold only affects subsequent labor market outcomes through displacement. While exclusion is an assumption, it is useful to note that there are no other formal rules pertaining to the LIFO threshold. Also, the reduced form coefficient is interpretable as the average effect of being exposed to a higher risk of displacement in the event of downsizing.

In the main specification I use a bandwidth of ± 15 while confirming the robustness of these results by varying both the bandwidth and the functional form of $h(\cdot)$ as suggested by Lee and Lemieux (2010).[14] In

[14] I also run the main regressions using the optimal bandwidth selector suggested by Calonico, Cattaneo and Titiunik (2014). As can be seen in Appendix B, the results remain virtually unchanged.

all regressions I cluster the standard errors at the level of the order circuits.[15]

1.4.2 Selection around the discontinuity

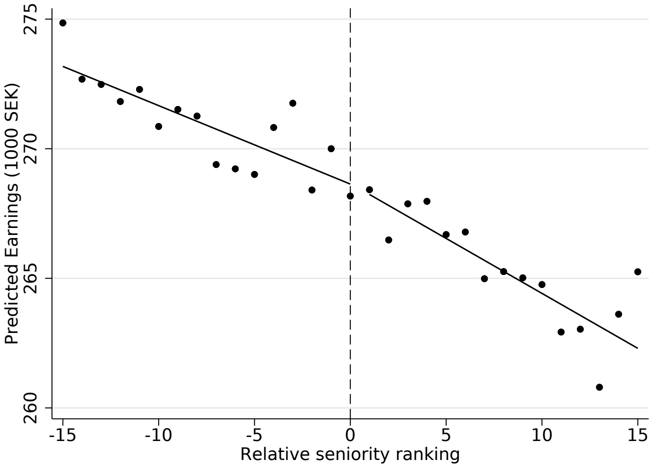
The empirical strategy relies on the assumption of non-manipulation of the forcing variable. Specifically, firms should not be able to perfectly choose which workers' gets notified and eventually displaced. As described in Section 1.2, default order circuits may be circumvented and formed endogenously in a firm/union negotiation which might render control over which workers get notified and eventually laid off. This implies that, even if data on actual order circuits where available, one may be reluctant to use these. However, by proxying order circuits with combinations of establishment and occupation, I avoid potential manipulation as the proxy functions as an instrument, only picking up establishment/occupation combinations that adhere to the LIFO rule. If the relative tenure ranking within the establishment/occupation combination were not predictive of actual order circuits, due to, e.g., deviations agreed upon between local union representatives and the employer, the first stage coefficient would be zero.

One potential concern is that firms set the cut-off endogenously by choosing how many workers to notify and eventually displace. A firm that intends to lay off n workers but realizes that worker $n+1$ in the seniority ranking is a lower productivity worker the firm can instead decide to notify and lay off $n+1$ workers. This could create non-random selection into displacement which could invalidate the RD research design by creating discontinuous differences in worker characteristics around the threshold. Formally, the key identifying assumption can be stated as

$$\lim_{\Delta \to 0^+} \mathbb{E}[\varepsilon_i \mid RR_i = \Delta] - \lim_{\Delta \to 0^-} \mathbb{E}[\varepsilon_i \mid RR_i = \Delta] = 0 \tag{1.5}$$

[15] Card and Lee (2008) propose clustering the standard errors on the running variable when using a RD design. Doing this generally renders somewhat smaller standard errors as does clustering at the establishment level or the interaction of the two. In my main specification I take the most conservative approach and cluster standard errors on the circuit level. Results with other levels of clustering is available upon request.

FIGURE 1.2. SELECTION ON OBSERVABLES



Notes: The figure shows predicted annual earnings as a function of a workers relative ranking within an order circuit (in discrete bins), normalized to zero at the threshold. The dependent variable is generated by taking the fitted values from a regression of annual earnings on age, tenure and dummies for female, immigrant, level of education. The regression include a linear polynomial function interacted with the threshold as well as order circuit fixed effects and is run using a bandwidth of ±15. The point estimate of the jump at the threshold is -0.104 with a standard error of 0.944. Standard errors are clustered at the order circuit level.

meaning that the distribution of unobserved worker characteristics be continuous at the threshold. Although the continuity assumption cannot be fully tested, its validity may usually be assessed by checking the density of observations around the threshold as well as mean observable worker characteristics.

As the threshold is defined by where in the seniority distribution the last worker is notified, standard density tests as suggested by McCrary (2008) are not longer valid as the density around the threshold is balanced almost by construction.[16] For completeness, however, Figure A-1.1 shows the density around the threshold. Due to having restricted the sample to at least 10 workers getting notified within a circuit the frequency of observations are about the same up until $RR_{ic} > 10$ where it starts to drop. Since density tests are invalid in this particular setting,

[16] I say *almost* due to the fact that I allow for ties in relative ranking of both tenure and age at notification are the same for workers within the same order circuit. Also note that, by construction, circuits where the entire workforce is notified are excluded.

I rely on balancing of average worker characteristics at the threshold to test the continuity assumption.

Figure 1.2 plots predicted annual earnings estimated by taking the fitted values from a regression of annual earnings on age, tenure and dummies for female, immigrant and level of education. Whereas the overall downward trend in predicted earnings stems from workers with higher relative ranking being on average lower tenured workers, there is no indication of selection around the discontinuity as the estimated jump at the threshold is less than 104 SEK (10.5 USD) and statistically insignificant. Thus predicted annual earnings evolves smoothly around the threshold.

Next, in an additional test of the continuity assumption, column (1)–(3) in Table 1.2 show estimates from regressing the instrument Z_i on a set of pre-determined covariates and the control function $h(RR_i)$. Irrespective of the choice of functional form, or the exclusion of circuit FE's, none of the individual variables are predictive of treatment status as coefficients are typically small as well as statistically indistinguishable from zero. Most importantly, all specifications in column (1)-(3), are unable to reject the hypothesis of all coefficients being jointly zero as can be seen in the lower end of Table 1.2 showing the F-statistic and the p-value from a joint significance test. Further, columns (4) and (5) show results from separate regressions for each baseline covariate, regressed on the instrument and a first and second order polynomial function, respectively. The point estimate in column (5) suggest that the difference in annual earnings between workers just to the right and left of the threshold is 0.7%. Figure A-1.3 show graphically the bivariate balancing tests corresponding to column (4) and (5) as well as for monthly wages for which I have access to for a smaller sample.

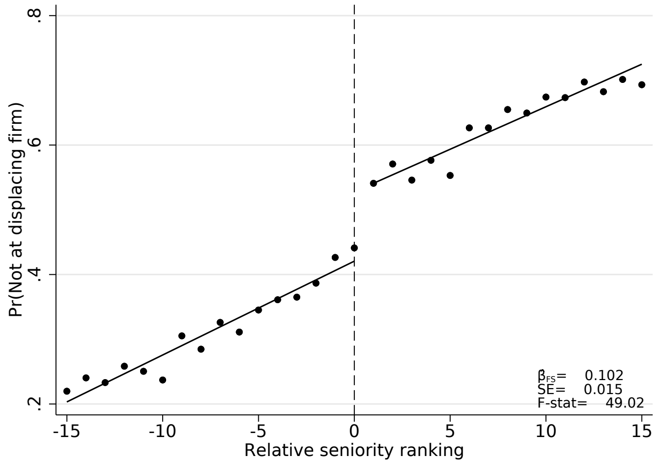
Taken together, the fact that observable characteristics and earnings are neither jointly nor individually predictive of treatment speaks strongly in favor of the continuity assumption. It suggests that employers are unable or unwilling to adjust the number of workers being notified/displaced such that selective displacement occurs. Arguably, the incentives for laying off $n + 1$ workers are also small as the marginal worker being laid off have first priority of recall up to 9 months after displacement. In sum, I find no evidence of firms setting the cut-off en-

Table 1.2. Balancing of baseline covariates

	(1)	(2)	(3)	(4)	(5)
Earnings	0.0001	0.0030	0.0030	0.0049	0.0073
	(0.0056)	(0.0078)	(0.0044)	(0.0101)	(0.0124)
Female	-0.0063	-0.0066	-0.0018	-0.0127	-0.0151
	(0.0046)	(0.0063)	(0.0037)	(0.0111)	(.0144)
Immigrant	0.0057	0.0041	0.0002	0.0059	-0.0071
	(0.0052)	(0.0065)	(0.0041)	(.0105)	(0.0143)
Age	-0.0000	-0.0001	0.0000	-0.1181	0.2001
	(0.0002)	(0.0002)	(0.0002)	(0.3532)	(0.4513)
Highest attined Education					
Primary school	ref.	ref.	ref.	0.0031	0.0271
				(0.0146)	(0.0207)
High school	-0.0017	-0.0022	-0.0028	-0.0048	-0.0272
	(0.0045)	(0.0049)	(0.0035)	(0.0149)	(0.0208)
College	-0.0003	-0.0001	-0.0036	0.0017	0.0002
	(0.0094)	(0.0129)	(0.0079)	(.0067)	(0.0088)
Order of polynomial					
1st degree	✓	✓		✓	
2nd degree			✓		✓
Circuit FE		✓	✓	✓	✓
F-statistic	0.542	0.364	0.306	.	.
p-value	0.776	0.901	0.934	.	.
R^2	0.740	0.733	0.883	.	.
# clusters	621	621	621	621	621
N	16,633	16,633	16,633	16,633	16,633

Notes: The table show balance tests of baseline covariates at the LIFO threshold. Columns (1)-(3) show results from regressing the instrument (being above the threshold) on a set of baseline covariates and a polynomial control function in relative ranking interacted with the threshold. Annual earnings in measured in the year prior to notification and is normalized to reflect percentage point deviations from the mean of the workers below the threshold. The bottom of the table displays the F-statistic and the corresponding p-value from testing the hypothesis that all coefficients being jointly equal to zero. Columns (4)-(5) report results from balancing tests where each covariate has been regressed separately on the instrument and a polynomial control function in relative ranking interacted with the threshold. All regressions use a bandwidth of ±15. Standard errors clustered at the level of the order circuit and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Figure 1.3. Probability of having left notifying firm



Notes: The figure shows the probability of displacement as a function of a workers relative ranking within an order circuit (in discrete bins), normalized to zero at the threshold. The regression include a linear polynomial function interacted with the threshold as well as order circuit fixed effects and is run using a bandwidth of ±15 corresponding to column 1 in Table 1.3. The point estimate of the jump at the threshold is 0.102 with a standard error of 0.015 which corresponds to an F-statistic of 49.02. Standard errors are clustered at the order circuit level.

dogenously such that it would invalidate the RD-design and thus the estimates can be interpreted as causal. Nevertheless, as the number of notified workers is set endogenously by the employer, imbalances may still exist on unobservable worker characteristics. While this, per definition, cannot be tested, selective displacement of low productivity workers would imply that my estimates of earnings losses and its persistence are exaggerated. However, in light of the finding that earnings losses are transitory rather then persistent, this would imply that displaced workers have smaller earnings losses and recover even faster.

1.4.3 Layoff and the LIFO-threshold

Figure 1.3 shows the probability of being displaced as a function of workers relative tenure ranking within an order circuit where displacement is defined as having left the notifying firm within 15 months after notification.[17] As predicted by the LIFO rule, there is a discontinuous

[17]The maximum notification time is 12 months and the average difference between workers' individual notification dates and the date the firm sends in the notification

jump at the threshold where the probability of displacement increases by 10.02 percentage points which translates to a 24 percent increase in marginal likelihood of getting displaced when surpassing the threshold.[18] In Appendix A, Figure A-1.2 shows the corresponding first stage regression using individual worker layoff notification as the dependent variable where again the probability of notification jumps discontinuously at the threshold 12.6–20 percentage points depending on the functional form of the control function.

If the LIFO rule was fully binding, this would imply a sharp jump in the probability of displacement going from 0 to 1 at the threshold. However, as seen in Figure 1.3 (and A-1.2), workers just below the threshold have about 42 (46) percent chance of being displaced (notified). The "fuzzyness" arises partly from two sources. First, tenure is measured with some error (see Appendix C). Second, the 2-digit occupational codes is only a proxy for workers CBA affiliation as the latter is not observed in data. This renders the establishment×occupation combination only approximate of statutory order circuits. Finally, actual circuits may deviate from the statutory circuits if agreed upon by the employer and local union representatives. Note, however, that the first stage ranks workers on tenure within the (proxied) statutory circuits. Hence, even if the actual circuits deviate due to local agreements or manipulation by the employer, this do not induce bias but only attenuates the first stage coefficient towards zero. Nevertheless, as seen in Figure 1.3, using (proxied) statutory circuits captures some compliance to the LIFO rule as the jump at the threshold is both precisely estimated as well as stable across various specifications.

Table 1.3 shows first stage estimates varying the bandwidth and functional form of the control function. Column (1) shows the estimate corresponding to Figure 1.3 where the estimated jump in the probability of displacement is 10 percentage points. The instrument is highly predictive of displacement with an F-statistic of 49 which is well above conventional levels for evaluating instrument relevance. It is also re-

to the PES is 70 days. Hence, not working at the notifying firm 15 months after notification is a fairly good proxy of displacement due to the downsizing. Nevertheless, the first stage is not sensitive to changing this to any number ≥ 12 months.

[18] Workers just below the threshold have a 42 percent likelihood of displacement so the marginal likelihood is calculated $0.102/0.419 = 0.243$.

TABLE 1.3. FIRST STAGE ESTIMATES

	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbf{1}[RR > 0]$	0.1022***	0.1033***	0.0873***	0.1304***	0.0643***	0.0960***
	(0.0146)	(0.0145)	(0.0157)	(0.0139)	(0.0185)	(0.0157)
Order of polynomial						
1st degree	✓	✓	✓	✓		
2nd degree					✓	✓
Covariates		✓	✓	✓	✓	✓
Circuit FE	✓	✓	✓	✓	✓	✓
Bandwidth ±	15	15	11	30	17	30
F-stat	49.02	50.93	30.77	87.96	12.10	37.16
p-value	0.000	0.000	0.000	0.000	0.001	0.000
R^2	0.365	0.374	0.362	0.380	0.376	0.394
# clusters	621	621	621	621	621	621
N	16,747	16,633	13,343	19,696	17,958	24,169

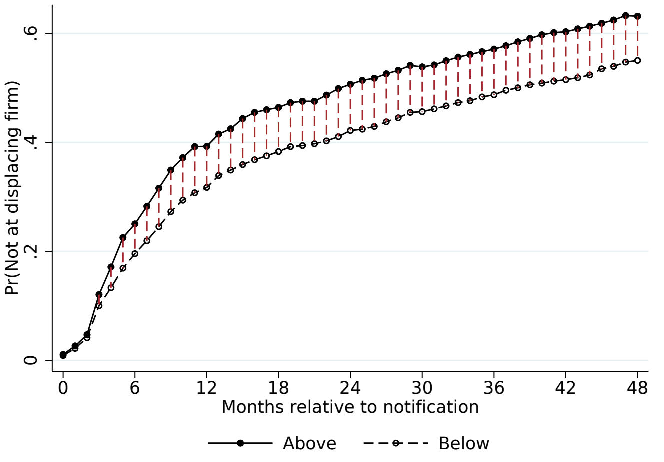
Notes: The table shows the estimated first stage coefficient γ of equation (1.3) which is the difference in probability of displacement for worker just below and above the LIFO threshold. The bottom of the table displays the first stage F-statistic and its corresponding p-value used to evaluate instrument relevance. Column (3) and (5) uses the optimal bandwidth selector suggested by (Calonico, Cattaneo and Titiunik, 2014). All regressions includes a polynomial control function which is interacted with the threshold which are of second order in columns (5)-(6). Where indicated I control for baseline covariates which are dummies for female, immigrant, level of education. Standard errors clustered at the level of the order circuit and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

assuring that adding covariates (column 2) do not change the estimated first stage coefficient by much, which confirms balancedness around the threshold. Column (3) of Table 1.3 display the first stage regression using the optimal bandwidth selector suggested by Calonico, Cattaneo and Titiunik (2014). Using this bandwidth of ± 11 the jump at the threshold is estimated to 0.087 with an F-statistic of 30.8. The bandwidth selector is, however, unable to account for order circuit fixed effects which may generate a too narrow bandwidth as it overstates the identifying variation. To further investigate the robustness of the first stage, column (5) and (6) fits a second order polynomial to the control function. Fitting a higher order polynomial to the (optimal) bandwidth of 17 somewhat reduces the first stage estimate compared to, e.g., column (3). However, it remains a strong predictor despite the narrow bandwidth where one worry may be that the model over-fits the data. Using a second order polynomial and doubling the bandwidth to 30 (column 6) renders more precise estimates similar to the preferred specification in column (1).

Displacement was defined above as having left the firm within 15 months of notification. This allows for a lag between the firm reporting the notification to the PES and at a later stage notifying the individual worker as well as individual worker's notification times. Nevertheless, there is a dynamic dimension to this first stage as some workers may separate from the establishment later and some workers may be recalled. Moreover, if the firm was doing poorly, future layoffs might be expected which should affect workers who just managed to keep their employment during the first downsizing event. To investigate whether the difference in employment at the notifying firm persists over time, I take advantage of the monthly markers provided by employers along with the annual income statement to trace the dynamic pattern of when workers separate from the notifying firm.

Figure 1.4 plots the results from 48 separate RD regressions for each month relative to the month of notification where I regress the monthly indicator of having separated from the notifying firm on the instrument Z_{ic}. For each time point, I plot the constant (hollow circles) and constant+γ (solid circles) estimate of equation (1.3) which corresponds to the predicted value of separation for workers just below and above of the threshold, respectively. The red dashed vertical line indicates signifi-

Figure 1.4. Probability of having left notifying firm relative month of notification



Notes: The figure shows the probability of having left the notifying firm for a given month relative the month of notification. For each time point, I plot the constant (hollow circles) and constant+γ (solid circles) estimate of equation (1.3) which corresponds to the average predicted value of each outcome for workers just to the left and right of the threshold, respectively. The dashed vertical line indicates significance at the 5-percent level where standard errors are clustered at the order circuit level. The regressions include a linear polynomial function interacted with the threshold as well as order circuit fixed effects.

cance at the 5-percent level, clustering the standard errors at the level of the order circuit. Due to statutory notification times workers just below and above the threshold start to diverge after 3 months, which is the average notification time in my sample. The gap widens up until about 12 months (99th percentile in notification times) where it stabilizes around an 10 percentage point difference. Importantly, the difference in probability of separation (i.e., not working at the notifying firm in a given month) remains stable throughout. Hence, it does not seem to be the case that workers are getting displaced and later recalled to any large extent, nor that workers surviving a first layoff notification later become displaced in additional notifications.[19]

[19] Figure A-1.4 shows the probability of recall within 2 years after notification by relative seniority ranking where there is no significant difference crossing the threshold.

1.5 Consequences of layoff for workers

This section investigates earnings losses upon job loss induced by the LIFO rule. I then proceed by decomposing the overall earnings effect into seperate estimates for employment, wages and hours. The section ends by evaluating the relative importance of these three channels for the total earnings loss.

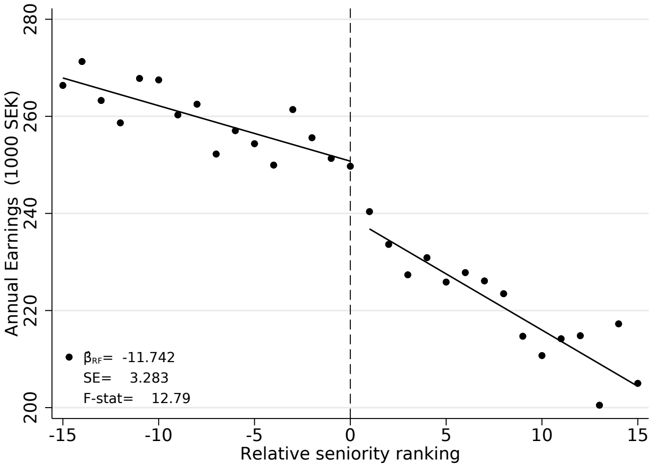
1.5.1 The total earnings effect

Figure 1.5 (a) gives a snapshot of annual earnings around the threshold in the year after notification.[20] There is a clear downward jump at the discontinuity, where workers just surpassing the threshold earn on average -11.74 thousand SEK less then their coworkers just below the threshold who remained employed at the notifying firm to a greater extent. The effect is precisely estimated with a standard error (SE) of 3.28 which is significant at the 1 percent level. Adding a second order polynomial barely changes the estimate -11.29 ($SE = 4.75$; $p = 0.018$). Figure 1.5 (b) plots the earnings differential but four years after notification and even here the difference in annual earnings remains but has become somewhat smaller with workers above the threshold earning on average 9.31 thousand SEK less then workers just below. Not surprisingly, the variance in earnings has increased considerably compared to the first year after notification. The slope of the control function has also changed which likely reflects that the displaced workers' current earnings no longer correlates with their previous relative ranking within the (old) notifying firm.

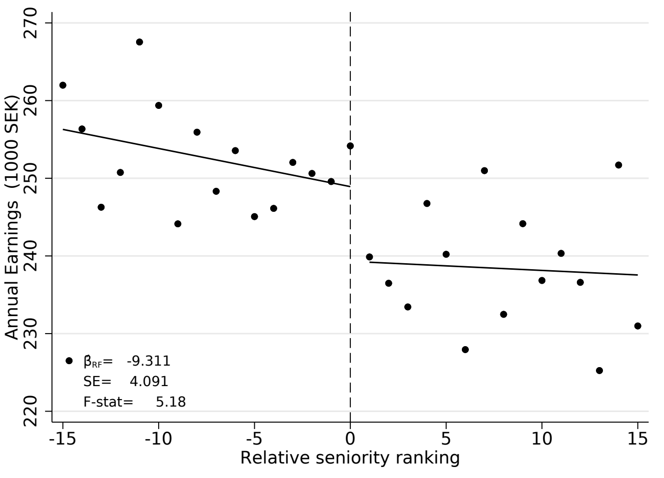
[20] All earnings and wages have been deflated to 2005 values in thousands of Swedish krona (SEK). One thousand SEK roughly corresponds to 110 US dollar or 97 Euros.

FIGURE 1.5. ANNUAL EARNINGS BY RELATIVE SENIORITY RANKING

(a) *1 year after notification*

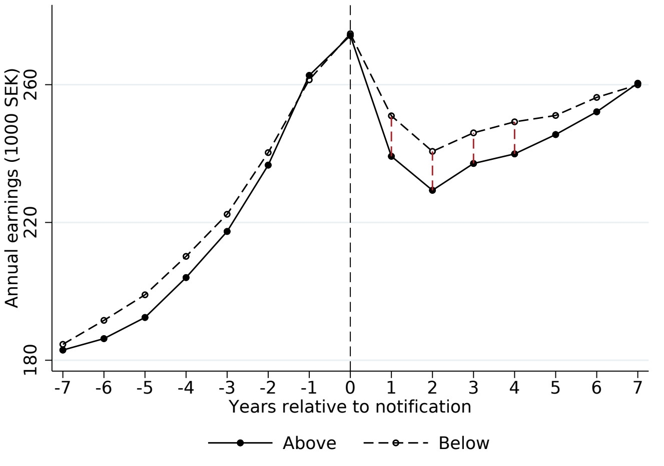


(b) *4 years after notification*



Notes: The figure shows annual earnings (in 1000 SEK's) in (a) the first year and (b) four years after notification as a function of workers' relative ranking within an order circuit (in discrete bins), normalized to zero at the cut-off. The regression include a linear polynomial function interacted with the threshold as well as order circuit fixed effects and is run using a bandwidth of ±15. Standard errors are clustered at the level of the order circuit.

FIGURE 1.6. EVOLUTION OF ANNUAL EARNINGS RELATIVE TO YEAR OF NOTIFICATION



Notes: The figure shows annual earnings relative to the year of notification. For each time point, I plot the constant (hollow circles) and constant+γ (solid circles) estimate of equation (1.3) with annual earnings as the dependent variable which corresponds to the average predicted value of each outcome for workers to the left and right of the threshold, respectively. The dashed vertical line indicates significance at the 5-percent level where standard errors are clustered at the order circuit level. The regressions include a linear polynomial function interacted with the threshold as well as order circuit fixed effects.

To trace out the dynamic response and examine the persistence of these earnings losses, Figure 1.6 shows the evolution of annual earnings for workers just above (solid circles) and below (hollow circles) the threshold by year relative to notification. Again, each time point corresponds to a separate RD plotting the intercepts of the reduced form estimate (e.g, the estimate in $t + 1$ and $t + 4$ corresponds to Figure 1.5 (a) and (b), respectively). First, we note that up to seven years prior to the notification event, annual earnings evolves in parallel for workers at the threshold which again provides reassurance that there is no manipulation and selective firing among employers.[21] Following the layoff notification in $t = 0$, earnings drop and due to the "fuzzy" nature of the LIFO rule, it does so for workers both to the left and right of the threshold. The drop is, however, significantly larger for work-

[21]Cumulated annual earnings over the 6 years pre notification are also not different for those above and below the threshold.

ers just surpassing the LIFO threshold where the difference stems from workers/circuits complying with the instrument.

The largest losses occur within the two subsequent years following notification where workers just surpassing the threshold earn about 11.3 thousand SEK less than workers just below the threshold. After two years both groups recover and increase their annual earnings. There is still, however, a significant earnings differential between the two groups estimated to -8.89 (SE = 3.76) and -9.37 (SE = 4.09) in year 3 and 4, respectively. Having a relative ranking just above the cut-off and hence having a 10 percentage point higher probability of layoff have a significant negative impact on a workers' annual earnings even 4 years after notification. Thereafter, workers start to recover and those just above the threshold do so at a faster rate, completely closing the earnings gap in year 7 after notification where the estimated difference is 0.45 ($SE = 5.27$).[22]

The above results showed the reduced form response, that is the effect of being just above the threshold within an order circuit, thereby having about a 10 percentage point higher likelihood of displacement. To quantify the effect of actual displacement, I instrument displacement D_{ic} with the indicator for being above the threshold Z_{ic} while controlling linearly for workers relative rank (RR) within a circuit. These estimates should be interpreted as a LATE for those order circuits and workers complying with the LIFO rule. Panel A in Table 1.4 shows IV-estimates on annual earnings each year relative to notification. During the first year after notification, displaced workers loose about 115 thousand SEK on average due to the displacement compared to their non-displaced coworkers which corresponds to a 38 percent loss. Although the losses become smaller over time, compared to their coworkers (who may or may not still be at the notifying firm) the earnings differential is still significant and substantial up to 4 years after notification. The earnings gap starts closing in year 5 after notification, and after 7 years the estimated ear-

[22] I investigate the robustness of the results by replicating Figure 1.6 using the optimal bandwidth selector suggested by Calonico, Cattaneo and Titiunik (2014). Figure A-1.10 shows the results from these regressions using both a first and second order polynomial. The results are remarkably stable when using the optimal bandwidth selector even though optimal bandwidths changes over time. The inclusion of a second order polynomial on average decreases the estimated earnings differential but the overall pattern remains consistent and qualitatively the same.

Table 1.4. IV-estimates on earnings by year relative to notification

Panel A	Annual Earnings						
	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$	$t+7$
Displaced	-114.91***	-107.88***	-84.43**	-85.82**	-50.43	-38.03	4.33
	(30.58)	(32.86)	(35.04)	(37.47)	(39.03)	(44.84)	(50.52)
Control mean	299	286	282	286	273	273	258
% of Control	-0.38	-0.38	-0.30	-0.30	-0.18	-0.14	0.02
Panel B	Annual Earnings + UI						
	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$	$t+7$
Displaced	-55.95**	-86.88***	-82.45**	-88.92**	-52.76	-37.34	-1.55
	(27.57)	(31.01)	(33.68)	(36.42)	(38.16)	(43.89)	(49.56)
Control mean	289	289	288	294	281	278	265
% of Control	-0.19	-0.30	-0.29	-0.30	-0.19	-0.13	-0.01
F-statistic	49	50	48	48	41	35	31
N	16,747	16,431	15,774	14,950	13,121	11,727	11,208

Note: The table shows IV estimates on annual earnings (top row), annual earnings including UI payments (bottom row) by year relative to notification. Displacement has been instrumented with being just above the threshold. The bottom of the table show the first stage F-statistic. All regressions include a first order polynomial function interacted with the threshold and order circuit fixed effects. Standard errors clustered at the level of the order circuit and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

nings gap is even slightly positive although statistically indistinguishable from zero.

To get closer to the cost of job loss for the worker, I add UI payments in panel B of Table 1.4. An argument brought forward against, e.g., relaxing employment protection is that it may generate uncertainty for workers about their future income stream. Others have instead argued that generous unemployment insurance (UI) benefits will compensate workers loss of employment protection. As such, more flexibility in the labor market can be achieved while at the same time workers are insured against transitory income shocks. Sweden has with its 60 weeks coverage and 80-70 percent replacement rate (subject to a cap) one of the most generous UI schemes. As can be seen in the lower panel of Table 1.4, the transitory income shock is indeed offset by UI payments, particularly in the first year after notification where some 50 percent of losses are now covered by UI. As expected, the offsetting force decreases over time and after 3 years, UI coverage has run out. Thus while UI appears to dampen workers' earnings losses during the first year it does not fully offset the workers loss of income.

What drives this earnings differential? Although the earnings gap has closed by year 7 after notification, displaced workers have on average forgone about 420 thousand SEK in pre tax earnings during the 7 years post displacement. Trivially, as workers surpassing the threshold have a higher likelihood of getting laid off, a large part of the earnings differential should be driven by non-employment. Displaced workers may also incur lower wages and/or face more volatile employment in terms of e.g. fewer hours. Following Cederlöf et al. (2019), the difference in earnings between displaced (D) and non-displaced (S) workers at the firm can be written as,

$$\Delta y = w^D h^D l^D - w^S h^S l^S \tag{1.6}$$

where w and h is the hourly wage rate and hours worked during a month, respectively, whereas l is the number of months worked during a year. This expression can be rewritten as,

$$\Delta y = w^S h^S \underbrace{(l^D - l^S)}_{\text{Extensive margin}} + l^D [h^s \underbrace{(w^D - w^S)}_{\text{Wage effect}} + w^D \underbrace{(h^D - h^S)}_{\text{Intensive margin}}] \tag{1.7}$$

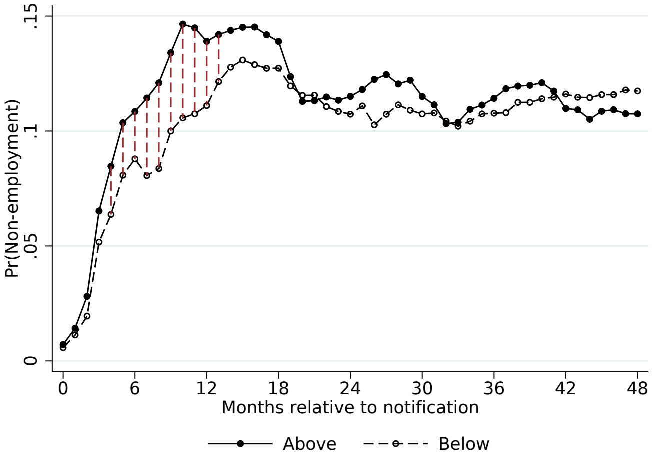
where the first component reflects the part of the earnings differential stemming from differences in employment. The second and third component reflects the possibility that displaced workers may end up in employment paying lower wages or providing fewer hours. This decomposition may be applied either separately for each year or averaged over some fixed time interval T. In the remainder of section 1.5, I unpack the total earnings effect and estimate the effect of layoff separately on employment, wages and hours worked averaged over a period of three years. I then put these pieces of evidence together and decompose the earnings losses using equation (1.7) to evaluate the relative importance of each adjustment margin.

1.5.2 Earnings losses by adjustment margins

Extensive margin Separation from the notifying firm does not mechanically induce non-employment as workers may well find work, e.g., within their notification period. To estimate the effect on non-employment I take advantage of the monthly employment markers to trace out the dynamic response. The monthly markers are noisy measures of labor supply due to some employers (both incumbent and new ones) routinely reporting workers having started work in January while the actual employment began in, e.g., March. As this type of measurement error may be more common among displaced workers, estimates should be interpreted as a lower bound.

Figure 1.7 plots the reduced form probability of non-employment by month relative to notification where again each time point is a separate RD regression. Just as with separation from the notifying firm, non-employment starts to appear 3 months after notification. After 4 months the employment gap between workers just above and below the threshold turns significant (as indicated by the vertical dashed red line) and remains so for up to 13 months after notification. During this time, workers just surpassing the threshold have about a 3–4 percentage points higher probability of being non-employed compared to workers within the same order circuit where just below the threshold. Scaling these estimates with the first stage implies that workers that are displaced are about 30-40 percent more likely to experience non-employment during month 4 to 13 after notification. There are some suggestive evidence that

FIGURE 1.7. PROBABILITY OF NON-EMPLOYMENT BY MONTH RELATIVE TO NOTIFICATION



Notes: The figure show the probability of non-employment for a given month relative the month of notification. For each time point, I plot the constant (hollow circles) and constant+γ (solid circles) estimate of equation (1.3) which corresponds to the average predicted value of each outcome for workers just to the left and right of the threshold, respectively. The dashed vertical line indicates significance at the 5-percent level where standard errors are clustered at the order circuit level. The regressions include a linear polynomial function interacted with the threshold as well as order circuit fixed effects.

these employment differences remain for longer as there is a 2 percentage point difference in the probability of non-employment during month 26 and 27, significant at the 10 percent level. However, from month 30 and onwards there are no differences in the likelihood of non-employment

How many months of non-employment did displacement generate on average? To answer this question I run a regression where cumulated months of employment is the dependent variable, again instrumenting displacement D_{ic} with Z_{ic}. Table 1.5 provides the results from these regressions along with the estimated first stage F-statistic at the bottom of the table. Workers who where displaced, due to the LIFO rule, loose on average around 5 months of employment over 3 years compared to workers who stayed with the firm (at least 15 months after notification). As also indicated by Figure 1.7, there appears to be no differences in employment levels after 3 years since the estimated coefficients on cumulated months of employment becomes smaller and turns insignificant.

Table 1.5. IV-estimates on cumulated months of employment

	(1) $t+1$	(2) $t+2$	(3) $t+3$	(4) $t+4$	(5) $t+5$	(6) $t+6$	(7) $t+7$
Displaced	-3.34***	-4.18***	-5.01**	-4.51	-3.57	-5.65	-9.39
	(0.80)	(1.47)	(2.24)	(2.95)	(3.92)	(4.89)	(5.99)
F-statistic	49	50	48	47	41	35	30
N	16,747	16,431	15,774	14,950	13,121	11,727	11,208

Note: The table shows IV estimates on cumulated number of months in non employment by year relative to notification. Displacement has been instrumented with being just above the threshold. The bottom of the table show the first stage F-statistic. All regressions include a first order polynomial function interacted with the threshold and order circuit fixed effects. Standard errors clustered at the level of the order circuit and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Wage effect Some studies finding persistent earnings losses following displacement have also found that workers suffer considerable wage cuts when switching jobs. E.g. Schmieder, von Wachter and Heining (2018) show that even though employment plays an important role for long-term earnings losses, a majority of these losses can be attributed to workers receiving lower wages at their new employers. To estimate differences in wages between displaced and non-displaced workers I use the Swedish wage register which is an annual survey covering a large share of the private sector workers and all public sector workers. These data contain information on hours worked and full-time equivalent wages conditional on the worker working at least one hour during a sampling week between September-November when the data is collected. Due to the wage register being a random sample of the working population not all employed workers are observed each year thus reducing the sample size substantially. To attain more precession, I pool data and estimate the effect on wages averaged over three or six years which gives a sample containing about 60 percent of the original sample. Moreover, as there are no employment differences beyond three years, considering averages over this period avoids the estimates from being influenced by sample selection.

Columns (1) and (2) of Table A-1.3 show differences in full-time equivalent monthly wages, averaged over three and six years post notification, respectively. It appears to have been some adjustment of wages on the short run as workers just above the threshold earn on average 460 SEK

TABLE 1.6. CUMULATED AVERAGE EARNINGS LOSSES AFTER 3 YEARS BY MARGINS OF ADJUSMENT

		Adjustment margins		
	Average total earnings loss	Months worked	Monthly wage	Monthly hours worked
	(1)	(2)	(3)	(4)
Displaced	-108.93***	-1.67**	-4.65**	-17.06
	(29.38)	(0.75)	(2.29)	(15.49)
Control mean	291.4	11.4	22.9	140.7
% of Control	-0.37	-0.15	-0.20	-0.12
F-statistic	48	47	26	26
# clusters	585	585	570	570
N	15,774	15,774	9,645	9,645

Note: The table shows IV estimates on worker outcomes cumulated and averaged over 3 years post notification. Earnings and wages are in thousands of Swedish krona in 2005 values. Displacement has been instrumented with being just above the threshold. The bottom of the table show the first stage F-statistic. All regressions include a first order polynomial function interacted with the threshold and order circuit fixed effects. Standard errors clustered at the level of the order circuit and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level, *** $p < 0.01$ level.

less then workers just below the threshold. However, this difference is both smaller and insignificant when considering average wages over six years. This indicates that displaced workers may accept lower wages initially but that these wages tend to rise more rapidly than for those of the control group.

Hours response Similar to the analysis on wages, I use the Swedish wage register to estimate differences in hours worked for workers just above and below the threshold, pooling data over 3 and 6 years. Columns (3) and (4) show the reduced form effect on hours. Interpreting the estimates at face value suggest that workers just above the threshold who where displaced to a larger extent works fewer hours during the first 3 years after notification but that these differences disappear after 6 years. However, the estimates are very imprecise and not significantly different from zero at conventional levels. This suggest that hours worked play a minor role in explaining the overall earnings gap.

Summary Table 1.6 summarizes of the effect of job loss averaged over a period of 3 years post notification. Column (1) show estimates from regressing average total earnings loss on displacement D_{ic} which has been instrumented with Z_{ic}. This corresponds to the weighted average of columns (1)-(3) in Table 1.4 showing that displaced workers having forgone about 109 thousand SEK each year 3 years after job loss. Similarly, columns (2)-(4) show IV-estimates for each adjustment margin. Column (2) of Table 1.6 indicates that displaced workers work on average 1.7 months less during the 3 years post displacement and for those who become employed face lower wages by about 4,650 SEK which corresponds to 20 percent lower wages compared to the non-displaced workers below the threshold. Again, since this effect is measured over a period where employment probabilities between the two groups have been equalized, both groups should be comparable and thus the estimate should reflect a pure wage effect absent of selection. The hours response is again very imprecisely estimated and thus I interpret this effect to be of minor importance in explaining the total earnings loss.

1.5.3 Decomposing the earnings effect

I now return to the decomposition of the difference in earnings as described in equation (1.7) considering a period of 3 years ($T = 3$). I use of the estimates from section 1.5.2 to determine the relative contribution of each adjustment margin. Plugging in the estimates into equation (1.7) yields

$$-109 \approx \underbrace{23 \overbrace{(-1.67)}^{\text{Extensive margin}}}_{35\%} + 9.7[\underbrace{140 \overbrace{(-0.033)}^{\text{Wage effect}}}_{42\%} + \underbrace{0.13 \overbrace{(-17.1)}^{\text{Intensive margin}}}_{19\%}] + \underbrace{\varepsilon}_{3\%} \tag{1.8}$$

The total earnings effect on the left-hand-side of equation (1.8) comes from column (1) of Table 1.6. The first component on the left-hand-side is taken from column (2) of Table 1.6 which also is a 3 year weighted average of estimates in Table 1.5. Estimates for the second and third component of the equation are taken from columns (3) and (4) of Table

1.6, respectively.[23] Over 3 years, about 35 percent of the average losses incurred by a displaced worker can be attributed to non-employment whereas 42 percent to lower wages. Plugging in the point estimate for hours worked suggest that about 19 percent of the earnings loss comes from reductions in hours. As I take the left-hand-side of the equation as given and try to predict it by separate estimates from each margin of adjustment I end up with a residual, being the difference between the estimated total earnings loss and that predicted jointly by the three adjustment margins. The share of earnings loss is left unexplained when joining the separate predictions is 3 percent but since the hours response is so imprecisely estimated the residual may be as big as 19+3=22%.

1.6 Understanding earnings losses upon job loss

The main finding in section 1.5, that earnings losses upon displacement are transitory, rather than persistent, appears to be at odds with previous literature which has found that displaced workers suffer both short and long run earnings losses. As noted above, the finding of persistent earnings losses have also been replicated for several countries over multiple time horizons.[24] For example, Sullivan and von Wachter (2009), Davis and von Wachter (2011), Lachowska, Mas and Woodbury (2018) all find earnings losses in the United States ranging between 15-20 percent up to 20 years after displacement. Although somewhat lower, Schmieder, von Wachter and Heining (2018) and Eliason and Storrie (2006), Seim (2019) find the same results in Germany and Sweden, respectively.

This begs the question, why are the long-run earnings losses so different in this setting? Four potential explanations comes to mind. First, relying on the LIFO rule for quasi-experimental variation renders a different composition of workers than the mass layoff strategy, as shown in section 1.3.1. The LIFO rule places restrictions on employers regarding who to displace, identification makes conditioning on (male) workers

[23]Since the wage effect in equation (1.7) is expressed in hourly wages whereas the estimated wage differential pertains to full-time equivalent wage, I divide this estimate by 140 ($\frac{4.65}{140} = 0.033$) which corresponds to the second component in equation (1.8).

[24]Table A-1.1 lists some of the most influential and most recent studies estimating earnings losses upon displacement.

having high tenure superfluous thereby altering the composition of displaced workers which become more representative for the average laid off worker as seen in Table A-3.1. Second, the LIFO rule alters the selection of workers which in turn could mitigate a negative signal sent to prospective employers (Gibbons and Katz, 1991). In fact Sorensen (2018) finds smaller, although still largely persistent, earnings losses for workers likely to have gotten laid off due to a tenure rule. Third, extraordinary events such as a mass layoff may amplify long-run earnings losses if such an event gives rise to general equilibrium effects by, e.g., creating labor congestion on the local labor market. Fourth, as mass layoff more often occur in economic downturns, estimates on job loss may be influenced by economic conditions. For example, Schmieder, von Wachter and Heining (2018) and Davis and von Wachter (2011) show that earnings losses are larger and more persistent in economic downturns.

To better understand the causes for earnings losses upon job loss and what determines its persistence, I begin by estimating earnings losses applying the canonical event study approach where I define a new sample of mass layoffs from the matched employer-employee data using the standard restrictions in the literature. The goal of this analysis is to provide estimates that are as comparable as possible with previous studies from the United States, and elsewhere, and to verify that there is nothing special in the Swedish context or time period making earnings losses transitory. As I find highly persistent earnings losses from this analysis, I turn to exploiting the large heterogeneity across layoffs in my data. I do this by conducting the RD-analysis separately for each layoff. I then examine whether these estimated earnings losses vary systematically with characteristics of the marginal worker within the circuit, other characteristics of the circuit or plant, as well as other measures which reflect the current state of the labor market. These correlations should be informative about when and where displaced workers suffer the largest earnings losses and what correlates with high persistence of these losses. I end the section by trying to separate partial from general equilibrium effects generated. Large layoffs may generate general equilibrium effects so I exploit geographical variation in the size of layoff relative to the local labor market.

1.6.1 Estimating earnings losses using mass layoff

Using the matched employer-employee data, I follow Jacobson, Lalonde and Sullivan (1993) and define a mass layoff to be an event where at least 30 percent of the workforce leaves a plant within in a year t.[25] To be sure that 30 percent is indeed a significant event, I consider only plants with 50 or more employees in a given year as smaller firms are subject to larger percentage fluctuations in employment. I define a worker's main employer as being the one which gives him the highest earnings in a year and the worker is displaced if he is notified in year t and leaves the plant between year t and $t+1$ or $t+2$.[26] Finally, to facilitate comparisons with the earlier literature I consider only male workers between age 25 to 50 with at least 6 years of (consecutive) tenure.[27]

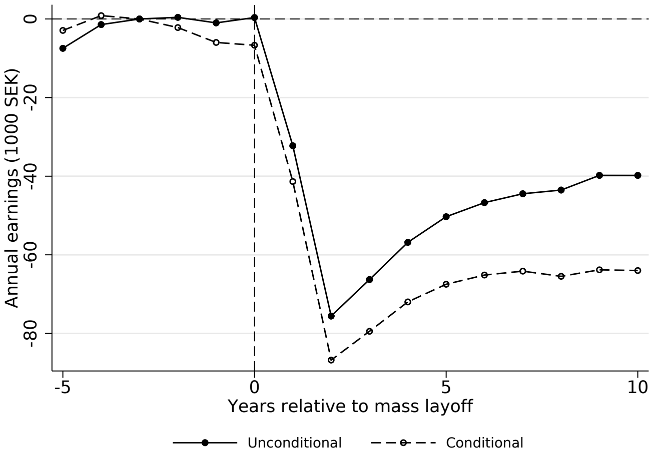
To create a control group for displaced workers, I take a 10 percent sample of all workers from plants which did not carry out a mass layoff. I then restrict attention to workers satisfying the baseline restrictions made on the displaced workers and use propensity score matching and match workers on 3-digit industry, tenure, age, level of education and earnings in $t-2, t-3$ and $t-4$, by each year of displacement. Using a nearest-neighbor algorithm each displaced worker is then assigned a comparison worker (without replacement) in a non-displacing firm. This yields a group of non-displaced workers that are almost identical to workers who eventually will become displaced as can be seen in Table A-1.4. A common, although debated, restriction made on the control group is conditioning them staying employed throughout the entire sample period. As highlighted by Krolikowski (2018), this renders one to attribute all future job instability of the treated workers to the initial displacement thus exaggerating the impact of displacement on earnings

[25] Some studies define displaced workers as workers leaving the firm within 1-2 years prior to the mass layoff event in order to account for possible initial selection.

[26] A main obstacle in the literature is the inability to separate between involuntary and voluntary separations. This is also the main reason for focusing on high tenured workers. I make use of the notification data and define a displaced worker as getting notified and leaving the plant in $t+1$ or $t+2$. Figure A-1.5 show results when defining a worker as being displaced as the plant in $t+1$ or $t+2$ without conditioning on being notified. Indeed, it turns out that earnings losses are smaller and hence the standard definition on a displaced worker has an upward bias as it may include voluntary quits.

[27] See Table A-1.1 for a summary of restrictions made in some of the most cited and recent studies estimating earnings losses using mass layoffs.

FIGURE 1.8. EARNINGS LOSSES UPON MASS LAYOFF



Notes: The figure shows the difference of annual earnings between displaced and non-displaced workers by years relative to a mass layoff event where annual earnings is normalized to zero at $t-3$. Displacement is defined as a worker being notified and leaving the plant in year $t+1$ or $t+2$ during a mass layoff event. The plotted estimates are δ_k from equation (1.9) where the solid black line show losses of displaced workers when not conditioning on the control group being employed in $t>0$. The dashed black line conditions on the control group being employed throughout the entire sample period.

losses. I therefore compare displaced workers with and without this restriction in order to align with the previous literature.

Following standard procedure, I estimate earnings losses upon displacement using a distributed lag model of the form

$$y_{it} = \gamma_t + \alpha_i + \theta_{it} + \sum_{k=-5}^{10} \delta_k D_{it}^k + u_{it} \tag{1.9}$$

where y_{it} is annual earnings for worker i in year t. γ_t and α_i are calendar-year and worker fixed effects, respectively. As the control group is created separately for each year of displacement, I also include displacement year fixed effects θ_{it}. The D_{it} are dummy variables equal to 1 for the k^{th} year before or after displacement where $k=-3$ is the omitted category. The coefficient of interest is δ_k which reflect differences in annual earnings between displaced and non-displaced workers by each year relative to the year prior the mass layoff event.

Figure 1.8 shows the difference in earnings by pooling workers displaced 2005-2015 along with their matched non-displaced workers. Due to propensity score matching, both groups have almost identical trends in annual earnings in the pre-displacement period suggesting that the matching procedure has created a comparable control group. The solid black line shows the earnings differential when not conditioning on future employment for the control group whereas the dashed line has the control group being employed with his main employer throughout the sample period.

Initial earnings losses of displaced workers amounts to a little more than 73,000 SEK two years after the layoff event which corresponds to about 25 percent of pre-displacement income. As time goes by, displaced workers recover some of their initial losses but even after 10 years the earnings differential between displaced and non-displaced workers are on average about 41,000 SEK (14.2 percent). These results are very similar to what has previously been found for mass layoffs in Sweden (Seim, 2019, Eliason and Storrie, 2006). In line with Krolikowski (2018), I also find that a key factor in explaining the high persistence of earnings losses lies in the handling of the control group. The dashed line in Figure 1.8 shows estimates conditioning on future employment of the non-displaced workers. This increases earnings losses after 10 years by almost 50 percent (64,000 SEK which is 21.8 percent of pre-displacement earnings). Overall, there appears to be nothing special with the Swedish context or time period which could explain the absence of long term earnings losses in the main analysis. Rather, mass layoffs seem to be something fundamentally different than displacements due to more regular employment adjustments.

1.6.2 Exploiting heterogeneity across layoffs

To better understand what drives earnings losses upon displacement and its persistence, I exploit the large heterogeneity across layoffs in the data. I start by by replicating the RD-analysis separately for each layoff/circuit and estimate earnings losses one and six years after notification. I then regress the estimated (reduced form) earnings differentials for each circuit on characteristics of the notified workers just at the threshold (the marginal workers), characteristics of the plant/circuit, as well as indicators for overall macro economic conditions. Although these estimates contain a lot of noise, they should provide some information about what correlates with both short and long-run earnings losses. What stands out from these correlations is that the size of the layoff appears to be crucial in explaining both the magnitude and persistence of earnings losses.

Table 1.7 shows the results from this exercise: columns (1)-(3) pertain to earnings losses one year after notification while columns (4)-(6) pertain to earnings losses six years after notification. Positive estimates should be interpreted as a reduction in the absolute size of the estimated jump at the threshold whereas negative estimates implies greater displacement losses.

Column (1) and (4) of Table 1.7 focuses on the sample characteristics of the marginal workers and how these correlate with short and long-run losses, respectively. Layoffs where the marginal workers are older tend to have greater earnings losses; each year correlates with an increase (in absolute terms) in the estimated jump by -1.05 thousand SEK. By contrast, circuits with more high tenured workers have a smaller earnings losses. This, however, is most likely an artifact of notifications times being a function of tenure by law and can be as long as one year. For the long run losses, six years after notification, the significant worker characteristic seems to be age. Figure 1.9 (a) plots the reduced form earnings losses 6 years after notification in 20 equally sized bins of the marginal worker age in the order circuit. The solid line depicts the marginal effect of worker age which has a clear downward slope thus indicating that in circuits where the marginal worker is older, the earnings drop at lay off is also larger. This in turn is well in line with previous findings in the mass layoff literature (see e.g. Seim, 2019, Eliason and Storrie, 2006).

Column (2) and (5) adds industry fixed effects as well as characteristics of the layoff which varies at the plant or circuit level. Routineness is an index provided by Acemoglu and Autor (2011) describing the intensity of tasks in each occupation that are considered routine. The phenomenon of routine biased technological change (as documenented by, e.g., Autor, Levy and Murnane, 2003, Goos and Manning, 2007, Acemoglu and Autor, 2011, Goos, Manning and Salomons, 2014, Cortes, 2016) imply that the cost of job loss for workers performing routine

TABLE 1.7. CORRELATION OF EARNINGS DIFFERENTIAL AND LAYOFF CHARATERISTIC

	Dependent variable: RD-estimate					
	1^{st} year after notification			6^{th} year after notification		
	(1)	(2)	(3)	(4)	(5)	(6)
Marginal worker charateristics						
Tenure	2.62***	3.35***	3.25***	1.14	2.14*	1.99
	(0.69)	(0.76)	(0.76)	(1.11)	(1.24)	(1.29)
Age	-1.05**	-0.79*	-0.98**	-1.63***	-1.48**	-1.69***
	(0.42)	(0.41)	(0.43)	(0.60)	(0.60)	(0.62)
Female	1.12	-1.05	-1.42	-14.80	-24.54	-27.34
	(8.77)	(8.94)	(9.06)	(15.81)	(16.86)	(17.57)
Circuit/Plant charateristics						
Share notified						
$< 10\%$		ref.	ref.		ref.	ref.
$10\% - 20\%$		-22.89**	-23.95**		-2.56	-7.70
		(9.80)	(9.77)		(18.04)	(18.31)
$20\% - 30\%$		-18.63*	-19.10*		-19.36	-25.22
		(10.00)	(10.14)		(18.60)	(19.36)
$> 30\%$		-37.55***	-38.22***		-42.90**	-48.91**
		(11.20)	(11.59)		(21.72)	(22.27)
Routineness		-5.73	-6.16		-11.79	-11.50
		(4.49)	(4.49)		(8.10)	(8.14)
Economic indicators						
Δ GDP			2.03			1.53
			(4.19)			(6.86)
Δ Employment rate			-0.50			2.12
			(3.66)			(6.47)
Δ Unemployment rate			-2.73			0.65
			(5.13)			(8.90)
Average outcome		-17.63			-14.33	
Industry FE		✓	✓		✓	✓
Year FE			✓			✓
N	553	547	547	390	390	390

Notes: The table show estimates from pooling RD-estimates of the earnings differential estimated separately for each order circuit and regressing these on characteristics for the marginal workers within the circuit, plant or circuit characteristics as well as macro economic indicators which reflect the current state oft he labor market at the time of notification. Standard errors are robust to heteroskedasticity and and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

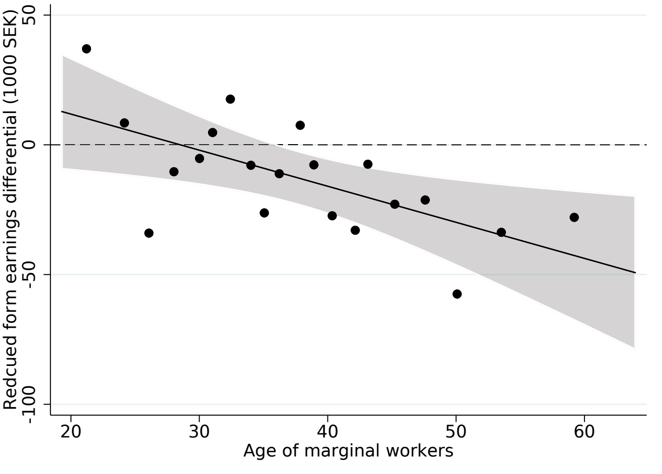
tasks would be greater as demand for there services are generally low. Although insignificant, displaced workers performing routine tasks also seem to have greater earnings losses both in the short and long-run as a 1 standard deviation increase in routineness correlates with an increased earnings gap of about 12 thousand SEK after six years.

To see how earnings losses vary with the size of the layoff, column (2) and (5) also add the share of notified workers at the establishment. In reference to layoffs where less than 10 percent of the workforce is notified, layoffs exceeding 30 percent of the workforce are associated with an estimated earnings differential of about -38 thousand SEK in the year after notification. Examining the persistence of these earnings losses in column (5), reveals an interesting pattern. Although increasing in the size of layoff, there are no significant differences in earnings between establishments that notify $< 10, 10-20, 20-30$ percent of their workforce. However, for layoffs where more than 30 percent of the workforce are notified, earnings losses appear to be far more persistent and significantly so. One may be worried that this is primarily driven by businesses cycles as larger layoffs tend to occur during poor economic conditions, but controlling for macro economic conditions in column (3) and (6) and adding fixed effects for the year of notification does not alter the pattern.

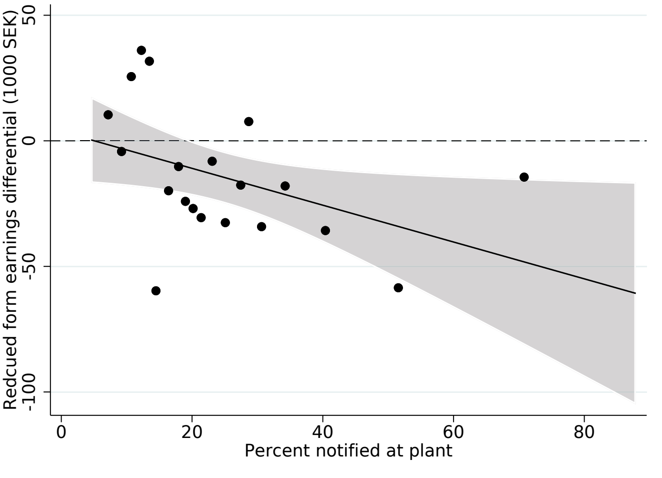
From this exercise one of the four above mentioned potential explanations to the difference in results between the mass layoff estimates and the RD-analysis stands out, namely the size of the layoff. Figure 1.9 (b) probes this result further by plotting the reduced form earnings losses 6 years after notification by the percentage notified at the plant. It shows that earnings losses increase with the size of the layoff and that the associated earnings losses becomes significant when at least 20% of the workforce are laid off. Importantly, this is not driven by differences in the first stage as there is no correlation between the within circuit first stage coefficient and the size of the layoff as shown in Figure A-1.6. As an additional robustness check, Figure A-1.7 in Appendix A plots the reduced form earnings differential estimated by pooling layoffs by the share of workers notified at the plant where standard errors are clustered at the level of the order circuit. The results confirm that not only are layoffs that exceed 30 percent different from smaller layoffs, but they

FIGURE 1.9. MARGINAL EFFECT ON EARNINGS LOSSES BY LAYOFF CHARACTERISTICS

(a) *Age of marginal workers*



(b) *Size of layoff*



Notes: The line in the figure shows the marginal effect on reduced form estimated earnings differentials by (a) age of the marginal workers and (b) the percent notified at the establishment. The solid circles show in 20 equally sized bins estimated earnings differentials which are estimated by regressing annual earnings on a dummy for being above the LIFO threshold while including a linear polynomial function interacted with the threshold as well as order circuit fixed effects, using a bandwidth of ±15.

are also the only ones where persistent earnings losses different from zero can be found.

1.6.3 General equilibrium effects

Why do larger layoffs bring about greater earnings losses? On the one hand, at the individual level, larger layoffs should reduce negative signaling by employers which in turn should reduce workers earnings and employment losses (Gibbons and Katz, 1991). On the other hand, large layoffs may cause workers to loose more human capital if larger layoffs are more common in industries where the degree of firm specific human capital is high. On the aggregate level, mass layoffs tend to be more frequent during periods of economic distress and it has been documented by, e.g., Davis and von Wachter (2011), Schmieder, von Wachter and Heining (2018) that earnings losses are both larger and more persistent when workers are laid off in economic recessions. However, to what extent these differences are driven by individual factors as different kinds of workers are displaced in booms compared to busts, is largely unknown. Moreover, large layoffs themselves may have negative externalities thereby amplifying earnings losses by general equilibrium effects caused by, e.g., congestion of labor in the local labor market. The answer to what drives these losses has consequences for the appropriate policy response. If individual worker factors are the main cause, targeting disadvantaged workers in risk of high earnings losses may be beneficial. If, however, earnings losses are due to general equilibrium effects, extensions of UI or financial help for firms to hoard labor may be a more appropriate response.

Section 1.6.2 shows that the estimated earnings losses are insensitive to controlling for characteristics of the marginal workers as well as macro economic conditions. The underlying mechanism is, thus, likely to be that large layoffs themselves generate negative general equilibrium effects in the local labor market. In an attempt to separate the partial effect of displacement which directly affect the displaced workers through, e.g., signaling or the loss of human capital, from that of the general equilibrium effect indirectly affecting workers, I make use of the fact that layoffs vary in their size relative to the local labor market. Not only is there substantial heterogeneity in the size of the layoff relative to the size of the establishment but there is also considerable geographical heterogeneity across layoffs. Figure A-1.8 (a) shows, for layoffs in 2008-2009, how the share of notified workers relative to the size of the downsizing establishment varies between local labor markets where, in absence of a better measure, a local labor market is defined to be the municipality. The dark red labor markets are those where the average layoff comprise more than 30 percent of an establishment and thus would constitute a mass layoff. Although, the heavy metal industries are located in the northern part of Sweden, mass layoffs appear to be largely spread out across the country. Figure A-1.8 (b) instead depicts the size of an average layoff in relation to the size of the local labor market defined as the number of notified workers divided by the number of employed workers earning above 10,000 SEK in a year in the local labor market where the establishment operates. Again, the layoff shocks are spread out across the country but for many labor markets where the layoff events were relatively large in relation to the size of the establishment they are in fact small when set in relation to the size of the local labor market whereas others comprise more than 3 percent of total employment. Figure A-1.9 depicts in another way the correlation between the relative size of layoff in relation to establishment versus local labor market size.

Holding constant the size of the layoff relative to the size of the establishment, I can exploit variation in the size of layoffs in relation to the local labor market to decompose the earnings effect driven by the size of layoff in relation to the establishment versus to the local labor market. Table 1.8 shows results from regressing the reduced form RD-estimates of earnings losses after 6 years on the log of the share of notified workers relative to the number of workers in the local labor market. In every regression I control for worker characteristics, year and industry fixed effects and the number of workers notified in the layoff. Column (1) shows how earnings losses change with the size of the layoff where the estimate should be interpreted as increasing the size of the layoff in relation to the size of the local labor market by 1 log point. This correlates with an increase in the earnings differential by about 10.6 thousand SEK. The effect in column (1) likely contain both the partial as well as the general equilibrium effect as it does not hold constant the relative size

TABLE 1.8. GENERAL EQUILIBRIUM VS. PARTIAL EQUILIBRIUM EFFECT

	Dependent variable: Earnings differential in $t+6$				
	(1)	(2)	(3)	(4)	(5)
General equilibrium effect					
$\log\left(\frac{\text{\# notified in layoff}}{\text{\# workers municiplaity}}\right)$	-10.62**	-9.04**	-8.50*	-8.64*	-9.28**
	(4.41)	(4.48)	(4.60)	(4.56)	(4.58)
Share notified at establishment					
Level		-0.81*	-2.25		
		(0.44)	(1.37)		
Level^2			0.02		
			(0.02)		
log()				-23.82**	
				(11.94)	
10% – 20%					4.72
					(19.46)
20% – 30%					-2.92
					(21.54)
> 30%					-34.05
					(23.51)
Worker controls	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
R^2	0.093	0.104	0.107	0.106	0.110
N	433	433	433	433	433

Notes: The table show estimates from pooling RD-estimates of the earnings differential estimated separately for each order circuit and regressing these on the share of notified workers relative to the size of local labor market. The size of the labor market is defined as the number of employed workers earnings above 10,000 SEK during a year. All regressions controls for the absolute size of layoff, characteristics for the marginal workers within the circuit, plant or circuit characteristics as well as macro economic indicators which reflect the current state oft he labor market at the time of notification. Standard errors are robust to heteroskedasticity and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

of the layoff. Columns (2)-(5) controls for the size of the layoff where I vary the functional form of the latter in order to flexibly control for the partial effect. When controlling for these two variables the log size of the layoff relative to the local labor market should reflect the general equilibrium effect. The drop in the coefficient from column (1) to (2)-(5) reflects the size of the partial equilibrium effect.

As can be seen in columns (2)-(5) the functional form of the added control vector is not of major importance but rather the estimate of interests varies little. The most conservative estimate in column (3) suggest that general equilibrium effects account for about 80 percent of the estimated earnings losses ((10.62-8.5)/10.62 =.8). While being approximate, these results suggest that the size of the layoff relative to the local labor market is the main driver of persistence in earnings losses. Note that these are not estimated spillover effects but rather how general equilibrium effects affect the individual worker by, e.g., generating an abundance of labor in the local labor market and therefore prolonging non-employment rendering human capital of the individual worker to depreciate.

1.7 Conclusions

This paper examines the question of how workers are affected by job loss in terms of the future earnings, wages and employment. The empirical approach builds on exploiting the use of a last-in-first-out (LIFO) rule used at layoffs in Sweden. These rules provide quasi-experimental variation in the probability of displacement whereby future earnings of displaced and non-displaced workers are compared in a (fuzzy) regression discontinuity design. Whereas previous evidence on earnings losses upon displacement pertains to (primarily) male, high tenured workers displaced in mass layoffs, this paper studies a broader and more representative population of workers laid-off due to more common and regular adjustments to employment.

The results show that workers experiencing involuntary job loss suffer large and significant earnings losses during the first two years after displacement. These initial losses amount to 38 percent of their non-displaced coworkers' earnings; the losses are mainly attributed to lack of employment, but also lower wages in subsequent jobs. However, as time progresses, earnings of displaced workers rise and 7 years after displacement the earnings gap between displaced and non-displaced workers is closed. Importantly, this is not driven by non-displaced workers getting laid-off to a larger extent at a later point in time.

The finding of earnings losses upon displacement being transitory, rather then persistent, stands in contrast to the conventional wisdom established by a large literature studying worker job loss through mass layoffs. I therefore replicate the canonical mass layoff approach (Jacobson, Lalonde and Sullivan, 1993). Here, I do find earnings losses to be significant and highly persistent which suggests that studying displacement due to mass layoff is fundamentally different than job loss due to more regular employment adjustments. Exploiting heterogeneity across LIFO thresholds, I explore several possible mechanisms that may cause earnings losses to become persistent. I find that while older workers tend to have more persistent earnings losses, the main driver of persistent earnings losses seems to be the size of the layoff. I then ask the question why large layoffs generate persistent earnings losses. The main candidate explanation is that large layoffs generate negative general equilibrium effects in the local labor market. I show that large layoffs relative to the local labor market give rise to large losses, holding constant the size of the displacement relative to the establishment. The results suggest that the observed persistence in earnings losses can mainly be attributed to general equilibrium effects caused be, e.g., congestion in the labor market after the layoff event.

The findings of this paper shed new light on the question on how workers are affected by job loss and whether displacement create lasting scars or merely temporary blemishes. The previous literature suggests lasting scars. My evidence suggests that this may be true for high tenured workers laid off during mass layoffs, earnings losses are temporary when considering more regular and less drastic adjustments to employment. This result has important implications for the targeting of public policy. It also speaks to the modeling the consequences of adverse shocks in the labor market. The way for a mechanism generating long-run persistence at the individual level seems to have been overemphasized.

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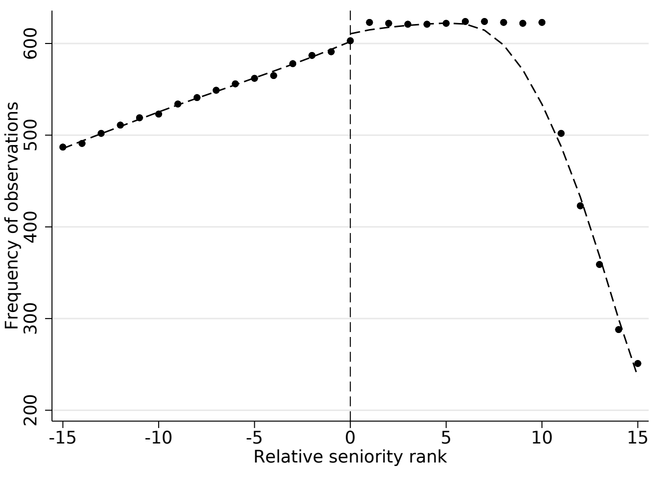
Appendix

A Figures and tables

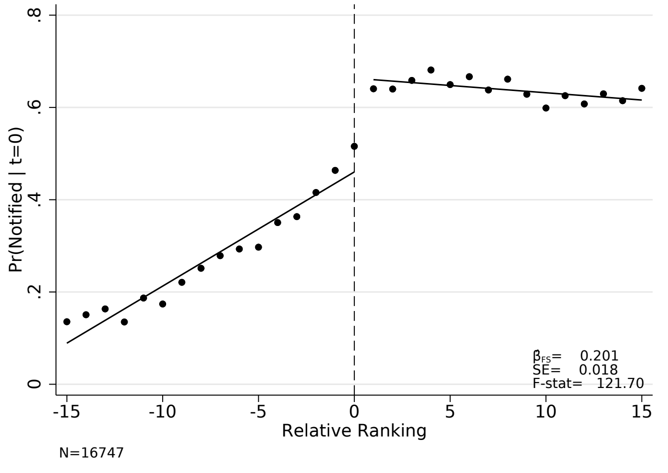
Figure A-1.1. Frequency of observations around the threshold



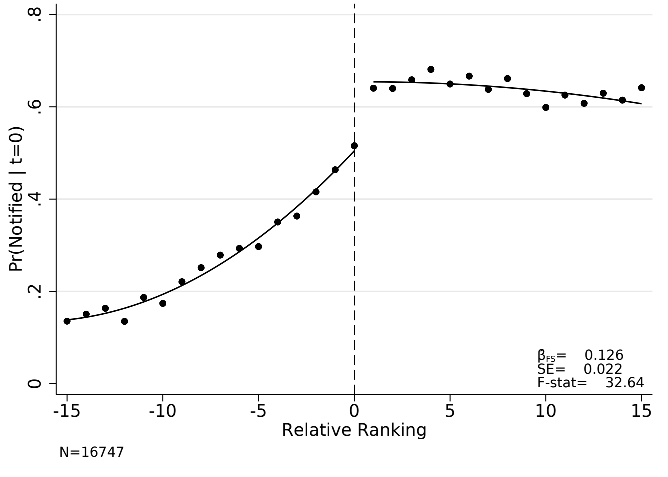
Note: The figure show the frequency of observations around the LIFO threshold.

FIGURE A-1.2. PROBABILITY OF INDIVIDUAL LAYOFF NOTIFICATION

(a) *Linear fit*



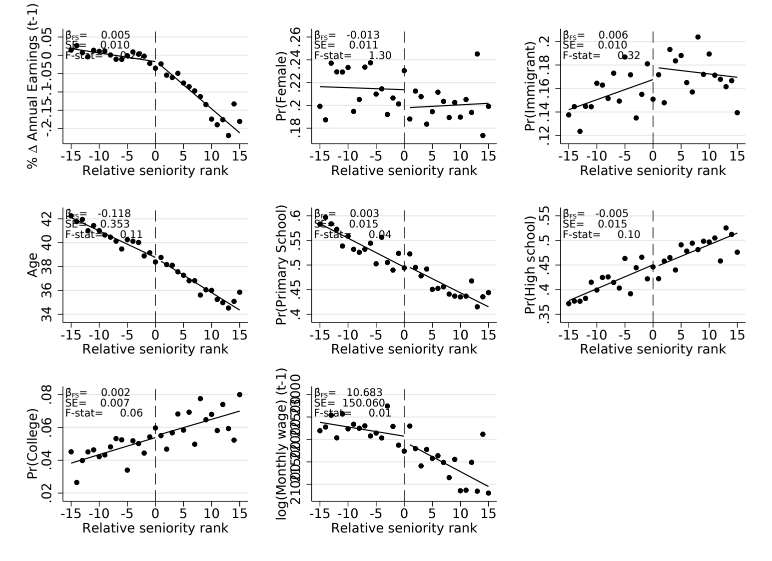
(b) *Quadratic fit*



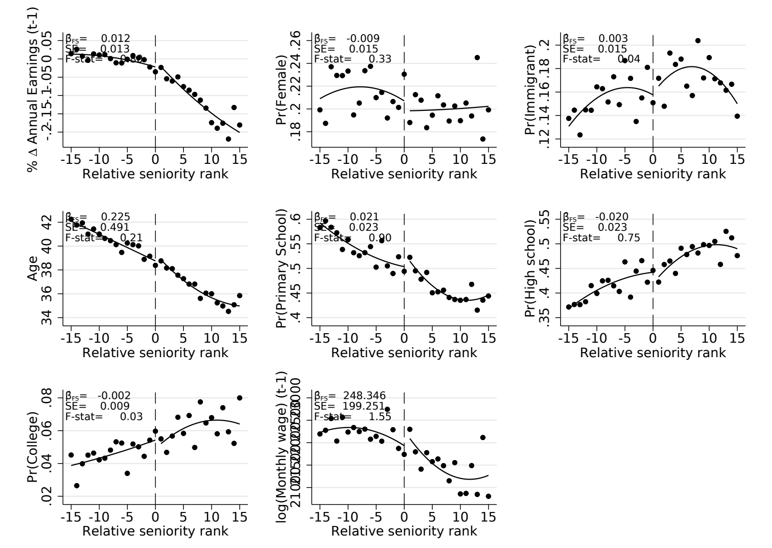
Note: The figures show the probability of receiving a layoff notification as a function of a workers relative ranking within an order circuit (in discrete bins), normalized to zero at the threshold. The regression include in (a) a first order and (b) second order polynomial function interacted with the threshold as well as order circuit fixed effects and is run using a bandwidth of ±15. The point estimate of the jump at the threshold are supplied in the graph along with standard errors clustered at the order circuit level and F-statistic.

Figure A-1.3. Balancing of individual covariates

(a) *Linear fit*

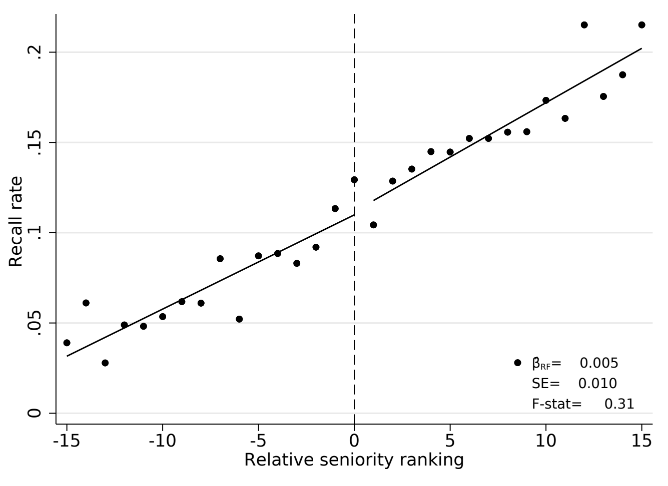


(b) *Quadratic fit*



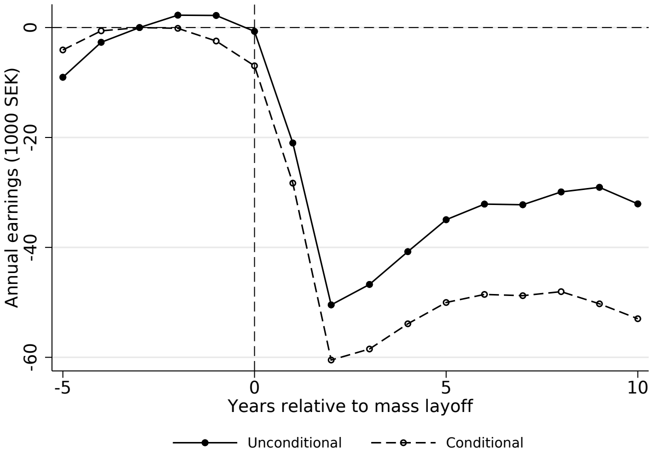
Note: The figures show balancing at the threshold of pre-determined worker characteristics using a (a) first order and (b) second order polynomial function interacted with the threshold as well as order circuit fixed effects and is run using a bandwidth of ±15. The point estimate of the jump at the threshold are supplied in the graph along with standard errors clustered at the order circuit level and F-statistic.

FIGURE A-1.4. PROBABILITY OF RECALL WITHIN 2 YEARS OF NOTIFICATION



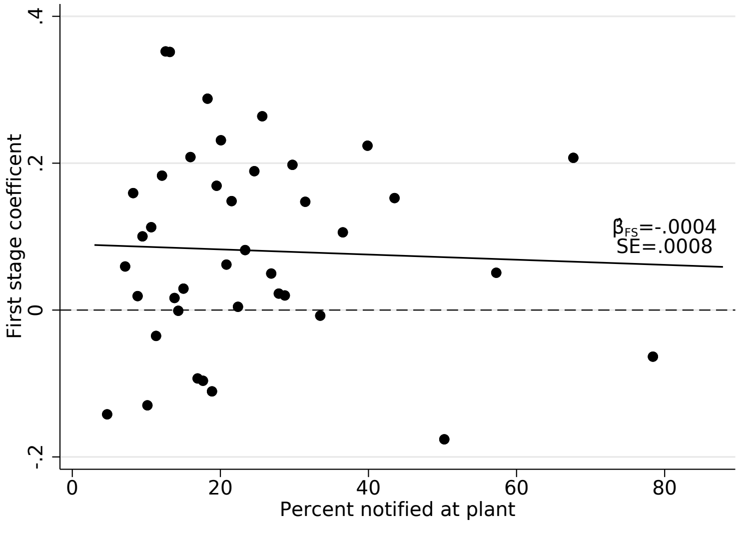
Note: The figure show the probability of workers being recalled by relative seniority ranking. Recalls are defined as leaving and returning to the notifying within 2 years after notification. The regression include a first order polynomial function interacted with the threshold as well as order circuit fixed effects and is run using a bandwidth of ±15. The point estimate of the jump at the threshold are supplied in the graph along with standard errors clustered at the order circuit level and F-statistic.

FIGURE A-1.5. EARNINGS LOSSES UPON MASS LAYOFF INCLUDING NON-NOTIFIED WORKERS



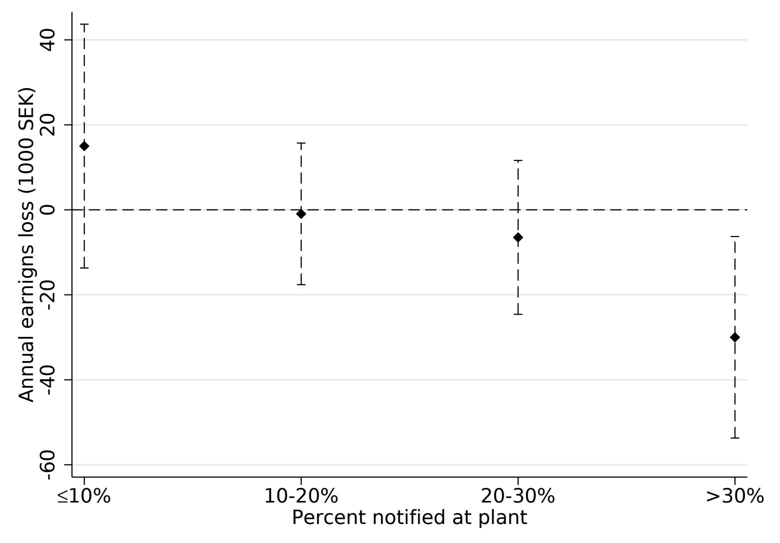
Notes: The figure shows the difference of annual earnings between displaced and non-displaced workers by years relative to a mass layoff event where annual earnings is normalized to zero at $t-3$. Displacement is defined as a worker leaving the plant in year $t+1$ or $t+2$ during a mass layoff event. The plotted estimates are δ_k from equation (1.9) where the solid black line show losses of displaced workers when not conditioning on the control group being employed in $t>0$. The dashed black line conditions on the control group being employed throughout the entire sample period.

Figure A-1.6. Correlation between first stage and size of layoff



Note: The figure shows the within circuit first stage coefficient by the share of workers notified at the plant in 20 equally sized bins. The solid line is the predictions from regressing the first stage coefficient of each circuit on the percent of notified workers in each establishment, using standard errors robust to heteroskedasticity.

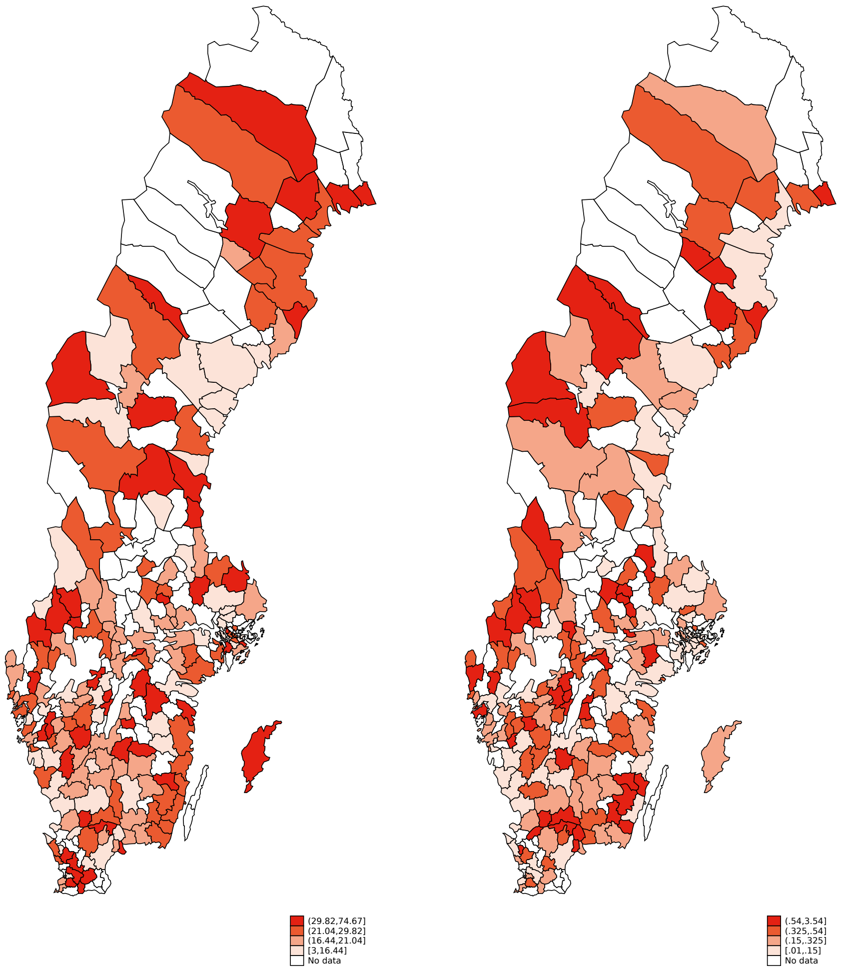
Figure A-1.7. Earnings differential by size of layoff



Note: The figure shows estimated earnings differentials along with 95 percent confidence intervals. Estimates are obtained by pooling order circuits into four categories by size of layoff measured by the share of workers notified within an establishment and separately regressing annual earnings on an indicator for being above the threshold. All regressions include a linear polynomial function interacted with the threshold as well as order circuit fixed effects. Standard errors are clustered at the level of the order circuit.

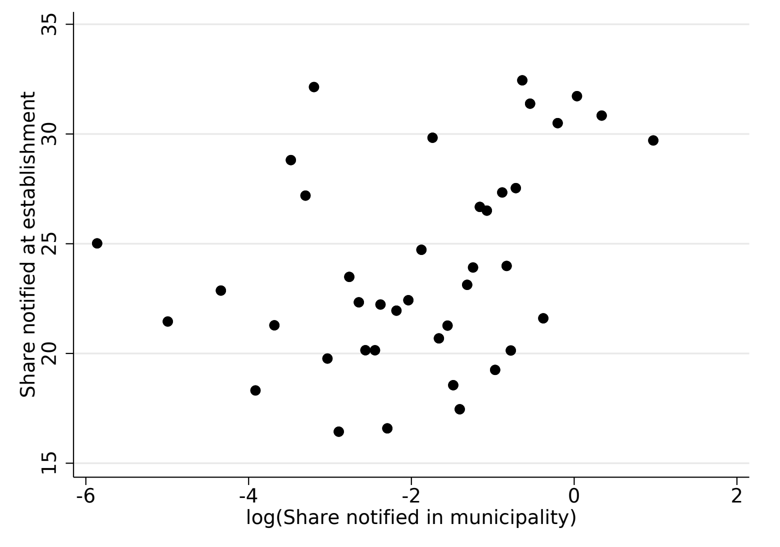
Figure A-1.8. Heterogeneity in layoffs across local labor markets

(a) *Share of establishment* **(b)** *Share of local labor market*



Note: The figure depicts heterogeneity in the size of layoffs by municipalities in Sweden between the years 2008-2009. The size of layoff is measured in (a) as the share of workers laid off within an establishment and in (b) as the number of notified workers divided by the number of employed workers in the municipality earning above 10,000 SEK during a year.

FIGURE A-1.9. CORRELATION BETWEEN ESTABLISHMENT VS. LOCAL LABOR MARKET SIZE OF LAYOFF



Note: The figure shows the correlation between the size of a layoff event measured relative to the size of the establishment versus local labor market in 40 equally sized bins. The size of the local labor market is defined as the number of employed workers in a municipality earnings above 10,000 SEK during a year.

TABLE A-1.1. OVERVIEW OF STUDIES OF JOB DISPLACEMENT

			Data		Sample restrictions (main analysis)			Percent earnings losses	
Author(s) (year)	Journal	Number of citations	Country (state)	Time	Tenure (years)	Gender	Share Laid off	Short-run (years)	Long-run (years)
(Jacobson, Lalonde and Sullivan, 1993)	AER	2291	United States (Pennsylvania)	1975–1985	≥ 6	Males only	$\geq 30\%$	40 (1)	25 (6)
(Sullivan and von Wachter, 2009)	QJE	728	United States (Pennsylvania)	1974–1991	≥ 6	Males only	$\geq 30\%$	40–50 (1)	15–20 (9)
(Davis and von Wachter, 2011)	Brookings papers	451	United States	1974–2008	≥ 3	Males only	$\geq 30\%$	25–39 (1)	15–20 (10-20)
(Couch and Placzek, 2010)	AER	449	United States (Connecticut)	2002–2014	≥ 6	Both	$\geq 30\%$	32–33 (1)	12–15 (6)
(Eliason and Storrie, 2006)	JOLE	249	Sweden	1983–1999	None	Both	100%	11.4* (0)	9.4* (12)
(von Wachter, Song and Manchester, 2009)	IZA WP	98	United States	1974–2004	≥ 6	Male focus	$\geq 30\%$	30 (1)	20 (15–20)
(Lachowska, Mas and Woodbury, 2018)	NBER WP	12	United States Washington	2002–2014	≥ 6	Both	$\geq 30\%$	48 (.25)	16 (5)
(Seim, 2019)	Labour Economics	·	Sweden	1999–2009	≥ 1.5	Males only	$\geq 80\%$	23.5 (1)	16.4 (7)
(Schmieder, von Wachter and Heining, 2018)	Mimeo	·	Germany	1980–2008	≥ 3	Males only	$\geq 30\%$	30 (0)	15 (10)

Notes: The table summarizes results and sample restrictions made in some of the most cited studies using of earnings losses upon job displacement. Papers are listed in order of number of citations which are taken from Google scholar in May 2019. All papers listed above, expect (Eliason and Storrie, 2006), include the additional sample restrictions of establishments/firms having no less than 50 employees at the year of mass layoff.
*The paper only reports nominal losses in Swedish krona so estimates are calculated based upon summary statistics provided in the paper.

TABLE A-1.2. CHARACTERIZING COMPLIERS IN LIFO SAMPLE

	(1) Full LIFO Sample	(2) Compliers	(3) Never-takers	(4) Always-takers
Age	38.58	38.84	38.48	38.47
	(0.09)	(0.32)	(0.21)	(0.24)
Female	0.21	0.21	0.19	0.23
	(0.00)	(0.01)	(0.01)	(0.01)
Tenure	6.47	7.47	5.38	6.81
	(0.04)	(0.15)	(0.09)	(0.10)
Annual earnings (t-1)	25.24	24.44	26.18	24.88
	(0.08)	(0.25)	(0.20)	(0.20)
Level of education				
Primary school	0.50	0.53	0.47	0.50
	(0.00)	(0.01)	(0.01)	(0.01)
High school	0.45	0.44	0.45	0.44
	(0.00)	(0.01)	(0.01)	(0.01)
College	0.05	0.02	0.08	0.05
	(0.00)	(0.01)	(0.00)	(0.00)

Notes: The table shows mean characteristics for different populations, compliers, never-takers and always-takers of the full sample used in the main analysis. The complier analysis follows (Abadie, Angrist and Imbens, 2002, Abadie, 2003). Standard errors calculated by bootstrap using 500 iterations and shown in parentheses.

TABLE A-1.3. REDUCED FORM EFFECT ON WAGES AND HOURS

	Monthly wages		Hours worked	
	(1)	(2)	(3)	(4)
	$t+3$	$t+6$	$t+3$	$t+6$
Above threshold	-0.46**	-0.17	-1.63	0.29
	(0.21)	(0.30)	(1.48)	(1.52)
Control mean	21.13***	21.38***	134.25***	133.27***
	(0.12)	(0.16)	(0.87)	(0.88)
# clusters	550	425	550	425
N	9,625	8,030	9,625	8,030

Note: The table shows reduced form estimates from a regression of wages and hours worked averaged over 3 and 6 years post notification on a dummy for being above the LIFO threshold. The regression include a linear polynomial function interacted with the threshold as well as order circuit fixed effects and is run using a bandwidth of ±15. All regressions include a first order polynomial function interacted with the threshold and order circuit fixed effects. Standard errors clustered at the level of the order circuit and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level, *** $p < 0.01$ level.

Table A-1.4. Characteristics of displaced vs. non-displaced workers in mass layoff sample

	(1) Displaced workers	(2) Non-displaced workers	(3) Difference col. (1)-(2)	(4) p-value
Worker characteristics				
Age	42.47	42.48	-0.02	0.88
Tenure	10.94	11.10	-0.16	0.04
Primary school	0.57	0.59	-0.02	0.01
High school	0.35	0.34	0.01	0.06
College	0.08	0.07	0.00	0.22
Earnings (t-1)	305.46	296.59	8.86	0.00
Earnings (t-2)	297.36	293.10	4.26	0.01
Earnings (t-3)	291.72	288.05	3.67	0.01
Industry shares				
Agricultural	0.00	0.00	0.00	0.38
Mining	0.00	0.00	0.00	0.83
Manufacturing	0.61	0.61	0.00	0.95
Construction	0.06	0.06	-0.00	0.76
Retail	0.10	0.10	-0.00	0.88
Transport	0.09	0.10	-0.01	0.19
Financial	0.00	0.00	0.00	0.18
Non-financial	0.06	0.06	-0.00	0.96
N	11,440	11,440	22,880	

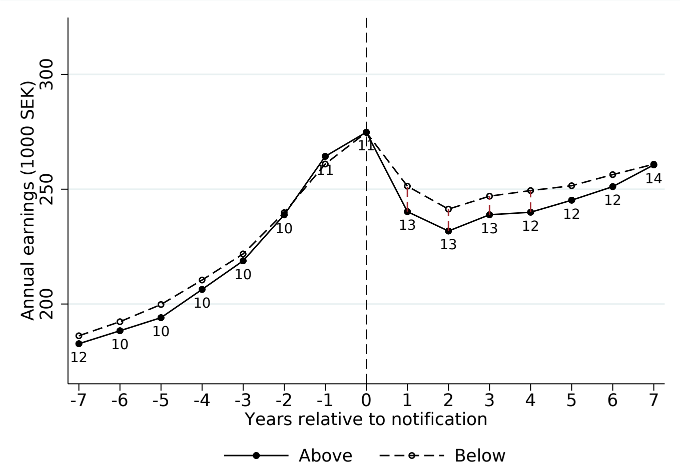
Notes: The table show in column (1) and (2) average characteristics of displaced and non-displaced workers, respectively, used in the mass layoff analysis in section 1.6. Column (3) show differences in means for the two groups and column (4) the p-value for a test of equality of means. For details on how the two groups are created and matched see section 1.6.1.

B Optimal bandwidth

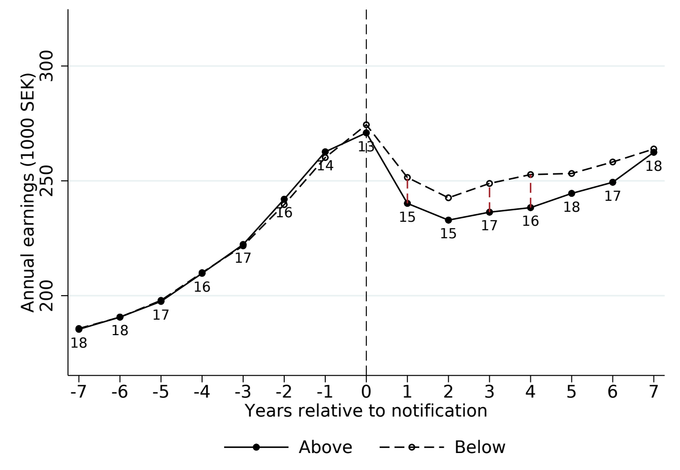
Figure A-1.10 reproduces Figure 1.6 using the optimal bandwidth selector suggested by Calonico, Cattaneo and Titiunik (2014).

FIGURE A-1.10. EVOLUTION OF ANNUAL EARNINGS RELATIVE TO YEAR OF NOTIFICATION (OPTIMAL BANDWIDTH)

(a) *Linear fit*



(b) *Quadratic fit*



Note: The figure show annual earnings relative to the year of notification. For each time point, I plot the constant (hollow circles) and constant+γ (solid circles) estimate of equation (1.3) which corresponds to the average predicted value of each outcome for workers to the left and right of the threshold, respectively. Indicated below each point is the optimal bandwidth suggested by Calonico, Cattaneo and Titiunik (2014). The dashed vertical line indicates significance at the 5-percent level where standard errors are clustered at the order circuit level. The regressions include a linear polynomial function interacted with the threshold as well as order circuit fixed effects.

C Calculation of tenure

A worker's relative seniority is directly related to tenure since it is defined as his tenure relative to the tenure distribution of the rest of the workforce within an order circuit. Hence, any measurement error in workers' individual tenure or the full tenure distribution within the circuit induces measurement error in the forcing variable as defined in equation (1.1).

I construct tenure using the indicators of first and last month worked at a firm which are reported by the employer along with the annual income statement in the matched employer-employee data. Employment spells may be interrupted by months of non-employment if the employer has reported two or more spells of employment where e.g. the first spell may last January to March and the second spell e.g. August to December. As such I can create monthly markers indicating whether the worker is employed in any given month at a particular firm or an establishment. Using these monthly indicators I rank workers, who are employed at the time of notification, by their date of first employment at the firm. As noted in section 1.3 there may be false ties in tenure due to employers too often reporting January as the month where the worker started employment. To avoid such ties which are due to measurement error, I divide workers with the same start date into quartiles of annual earnings in the first year of employment, where workers in lower quartile are assumed to have started employment later than workers in higher quartiles. I drop entire circuits where more than 2/3 of workers have a tenure equal to the mode of the circuit as these ties are most likely due to so called false firm deaths where firms for other reason than bankruptcy change identification number. Finally, when constructing the forcing variable, relative ranking, I break ties in tenure by age at notification (following the LIFO rule).

Even if tenure was perfectly measured, there are still potential sources of measurement error in the forcing variable. The LIFO rule which applies at the CBA $\times$ establishment level is proxied by 2-digit occupational codes. The lack of a one-to-one mapping between CBA's and occupation codes makes it difficult to precisely define the relevant workforce subject to the notification. Thus the full tenure distribution within the order circuit may be obscured by including workers in an order circuit which they do not belong to. Misallocating just one worker will render the circuit too large or too small and thereby leading me to place the discontinuity in the wrong place in the tenure distribution when normalizing the running variable with the number of notified workers (N_c). To minimize the risk of missmeasuring order circuits I restrict the circuits to be no larger than 100 workers.

Importantly, the above listed causes which may generate measurement error does not affect the consistency or causal interpretation of my estimates but only induces noise in the forcing variable (RR), thereby attenuating the first stage.

Chapter 2

How Does Advance Layoff Notice Affect the Labor Market Prospects for Workers?*

with Peter Fredriksson, Arash Nekoei and David Seim

*We thank Caroline Johansson, Alexander Lindholm and seminar participants at AEA 2018, Aarhus University, CEPR-conference on "Labour Market Participation: Forces at Work and Policy Challenges", CREST, Stockholm University, Labor Economics Workshop, Nordic Summer Institute in Labor Economics, the 10th Nordic Register Data and Economic Modeling Meeting, and the UCLS annual conference. We gratefully acknowledge funding from Riksbankens Jubileumsfond, Handelsbanken, and FORTE Grant 2015-00490.

2.1 Introduction

Layoff rules are sometimes criticized for hampering the speed of adjustment after adverse shocks and creating inefficiencies in the allocation of resources. However, layoff rules also provide insurance for workers – in part by mandating that firms share information on future reductions in labor demand – and they may force firms to share the costs associated with layoff.

In this paper we examine how advance layoff notice affects the labor market prospects for workers. More precisely, we utilize quasi-random variation in the length of notice periods to estimate the effects of advance notice on exposure to non-employment, job mobility, subsequent wages and earnings. The quasi-random variation comes from collective bargaining agreements, which stipulate that individuals above a certain age get longer notice periods. We estimate the causal effects of longer advance notice in a (fuzzy) regression discontinuity design.

Employment protection legislation in most countries features advance layoff notice. The length of the notice period typically increases with tenure and tends to be longer for white-collar workers than for blue-collar workers. Notice periods are also longer in Northern and Continental Europe than in Anglo-Saxon countries (OECD, 2013). In the U.S., labor laws do not feature notice periods in the case of individual dismissals, but in the case of plant closures and mass layoffs, legislation sometimes mandates that workers are given two months of notice.[1]

Contracts may also provide employment protection and advance notice. In the U.S., for example, collective bargaining agreements often include notice periods and severance pay. Such agreements provide workers with extra protection relative to the law. In the Swedish context, the law provides a set of default rules and the provisos in the collective agreements provide workers with additional protection.

Previous empirical analyses of employment protection originate from the seminal work of Lazear (1990).[2] Using a panel of OECD countries, Lazear (1990) finds that severance pay increases unemployment. In a different panel of countries, Heckman and Pagés (2004) document a neg-

[1] Employers are subject to the so-called WARN act if they have at least 100 employees. The WARN act stipulates, for example, that an employer that closes a plant with at least 50 employees must give advance notice.

[2] Aguirregabiria and Alonso-Borrefo (2014) provide an extensive literature review.

ative association between job security provisions and employment rates. Another strand of the literature exploits policy reforms to obtain quasi-experimental effects of employment protection on various outcomes. Kugler and Pica (2008) show that mandated severance pay reduces employment in Italy, while Autor, Kerr and Kugler (2007) find similar effects on employment from increased dismissal costs.

We contribute to this literature along two margins. First, the employment outcomes in previous studies originate from both hiring and separations margins. By focusing on separations, we specifically investigate the role of advance notice for a laid-off worker, thereby uncovering the insurance role of such employer protection legislation. Second, our setting provides the ideal testing ground from an identification perspective. We exploit exogenous variation in the length of advance notice within establishments and displacement events, across individuals. This permits a compelling analysis of the causal effects of advance notice.

We also contribute to an older literature, which investigates the role of advance notice for laid-off workers using the Displaced Worker Survey (see Ruhm 1992 and Ruhm 1994 and the survey in Addison and Blackburn 1994). That literature exploits cross-sectional variation in advance notice periods instigated by the Worker Assistance and Retraining Notification (WARN) Act and finds that joblessness falls upon notification of job loss.[3] Our paper breaks new ground by providing quasi-experimental variation applied to administrative data on long-term outcomes.

Advance notice policies are clearly related to severance pay and unemployment insurance policies. Pissarides (2001) analyzes a model where firms can offer severance pay and advance notice as part of an optimal contract, but unemployment insurance (UI) is exogenously set by the government. His analysis implies that optimal contracts are more likely to involve severance pay or advance notice when UI replacement rates are low. In his model, advance notice plays a role over and above severance payments if and only if the insurance properties are sufficiently superior.[4]

[3] Krolikowski and Lunsford (2020) assemble a database of large displacement events in the U.S.. This is feasible because the WARN Act prescribes that large layoff events be reported to State Dislocated Worker Units (SDWUs).

[4] A severance payment is a pure transfer from the worker to the firm, and thus does not affect the private surplus of the match. By contrast, advance notice may have a negative effect on the private surplus, if the match is kept alive after it has

Our empirical work relates to the model in Pissarides (2001) in the sense that we think of (voluntary) severance pay as an outcome of other policy parameters, in our case mandatory advance notice. We examine whether firms are willing to make – and whether workers accept – an upfront severance payment in order to avoid the notice period. To our knowledge, this is the first paper that provides such an analysis.

The major results of the paper are the following. Longer notice periods cause pro-longed periods of adjustment. For workers who are eligible for a longer notification period (the treatment group), the probability of remaining in the displacing firm increases during the first two years after notification, and the probability of moving to another firm falls during the same time period. As a result of an extension of the notice period, workers are less exposed to unemployment and non-employment, and spend less time outside the labor force. After two years, all employment responses have subsided and there are no differential effects on employment outcomes for the treatment and control groups. We also show that the treatment group experiences smaller wage losses when finding a new job than the control group: wages in new jobs are three percent higher for the treatment group than for the control group. Moreover, firms make severance payments to workers in order to avoid the notice period: the extra payment accruing to the treatment group amounts to almost 60% of the monthly wage. Finally, we show that workers who are eligible for higher UI get lower severance payments. This is consistent with the view that they are more willing to accept lower severance pay since they have a better outside option should they leave the firm for unemployment.

The extra severance payment is part of the overall effect on earnings for treated workers. When we decompose the earnings effect accruing to workers during the first two years after notification, we find that around half of the earnings effects has to do with less exposure to non-employment and a third is due to the increase in severance pay. The wage effect amounts to a fifth of the overall earnings effect. Over time,

become unproductive. These costs have two components: first, there may be variable costs associated with keeping an unproductive job alive; second, for the worker-firm pair, unemployment income is a pure subsidy which is forgone by keeping the match alive. In the framework of Pissarides (2001), the insurance value of advance notice must thus outweigh these losses relative to the severance pay.

the wage effect becomes less important as individuals with short notification periods – who find a lower quality job initially – move on to better-paying jobs at a greater rate than individuals with long notification periods.

The remainder of the paper unfolds as follows. Section 2.2 provides the relevant details of Swedish labor law and the collective bargaining agreements that we use for identification. Section 2.3 describes our data consisting of all layoff events involving at least 5 workers. Within that population, we exploit variation in notification periods by worker age at the time of notification. Section 2.4 probes the relationship between the forcing variables and the lengths of notification periods. Section 2.5 examines how the labor market prospects of workers are affected by having longer advance notice periods. Section 2.6 concludes.

2.2 Institutional details

The current formulation of Swedish labor law stipulates that employers wishing to lay off an employee must give written advance notice to the worker.[5] The length of the notification period varies discontinuously with tenure, with threshold values being multiples of 24 months of tenure. The minimum notice period is 1 month for employees with less than 24 months of tenure, and 2 months for those with at least 24 months of tenure. The maximum notice period is 6 months which applies to workers with at least 10 years (120 months) of tenure.[6] For comparison, the Worker Adjustment and Retraining Notification Act (WARN) in the U.S. obliges employers with more than 100 full-time employees to give a written notice at least 60 days in advance of layoff.[7]

Importantly, the labor law is dispositive. This means that the law provides a set of default values, but alternative rules can be agreed upon in collective bargaining agreements (CBAs). For instance, the white-

[5] Verbal announcements are legally binding as well but come with pecuniary penalties.

[6] In the prior formulation of the law, age governed the length of the notification period. This formulation of the law is relevant for employment spells starting prior to 1 January 1997.

[7] The state of New York extended the mandatory notice period to 90 days in 2009, and New Jersey is currently considering a similar extension (Krolikowski and Lunsford, 2020).

collar agreement within manufacturing stipulates that workers above age 55 with 10 years of tenure get an additional 6 months of notice. After being notified, individuals may also agree to severance pay packages which are different (and perceived as more generous from the worker's point of view) from the default rules.[8]

We have collected information from all major collective agreements on the Swedish labor market for the relevant time period (2005-2014). Age rules exist mainly for white-collar workers in the private sector. The most common formulation by far is the one described above. A worker who is 55 years old at the time of notification and has at least 10 years of tenure receives an additional six months of notification.[9] Ideally, we would have liked to match the information from the collective agreements to the individual-level micro data. However, these micro data do not include information on which collective agreement a worker belongs to at a given point in time. Therefore, we simply implement the age-55 rule for all white-collar workers in the private sector without taking into account which agreement they belong to.

In our empirical analyses, we focus on the age-55 threshold. An important reason for this is that tenure is difficult to measure exactly, and any measurement error in tenure will weaken the predictive value of the tenure rules.[10] For age, per se, there is no measurement error involved.

[8]Further rules are tied to the size of the displacement event. For instance, a firm intending to displace 5-25 workers should give all notified workers at least 2 months of notification, while a firm notifying 26-100 workers at the same time must give all workers at least 4 months of notification. In the extreme case where more than 100 workers are notified, they all get at least 6 months of notification.

[9]With that said, other age rules do exist but they are much less prevalent. The white-collar agreement in retail trade during 2004-2007, for instance, has a combination of age and tenure. For given tenure, notification periods are prolonged discretely at age 25, 30, 35, 40, and 45. This age proviso was later changed to the one described in the main text.

[10]Measurement error in tenure at notification has three sources. One issue is that January is reported too often as the starting month of an employment spell. This potential problem comes from the fact that employers should report the duration of the employment spell within the year in the annual income statements for the employees. The risk is that employers routinely report months 1-12 as the employment spell rather than the true employment spell (say 4-12), since the spell length does not affect taxes due. A second issue is the precise definition of tenure. The formal rule allows for breaks in tenure due to, e.g., parental leave and sick-leave. The challenge is to determine whether a break in two consecutive spells at a firm is due to the worker being on leave or whether the break should reset the tenure clock. A third issue is that there may be measurement error in notification dates, meaning that tenure at

2.3 Data and estimation sample

We focus on displacement events occurring during 2005-2014. A firm intending to lay off at least five workers simultaneously must notify the Public Employment Service (PES) in advance. In a first stage, the firm reports the number of workers it intends to displace to the PES along with the reason for downsizing.

In a second stage, a list of names of the displaced workers and their displacement dates (DD) must be submitted to the PES. Our data contain the date when the list arrives at the PES. We assume that this is also the date when the worker learns about her future displacement and define it as the notification date (ND). The reported dates are scrutinized by the PES, which can even take employers who do not abide by labor laws to court. This implies that ND should be a valid proxy for when the employee is informed about the upcoming job loss.

A worker shows up in our data at this second stage. From the information supplied at this point in time, we calculate the length of the notification period (D) as:

$$D_i = DD_i - ND_i \tag{2.1}$$

The typical configuration in the data is that all workers involved in a given displacement event are notified at the same date, but that future displacement dates vary depending on individual variation in age or tenure. An issue for the empirical analysis is that there is measurement error in the notification dates. This implies that (even though there is no measurement error in age) there is some measurement error in age at notification which is the main assignment variable.

We match the displacement data with four administrative datasets: (i) Register-based employment statistics (RAMS by Swedish acronym); (ii) the Integrated Database for Labour Market Research (LISA); (iii) Wage and hours survey and (iv) Unemployment spell registries. The first three are obtained through Statistics Sweden and the last one is retrieved from the PES. The first dataset contains the universe of matches be-

notification would be measured with error even if tenure itself is measured perfectly. This issue also implies that there will be some measurement error in age at notification. See Davezies and la Barbanchon (2017) for an analysis of the consequences of measurement error in the forcing variable.

tween employers and employees during the time period 1990-2016. From the matched employer-employee data, we get information on job-related characteristics of workers, such as her wage earnings, and the characteristics of firms, such as industry codes, both overall and for workers and firms involved in displacement events. The second database (LISA) provides a range of individual-level characteristics, including demographic variables. It contains all individuals, aged 16 and older, residing in Sweden at the end of the calendar year. We use this dataset to define our analysis sample as individuals who are notified of a displacement event and are residents in Sweden at the end of the notification year.

The Wage and hours survey provides information for a large subset of the employed population. Employers report contracted monthly wages and hours worked in a measurement week (during September-November) for each employee. The wage concept includes all fixed wage components, as well as piece rates, performance pay, and fringe benefits. All workers in the public sector and around 50% of all private-sector workers are included in the data.

Table 2.1 presents descriptive statistics of our analysis sample as well as three relevant comparison groups. Panel A shows individual-level characteristics, while Panel B presents firm-level statistics. The first column focuses on all notified individuals in our data. Column (2) shows that the set of notified individuals compares relatively well with the average employed worker in the same industries. The main difference is that educational attainment is lower among notified individuals, which in turn is driven by the fact that blue-collar workers are overrepresented among notified individuals. Furthermore, column (3) indicates that educational attainment is lower compared to the average employed workers. The fact that private sector manufacturing firms are overrepresented also implies that males are overrepresented among notified individuals.

Column (4) of Table 2.1 shows characteristics of our estimation sample. It consists of private-sector white-collar workers aged 52 to 58 who are notified of displacement. Since individuals in this sample are older white-collar workers, they have higher wages, earnings, and educational attainment, than the average individual notified of displacement.

Table 2.1. Descriptive statistics

	Notified individuals (1)	Employed workers same industries (2)	Employed workers (3)	Notified individuals age-55 sample (4)
Panel A: Individual-level characteristics				
Female	0.35	0.37	0.48	0.44
Immigrant	0.17	0.14	0.14	0.10
Age	40.93	41.54	42.00	55.02
Tenure	5.36	5.94	5.79	7.28
Earnings_{t-1} (1000 SEK)	256.9	249.9	224.8	356.4
Wage_{t-1} (1000 SEK)	24.6	25.8	24.8	31.1
Educational attainment				
Compulsory ed.	0.16	0.17	0.16	0.12
Upper-secondary ed.	0.60	0.50	0.47	0.50
College ed.	0.24	0.31	0.36	0.37
Panel B: Firm-level characteristics				
Firm size (# employees)	599.04	53.11	62.47	1061.96
Agricultural	0.00	0.00	0.04	0.00
Mining	0.00	0.00	0.00	0.00
Manufacturing	0.36	0.39	0.11	0.32
Electricity	0.00	0.00	0.01	0.01
Construction	0.08	0.05	0.06	0.03
Wholesale and retail	0.12	0.14	0.11	0.16
Transport	0.11	0.09	0.08	0.20
Financial Services	0.01	0.00	0.02	0.01
Non-Financial services	0.15	0.21	0.14	0.15
Public administration	0.02	0.01	0.05	0.01
Education	0.02	0.02	0.10	0.03
Human health	0.03	0.04	0.15	0.04
Entertainment	0.02	0.01	0.07	0.03
Other	0.08	0.03	0.07	0.01
Observations	418,111	5,827,312	5,827,888	8,955

Notes: Column (1) contains all notified individuals. In column (2) the distribution of employed workers over industries has been reweighted to match the industry distribution of notified workers. Column (3) focuses on the population of employed workers. Column (4) includes white-collar workers in the private sector who were aged 52-58 at the time of notification. Firm-level characteristics are computed at the individual level, except firm size where the unit of observation is a firm.

Panel B describes the firms involved in a displacement event in our data. Given that the firms in the data must have notified at least five workers simultaneously in order to be included in the data, it is no surprise that average firm size is large. It also conforms to the intuition that they tend to belong to cyclically sensitive sectors (e.g. manufacturing) to a greater extent than the average firm.

2.4 Age and notification times

As noted above, we exploit the discontinuity in notification periods at age 55 induced by collective bargaining agreements for identification. This age rule is a feature of many collective agreements for white-collar workers in the private sector. Our data allow us to identify private-sector white-collar workers, but we cannot identify which collective agreement they belong to. This is one reason that the age rule is only predictive. Other reasons include the fact that the workers may individually agree to severance packages in the case of layoff.

We define an instrumental variable related to age, A_i, as: $Z_{Ai} = \mathbf{1}[A_i \geq 55]$. Furthermore, we define an age-specific control variable as

$$g_A(a_i) = \left[g_A^0(A_i - 55) + \mathbf{1}[A_i \geq 55] \times g_A^1(A_i - 55)\right]$$

where a denotes normalized age (normalized to zero at the age-55 threshold). The slope of the age control function is allowed to differ above (g_A^1) and below (g_A^0) the threshold.

Our data include month and year of birth. Age is thus discrete and measured in months. Since age is discrete, we mainly rely on a parametric control function.[11] Note also that it is age at the time of notification that is relevant.[12] The first-stage regression is given by

$$D_i = \delta_A Z_{Ai} + g_A(a_i) + \nu_i, \tag{2.2}$$

[11]Our main analysis uses data for individuals who are aged ±3 years relative to the age-55 threshold. This corresponds relatively well to what the optimal bandwidth selector of Calonico, Cattaneo and Titiunik (2014) would suggest (see Appendix B).

[12]Since age is measured in months we cannot determine whether someone turning 55 during the same month as the notification takes place is just above 55 or just below 55. We therefore exclude observations exactly at the cut-off.

where δ_A is the first stage effect. The corresponding outcome equation is:

$$y_i = \beta D_i + f_A(a_i) + \epsilon_i, \tag{2.3}$$

where y denotes an outcome and β is the effect of increasing the notification period, and $f_A(a_i)$ an age control function. There is also an immediate reduced-form equation which is obtained by substituting equation (2.2) into (2.3):

$$y_i = \gamma_A Z_{Ai} + h_A(a_i) + u_i \tag{2.4}$$

In the remainder of this section, we investigate the properties of the age-55 threshold empirically. Section 2.4.1 thus examines whether there is manipulation of the age-55 threshold and Section 2.4.2 examines the impact of the age-55 threshold on the length of the notification period.

Excludability of the instrument is a more difficult question. Age rules may surface in various parts of the public support system.[13] However, there is no age-rule in the public support system specifically tied to the *age at notification*. Nevertheless, surpassing the age threshold, which increases the probability of being eligible for longer notification, may affect other outcomes which in themselves affect post-displacement outcomes. For this reason, we focus on reduced-form estimates, i.e., equation (2.4).

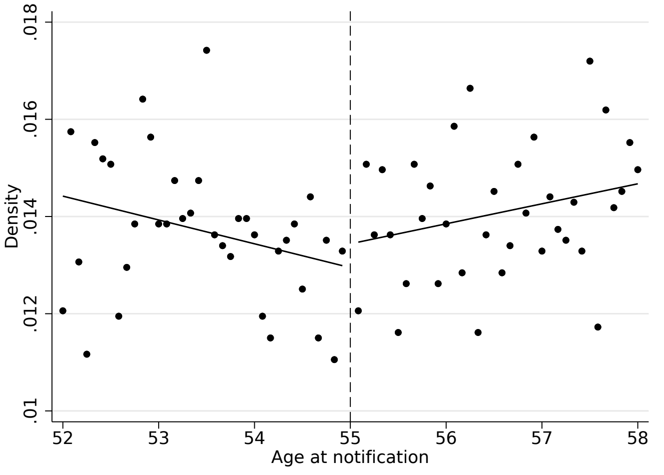
2.4.1 Bunching and balancing of covariates

A possible concern is that firms try to selectively displace low-cost workers, along the lines of the insider-outsider theory (see Lindbeck and Snower, 1989). In our setting, this would manifest itself through more laid-off workers just to the left of the age-55 threshold. Figure 2.1 examines whether there is manipulation around the age-55 threshold by comparing the number of observations in the vicinity of the threshold. There is no evidence of suspect bunching on either side of the threshold.

Table 2.2 investigates whether baseline covariates are evenly distributed across the age-55 threshold. Columns (1)-(4) examine overall balancing. We regress an indicator for being above the age-55 threshold on all baseline characteristics and polynomial control functions in age. We are

[13]For instance, there used to be a rule prolonging UI duration for workers who were at least 55 years old at the time of unemployment entry. This rule was abolished in 1998.

FIGURE 2.1. NUMBER OF OBSERVATIONS BY AGE AT NOTIFICATION



Notes: The figure shows the distribution of displaced individuals by age at notification (measured in months). The regression lines come from estimating a regression corresponding to equation (2.4) with the fraction of observations at each age bin as the outcome variable. The regression includes a linear age polynomial interacted with the threshold dummy. The estimated jump at the threshold is 0.0005 (standard error = 0.0006, p-value = 0.435).

mainly interested in the F-statistics, reported at the bottom end of the table, which test the null hypotheses that all coefficients on individual (and firm) characteristics are jointly zero. As indicated by the p-values of the F-tests we cannot reject these hypotheses. Also, the individual coefficients are typically small.

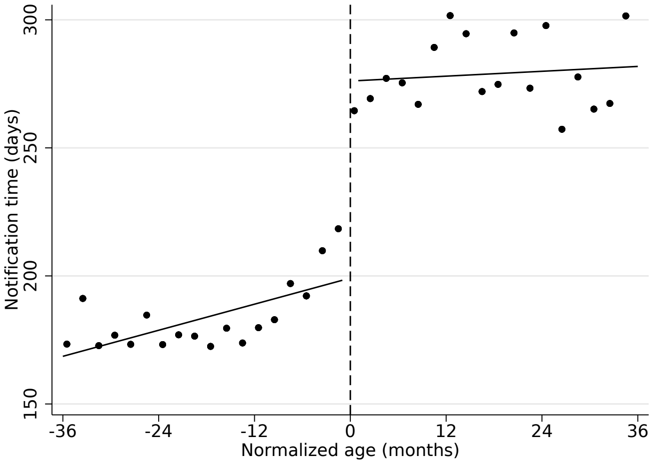
Column (5) reports bivariate tests of equality of baseline covariates above and below the threshold. These tests reinforce the view that the coefficients are generally small: those just above the threshold earned 0.26% less than those just below the threshold, for instance.

Table 2.2. Balancing of pre-determined covariates

	(1)	(2)	(3)	(4)	(5)
Earnings$_{t-1}$	-0.0029	-0.0028	-0.0040	-0.0001	-0.0026
	(0.0062)	(0.0066)	(0.0066)	(0.0040)	(0.0174)
Female	0.0035	0.0037	0.0037	0.0002	0.0128
	(0.0049)	(0.0052)	(0.0055)	(0.0033)	(0.0215)
Immigrant	-0.0033	-0.0028	-0.0027	-0.0009	0.0012
	(0.0093)	(0.0091)	(0.0092)	(0.0062)	(0.0146)
Tenure (years)	-0.0035	-0.0035	-0.0045	-0.0015	-0.0496
	(0.0030)	(0.0030)	(0.0032)	(0.0020)	(0.0334)
Highest attained education					
Primary	-0.0252	-0.0224	-0.0211	0.0016	0.0052
	(0.0230)	(0.0232)	(0.0226)	(0.0151)	(0.0129)
High school	-0.0301	-0.0277	-0.0281	-0.0040	-0.0399*
	(0.0196)	(0.0197)	(0.0193)	(0.0119)	(0.0233)
College	-0.0189	-0.0175	-0.0182	0.0015	0.0295
	(0.0219)	(0.0216)	(0.0212)	(0.0137)	(0.0232)
Firm characteristics		✓	✓	✓	
Polynomial order					
1st degree	✓	✓	✓		✓
2nd degree				✓	
Interacted w. threshold	✓	✓	✓	✓	✓
Month/Year FE			✓	✓	✓
F-statistic	1.20	1.38	1.55	1.18	·
p-value	0.312	0.196	0.126	0.311	·
R^2	0.768	0.768	0.768	0.905	·
# clusters	72	72	72	72	72
# observations	8,860	8,860	8,860	8,860	8,893

Notes: Earnings are measured relative to the control group. Standard errors are clustered on the discrete values of the forcing variable (age at notification). Regressions include individuals aged 52-58 at the time of notification. Columns (1)-(4) show the results of regressing an indicator for being above the age–55 threshold on baseline covariates and polynomial control functions in age. The bottom part of the table reports the F-statistic and the associated p-value from testing the null hypothesis that all coefficients on (individual and firm) baseline covariates are jointly zero. Firm characteristics included in columns (2)-(4) are workforce characteristics – average earnings, share of females, share of immigrants, average age, share of college-educated, and number of employed. All firm characteristics are balanced, except average age in columns (2) and (3) (for instance, in column (2) average age is 0.0008 years higher for individuals above the age-55 threshold). Column (5) reports the results of bivariate balancing tests where each covariate listed in the left-hand column is regressed on the treatment indicator and an interacted first order polynomial control function in age at notification. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 2.2. Notification times around the age-55 threshold



Notes: The figure shows notification times by age at notification in 2-month-bins. The regression lines come from estimating equation (2.2) with a linear age polynomial interacted with the threshold indicator. The estimated jump at the threshold is 80.8 days, with a standard error of 8.1. The regression also includes baseline covariates and month-by-year FEs. The specification corresponds to column (2) in Table 2.3.

2.4.2 Notification times and the age-55 discontinuity

How much do notification times increase when workers surpass the age-55 threshold? Figure 2.2 answers this question by relating notification times to age at notification. The figure shows that notification times are long. Just below the age-55 threshold, individuals have around 6.5 months of notice (196 days) and just above the threshold, notice times are roughly 9 months (277 days) on average. Notice times thus jump by slightly more than 2.5 months (81 days) at the age-55 threshold.

Table 2.3 shows different specifications of the "first-stage" regression, given by equation 2.2. In columns (1)-(3), the control function is linear and in columns (4)-(6), we include a second-order polynomial in the forcing variable. All specifications allow the slope of the control function to be different across the threshold. Column (1) shows the result of a specification which includes baseline covariates and a first-order polynomial in age. The notification time increases by 78 days at the age-55 threshold. Column (2) adds month-by-year fixed effects (FEs) and column (3) replaces the month-by-year FEs with displacement event FEs, which implies that we only utilize the variation across individuals involved in

TABLE 2.3. FIRST-STAGE ESTIMATES

	(1)	(2)	(3)	(4)	(5)	(6)
Above age-55	78.370***	80.821***	81.889***	50.148***	51.008***	55.499***
	(8.409)	(8.119)	(6.698)	(11.498)	(10.158)	(9.330)
Control mean	198.043***	196.489***	200.176***	217.832***	216.710***	216.506***
	(6.200)	(5.766)	(4.702)	(8.790)	(6.875)	(6.265)
Polynomial order						
1st degree	✓	✓	✓			
2nd degree				✓	✓	✓
Baseline covariates	✓	✓	✓	✓	✓	✓
Interacted w. threshold	✓	✓	✓	✓	✓	✓
Month/Year FEs		✓			✓	
Displacement event FEs			✓			✓
F-stat	86.87	99.10	149.47	19.02	25.22	35.38
R^2	0.091	0.186	0.245	0.092	0.187	0.247
# clusters	72	72	72	72	72	72
# observations	8,955	8,860	8,860	8,955	8,860	8,860

Notes: Standard errors are clustered on the discrete values of the forcing variable (age at notification). Regressions include individuals aged 52-58 at the time of notification. Baseline covariates are earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

a given displacement event for identification. None of these additions matter substantially: the estimated age-55 discontinuity increases marginally from 78 to 82 days.

What does matter for the magnitude of the estimated discontinuity is how flexibly we control for age at notification. The estimated discontinuity is reduced by roughly 26-30 days when we control for a second-order polynomial (see columns (4)-(6)). Most of this reduction is likely an artifact of the measurement error in notification dates. When notification dates are to some extent mismeasured, we get measurement error in age at notification. This measurement error, in turn, causes the discontinuity to look a like a non-linearity (see Figure 2.2). Since we think the reduction in the discontinuity estimate is mainly driven by measurement error, our preferred specification is the linear interacted one. When we show regression results, we therefore mainly use the specification shown in column (2).[14]

2.5 Worker outcomes

We begin our analysis of the individual effects of longer advance notice by examining the overall earnings effects. We decompose the overall earnings effect into a number of adjustment margins, and then look at the effects of being eligible for longer advance notice along each adjustment margin.

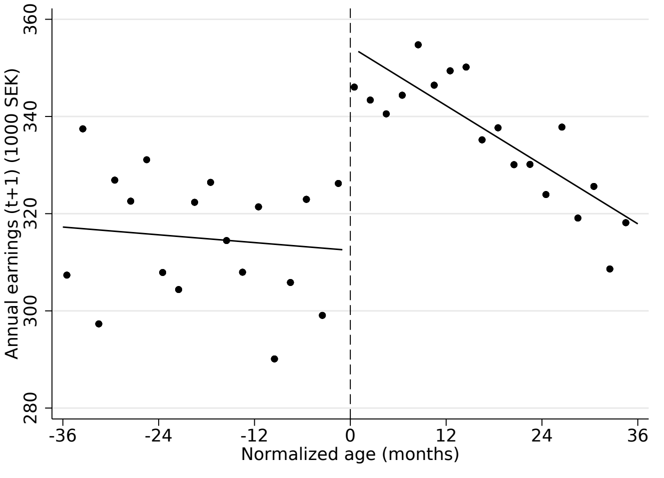
2.5.1 The overall earnings effect

Figure 2.3 shows reduced-form RD estimates for earnings in the calendar year after notification. Individuals just to the left of the threshold earn 314,000 SEK, while individuals just to the right earn 356,000 SEK.[15] Being eligible for longer notification thus increases annual earnings by 42,000 SEK which corresponds to almost 14% of the wage earnings for individuals just below the threshold.

[14]Of course, we graph the reduced-form relationship in conventional regression discontinuity graphs for the main outcomes, such that readers can judge for themselves what is an appropriate specification of the control function.

[15]All amounts have been deflated to 2010 values. In November 2019, the SEK/US Dollar conversion rate is 9.65 SEK/Dollar and the SEK/Euro conversion rate is 10.75 SEK/Euro.

FIGURE 2.3. EARNINGS IN THE YEAR AFTER NOTIFICATION BY AGE AT NOTIFICATION



Notes: Age at notification is in 2-month-bins. The regression lines come from estimating equation (2.4) with a linear age polynomial interacted with the threshold indicator for individuals aged 52-58 at the time of notification. The estimated jump at the threshold is 42.4 (1000 SEK), with a standard error of 6.0. The regression also includes baseline covariates and month-by-year FE:s. The specification corresponds to column (2) in Table 2.3.

What goes into this overall earnings effect? Trivially, earnings after notification have two sources, either the notifying firm or a new firm. But, as mentioned above, there might be (voluntary) severance packages as long as these are to the advantage of both the firm and the worker. The monetary component of these severance packages are recorded in the data as earnings. Thus, earnings from the notifying firms can consist of regular wage payments ($w_0 l_0$) and severance payments (SP). The total earnings effect (Δy) of being eligible for longer notification over some fixed time horizon can thus be written as

$$\Delta y = \Delta(w_0 l_0) + \Delta(w_1 l_1) + \Delta SP$$

where Δ denotes a treatment and control difference; w_1 is the wage associated with the new job and l_1 is the duration of the new job over a fixed time horizon (T), and w_0 and l_0 are defined correspondingly for the old job. Rewriting this equation using $\Delta w_0 = 0$ (the treatment and control group have the same wages prior to treatment) and $T =$

$l_0 + l_1 + NE$, where NE denotes non-employment, we get

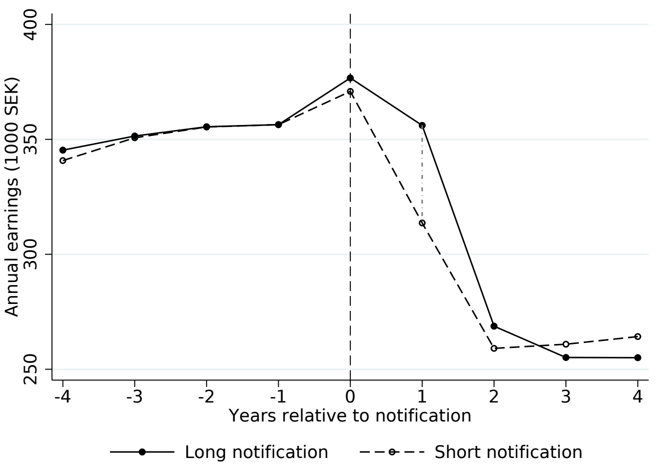
$$\underbrace{\frac{\Delta y}{w_0}}_{\substack{\text{Total}\\ \text{earnings}}} = - \underbrace{\Delta NE}_{\substack{\text{non-employment}\\ \text{duration}}} - \frac{w_0 - w_1}{w_0} \underbrace{\Delta l_1}_{\substack{\text{new job}\\ \text{duration}}} + \underbrace{\frac{\Delta w_1}{w_1}}_{\substack{\text{effect}\\ \text{wage}}} \frac{w_1}{w_0} l_1 + \underbrace{\frac{\Delta SP}{w_0}}_{\substack{\text{pay}\\ \text{severance}}} \quad (2.5)$$

The first and second component on the right-hand-side come from the fact that being eligible for a longer notice period likely implies that you will stay longer at the notifying firm. If there is some friction involved in changing jobs, the treatment group spends less time in non-employment ($\Delta NE < 0$) which will thus contribute to increase earnings. Longer duration at the notifying firm, on the other hand, likely implies a shorter duration of the new job ($\Delta l_1 < 0$). Since displacement involves wage losses, $w_1 < w_0$, the second component also contributes to increasing earnings. The third component reflects the possibility that workers eligible for longer displacement may be able to avoid some of the wage losses associated with displacement (see Nekoei and Weber, 2017 for evidence on such job quality effects in the case of unemployment insurance). Finally, the fourth component captures the possibility of severance packages being more generous for workers who are eligible for longer notification periods.

Figure 2.4 illustrates the earnings effects in another way. It shows the evolution of annual earnings in different calendar years relative to the notification event for those just above the age-55 threshold (black-circled line) and those just below the threshold (hollow-circled dashed line). The difference between the two lines at any given point is an RD-estimate, and the RD estimates that are statistically significant at the 5-percent level are indicated by dashed vertical lines. Note that any treatment effects may occur from year 0 (the notification year) and onwards.

The estimate shown in the first year after notification in Figure 2.4 corresponds to the RD estimate illustrated in Figure 2.3. The figure conveys several additional insights as well. First, it shows that the treatment and control groups are strongly balanced during the four years prior to notification. Second, it presents suggestive evidence of the existence of severance packages. There is a striking increase in annual earnings during the year of notification. If earnings only reflected regular wage

FIGURE 2.4. ANNUAL EARNINGS BY YEARS RELATIVE TO NOTIFICATION



Notes: The figure shows annual earnings by year relative to notification. At any given point in time, we plot estimates of the constant (hollow circles) and the constant+γ_A (black circles) from a regression corresponding to equation (2.4). Dashed lines indicate that the estimate of γ_A is significant at the 5% level. These regressions include a linear age polynomial interacted with the threshold indicator, baseline covariates, and month-by-year FEs. The specification thus corresponds to column (2) in Table 2.3. The analysis only includes individuals aged 52-58 at the time of notification.

payments during this year, we would expect earnings growth to be lower between $t = -1$ and $t = 0$ than during any other pre-notification year. The fact that this is not the case suggests the existence of severance pay packages. Third, after two years there are no discernible effects of being eligible for longer notification. A comparison of earnings in $t = 2$ and $t = -1$ suggests that the earnings losses associated with displacement amount to roughly 25% of pre-displacement earnings. These losses are similar in magnitude to those typically documented in the literature (see e.g. Jacobson, Lalonde and Sullivan, 1993). Fourth, because of the longer notice period, earnings losses are much lower for the treatment group (0%) than for the control group (11%) one year after notification ($t = +1$) when compared to the year before notification ($t = -1$).

In the remainder of this section, we unpack this overall earnings effect. We begin by investigating employment outcomes in Section 2.5.2, while Section 2.5.3 examines whether those with longer notification find better-paying jobs. Section 2.5.4 turns to severance payments. Section 2.5.5 pulls the different pieces together by using the evidence presented in previous sections to assign numbers to the decomposition in equation (2.5).

2.5.2 Employment outcomes

In this section we examine whether advance notice prolongs the adjustment process for workers, and whether a longer notification period shields them from non-employment. For this analysis, we mainly tap information on monthly employment indicators obtained from the employer-employee data (RAMS). These allow us to trace the dynamics following notification at a high frequency.

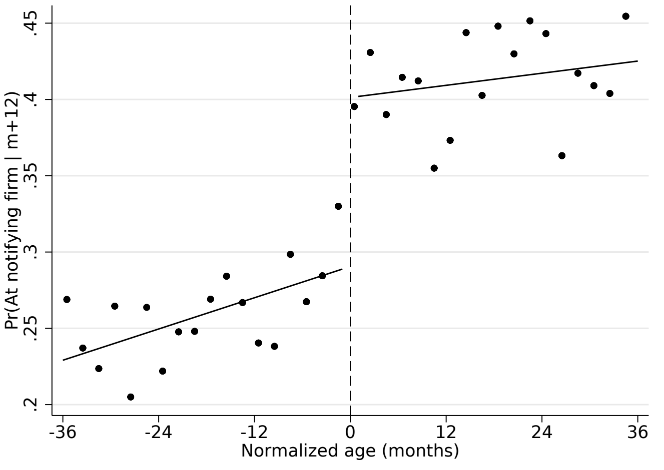
Figure 2.5 shows reduced-form RD estimates for two of the employment outcomes – the probability of remaining at the notifying firm and the probability of working at a new firm 12 months after notification. Panel (a) shows that the probability of remaining at the notifying firm increases from 29 percentage points to 40 percentage points when a worker surpasses the age-55 threshold. Panel (b), on the other hand, shows that the probability of being employed at a new firm is 7 percentage points lower for treated individuals. Combined, panels (a) and (b) imply that individuals treated with longer advance notice are: (i) less exposed to non-employment – the treatment effect corresponds to a reduction by 4 percentage points after 12 months, and (ii) involved in a lengthier adjustment process.

Figure 2.5 gives a snapshot for a particular point in time. Figure 2.6, instead, provides information pertaining to different points in time relative to notification, in a way akin to Figure 2.4. Panel (a) of Figure 2.6 shows the probability of remaining at the notifying firm at different points in time relative to the notification date.[16] The figure conveys several messages. First, prior to notification, and up to 2 months after notification, the probability of remaining at the notifying firm is the same across the two groups, but after 3 months a small gap starts to open up. Second, between months 6 and 11 a rather substantial gap

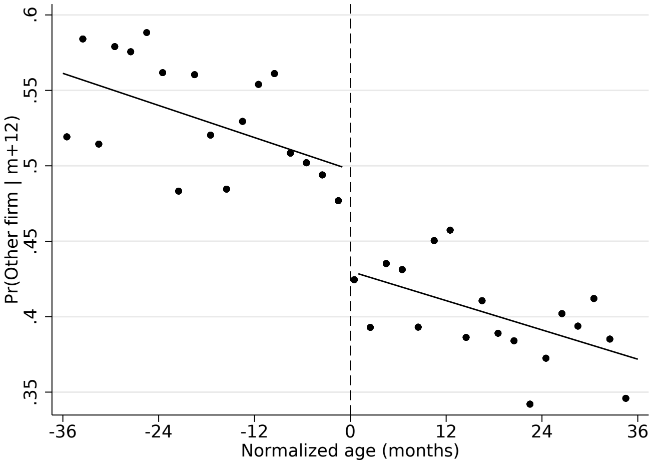
[16]In principle, all workers notified of displacement in a particular month should be observed at the notifying firm in the month before. Because of measurement errors (mainly in the notification dates), this is not the case in our data. According to panel (a), the fraction remaining in the notifying firm in $t = -1$ is 0.99, and according to panel (b), the fraction observed in another firm in $t = -1$ is 0.003.

Figure 2.5. Employment outcomes by age at notification

(a) *Probability of remaining at the notifying firm 12 months after notification*



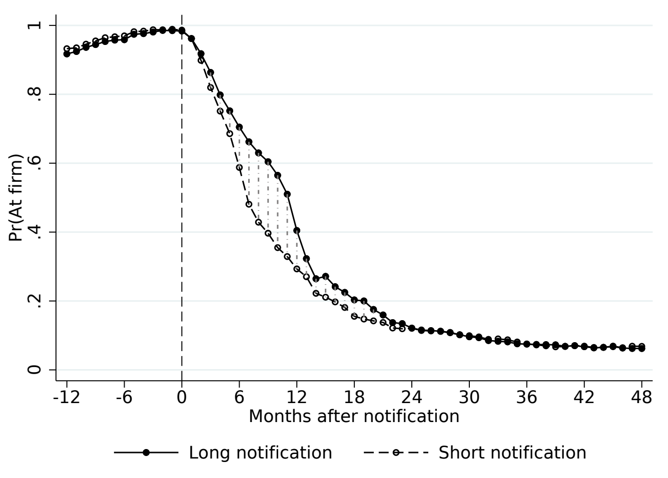
(b) *Probability of working at a new firm 12 months after notification*



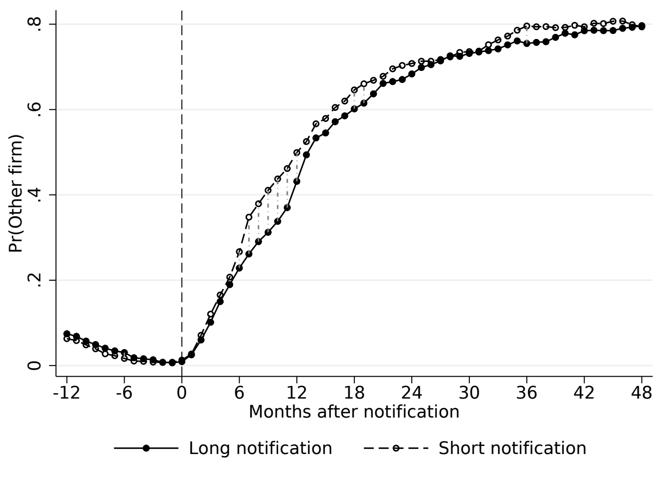
Notes: The figures show employment outcomes by age at notification (2-month-bins). The regression lines come from estimating equation (2.4) with a linear age polynomial interacted with the threshold indicator. The estimated jump at the threshold in panel (a) is 11.2 percentage points, with a standard error of 2.3 ppt, while the estimated jump at the threshold in panel (b) is -6.8 percentage points, with a standard error of 2.4 ppt. The regressions also include baseline covariates and month-by-year FE:s. The specification corresponds to column (2) in Table 2.3. The analysis only includes individuals aged 52-58 at the time of notification.

FIGURE 2.6. EMPLOYMENT OUTCOMES BY MONTH RELATIVE TO NOTIFICATION

(a) *Probability of remaining at the notifying firm*



(b) *Probability of working at a new firm*



Notes: The figures show each outcome by month relative to notification. At any given point in time, we plot estimates of the constant (hollow circles) and the constant+γ_A (black circles) from a regression corresponding to equation (2.4). Dashed lines indicate that the estimate of γ_A is significant at the 5% level. These regressions include a linear age polynomial interacted with threshold, baseline covariates, and month-by-year FE:s. The specification thus corresponds to column (2) in Table 2.3. The analysis only includes individuals aged 52-58 at the time of notification.

opens up between workers with long and short notification periods. Third, when the formal notification period expires for the treatment group (from month 12 and onwards), the gap between the two groups closes. The differential survival rates persist to around 20 months after notification, after which there are no treatment effects of being eligible for longer notification. Fourth, in the longer run, 7 percent of workers remain at the firm despite being notified of layoff. The potential explanations for this pattern include: notified workers replacing an unexpected departure of another worker, and market conditions improving for the notifying firm.[17]

Panel (b) of Figure 2.6 shows the probability of being observed at another firm by time relative to notification. The figure shows that there are significant treatment effects during months 6-12 after notification; after 12 months the difference between the two groups is small and it becomes non-existent after 2 years. In the longer run, 80 percent of notified workers have moved on to another firm, and this mobility rate does not differ across the treatment and control groups.

Figure 2.6 suggests that all of the employment adjustment occurs during the first two years after notification. Appendix Figure A-2.1 shows that these results are robust to using the optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). In Table 2.4 we summarize the employment impacts by cumulating them over the first two years. By doing so, we get an estimate on the duration (in months) at the notifying firm during the first two years, for example.[18]

The first two columns in Table 2.4 provide a summary picture of the evolution shown in Figure 2.6. As a result of being eligible for longer notification, individuals stay in the notifying firm an additional 1.8 months during the two years after notification. This is close to a month less than the additional 2.7 months of notice they were awarded at the time of notification (c.f. Table 2.3, column (2)).

Column (2) shows that the duration in the new firm is reduced by one month. Columns (1) and (2) imply that those eligible for longer notification are less exposed to non-employment with column (3) showing that this reduction is equal to three quarters of a month. Column (4)

[17]Importantly, this pattern is not driven by recalls. The vast majority of workers are observed at the notifying firms because they never left.

[18]This uses the fact that the survival function integrates to mean duration.

TABLE 2.4. CUMULATED DURATION (IN MONTHS) IN VARIOUS STATES, FIRST 2 YEARS AFTER NOTIFICATION

	(1) At notif. firm	(2) At other firm	(3) Non-employed	(4) Unemployed	(5) Out of labor force
Above age-55	1.815***	-1.047***	-0.769***	-0.540***	-0.228**
	(0.262)	(0.303)	(0.203)	(0.155)	(0.113)
Control mean	8.999***	11.036***	3.965***	3.259***	0.706***
	(0.230)	(0.274)	(0.166)	(0.125)	(0.077)
R^2	0.065	0.058	0.018	0.020	0.006
# clusters	72	72	72	72	72
# observations	8,856	8,856	8,856	8,856	8,856

Notes: Time out of the labor force is calculated as time in non-employment minus time in unemployment. Standard errors are clustered on the discrete values of the forcing variable (age at notification). Regressions include individuals aged 52-58 at the time of notification. The regressions include a linear age polynomial interacted with threshold, baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs), and month-by-year FE:s. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

shows that the effect of being eligible for longer notification is to reduce exposure to unemployment. Treated individuals are also less exposed to non-participation events as shown in column (5).

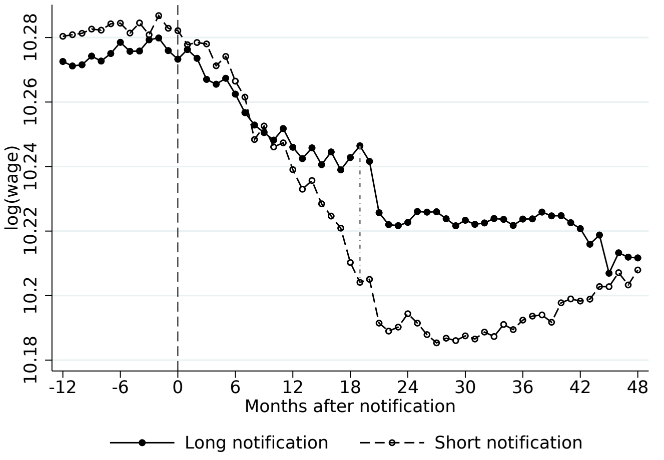
2.5.3 Wage effects

What about wages in the new job? Since longer notification means full insurance, without exposure to unemployment, one would expect individuals eligible for longer notification to find higher-quality jobs.

Figure 2.7 shows the evolution of wages by month since notification for those just above and below the age-55 threshold.[19] As before, the difference between the solid and dashed lines represent the RD-estimates. At this stage, we show wages for all employed individuals. However, employment rates differ across individuals with long and short notification, with the implication that the wage differences during the first 20 months do not necessarily have a causal interpretation (see Figure 2.6). But after 20 months, employment rates and mobility into an alternative

[19] We observe wages at a single point in time (September-November) each year. Thus the monthly variation comes from the variation in notification dates relative to the wage observation.

Figure 2.7. Wages by months since notification



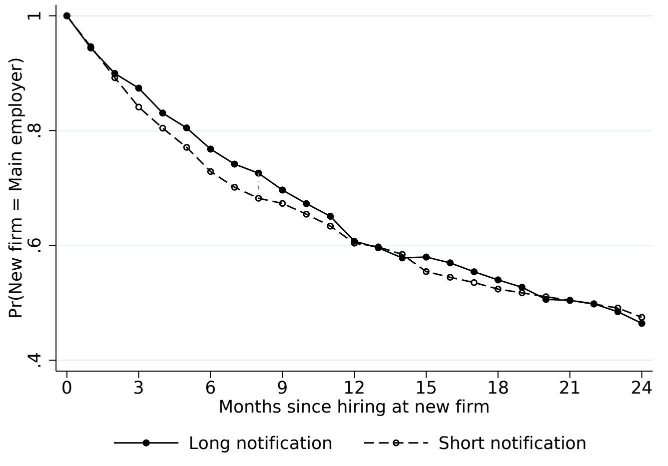
Notes: The figure shows the log monthly full-time equivalent wage by month relative to notification over time. At any given point in time, we plot estimates of the constant (hollow circles) and the constant+γ_A (black circles) from a regression corresponding to equation (2.4). Dashed lines indicate that the estimate of γ_A is significant at the 5% level. These regressions include a linear age polynomial interacted with the threshold indicator, baseline covariates, and month-by-year FE:s. The specification thus corresponds to column (2) in Table 2.3. The analysis only includes individuals aged 52-58 at the time of notification.

firm do not vary with the treatment. The difference in wages between the two groups at this point reflects differential losses associated with the move to another firm, which are caused by the treatment. On average, the wage difference between the two groups amounts to around 3 percent during months 20-36 after notification.[20]

An interesting pattern in Figure 2.7 is that wages seem to catch up for the control group after 36 months. This is consistent with the hypothesis that individuals with short notification have stronger incentives to move on to another job, and would therefore be climbing the wage ladder faster than the treatment group. If so, we should expect greater onward mobility in the control group than in the treatment group. To examine this issue, Figure 2.8 plots the survival functions in the first new job, separately for individuals with long and short notification. As

[20]Recall that these wage effects are estimated on the subsample with wage observations. These estimates are consistent with the dynamic earnings effects presented in Appendix Figure A-2.2, which, in turn, are aligned with the baseline estimates in Figure 2.4.

Figure 2.8. Probability of working at the first firm by months since employment start



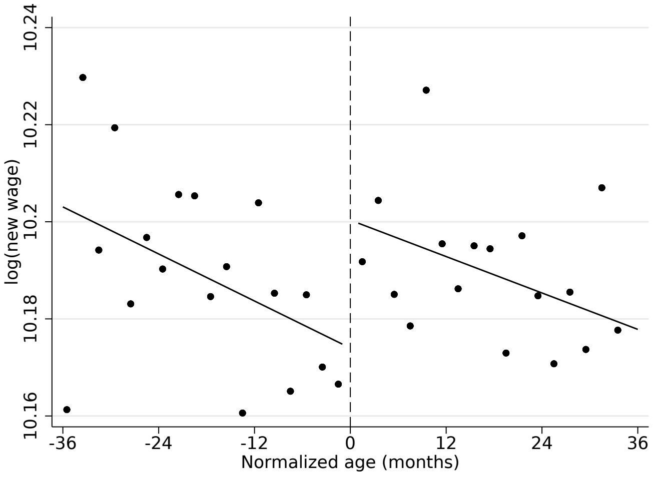
Notes: The figure shows the probability of working in the first new firm after notification over time. We define the first new job after notification as the first month of a new employment spell after the worker has left the notifying firm. The worker stays with that firm as long as imputed monthly earnings from that firm exceed those from some other firm (in months when the worker has no earnings from a different firm the threshold is zero). At any given point in time, we plot estimates of the constant (hollow circles) and the constant+γ_A (black circles) from a regression corresponding to equation (2.4). Dashed lines indicates that the estimate of γ_A is significant at the 5% level. These regressions include a linear age polynomial interacted with the threshold indicator, baseline covariates, and month-by-year FE:s. The specification thus corresponds to column (2) in Table 2.3. The analysis only includes individuals aged 52-58 at the time of notification.

before, the vertical distance represents the RD estimate and a statistically significant estimate is marked by a dashed line. The figure shows that those with short notification move on to other jobs at a greater rate than those with long notification, consistent with greater onward job mobility.

Now let us turn directly to the question of whether longer notification leads to higher-quality jobs. Since there are no employment effects after two years (see Figure 2.6), we can look at the first job that the individual obtains within the first two years without having to worry about employment rates varying by treatment status. The results are given in Figure 2.9 and Table 2.5.

The outcome in Figure 2.9 is the log of the full-time equivalent wage (adjusted using contracted hours). Because the wage information is

FIGURE 2.9. WAGE IN THE NEW JOB BY AGE AT NOTIFICATION



Notes: The figure shows log of the full-time equivalent monthly wage against age at notification, normalized around 55. Age at notification is in 2-month-bins. The regression lines come from estimating equation (2.4) with a linear age polynomial interacted with the threshold indicator for individuals aged 52-58 at the time of notification. The estimated jump at the threshold is 0.027, with a standard error of 0.015. The regression also includes the wage prior to notification, baseline covariates, and month-by-year FEs. The specification corresponds to column (2) in Table 2.3.

collected through a survey which covers around 50% of the baseline population, we lose a substantial fraction of observations. This implies that estimates are somewhat less precise. Nevertheless, Figure 2.9 shows that there is a positive wage effect of longer advance notice.

Column (1) of Table 2.5 reports the results of regressing log wages on the instrument, i.e., the estimated difference at the threshold in Figure 2.9. Longer notice, on average, allows workers to avoid 2.7% of the wage loss associated with moving to a new job after notification and this difference is statistically significant. In column (2) we use the difference in log wages as the outcome. Since prior wages are balanced, this should improve precision without affecting the estimates much. The estimates show that eligibility for longer notification reduces the wage loss associated with displacement. Whereas workers in the control group experience a wage loss of 8.6% as a result of displacement, longer notification limits the wage loss to 5.5%. The effect of being eligible for longer notification is therefore 3.1 percentage points. The additional insurance

TABLE 2.5. EFFECTS OF ADVANCE NOTICE ON INITIAL WAGES AND SUBSEQUENT WAGE GROWTH

	(1) ln(Monthly Wage)		(2) Δln(Monthly Wage)	
Panel A: Initial wage				
Above age-55	0.027*		0.031**	
	(0.015)		(0.014)	
Control mean	10.173***		-0.086***	
	(0.010)		(0.011)	
Panel B: Wage growth				
	1st year	2nd year	3rd year	4th year
Above age-55	0.019**	0.019	-0.009	-0.018
	(0.009)	(0.012)	(0.013)	(0.019)

Notes: The difference in wages is taken relative to the wage in the year prior to notification. Annual wage growth is defined relative to the initial wage for all individuals (movers as well as stayers) for whom we observe subsequent wages. Wage growth in panel B is relative to the year since start of first employment. Standard errors are clustered on the discrete values of the forcing variable (age at notification). Regressions include individuals aged 52-58 at the time of notification. The regressions include a linear age polynomial interacted with the threshold indicator, baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs), and month-by-year FE:s. Sample size in col. (1) panel a) is 3,056; sample size in col (2) panel A is 2,363. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

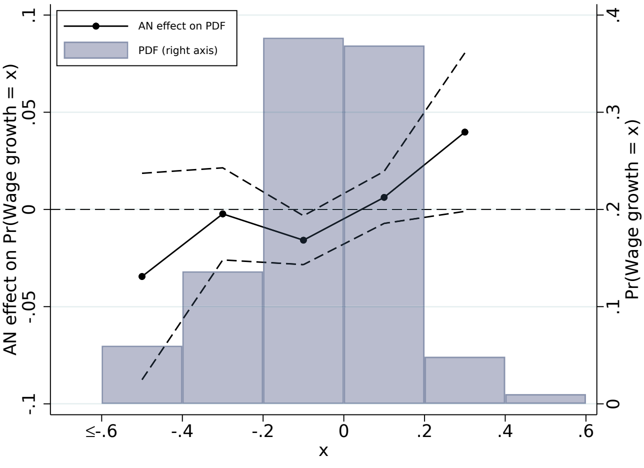
provided by longer notification thus allows workers to obtain a better job after displacement.

Figure 2.10 shows the distributional impact of being eligible for long notification.[21] It illustrates that individuals who are eligible for longer notice are exposed to wage losses to a lesser extent. Although longer notice also increases the probability of observing large wage increases (increases amounting to 20-40 log points), the average effect is driven by a lower probability of observing wage losses.

How does the average wage effect compare to the literature? The closest comparison comes from the literature on the wage effects of extending unemployment insurance (UI). With the exception of Nekoei and Weber (2017), the typical result in this literature is that job quality is unaffected by an increase in UI generosity (see Card, Chetty and Weber, 2007; Lalive, 2007; Schmieder, von Wachter and , 2016; van Ours and

[21]See Nekoei and Weber (2017) for an analysis of the distributional wage impact of prolonging UI duration.

Figure 2.10. Effects of eligibility for long Advance Notice (AN) on the distribution of wage changes



Notes: The figure shows the effect of longer advance notice on the distribution of wage changes. The blue bars show the probability density function over wage growth (read on the right axis). Each dot reflects RD-estimates (read on the left axis) from a regression corresponding to equation (2.4) within a wage growth bin. The dashed lines are 95 percent confidence intervals where standard errors have been clustered on the discrete values of the forcing variable (age at notification).

Vodopivec, 2008). Nekoei and Weber (2017) estimate a positive wage effect amounting to 0.5 percent as a result of a nine-week UI extension. They also point out that the wage effect of UI is ambiguous because of negative duration dependence: with extended benefits, the unemployed have longer time to look for a better match, but longer job search per se can have a negative effect on wages.

Our wage estimate is much larger than the estimate of the wage effect of UI generosity. But the treatment we consider is also different and more generous. With longer notification, workers have full insurance and also more time to search without being exposed to unemployment.

To what extent is the wage estimate driven by the ability to find better-paying firms? To provide some suggestive evidence on this question, we examine whether longer notification implies that the average wage of co-workers in the new firm is higher. Albeit statistically insignificant (the estimate is 0.016 with a standard error of 0.016), longer notification is associated with a higher average wage, suggesting that firm quality may partly explain our positive wage effects.

Panel B of Table 2.5 examines whether the wage effects extend beyond the initial wage. It seems that individuals with long notification are able to find better jobs, both in terms of wage levels (panel A) and initial wage growth (see the first column of panel B). Individuals with short notification have greater incentive to move on to better paying jobs, however. Onward mobility to higher-paying jobs implies that the longer run effects on wage growth are negative (although statistically insignificantly so). The evidence on wage growth thus lines up with the pattern we observed in Figure 2.7.

2.5.4 Severance pay

Next, we turn to severance payments. We think of these payments as monetary transfers from firms to workers in exchange for a shorter notice period. In order to observe such payments, it must both be in the interest for firms to offer them and workers to accept them. In this section we examine whether severance payments are affected by the treatment.

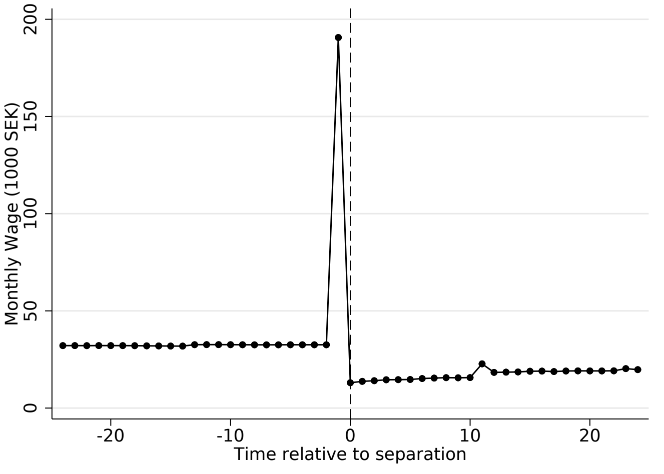
Identifying severance payments is empirically challenging. We would like to identify excess payments from the notifying firm to the worker. These kinds of payments are recorded as earnings in the data, as they are subject to regular labor income taxation, which we only observe at the annual level.

As a descriptive exercise, we focus on workers whose last month worked at the notifying firm is January. For such workers, payments from the notifying firm to the worker in that year equal severance pay plus regular wage payments for one month at most. Figure 2.11 illustrates the evolution of monthly earnings for these workers. It shows that they receive total transfers of around 180,000 SEK in the last month with the firm. If we subtract off one month of wage payments obtained from the annual payments in the preceding year (around 30,000 SEK), a crude estimate of severance pay would be 150,000 SEK. In levels this is likely an upward-biased measure of severance pay, since it includes all payments that accrue when a worker leaves a firm.[22]

To examine whether workers who are eligible for longer notification get extra severance pay, we proceed as follows. We first calculate earnings

[22]For instance, the worker may have built up an entitlement to holiday leave. The monetary value of this entitlement will be paid out when the worker leaves the firm.

FIGURE 2.11. EARNINGS FOR WORKERS FOR WHOM JANUARY IS THE LAST MONTH WITH THE NOTIFYING FIRM



Notes: Monthly earnings for workers whose last recorded month with the notifying firm is January in the year of displacement.

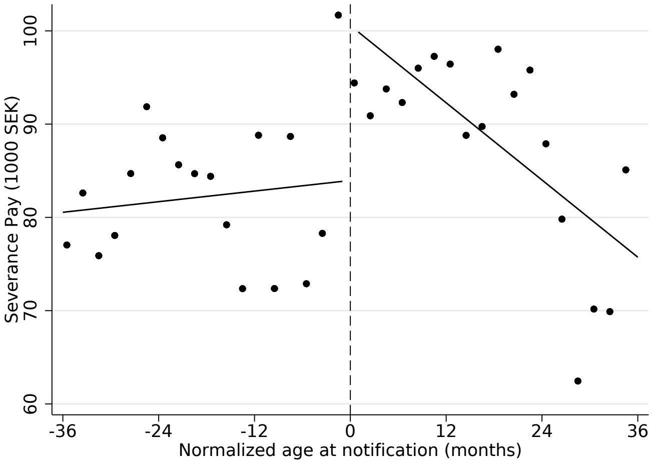
received from the notifying firm in the year of separation. If a given worker separates from this employer after, say, April, we subtract four months worth of average earnings, calculated from the previous year. We thus measure severance pay as excess earnings from the notifying employer relative to the previous year. As noted above, this will be a biased measure of severance pay in levels, but the bias gets differenced out when we compare workers across the threshold.

Figure 2.12 shows the results. When a worker surpasses the threshold, severance pay (SP) increases by 16,900 SEK. This magnitude corresponds to 56% of a full-time equivalent monthly wage. Notice that the estimated effect reflects both the extensive (the probability of observing $SP > 0$) and the intensive margin (the amount received given $SP > 0$).

In Appendix A1, we present a simple model of the choice to offer and accept severance pay. In the model, firms experience differentially sized negative productivity shocks. Firms that experience larger productivity losses, have higher willingness to pay to avoid the notice period (at least if the shock does not imply that firms trigger a credit constraint).

Workers, on the other hand, have differential propensity to accept a given severance pay offer. In particular, workers whose unemployment insurance (UI) entitlement replaces a greater share of their previous

FIGURE 2.12. SEVERANCE PAY BY AGE AT NOTIFICATION

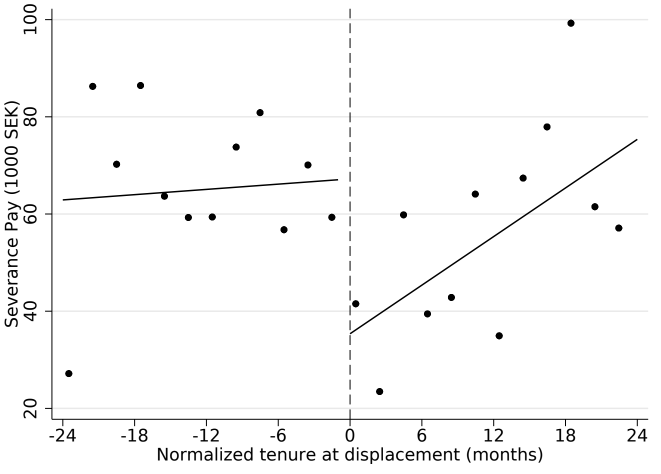


Notes: Age at notification is in 2-month-bins. The regression lines come from estimating equation (2.4) with a linear age polynomial interacted with the threshold indicator for individuals aged 52-58 at the time of notification. The estimated jump at the threshold is 16.9 (1000 SEK), with a standard error of 6.4. The regression also includes baseline covariates and month-by-year FE:s. The specification corresponds to column (2) in Table 2.3.

earnings are more likely to accept the offer. For analogous reasons the amount received is lower for workers with a higher entitlement to UI. From the worker's point of view, UI is forgone when the unproductive match is kept alive (see Pissarides, 2001). In the remainder of this section, we examine whether workers with higher UI accept lower severance pay amounts.

In this analysis, we divide the data by the workers' UI replacement rate. For members of UI funds, the public UI system offers a replacement rate of 80% up to a ceiling. The ceiling is low and the replacement rate for the average worker (given the UI rules during 2007-2015) in our sample is only 49%. However, for private sector white-collar workers, the collective agreement specifies a supplementary benefit scheme. Supplementary benefits are offered for workers above age 40 who have at least 5 years of tenure at the time of displacement. Basically, eligible workers are given a (de facto) replacement rate of no less than 70 percent. The tenure rule thus offers substantive variation in the UI replacement

FIGURE 2.13. SEVERANCE PAYMENTS BY TENURE AT DISPLACEMENT



Notes: Tenure at displacement in 2-month-bins. The vertical dashed line indicates 5 years (60 months) of tenure. The regression lines come from estimating equation (2.4) with a linear tenure polynomial interacted with the threshold indicator for individuals aged 52-58 at the time of notification who have 3-7 years of tenure at displacement (the analysis includes 2,636 individuals). The estimated jump at the threshold is -36.0 (1000 SEK), with a standard error of 10.6. The regression also includes baseline covariates and month-by-year FE:s. The specification corresponds to column (2) in Table 2.3.

rate: 70% for those above the tenure threshold, and for those below the threshold, the replacement rate may be as low as 49%.[23]

Figure 2.13 exploits the variation generated by supplementary benefit schemes. The vertical dashed line indicates 5 years of tenure at displacement. Workers to the right of this threshold have 70% of UI replacement from the supplementary UI scheme. Workers to the left of the threshold are not eligible for the supplementary scheme, and on average they have much lower replacement rates. There is clear evidence that those eligible for higher UI accept less generous severance payment deals. The downward jump at the threshold amounts to 36,000 SEK.

[23]The 49% is a lower bound since those below the threshold have greater incentives to buy top-up private insurance offered by the unions. We do not have information on how common this is.

TABLE 2.6. DECOMPOSITION OF 2-YEAR CUMULATIVE EARNINGS EFFECT

	Component/monthly wage	Percent of earnings effect
Component		
Employment effects		
non-employment	0.796	50
new job	0.056	4
Wage effects		
wage at new job	0.314	20
imbalance in initial wage	-0.148	-9
Estimated severance pay effect	0.559	35
Imputed severance pay effect	0.544	
Sum of estimated components	1.578	100
Estimated earnings effect	1.563	

Notes: Estimated earnings effect = $\Delta y / w_0^L$. Employment effects: non-employment = $-\Delta NE$; new job = $-\left[(w_0^L - w_1^L)\Delta l_1\right]/w_0^L$. Wage effects: wage at new job = $\left[l_1^S w_1^S \Delta \ln w_1\right]/w_0^L$; imbalance in initial wage = $+\left[l_0^S w_0^S \Delta \ln w_0\right]/w_0^L$ (notice that the difference in pre-treatment wages is insignificant; it equals -0.014 (SE = 0.015)). Estimated severance pay effect = $\Delta SP / w_0^L$. The index L (S) denotes eligibility for long (short) notification.

2.5.5 Pulling the pieces together

Let us return to the decomposition in equation (2.5). We are interested in decomposing the cumulative earnings effect over a 2-year horizon. If we estimate this effect directly, it corresponds to 1.56 months of pay as shown by the last row of Table 2.6. The remaining rows show the component parts using the results from Sections 2.5.2-2.5.4.

Notice that the decomposition is only approximate for two reasons. First, our earnings and wage effects are estimated based on calendar years $t \in \{0, 1, 2\}$, while employment indicators leverage the high-frequency data and consider the 24 months right after notification. Second, the wage effects are estimated on a subsample of the baseline population. While the earnings effects for the sample are close to the main results, the point estimates are not identical (see Appendix Figure A-2.2).

The employment effects come from Table 2.4. We use the wage estimate reported in column (2) of Table 2.5. The severance pay estimate comes from Figure 2.12. We also present an imputed severance pay effect, which is simply the residual after deducting the components relating to the wage effects and the employment effects from the estimated earnings effect. Despite the approximations involved in the decomposition, the estimated and imputed severance pay effect line up remarkably well.

The decomposition reveals that the most important component in the overall earnings effect is the non-employment effect (50 percent of the overall effect). Severance pay contributes to 35 percent of the earnings difference, while wages in the new job contribute to 20 percent.

2.6 Conclusions

We have estimated the impact of advance layoff notices on the future labor market prospects for workers using data from Sweden. Our main source of variation comes from collective bargaining agreements where individuals above a certain age are eligible for longer advance layoff notice.

We find that longer notice periods cause pro-longed periods of adjustment. For workers who are eligible for longer notification periods (the treatment group), the probability of remaining in the displacing firm increases during the first two years after notification and the probability of moving to another firm falls during the same time period. As a result of an extension of the notice period, workers are less exposed to unemployment and non-employment. After two years, all employment responses have subsided and there are no differential effects on employment outcomes for the treatment and control group. We also show that the treatment group experiences smaller wage losses when finding a new job than the control group: wages in the new job are 3% higher for the treatment group than for the control group. Advance notice thus offers insurance for workers.

Firms that experience unexpected and substantial productivity drops are likely to offer severance packages to workers in order to shorten the notice period, and we also see workers accepting them. The extra severance payment accruing to the treatment group amounts to almost 60% of the monthly wage. Finally, we show that workers who are eligible for higher UI get lower severance payments. This is consistent with the view that they are more willing to accept lower severance pay since they have a better outside option if they leave the firm for unemployment.

Is mandating advance layoff notice an efficient government policy? To the extent that advance notices forces firms to share (private) information on future reductions of labor demand, an efficiency case can be made for them. To the extent that productivity shocks are unexpected, however, there is a risk that advance layoff notice locks in workers in activities that are not sufficiently productive. Nevertheless, as our current analysis has shown, some of these losses can be undone if firms are able to offer severance in order to shorten the length of the unproductive period. In future work, we intend to further analyze the efficiency properties of advance notice periods.

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Appendix

A A toy model of severance payments

Production is constant returns to scale and so we take a firm to be a job. Assume that displacement shocks arrive at rate λ. Given the arrival of a displacement shock, firms make a draw from the productivity distribution; the new productivity draw is δy, where y denotes productivity prior to the shock. A displacement shock is by definition such that $\delta y < w$. A non-productive job cannot be shut down immediately because of mandatory advance notice. A job is either either productive (p), non-productive (n) ,or vacant (v).

Workers and firms are generically identical, but have experienced different draws of the random displacement shock. To simplify matters further, we take utility to be linear and assume that the notification periods ends stochastically at rate μ, so that $1/\mu$ is the expected duration of the notice period. When the notice period ends, the non-productive job becomes vacant and the worker is transferred to unemployment.

Firm values satisfy

$$rJ^p = y - w + \lambda(E(J^n) - J^p)$$

$$rJ^n = \delta y - w + \mu(J^v - J^n) + \alpha s^n(J^v - J^n)$$

$$rJ^v = -k + q(J^p - J^v)$$

where $\delta y < w$, and there is a distribution of δ among the unproductive firms, so $\delta \in \left[0, \bar{\delta}\right]$ where $\bar{\delta}$ denotes reservation productivity. By the usual free-entry condition, $J^v = 0$.

Worker values are given by

$$rW^p = w + \lambda(W^n - W^p)$$

$$rW^n = w + \mu(W^u - W^n) + \alpha s^n(W^p - W^n)$$

$$rW^u = b + \alpha s^u(W^p - W^u)$$

Using $J^v = 0$, we have

$$J^n = \frac{\delta y - w}{r + \mu + \alpha s^n} < 0$$

If $s^n = s^u$, we have

$$W^n - W^u = \frac{w-b}{r+\mu+s\alpha} > 0$$

Imagine now that the firm and the worker bargain over a severance payment, SP (we assume that wages are not renegotiated). The bargaining weight is β. By accepting the package, workers get $SP + W^u$; they give up W^n. The firm surplus generated by the package is $J^v - SP - J^n$. The solution is given by

$$SP - (W^n - W^u) = \beta(-J^n - (W^n - W^u)) \tag{2.6}$$

Workers agree to SP if

$$-(J^n + W^n - W^u) \geq 0$$

In other words, if the total surplus value associated with an unproductive job (the expression within parenthesis) is negative, then there is a severance payment that would convince the worker to leave the job. Now,

$$J^n + W^n - W^u \leq 0 \Longleftrightarrow \delta \leq \frac{b}{y}$$

This equation defines a cut-off value δ_S when severance payments are made

$$\mathbf{1}(SP > 0) = \mathbf{1}(\delta < \delta_S),\ \delta_S = \frac{b}{y}$$

If $b/y = 0.6$, for instance, then all displacement shocks resulting in a productivity drop of more than 40% will be settled using a severance payment. The share of displacements where severance payments are made is given by $F(\delta_S)$.

What about the size of the severance payment? By manipulating (2.6), and using the definition of the cut-off value, we get

$$SP = \frac{w - b + \beta y(\delta_S - \delta)}{r+\mu+s\alpha},\ \delta \leq \delta_S$$

The marginal displacement with SP has

$$SP(\delta_S) = \frac{w-b}{r+\mu+s\alpha}$$

Thus the worker is exactly compensated for what (s)he gives up by accepting the SP-package. For inframarginal displacements involving SP, the worker, additionally, gets a share of the money saved for firms.

Severance payments are increasing in $(w - b)$, the size of the productivity drop $(1 - \delta)$, and the expected length of the notification period $(1/\mu)$.

Firms' maximum willingness to pay for an increase in the notification period is proportional to $(-J^n)$, the present value cost of keeping the unproductive job alive for the firm. Workers with longer notification periods demand higher compensation in order to end the period. The minimum value that the worker is willing to accept rises with the notification period and the increase is proportional to $(W^n - W^u)$.

Firms' willingness to pay to avoid the notification period increases with the productivity drop. Workers' willingness to accept is independent of the productivity drop. Shifts in the UI replacement rate change workers' willingness to accept but leave firms' willingness to pay unaffected. Shifts in b effectively trace out the willingness-to-pay function.

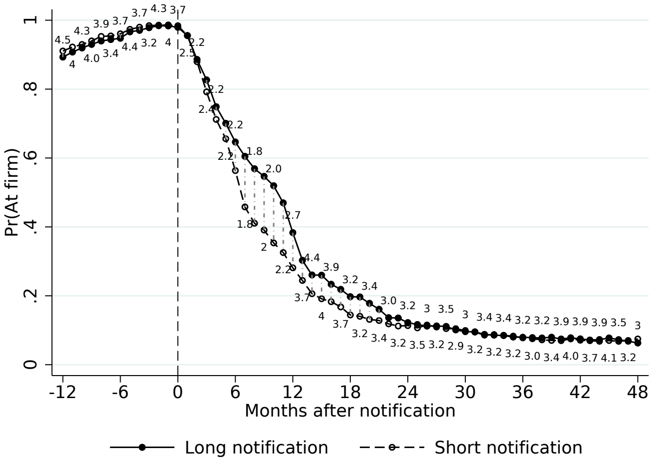
B Optimal bandwidth

Figure A-2.1 reproduces Figure 2.6 but with the optimal bandwidth selector of Calonico, Cattaneo and Titiunik (2014). The figure is built from 48 separate RD-regressions, and, consequently, there are 48 optimal bandwidths. In general, optimal bandwidths are in between 2.0 and 4.0 years, and our default bandwidth of 3 is thus well in-line with the optimal ones. There are instances when bandwith-selector picks 1.8 or 4.5 years as the optimal ones, but these are rare occasions.

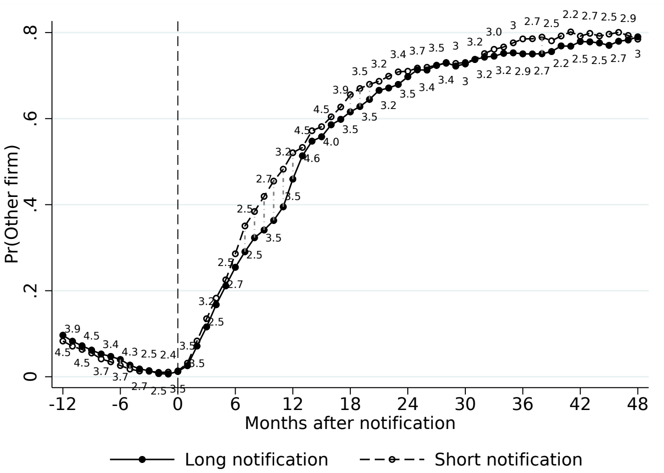
The most important message from A-2.1, however, is that none of our results change when we use this approach rather than the one we opt for in the main text. Conceptually, since age is discrete in our data, we prefer the parametric approach of the main text. Our default approach also avoids the slightly cumbersome exercise of using potentially different data sets for each single point estimate.

Figure A-2.1. Employment by month relative to notification (optimal bandwidth)

(a) *Probability of remaining at notifying firm*



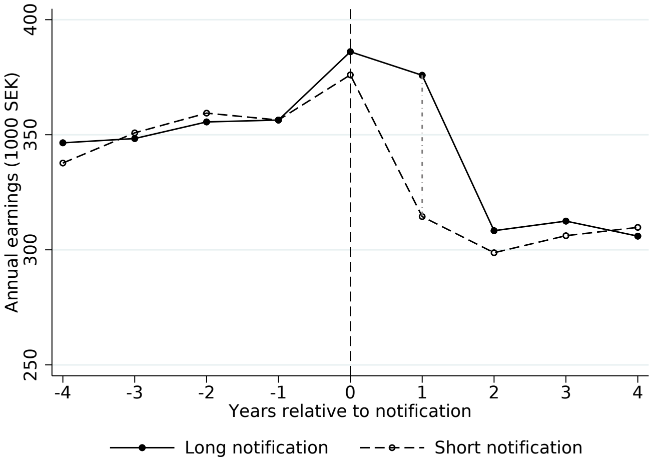
(b) *Probability of working at a new firm*



Notes: The figures show each outcome by month relative to notification. At any given time point, we plot estimates of the constant (hollow circles) and the constant+γ_A (black circles) from a local linear regression corresponding to (2.4) with a bandwidth which is indicated by the number above each point in the graph. Dashed lines indicate that the estimate of γ_A is significant at the 5%-level. The regressions also include baseline covariates and month-by-year FE:s.

C Wage sample estimates

Figure A-2.2. Earnings outcomes by years relative to notification, wage sample



Notes: The figure shows annual earnings by year relative to notification. At any given time point, we plot estimates of the constant (hollow circles) and the constant+γ_A (black circles) from a regression corresponding to (2.4). Dashed lines indicates that the estimate of γ_A is significant at the 5%-level. These regressions include a linear age polynomial interacted with threshold, baseline covariates, and month-by-year FE:s. The specification thus corresponds to column (2) in Table 1.3. The analysis only include individuals aged 52-58 at the time of notification who have a new job with an observed wage within two years after displacement.

Chapter 3

What Makes a Good Caseworker?*

with Martin Söderström and Johan Vikström

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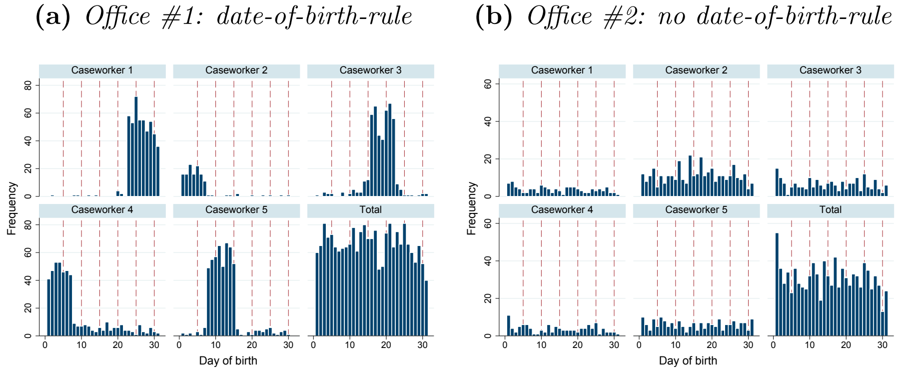
3.1 Introduction

Countries around the world use job-search assistance, monitoring schemes, and labor market programs to try to bring unemployed workers back to work. By now, there is extensive evidence on these policies (see, e.g., Card et al., 2010, 2017). However, much less is known about the caseworkers who provide the job-search assistance, carry out the monitoring, and assign job seekers to programs (see the literature review below and McCall et al., 2016). This is unfortunate, since a comprehensive picture of labor market policies requires that we understand the role of the human resources used to provide the services. It is, for example, important to know who becomes a caseworker, why some caseworkers perform better than others, and for whom caseworkers matter the most. While these are important questions, the evidence is scarce for two important reasons. First, in most cases, there is non-random sorting of job seekers to caseworkers, often because the most productive caseworkers are assigned the most disadvantaged job seekers. Second, high-quality data on caseworkers is often lacking. Usually, data do not link caseworkers to job seekers, and in the rare cases when such information is available, typically little is known about the caseworkers.

This paper addresses both of these issues. First, we break the caseworker-job seeker sorting by exploiting that many local employment offices in Sweden use date-of-birth-rules to allocate job seekers to caseworkers, creating as-if random allocation. Second, we have access to uniquely fine-grained information on caseworkers, such as labor market experiences and cognitive ability, and we can link job seekers to caseworkers. The quasi-random allocation and the fine-grained data allow us to provide new and credible evidence on the importance of caseworkers and what makes a good caseworker.

In Sweden, employment services are provided by caseworkers at local public employment offices. These offices have extensive discretion to design the rules for allocating job seekers to caseworkers. It turns out that many offices use job seekers' date of birth (day in the month) to allocate them to caseworkers, as illustrated in Figure 3.1. Office (a) uses a date-of-birth-rule, where, for example, caseworker 1 is assigned job seekers born on the 23rd to 31st of each month. Since the exact birth date is unrelated to both observed and unobserved characteristics, this

FIGURE 3.1. ALLOCATION OF JOB SEEKERS TO CASEWORKERS OVER DAY-OF-BIRTH (1–31), AT TWO LOCAL OFFICES



Notes: Number of job seekers born on each day-in-month per caseworker.

is as good as random allocation. Office (b), on the other hand, does not use a date-of-birth-rule as birth dates are evenly distributed across caseworkers. Instead, these non-date-of-birth offices use allocation rules that introduce non-random sorting, often because disadvantaged job seekers are allocated to more experienced caseworkers.

Figure 3.1 also shows that within the offices that use a date-of-birth rule, all job seekers are not allocated using the rule. Some offices make exemptions for special groups, such as youths, disabled workers, or immigrants, and allocate these groups to caseworkers who are believed to be able to provide the best support to them. Since these exemptions may introduce sorting, we use an IV-framework exploiting that we can identify the caseworker each job seeker would have had if they had been allocated using the date-of-birth rule. This rule-predicted caseworker is then used as an instrument for the caseworker assigned to the job seeker. This identification strategy adds to the existing literature on caseworkers, which mainly includes studies based on conditional independence assumptions, assuming that the allocation of job seekers to caseworkers is random conditional on observed job seeker characteristics (see, e.g., Lechner and Smith, 2007, Behncke et al., 2010b,a, Arni et al., 2017, Arni and Schiprowski, 2019).[1]

[1]One recent exception, however, is Schiprowski (2020), which exploits unplanned absences to study the effects of a meeting with a caseworker and to study productivity differences across caseworkers. Our paper uses detailed data on caseworkers and

Our unique administrative data on caseworkers include rich measures of labor market history, such as information on previous occupations and personal experience of unemployment. For most male caseworkers, we also have information on cognitive and non-cognitive ability from enlistment tests. Staff records provide information on experience (tenure at the public employment service) and wages for each caseworker. To this, we add information on demographics such as gender, level and type of education, and country of origin.

Using the fine-grained caseworker data, we initially study who becomes a caseworker. One conclusion is that caseworkers in Sweden are a heterogeneous group that includes former blue-collar workers; individuals with university degrees in social work, business economics, and human relations; and both natives and non-natives. Interestingly, caseworkers have, on average, lower cognitive skills, substantially more experience of unemployment, but similar non-cognitive skills as other public sector employees with similar types of occupations.

We then study caseworker performance in three different parts. In the first part, we analyze how different observed caseworker characteristics are related to caseworker performance as measured by the reemployment rate among their job seekers. Even though we are able to study a heterogeneous group of caseworkers, few observed caseworker characteristics predict caseworker performance. The most important characteristic is the gender of the caseworker: job seekers with female caseworkers have 3.1% shorter unemployment durations than those with a male caseworker. There is also some evidence that caseworkers' with higher wages perform better, but this may, of course, reflect both that high-performing caseworkers are rewarded with higher wages and/or that higher wages motivate caseworkers to perform better. However, many other caseworker characteristics, such as type of education, level of education, experience from previous occupations, and personal experience of unemployment, are not related to caseworker performance.[2] Moreover, caseworkers with higher cognitive ability do not perform bet-

the date-of-birth allocation to provide more comprehensive evidence on caseworker performance.

[2]This is consistent with results from the teacher literature, which finds little evidence of a relationship between teacher quality and observed teacher characteristics (Rockoff, 2004, Rivkin et al., 2005, Rockoff et al., 2011).

ter than low-ability caseworkers. There is, however, some suggestive evidence indicating that caseworkers with higher non-cognitive ability may have a positive impact on job seekers job-finding rate early on in the unemployment spell.

Based on the actions taken by the caseworkers, we also examine caseworker traits. Inspired by Arni et al. (2017), we define "supportive" caseworkers as those who more often use supportive actions, such as sending their job seekers to labor market training, whereas "restrictive" caseworkers are those who more often use restrictive policies such as workfare. Furthermore, we define "active" caseworkers as those who more frequently meet with their job seekers. Our results show that "active" caseworkers perform better than other caseworkers. This adds to the rather few existing studies: Arni et al. (2017) find that caseworkers who emphasize support have better outcomes, while Behncke et al. (2010b) show that tougher caseworkers are more successful than supportive ones.[3]

The second part of the paper examines caseworker-job seeker matching, and focus on caseworker-job seeker similarity, since it has been argued that sharing the same social background can enhance communication and trust. This is also what we find. If you are assigned a caseworker with the same gender this leads to a higher job-finding rate, but it is not the case that immigrant caseworkers provide better support to immigrants.[4] Using our fine-grained data, we are also the first to show that matching job seekers to caseworkers with similar labor market experiences and/or similar educational background leads to substantially shorter unemployment durations. Besides improved communication and trust, this may also reflect that experience from working in the same sector as the job seeker enables caseworkers to understand the individual-specific labor market opportunities, and that caseworkers can

[3]Huber et al. (2017) re-analyze the data used by Behncke et al. (2010b), and find that tougher caseworkers are not better because they assign workers to effective labor market programs, so that the differences are explained by other dimensions of the counseling and monitoring process.

[4]Behncke et al. (2010a) find that similarity in four dimensions at the same time (age, gender, education and nationality) improves employment outcomes. Studies in education (similarity between teachers and students) have found that similar ethnic background improves student outcomes (Dee, 2004), while there are mixed results for having the same gender (Neumark and Gardecki, 1998, Bettinger and Long, 2005, Dee, 2007, Hilmer and Hilmer, 2007).

use their social networks to help job seekers with similar labor market experiences.

The third and final part of the paper examines the overall importance of caseworkers by estimating caseworker fixed effects. This takes both differences due to observed and unobserved caseworker characteristics into account. The overall conclusion is that there are economically important differences between caseworkers. A one standard deviation increase in the distribution of caseworker fixed effects not only increases the job-finding rate among the job seekers by around 0.1 standard deviation but also renders about 5 percent higher earnings after three years. This confirms that caseworkers indeed can affect how quickly job seekers get back to work, a result consistent with Schiprowski (2020).[5] It is also in line with the results from other economic contexts; a large literature has documented substantial differences in teacher quality (Rockoff, 2004, Rivkin et al., 2005, Rothstein, 2010, Chetty et al., 2014a,b), and several studies have shown that managers matter for firm policies and firm performance (Bertrand and Schoar, 2002, Bloom et al., 2014, Lazear et al., 2015).

The outline of the paper is as follows. Background and institutional details are given in Section 3.2. In Section 3.3 we describe our data, and in Section 3.4 we study who becomes a caseworker. Section 3.5 presents our empirical strategies. Our results are presented in the following three sections: what makes a good caseworker in Section 3.6, the effects of caseworker-job seeker matching in Section 3.7, and the overall importance of caseworkers in Section 3.8. Section 3.9 concludes.

3.2 Background: Caseworkers in Sweden

Sweden has a long tradition of active labor market programs. Historically, Sweden has had a relatively low unemployment rate and extensive usage of active labor market programs, but over the last decades the un-

[5] A previous study on caseworkers in Sweden is Lagerström (2011). He sent out a survey to some local offices in the fall of 2002 asking how they assign job seekers to caseworkers. This revealed a number of offices that claim that they randomized job seekers to caseworkers in various (often unspecified) ways. With our data we see that many of these offices make exemptions, implying that a non-negligible share of the job seekers are not randomly allocated. But, note that these exemptions are handled by our empirical strategy.

employment rate as well as the program participation rate have moved towards more average European levels. In the middle of the period studied in this paper (2007), the unemployment rate was around 7%. In the mid 1990s, around 1.2% of the Swedish workforce participated in labor market training every year, but by 2007, that number was down to 0.04%. Other programs experienced similar declines.

All labor market policies in Sweden are organized by the Swedish Public Employment Service (PES). In the early 2000s, the PES was organized in 21 largely independent regional units, which in turn consisted of several local offices with caseworkers providing services to all job seekers in the local area. In 2007, there were around 300 local offices and 6 000 caseworkers.

Most job seekers in Sweden register at the local PES office, because registering is a requirement for obtaining unemployment insurance benefits and receiving support from the PES. Once job seekers register, they are assigned a caseworker, who is responsible for giving adequate support to the job seeker during the unemployment spell. Caseworkers have a wide range of tools at their disposal. They can decide how frequently to meet with each job seeker, because unlike many other countries, there are no formal requirements for the meeting frequency. The content and focus of the meetings are also at the discretion of the caseworker. Caseworkers also decide on what kind of labor market programs that are suitable. During our period of study, the main programs were: an intensified counseling program, vocational training (3–6 months), work practice (practice at a private or public firm), preparatory programs (as preparation for other programs), businesses start-up grants, and wage subsides.[6] Finally, to make sure that the job seekers follow the job search requirements, caseworkers are responsible for monitoring of job-search behavior.

To implement these services, caseworkers have guidelines, recommendations, rules and laws to follow. However, survey evidence in Lagerström (2011) and Lundin (2004), reveals that caseworkers have a substantial degree of discretion when deciding which programs and services job seekers should get. One reason is that the guidelines and recommendations give caseworkers a great deal of leeway. Another reason is that

[6]These programs are described in more detail in e.g. Calmfors et al. (2001).

caseworkers typically are evaluated based on specific goals (e.g., number of job seekers who find a job), and not based on the programs and strategies they use (Lundin, 2004). Altogether, it means that caseworkers are responsible for providing a range of key services to the job seekers, and they have substantial discretion when choosing and implementing these services.

Even though caseworkers are responsible for providing professional support and counseling, there is no specific education for becoming a caseworker in Sweden. Moreover, before and during the period studied in this paper, the PES argued that the diversity of jobs in the labor market requires caseworkers with different types of education and background. Therefore, there were only two formal criteria for becoming a caseworker: at least an upper secondary education degree and at least three years of work experience. This has led to a diverse group of caseworkers in Sweden, with people from different backgrounds and with different prior experiences, which offers good opportunities to study what makes a good caseworker.

Caseworkers' wages are set through individualized bargaining with re-negotiations every year. Each local office has its own yearly budget that it is free to use for salaries, office space and other costs. Thus, after bargaining with each caseworker, the local management can use the local budget to set individualized wages.

The local PES offices are supposed to adjust the activities and organization to the local needs (Lundin and Thelander, 2012). Among other things, this meant that the local offices were free to decide how they allocate job seekers to caseworkers. It turns out that some offices try to match job seekers to the caseworker that they think can give the best support. Other offices have caseworkers who specialize in job seekers from certain industries (e.g., construction) or job seekers from certain groups (e.g., immigrants and disabled workers). However, many offices use simple date-of-birth rules to allocate job seekers to caseworkers (described in detail in Section 5). Interviews with caseworkers reveal that date-of-birth-rules are viewed as a transparent and easy way to equalize the workload across caseworkers and to monitor their performance.

3.3 Data

We have detailed data on both caseworkers and job seekers that can be linked through unique caseworker identifiers.[7] In this section, we provide an overview of these data records while we describe the data more in detail in the analyses.

We use data for the period 2003–2010, because we have caseworker data starting from 2003 and after 2010 fewer offices use date-of-birth-rules. The basis for the data on caseworkers is staff records at the PES. It contains, among other things, information on which office a caseworker operates, their monthly wages, and the start and end date of each employment episode at the PES. The latter is used to construct information on caseworker experience (tenure) at the PES. Crucially, the staff records provides a link between the caseworker identifier and her (scrambled) social security number which enables us to add information from various administrative records.

We match the PES staff register with the population-wide register from Statistics Sweden, called Louise, which contain demographics, such as age, gender, and country of origin for each caseworker as well as educational information (level and field of education).

To examine the importance of different labor market experiences, we use information from unemployment and employment records, the latter containing the universe of employer-employee matches between 1993 and 2015. From unemployment records from the PES, we know if the caseworkers have personal experience of unemployment. Employment records include information on all jobs that the caseworkers had in the last 10 years, with information on, for example, sector of employment. It is used, for instance, to examine whether experience from the same sector as the job seeker matters for caseworker performance.

We also have access to measures of *cognitive* and *non-cognitive* abilities from military enlistment tests for a large share of the male caseworkers. Essentially, all men born between 1951 and 1981 were obliged to participate in an enlistment process at the age of 18, which included ability tests. The measure of cognitive ability is an index incorporating problem solving, induction capacity, and numerical, verbal and spa-

[7]Each caseworker has a unique five-letter signature that is used at the PES, for instance, when documenting meetings between caseworkers and job seekers.

tial comprehension. The non-cognitive ability is assessed by a certified psychologist. Both ability measures are normalized, and range from 1 (worst) to 9 (best), with a mean of 5. The quality of the Swedish enlistment tests is considered high, and the measures have a strong predictive power on future earnings.[8]

Detailed information on the type of support given to each job seeker is used to construct information on caseworkers' *traits*. As explained in detail in Section 3.6, based on the actions caseworkers take, we characterize them as "supportive", "restrictive" and/or "active" caseworkers.

The data on job seekers are based on similar administrative records. The register data from the PES cover all registered job seekers and contain day-by-day information on unemployment status, unemployment durations and re-employment transitions. It also include information on all participation in labor market programs, all meetings between caseworkers and job seekers, as well as other types of service provided to the job seekers. The PES registers also have different personal characteristics. Importantly, it includes information on the job seekers' exact date-of-birth, which is key when we exploit the date-of-birth-rules. Since we can link unemployment spells for the same job seeker over time, these data are also used to construct detailed measures of unemployment history. As for the caseworkers, we match job seekers to population-wide administrative records containing socio-economic information such as age, gender, marital status, educational attainment, and immigrant status. Information on employment history, previous occupations and annual earnings is obtained from the employment records.

We will consider several outcomes. These include indicators for whether the unemployment spell has ended before 90/180/360 days and log unemployment duration as well as annual earnings.

3.4 Who becomes a caseworker?

We now use the detailed data to describe the caseworkers, and compare with other public sector employees with similar occupations and with the full population in ages 20–65. This is in the spirit of Dal Bó et

[8]See Lindqvist and Vestman (2011) for a more detailed description of the enlistment tests and the measures of cognitive and non-cognitive abilities.

Table 3.1. Descriptive statistics for caseworkers and other public employees

	Caseworkers (1)	Public Employees (2)
Demographics		
Age	46.5	45.7
Female	0.62	0.65
Native	0.89	0.92
Married	0.52	0.52
Children	0.41	0.35
Education level		
Upper Secondary	0.29	0.28
University	0.67	0.67
Education field		
Social Degree	0.15	0.15
Business Degree	0.30	0.28
# observations	8 794	60 160

Notes: Statistics for caseworkers and public employees in 2005. Public employees includes officials, psychologists, social workers and caseworkers at central government authorities (other than the PES).

al. (2017) for politicians. The other public sector employees include officials, psychologists and social workers at other central government authorities.

Table 3.1 reports demographics for the caseworkers and the group of other public sector employees. The statistics convey two main messages: there is a great deal of heterogeneity among caseworkers, and in terms of demographics and education, caseworkers and other public employees are rather similar. They are both, on average, about 46 years of age, almost two thirds are women, 9 out of 10 are born in Sweden, about half of them are married, and about 40% have at least one child. Almost all individuals have completed at least upper secondary education (as expected according to the rules for becoming a caseworker) and two out of three have a university degree.

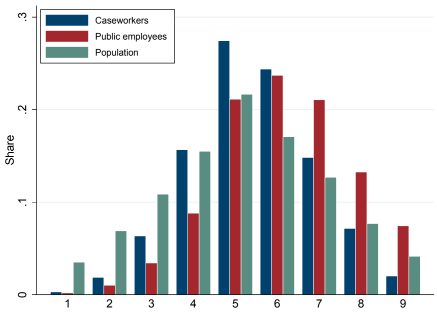
For males, Figures 3.2 (a) and (b) show the distribution of cognitive and non-cognitive scores for caseworkers, other public employees and the population. As by design, for the population, these scores follow a normal distribution with a mean of 5. The comparison of the three groups reveals some interesting differences. Caseworkers and public employees have, on average, higher cognitive scores than the full population. This comes as no surprise, as the population includes both employed and non-employed workers, and the employed typically have higher ability than the non-employed. Interestingly, caseworkers' cognitive ability is compressed towards the middle of the distribution: a large fraction of caseworkers have a score of 5 or 6, and relatively few caseworkers have very low (1 or 2) or very high scores (8 or 9). We also see that caseworkers on average have lower cognitive ability than the other public employees. In particular, the other public sector employees include substantially more individuals with a score above 6, especially more individuals with the top score.

For non-cognitive ability, there are smaller differences between the caseworkers and other public employees (Figure 3.2 (b)). In contrast with the patterns for cognitive ability, there are substantially more caseworkers with a high non-cognitive score (7 or 8). Taken together, this means that the PES, as compared to other central authorities, is able to hire people with good non-cognitive skills, but less able to recruit people with good cognitive skills.

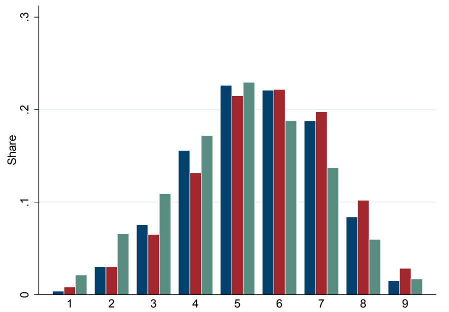
Next, Figure 3.2 (c) shows that more than 40% of the caseworkers have worked at just one workplace (the PES) in the last 10 years. The corresponding share among other public employees is less than 10%. In fact, the average caseworker-experience is 13.5 years, so there is rather limited movement in and out of the caseworker profession. Finally, Figure 3.2 (d) shows that caseworkers have more extensive unemployment history than other public sector employees. The share with some unemployment in last 10 years is 7 percentage points higher among caseworkers than the other public sector employees. Moreover, a non-negligible share of the caseworkers have several years of experience being unemployed. Actually, about 40% of the caseworkers are hired directly from the pool of unemployed, which is more than twice as common as for other public sector employees (Liljeberg and Söderström, 2017). Thus, the PES recruits many people with weak attachment to the labor market, and in particular, quite many directly from the pool of unemployed.

Figure 3.2. Abilities and measures of labor market history, for caseworkers, other public employees, and the population

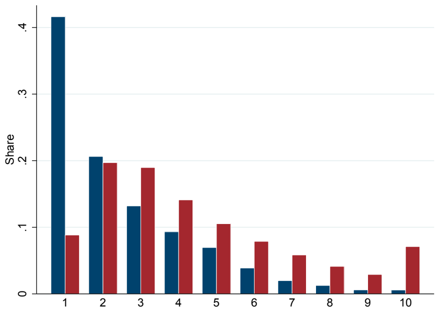
(a) *Cognitive score*



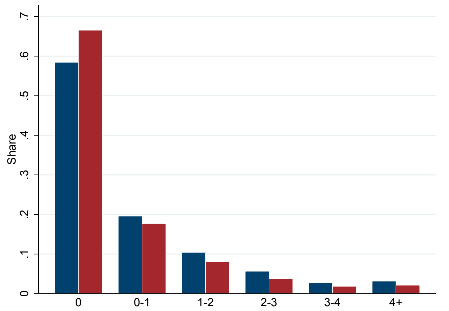
(b) *Non-cognitive score*



(c) *# workplaces last 10 years*



(d) *# years in unemployment*



Notes: Statistics for 2005. Public employees includes officials, psychologists, social workers and caseworkers at central government authorities (other than the PES), and the full population is everyone in ages 20–65. The top panel shows the distributions of cognitive and non-cognitive scores from military enlistment. The lower panel shows the number of workplaces, and years in unemployment the last 10 years.

3.5 Empirical strategy

3.5.1 Identification using date-of-birth-rules

We exploit that the day in the month you are born (1st to 31st) is uncorrelated with individual characteristics. Thus, if job seekers are allocated to caseworkers using a date-of-birth rule, this creates as-if random allocation, since all caseworkers within a local office will have job seekers with similar observed and unobserved characteristics. By construction, the allocation occurs within offices, so all empirical models include office and year fixed effects.

As already noted in the introduction, Figure 3.1 illustrates the date-of-birth-rules by showing the distribution of job seekers' date of birth for caseworkers at two offices. Figure (a) depicts an office that uses a date-of-birth-rule: caseworker 1 is responsible for job seekers born on the 23rd–31st of each month, caseworker 2 for the 1st–8th, and so on. The office in Figure (b) does not use a date-of-birth-rule, leading to an even distribution of the dates of birth across caseworkers. These offices without date-of-birth rules use different allocation rules, such as trying to match productive caseworkers to the most disadvantaged job seekers or letting caseworkers specialize in different occupational groups. In both cases, this creates non-random sorting, which is why we exploit the as-if random allocation created by the date-of-birth rules.

Figure 3.1 (a) also shows that offices occasionally make exemptions from the date-of-birth rules, however. For instance, caseworker 4 with dates 1st–8th also has some job seekers born on other days of the month. Some reasons for such exemptions are that some job seekers have a preference for a specific caseworker, and that job seekers with special needs occasionally are exempted. Even though the exemptions are rather rare, they still create non-random sorting. We therefore use an IV framework, where the caseworker that a job seeker would have had according to the date-of-birth rule (predicted caseworker) is used as an instrument for the actual caseworker. For instance, when studying what makes a good caseworker, the characteristics of the actual caseworker are instrumented by the same characteristics for the predicted caseworker. As evident from Figure 3.1, there is often a close connection between the predicted caseworker and the actual caseworker, leading to strong instruments.

Moreover, since the predicted caseworker is based only on date of birth, it is as-if random.

Optimally, this procedure is supported by complete information on the offices that use a date-of-birth rule as well as information on the days of the month each caseworker is responsible for. The latter is relevant, since caseworkers at some larger offices have 3–4 days of the month, whereas caseworkers at smaller offices may be responsible for 7–8 days of the month. Unfortunately, the PES never collected this type of information. However, as apparent from Figure 3.1, data often immediately reveal if the office uses a date-of-birth rule. To show this, we test if the job seekers' dates of birth are evenly distributed across caseworkers within each office.[9] The distribution of the resulting F-tests in Figure A-3.1 in the appendix (with truncation at 400) shows that many offices clearly use a date-of-birth-rule (high F-value), but also that many offices do not (low F-value).

Since we also lack institutional information on which days of the month each caseworker is responsible for, data is also used to construct information on the predicted caseworker. For each office and each day of the month, the predicted caseworker is the caseworker with the largest number of job seekers born on that day. For instance, the caseworker with the most job seekers born on, for example, the 10th, will become the predicted caseworker for all job seekers born on the 10th. For many offices, including the date-of-birth office in Figure 3.1 (a), this procedure will capture the actual date-of-birth rule very well.

One advantage of this strategy is that it is applicable for all offices, even for offices without a date-of-birth rule. The only difference is that the predicted caseworker is a strong instrument for the actual caseworker at the date-of-birth offices, but not for offices without a date-of-birth rule, since for them the predicted caseworker is uncorrelated with the actual caseworker. However, note that this only weakens the first stage: the predicted caseworker instrument is always random, as it is based solely on the date of birth. This implies that we can include all offices in the analyses, noting that the complier population behind the local average treatment effect (LATE) consists of job seekers at the date-of-

[9]Specifically, we regress the job seekers' date-of-birth (1–31) on caseworker dummies (within office and year) and examine the joint F-statistic.

birth offices.[10,11] Also, as mentioned above, this IV-strategy handles all selective exemptions from the date-of-birth-rules.

We also note that some offices use special date-of-birth-rules for youths (aged 24 or younger). One example is the office in Figure A-3.2 in the appendix, which clearly has separate date-of-birth rules for youths and older job seekers. We therefore allow the predicted caseworker to be different for younger and older workers born on the same day of the month.

3.5.2 Part I: What makes a good caseworker?

The as-if random date-of-birth strategy is used in all three parts of the paper. The first part examines how caseworkers' demographics (e.g., gender, age), labor market experiences, abilities and wages are related to job-seeker outcomes. That is, we study how caseworker performance is related to different observed characteristics (CW^X). Specifically, for job seeker i in office j in year t with caseworker k our model is:

$$y_{ijkt} = \alpha_1 + \beta_1 CW^X_{ijkt} + (\phi_j \times \gamma_t \times \theta^{25}_i) + \eta_{ijkt}. \tag{3.1}$$

We instrument the characteristics of the actual caseworker, CW^X, with the same characteristics for the predicted caseworker using 2SLS. Note that we adjust for office (ϕ_j), year (γ_t) and age-group (θ^{25}_i) fixed effects, and the interactions between them. This is because the date-of-birth rules create as-if random allocation within offices, and since the rules may change over time we include office and year interactions. Finally, as mentioned above, some offices use separate rules for youths and older workers, which motivates the age-group fixed effects. We cluster standard errors at the caseworker level.

[10] As a background to the LATE, Table A-3.1 in the appendix presents sample statistics for caseworkers and job seekers at all offices and for offices with a date-of-birth rule (defined by a F-value, as reported in Figure A-3.1, larger than 400). It shows that the date-of-birth offices are rather similar to the other offices, in terms of both their caseworkers and job seekers. The only notable difference is that the date-of-birth offices are larger, implying that caseworkers at the date-of-birth offices have more job seekers than caseworkers at other offices.

[11] We have also re-estimated our model using only the date-of-birth offices, leading to similar conclusions (see column 5 of Table A-3.5 in the appendix).

TABLE 3.2. FIRST-STAGE REGRESSIONS OF ACTUAL CASEWORKER CHARACTERISTICS ON THE DATE-OF-BIRTH-PREDICTED CASEWORKER CHARACTERISTIC

	Caseworker experience (1)	Caseworker univeristy education (2)
Instruments		
Predicted caseworker experience	0.334*** (0.011)	-0.000 (0.000)
Predicted caseworker university education	-0.155 (0.136)	0.340*** (0.007)
Joint F-statistic	629	
F-statistic	475	1,110
# observations	2,220,061	2,220,061

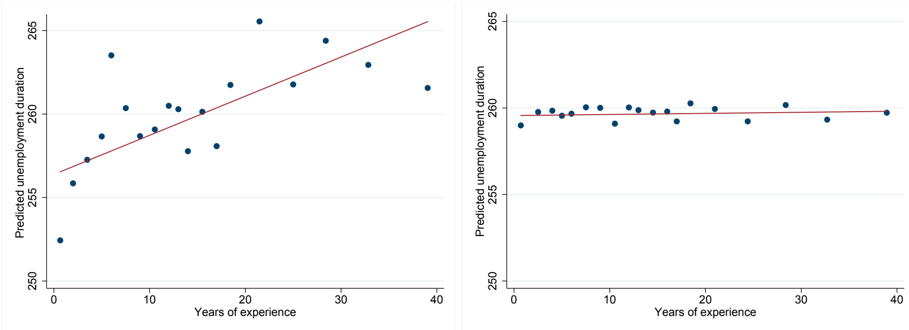
Notes: The sample consists of registered unemployed individuals between 2003–2010. Actual caseworker characteristics has been regressed on predicted caseworker characteristics, jointly. For details on how predicted caseworker is defined, see section 3.5.1. All models also include interacted year fixed effects, office fixed effects, and a dummy for above the age of 25. Joint F-statistic is from the joint test that all coefficients are equal to zero. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Sampling Fore some smaller offices, the procedure to define the predicted caseworkers is noisy and we therefore exclude offices with fewer than 200 registered job seekers per year and caseworkers with fewer than 30 job seekers per year. We also exclude job seekers with an unemployment spell in the year before the current spell, because they are often exempted from the date-of-birth allocation. The final data set consists of 2,220,067 unemployment spells, 1,601,217 unique job seekers and 3,985 caseworkers. The average job seeker is 32 years old, about half of them are females, almost 90% are Swedish and 30% have only primary school education (see Table A-3.1 in the appendix).

Relevance We now provide empirical evidence in support of our IV strategy. Initially, we examine the first-stage and show that our instruments (characteristics of the predicted caseworker) are correlated with the endogenous variables (characteristics of the actual caseworker). Since the identifying variation is across caseworkers within each office, year, and age group, these first-stage models also include a full set of office×year×youth fixed effects. The first-stage estimates for caseworker

Figure 3.3. Predicted unemployment duration versus actual (a) and predicted (b) caseworker experience

(a) *Actual caseworker* **(b)** *Predicted caseworker*



Notes: The figure plots job seekers' predicted unemployment duration and years of experience of a) actual caseworker and b) predicted caseworker. The sample include the inflow of job seekers 2003–2010. Each point are averages in bins of equal size with fitted linear regression lines. Predicted unemployment durations are generated by taking the fitted values from a regression of actual unemployment duration on duration of last unemployment spell, amount of welfare benefits last year, regional unemployment rate, age, age squared and dummies for UI eligibility, disability, immigrant, female and 6 levels of education, after adjusting for the interaction of office, year and above/below age 25 fixed effects.

experience (column 1) and caseworker education (column 2) in Table 3.2 show that we have a strong first-stage, with first-stage F-statistics of 475 and 1,110. The joint F-statistic for these two variables is also high (629). The full first-stage estimates for all caseworker characteristics reported in Table A-3.2 in the appendix show that we also have a strong first-stage for all variables.[12]

Independence Since our predicted-caseworker instruments are based on date of birth they should be as good as randomly assigned. Here, we show that this is indeed the case. Initially, we use the predicted unemployment duration for each job seeker as one unified measure of job-seeker quality. Specifically, we use the predicted unemployment duration for each job seeker from an OLS regression using standard covariates, such as age, gender, education, immigrant status, and labor mar-

[12] Note that each first-stage equation includes all instruments, but for each caseworker characteristic the most relevant instrument is the predicted caseworker equivalent. Moreover, the joint F-test for all instruments are high and well above the conventional rule-of-thumb, so that there is no problem with weak instruments.

TABLE 3.3. RANDOMIZATION TESTS: DATE-OF-BIRTH RULES AND ALLOCATION OF JOB SEEKERS TO CASEWORKERS

	Dependent variable: Predicted unemployment duration	
	Actual caseworker (1)	Predicted caseworker (2)
Caseworker experience	0.424***	0.015
	(0.078)	(0.026)
Caseworker univeristy education	11.575***	0.292
	(1.653)	(0.562)
F-statistic	28.442	0.238
p-value	0.0000	0.7882
# observations	2,172,036	2,172,036

Notes: OLS regressions for job seekers' predicted unemployment duration on actual/predicted caseworker characteristics. The sample consists of registered unemployed individuals between 2003–2010. All models also include interacted year, office fixed effects and a dummy for below or above the age of 25. F-statistic is for a joint test that all coefficients are equal to zero. Predicted unemployment durations are generated by taking the fitted values from a regression of actual unemployment duration on duration of last unemployment spell, amount of welfare benefits last year, regional unemployment rate, age, age squared and dummies for UI eligibility, disability, immigrant, female and 6 levels of education. Standard errors in parentheses clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

ket history.[13] Figure 3.3 (a) plots the predicted unemployment duration against the experience in years for the actual caseworker, revealing a striking selection pattern as more experienced caseworkers are assigned to job seekers with weaker attachment to the labor market (longer predicted unemployment durations). Since we use the full sample of offices, this captures sorting at offices without a date-of-birth rule as well as sorting due to the exemptions at the date-of-birth-rule offices. However, as expected, Figure 3.3 (b) shows that this sorting vanishes when we use the predicted caseworker: the experience of the predicted caseworker is totally unrelated to job-seeker quality (predicted unemployment duration). It holds because the exact date of birth, which the predicted instrument is based upon, is orthogonal to individual characteristics of the job seekers and thereby as good as randomly assigned.

[13]The exact variables used are the duration of the last unemployment spell, welfare benefits in the last year, regional unemployment rate, age, age squared and dummies for UI eligibility, disability, immigrant, female and levels of education (6 levels).

These patterns are confirmed by the regression estimates in Table 3.3. Column (1) shows that more experienced caseworkers and caseworkers with a university education are paired with job seekers who a priori have a higher predicted unemployment duration. However, column (2) shows that there is no correlation between experience and education of the predicted caseworker, and the predicted unemployment duration of the job seekers. It confirms that the independence condition holds.[14]

Finally, we note that one potential threat to causal identification is that immigrants whose birth date is unknown upon arrival to Sweden are registered as being born on specific dates, such as the 1st, 5th, and 10th, and this may lead to correlation between registered date of birth and job-seeker characteristics. But, sensitivity analyses reported in Table A-3.5 in the appendix show that this does not affect our results.[15,16]

3.5.3 Part II: Caseworker–job seeker matching

In the second part, we examine the matching of caseworkers and job seekers, and study if similarity between the job seeker and the caseworker matters. This is studied in several dimensions, using information

[14]We obtain similar evidence in favor of independence for all other caseworker characteristics. We have also correlated the instruments with each separate job-seeker characteristic. Here, Table A-3.3 shows that job-seeker characteristics such as age, disability and education are highly predictive of actual caseworker experience, but these correlations disappear once we use the predicted caseworker (1 out of 13 coefficients are significant at the 5% level, and all are much smaller than for the actual caseworker).

[15]To test for immigrant induced non-random sorting, we control for immigrant status and alternatively include day-of-the-month birth date fixed effects. The latter removes the selective birth date registration to certain days of the month, but note that our model is still identified, since there is variation in the predicted caseworkers across offices for individuals on the 1st. In both cases, our main results are confirmed.

[16]Monotonicity and the exclusion restriction also need to hold. Monotonicity requires, for instance, that job seekers with a experienced predicted caseworker should not obtain a less experienced actual caseworker than if they had a predicted caseworker with less experience. This cannot be tested formally, but it implies that the first-stage estimates should go in the same direction for all sub-samples (see, e.g., Bhuller et al., 2020). Table A-3.4 shows that the first-stage estimates for various sub-samples all are positive and significant, lending some support of the monotonicity assumption. Next, the exclusion restriction is violated if there are important unobserved caseworker characteristics that are correlated with the characteristics that we examine. Even though we have extensive data on the caseworkers, this, of course, cannot be ruled out.

on similarity in terms of gender, ethnicity, educational background and occupational background.

Empirically, we study one similarity dimension at a time, and create a matching variable ($Match^X$), that takes the value one if the job seeker and the caseworker are similar in the specific dimension, and zero otherwise. For example, for gender, the match-variable takes the value one if both the caseworker and the job seeker are males or if both are females. The model also includes the main effects of the gender of the caseworker and the job seeker. Thus, the model includes the match effect for characteristic X, the direct effect of having a caseworker with characteristic X, CW^X_{ijkt}, as well as the same characteristic for the job seeker, $Jobseeker^X_{ijkt}$:

$$y_{ijkt} = \alpha_2 + \delta_2 Match^X_{ijkt} + \beta_2 CW^X_{ijkt} + \lambda_2 Jobseeker^X_{ijkt} + (\phi_j \times \gamma_t \times \theta_i^{25}) + \eta_{ijkt}. \tag{3.2}$$

As before, we use the corresponding variables for the predicted caseworker to instrument for the caseworker-variables. For instance, the actual same-gender match variable is instrumented by the same match variable where the predicted caseworker is used instead of the actual caseworker.

As in the previous model, we include office (ϕ_j), year (γ_t), and age-group (θ_i^{25}; below/above 25 years of age) fixed effects, as well as the interactions between these fixed effects. First-stage F-statistics reported in the results section confirm that we have strong first-stages. As expected, similar randomization tests as above support the independence assumption (not reported).

3.5.4 Part III: How important are caseworkers?

To identify overall caseworker value-added, the third part estimates caseworker fixed effects. In the two previously explained parts of the paper we use the full sample of offices and use the characteristics of the predicted caseworker as instruments for the characteristics of the actual caseworker, noting that the complier population is based on the offices with date-of-birth allocation. However, when we study caseworker fixed effects we need one instrument for each caseworker effect, and by construction, we have no relevant instruments for the caseworkers at the offices without a date-of-birth rule. In this part, we therefore restrict the analysis to the offices with distinctive date-of-birth-rules, since for these offices there is a close connection between the actual and the predicted caseworker, leading to a strong instrument for each caseworker fixed effect. Specifically, we use the test reported in Figure A-3.1, and select offices with a F-value larger than 400. Sample statistics for these offices are shown in Table A-3.1 in the appendix. Furthermore, in order for our model to be identified we need to restrict our sample to caseworkers who at one time have become the predicted caseworker.[17] Our model is:

$$y_{ijkt} = \alpha + \mu_k + (\phi_j \times \gamma_t \times \theta_i^{25}) + u_{ijkt} \tag{3.3}$$

where μ_k is the caseworker fixed effects which are instrumented with indicators for each predicted caseworker. That is, for each endogenous variable (caseworker fixed effect) we have one instrument (indicator for the predicted caseworker), so that we have a just-identified model with many endogenous variables and equally many instruments. The F-statistics for the first-stages are illustrated in Figure A-3.3, and as before the independence condition holds because identification is based on date-of-birth.

This results in a set of estimated caseworker fixed effects, and the distribution of these fixed effects gives the relative difference in overall caseworker performance. However, by construction, these fixed effects are estimated with sampling error. This will exaggerate the variance of the fixed effects, as some of the observed variation is due to sampling noise. Thus, to quantify differences in caseworker performance, we have to separate the true variance of the caseworker fixed effects from this sampling variation. But this is a well-studied problem examined in detail, for instance, in the teacher fixed effects literature. Here, we follow the iterative procedure used by Leigh (2010), which uses the overall variance and the variance for each estimated fixed effect to decompose the observed variation into two parts: the variation in the true caseworker fixed effects and sampling variation.

[17] Occasionally, a caseworker is assigned as the predicted caseworker for one single day of the month, and another caseworker is the predicted caseworker for the two surrounding days. To avoid these noisy predictions, we also exclude caseworkers who are the predicted caseworker for one single day.

3.6 Part I: What makes a good caseworker?

We now exploit our fine-grained data on caseworkers and examine if caseworkers' demographics, education, labor market experiences, abilities, and wages predict who are successful caseworkers. In all subsections, we use the model in equation (3.1).[18]

3.6.1 Demographics and education

We have extensive demographic and educational information for all caseworkers, but to avoid spurious correlations we start with a relatively parsimonious model with the a priori most important variables. We use basic demographics (age, gender and immigrant status), the two main levels of education (high school and university) and the two most common fields of education (business economics, and social and behavioral sciences). Interestingly, Table 3.4 shows that many of the observed caseworker characteristics are unrelated to employment outcomes for the job seekers (i.e., unrelated to caseworker quality). The only exception with a significant coefficient is the gender of the caseworker. Being assigned a female caseworker shortens the unemployment duration by, on average, 3.1% (column 4), and increases the probability to leave unemployment within 90 and 180 days by 1 and 1.1 percentage points, respectively (columns 1 and 2).

We conclude that older and/or native caseworkers do not perform better than other caseworkers. Moreover, even though a higher level of education, in general, is related to higher ability and better skills, it cannot explain what makes a good caseworker. Finally, whether the caseworker has a degree in business economics or social and behavioral sciences, which include human resource management, also makes little difference. This suggests that general knowledge of the recruitment process is of minor importance for caseworker performance.

[18]Below we examine blocks of covariates at a time, but sensitivity analyses in Table A-3.5 in the appendix show that we get similar results when we regress each characteristic separately (column 1) or include all caseworker characteristics at the same time (column 4).

TABLE 3.4. CASEWORKER DEMOGRAPHICS, CASEWORKER EDUCATION AND JOB SEEKER OUTCOMES

	Leave unemployment within			log(duration)
	90 days (1)	180 days (2)	360 days (3)	(4)
Caseworker demographics				
Age	-0.000	-0.000	-0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.000)
Female	0.010***	0.011***	0.007***	-0.031***
	(0.003)	(0.003)	(0.003)	(0.009)
Swedish	0.005	0.006	0.007	-0.009
	(0.005)	(0.005)	(0.004)	(0.014)
Caseworker level of education				
High school degree	0.000	0.007	-0.003	-0.001
	(0.008)	(0.008)	(0.007)	(0.023)
University degree	-0.006	0.003	-0.008	0.025
	(0.008)	(0.008)	(0.006)	(0.023)
Caseworker field of education				
Business degree	0.001	-0.000	-0.002	0.002
	(0.003)	(0.004)	(0.003)	(0.010)
Social degree	0.007	0.001	0.001	-0.013
	(0.005)	(0.005)	(0.004)	(0.014)
Mean outcome	0.423	0.634	0.801	4.769
First-stage F-statistic	134	134	134	134
# clusters	6,816	6,816	6,816	6,816
# observations	2,218,754	2,218,754	2,218,754	2,218,754

Notes: IV estimates using where each characteristic of the actual caseworker is instrumented with the corresponding characteristic of the predicted caseworker. All models also include interacted year fixed effects, office fixed effects, and a dummy for above the age of 25. First-stage F-statistic is a joint test for all instruments. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

3.6.2 Experience and wages

Studies on other occupations suggest that experience matters for productivity and performance (e.g., Shaw and Lazear, 2008, Haggag et al., 2017). By exploiting information from the PES staff records we have exact information on experience (tenure) as a caseworker at the PES, and Table A-3.1 shows that there is a great deal of heterogeneity in caseworker experience. Here, we divide caseworkers into five groups – 0–2, 3–5, 6–10, 11–20 and more than 20 years of experience – using caseworkers with 0–2 years of experience as a reference group.

The results in Panel A of Table 3.5 reveal no significant differences between experienced and non-experienced caseworkers at conventional levels. However, there is some suggestive evidence indicating that caseworkers with the most experience are able to increase job seekers job-finding rate. First, there is a weakly significant effect on the probability of re-employment within 6 months by getting assigned a caseworker with more than 20 years of experience (column 2). Second, the log unemployment duration in column (4), there is a tendency towards better outcomes for more experienced caseworkers, and the effect when comparing caseworkers with more than 20 years of experience and newly hired caseworkers is of the about same magnitude as the difference between male and female caseworkers. Thus, even though there are no clear significant differences, we cannot rule out that caseworker-experience matters.

Next, the results in Panel B of Table 3.5 indicates that caseworkers with higher wages perform better. If the caseworker's wage goes up by SEK 1000 ($\approx$ \$110), the 90 and 180-day job-finding rate goes up with 0.2 percentage points (column 1) and the average duration shortens by about 0.6%. If we compare caseworkers at the 25th and 75th percentiles of the caseworker wage distribution, the wage difference is about SEK 2130, which would translate into an increased job-finding rate among their job seekers by about 0.4 percentage points or 1%. This correlation between wages and caseworker performance may, of course, reflect that high-performing caseworkers are rewarded with higher wages and/or that higher wages motivate caseworkers to perform better.

Table 3.5. Caseworker experiences, wages, labor market experience, abilities and job seeker outcomes

	Leave unemployment within			log(duration)
	90 days	180 days	360 days	
	(1)	(2)	(3)	(4)
Panel A: Caseworker experience (tenure)				
3-5 years	0.008	0.006	-0.002	-0.012
	(0.006)	(0.006)	(0.005)	(0.016)
6-10 years	-0.000	0.006	0.003	-0.007
	(0.005)	(0.005)	(0.004)	(0.014)
11-20 years	0.002	0.006	0.003	-0.010
	(0.005)	(0.005)	(0.004)	(0.014)
20+ years	0.006	0.010*	0.004	-0.024
	(0.005)	(0.005)	(0.004)	(0.015)
Mean outcome	0.423	0.634	0.801	4.769
First-stage F-statistic	525	525	525	525
# observations	2,220,061	2,220,061	2,220,061	2,220,061
Panel B: Caseworker wage (SEK 1000)				
Caseworker wage	0.002**	0.002**	0.001*	-0.006**
	(0.001)	(0.001)	(0.001)	(0.003)
Mean outcome	0.425	0.637	0.803	4.762
First-stage F-statistic	819	819	819	819
# observations	2,154,703	2,154,703	2,154,703	2,154,703
Panel C: Caseworker labor market experience				
Own unemployment	-0.001	-0.002	-0.002	0.003
	(0.003)	(0.003)	(0.003)	(0.009)
Experience from private sector	0.000	-0.003	0.002	-0.011
	(0.006)	(0.006)	(0.005)	(0.017)
Mean outcome	0.423	0.634	0.801	4.769
First-stage F-statistic	282	282	282	282
# observations	2,220,061	2,220,061	2,220,061	2,220,061
Panel D: Caseworker abilities				
Cognitive	-0.002	0.002	-0.004	0.004
	(0.004)	(0.005)	(0.004)	(0.013)
Non-Cognitive	0.008*	0.001	0.000	-0.010
	(0.005)	(0.005)	(0.004)	(0.014)
Mean outcome	0.444	0.657	0.821	4.695
First-stage F-statistic	203	203	203	203
# observations	254,172	254,172	254,172	254,172

Notes: IV estimates using where each characteristic of the actual caseworker is instrumented with the corresponding characteristic of the predicted caseworker. Tenure as caseworker at the PES in years. Wages based on staff records in SEK 1000. Own unemployment is an indicator for more than 30 days of unemployment in the last 10 years. Experience from manufacturing or retail is an indicator from working in these sectors in the last 10 years. Abilities on a scale from 1 to 9 standardized to have a mean of zero and a standrad deviaton of one. All models also include interacted year fixed effects, office fixed effects, and a dummy for above the age of 25. First-stage F-statistic is a joint test for all instruments. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

3.6.3 Experience of unemployment and other occupations

We have seen that caseworkers have far more personal experience of unemployment compared to other comparable public sector employees, and Liljeberg and Söderström (2017) show that around 40% of the new caseworkers are recruited from the pool of unemployed. This begs the question if experience of unemployment is good or bad. On the one hand, personal unemployment experience may give caseworkers some insight into the practical problems associated with job search, but on the other hand, extensive unemployment experience may correlate with less favorable unobserved caseworker-attributes.

Here, we define personal experience as more than 30 days of unemployment in the last 10 years, but we find similar results with other cut-offs. Panel C of Table 3.5 reveals no differences between caseworkers with and without unemployment in the past. It suggests that personal experience of unemployment are irrelevant for caseworkers.[19] This result is also highly policy-relevant, since the PES recruits many caseworkers from the pool of unemployed. Our results suggest that this does not lead to worse (nor better) outcomes for the job seekers.

Next, we exploit the information on all previous occupations held by the caseworkers, and examine if caseworkers with certain occupational experience perform better. It may, for instance, be important to have personal experience from the private sector or blue-collar work when providing job-search counseling. Even though quite many caseworkers have been at the PES for a long time, it also includes caseworkers with personal experience from other occupations. A starting point is that many job seekers search for private sector jobs. Hence, previous employment in the private sector is likely to have given caseworkers valuable experience that they may use to help their job seekers.[20] Therefore, we use previous experience from these two sectors (public and private) as valuable labor market experiences.

Interestingly, Panel C of Table 3.5 reveals no significant impact of this kind of previous labor market experience, and the point estimates go in different directions. This suggests that experience from different previ-

[19] Or the unobserved caseworker attributes perfectly cancel the benefits of unemployment experience.

[20] Private sector jobs is here defined as having worked in any of the following industries: manufacturing, construction, retail, hotel and restaurant.

ous occupations does not matter, at least not on average. However, it may be the case that previous labor market experiences are important only for certain groups of job seekers. For instance, caseworkers who previously worked in the private sector may provide better counseling to job seekers searching for private sector jobs. This highlights the questions about caseworker–job seeker matching that we consider in Section 3.7.

3.6.4 Cognitive and non-cognitive ability

Section 3.4 revealed an interesting pattern for caseworkers' cognitive and non-cognitive ability: a large share of the caseworkers is in the middle of the cognitive ability distribution, and compared to other public sector employees with similar occupations caseworkers have worse cognitive skills but similar non-cognitive skills. This illustrates the type of people the PES is able to recruit, but does it matter for the quality of the counseling support given to the job seekers? To examine this, we standardize the 1 to 9 cognitive and non-cognitive scores to have a mean of zero and a standard deviation of one, but we find similar results with more flexible specifications (e.g., a dummy for each score-level).

The results in Panel D of Table 3.5 indicate that cognitive ability is relatively unimportant: the estimates for all four outcomes are insignificant, close to zero, and go in different directions. For non-cognitive ability there exists a weakly significant effect on the 90 day job-finding rate where job seekers who are assigned a caseworker with one standard deviation higher non-cognitive ability are 0.8 percentage points more likely to leave unemployment. Overall, however, we find no strong evidence indicating that neither cognitive nor non-cognitive ability would be very important for caseworker performance. For public policy, this means that ability tests is not a very informative way to screen and recruit new caseworkers. It also suggests that the PES should not be overly worried about the fact that they largely is unable to recruit high-cognitive caseworkers. Lastly, note that these results imply that cognitive and non-cognitive abilities do not matter for the average job seeker, but they may matter for some groups of workers, for instance, in terms of the ability matching of caseworker and job seekers. We return to this below.

3.6.5 Traits

Next, we examine caseworker traits and whether supportive or restrictive caseworkers perform better. To this end, we mainly follow Arni et al. (2017) and label caseworkers as "supportive" if they are more prone to send job seekers to more supportive policies (training), and "restrictive" caseworkers are those who more often use restrictive policies (workfare). The idea is that training, which focuses on improving the skills of the job seekers, promotes employment through increased *support*, whereas workfare, which typically is used as a tax on leisure and to test whether the job seeker is ready to take a job, promotes employment through pressure and *restrictions*.[21] We also add a third trait and label caseworkers as "active" if they more frequently meet with their job seekers – i.e., whether they more actively try to help their job seekers or not. We argue that these traits capture how caseworkers approach their profession, so that it reflects a deeper caseworker trait that goes beyond the effect of using these specific policies per se, but by construction, any differences between the traits will also capture the direct effect of the policies.

To define the traits, we calculate each caseworker's propensity to use each policy, where supportiveness (restrictiveness) is based on the fraction of job seekers assigned to training (workfare). We do this separately for each job seeker using a leave-one-out mean. Similarly, active caseworkers are defined by their meeting frequency. We then standardized these propensities and obtain measures with a standard deviation of one. In a similar way as before, the trait of the actual caseworker is instrumented by the corresponding trait for the predicted caseworker.

The results in Table 3.6 show that "active" caseworkers perform significantly better than other caseworkers. For instance, being assigned an one standard deviation more active caseworker increases the 90-days and 180-day job-finding rate by 1.6 (column 1) and 1.4 percentage points, respectively. The latter corresponds to an increase by 2.2%. We find no evidence for the effectiveness of "supportive" caseworker although all estimates point in the same direction. Furthermore, there is no evidence

[21] We use information on a labor market training program called *Arbetsmarknadsutbildning* (AMU), typically lasting for six months, which is directed towards the upgrading or acquisition of skills that are in short supply. The workfare program is called work practice and involves 3–6 months of practice at a public or private firm.

Table 3.6. Caseworker traits on job seeker outcomes

	Leave unemployment within			log(duration)
	90 days (1)	180 days (2)	360 days (3)	(4)
Supportive	0.002	0.005	0.006	-0.015
	(0.004)	(0.004)	(0.003)	(0.012)
Restrictive	-0.002	-0.006	-0.005	0.008
	(0.005)	(0.006)	(0.005)	(0.016)
Active	0.016***	0.014***	0.008**	-0.016
	(0.005)	(0.004)	(0.004)	(0.014)
Mean outcome	0.423	0.635	0.802	4.767
First-stage F-statistic	237	237	237	237
# clusters	7,002	7,002	7,002	7,002
# observations	2,278,278	2,278,278	2,278,278	2,278,278

Notes: IV estimates using where each characteristic of the actual caseworker is instrumented with the corresponding characteristic of the predicted caseworker. All traits are indicators for above median propensity to assign to training (supportive), assign to work practice (restrictive) and to have meeting with their job seekers (active). All models also include interacted year fixed effects, office fixed effects, and a dummy for above the age of 25. First-stage F-statistic is a joint test for all instruments. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

of any effects on job-seeker outcomes for restrictive caseworkers. This adds to the existing literature, which has been inconclusive: Arni et al. (2017) find that caseworkers that place more emphasis on support have better outcomes, while Behncke et al. (2010b) and Huber et al. (2017) show that tougher caseworkers are more successful than supportive ones.

3.7 Part II: Caseworker–job seeker matching

The literature in sociology has shown that individuals with the same gender/ethnicity (or other social attributes) behave differently against each other than towards individuals of the other gender or other ethnic groups. For instance, sharing the same social identity could enhance communication and trust (see, e.g., Sherif et al., 1961). This suggests that caseworker–job seeker similarity could matter. Behncke et al. (2010a) also provide initial evidence of this. Here, we use our fine-grained data to study the importance of similarity in several dimensions, including demographic similarity, ability similarity, and similarity in the form of similar labor market experiences. The latter, which has not been studied before, may be especially relevant for several reasons. Besides promoting communication and trust, experience from working in the same sector as the job seeker may enable caseworkers to understand the individual-specific labor market opportunities, and thereby help them to provide more adequate counseling and support. Caseworkers may also be able to use their social networks from previous jobs to refer job seekers to suitable workplaces and to promote informal hiring channels, and these network mechanisms ought to be more relevant for job seekers with a similar occupational background as their caseworker.[22]

We use the model in equation (3.2) and study similarity in one dimension at a time. For presentation reasons, we focus on job-finding within 180 days, but find similar results for the other outcomes (not reported).

Initially, Panel A of Table 3.7 shows that there is a positive match-effect of gender-similarity. Job seekers that are assigned a caseworker with the same gender increase their likelihood of finding a job within 180 days by 0.4 percentage points or 0.6%. As above, we also see that overall female caseworkers perform better than male caseworkers. Interestingly, columns (2) and (3) show that both males and females are better off with a female caseworker. In particular, female job seekers benefit from being assigned a female caseworker (column 2). This is partly due to the positive match effect but predominantly because of the positive female caseworker effect. For male job seekers (column 3), the gender of the caseworker is less important. From a similarity perspective a male caseworker is preferred, but this is counteracted by the fact that female caseworkers perform better than male caseworkers. This relates to results from the teacher literature, with mixed evidence on the effects on student outcomes for teacher–student gender similarity (Neumark and Gardecki, 1998, Bettinger and Long, 2005, Dee, 2004, Hilmer and Hilmer, 2007).

[22]The social network mechanism is supported by evidence that show that informal hiring channels are important (see, e.g., Hensvik and Nordström Skans, 2016, Dustmann et al., 2016).

Table 3.7. Caseworker and job seeker similarity: demographics and ability

	(1)	(2)	(3)
	All	Job seeker characteristic	
Panel A : Gender similarity		Female	Female
		Female caseworker	Male caseworker
Match effect	0.004**	0.015***	0.007*
	(0.002)	(0.004)	(0.004)
Female caseworker	0.011***		
	(0.003)		
First-stage F-statistic	1276	1276	1276
# observations	2,220,061	2,220,061	2,220,061
Panel B : Immigrant similarity		Native	Foreign born
		Native caseworker	Native caseworker
Match effect	-0.003	0.004	0.010
	(0.004)	(0.005)	(0.008)
Native caseworker	0.007		
	(0.006)		
First-stage F-statistic	371	371	371
# observations	2,220,061	2,220,061	2,220,061
Panel C : Ability similarity		High ability	Low ability
		High ability caseworker	High ability caseworker
Match effect	0.001	0.007	0.005
	(0.004)	(0.008)	(0.010)
High ability caseworker	0.006		
	(0.008)		
First-stage F-statistic	416	416	416
# observations	274,195	274,195	274,195

Notes: IV estimates where each match-effect and main caseworker effects is instrumented with the corresponding variable for the predicted caseworker. High ability caseworker is above median caseworker cognitive ability, and high ability for the job seeker is above median predicted unemployment duration. All models also include interacted year fixed effects, office fixed effects, and a dummy for above the age of 25. First-stage F-statistic is a joint test for all instruments. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Panel B of Table 3.7 shows that matching of caseworkers and job seekers based on immigrant status appears to be unimportant. Thus, it is not the case that non-native caseworkers can provide better counseling support to non-native job seekers. Next, Panel C examines ability-similarity. Here, we unfortunately do not have access to ability measures for job seekers. Instead, we use the predicted unemployment duration as a proxy for general ability, and define ability similarity based on ability above/below the median for job seekers and caseworkers, respectively. However, we find no evidence that matching (male) caseworkers and job seekers based on ability is important.

Panels A and B of Table 3.8 examine caseworker–job seeker matching based on occupational experiences and educational background. For the latter, similarity is defined by having a university degree or not. For labor market experiences we use the same measure of labor market experience as used above: having worked in the private sector. Specifically, the match variable takes the value one if the caseworker has some personal experience from the private sector in the last 10 years worked and if the job seeker worked in the private sector just prior to becoming unemployed, or if both of them have no experience from the private sector. There is some suggestive evidence that similarity and matching in these labor market dimensions are important. Sharing the same labor market experience increases the 180-day job-finding rate by 0.7 percentage points which corresponds to 1.1%. The corresponding estimate for sharing a similar educational background is 0.6 percentage points.

We conclude from this exercise that sharing the same gender as your caseworker is beneficial for job seekers. However, for male job seekers this effect is counteracted and dominated by the fact that female caseworkers perform better than their male correspondents. Moreover, it may be important to allocate job seekers to caseworkers who have experience from similar sectors as job seekers, as this may allow them to use their own social networks to mediate jobs. Finally, being paired with an equally educated caseworker do help job seekers find work faster which may reflect that caseworkers understand the relevant job-market for that job seekers' qualifications.

Table 3.8. Caseworker and job seeker similarity: demographics and ability

	(1) All	(2)	(3)
		Job seeker characteristic	
Panel A : Experience private sector		Private sector	Other sector
		Caseworker exp. private sector	Caseworker exp. private sector
Match effect	0.007*	0.008	-0.005
	(0.004)	(0.007)	(0.007)
Caseworker exp. private sector	0.001		
	(0.006)		
First-stage F-statistic	276	276	276
# observations	1,664,379	1,664,379	1,664,379
Panel B : University degree		University edu.	No university edu.
	(1)	Caseworker with university edu. (2)	Caseworker with univeristy edu. (3)
Match effect	0.006**	0.006	-0.006*
	(0.003)	(0.005)	(0.003)
University degree caseworker	0.000		
	(0.004)		
First-stage F-statistic	1081	1081	1081
# observations	2,220,061	2,220,061	2,220,061

Notes: IV estimates where each match-effect and main caseworker effects is instrumented with the corresponding variable for the predicted caseworker. Caseworker has experience from the privatye sector if having ever worked manufacturing, construction, retail, hotel and restaurant within the last ten years. For job seekers experience from the privae sector is based on the last job just prior to becoming unemployed. All models also include interacted year fixed effects, office fixed effects, and a dummy for above the age of 25. First-stage F-statistic is a joint test for all instruments. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

TABLE 3.9. DISTRIBUTION OF CASEWORKER FIXED EFFECTS

	Unadjusted standard deviation	Adjusted standard deviation		
	Level	Level	Percent	Standard deviations
	(1)	(2)	(3)	(4)
Leave unemployment within				
90 days	0.156	0.050	0.112	0.100
180 days	0.174	0.047	0.070	0.099
360 days	0.093	0.000	0.000	0.000
Log unemployment duration	0.469	0.028	0.006	0.022
Annual earnings in year				
$t+1$	44.637	3.843	0.041	0.039
$t+2$	39.733	7.615	0.066	0.069
$t+3$	41.322	6.333	0.049	0.054

Notes: The table reports the standard deviation of estimated caseworker fixed effects (IV-estimates). The sample is all offices with date-of-birth assignment (i.e $F \geq 400$ as defined in section 3.5.1). Column 1 reports the unadjusted standard deviations and Columns 2–4 the empirical Bayes adjusted standard deviations (see Section 3.5.4 for details). Column 2 is in levels, Column 3 in percent of the mean outcome and Column 4 in relation to the standard deviation of the outcome. All models include interacted year fixed effects, office fixed effects, and a dummy for above the age of 25.

All these results relate and extend the previous findings in Behncke et al. (2010a). They find an effect of similarity, but only when it comes to sharing characteristics in four dimensions (simultaneously): caseworkers and job seekers sharing the same gender, age group, nationality, and education.

3.8 Part III: How important are caseworkers?

So far, we have used observed caseworker characteristics to examine what makes a good caseworker and to study caseworker–job seeker matching. We now examine the overall importance of caseworkers, and study the distribution of caseworker fixed effects, quantifying both observed and unobserved differences between caseworkers. Following the estimation procedure described in section 3.5.4, the results are summarized in Table 3.9. Column (1) shows the unadjusted standard deviation of the estimated caseworker fixed effects for leaving unemployment within 90 days, 180 days, 360 days and log duration as well as earnings after 1-3 years, respectively. Columns (2)–(4) display the adjusted standard deviation, after we applied the shrinkage procedure in Leigh (2010).

Overall, we find substantial differences in caseworker performance. Even though the shrinkage procedure reduces the standard deviations of the fixed effects, differences between caseworkers remain. The estimates in Table 3.9 show that moving one standard deviation in the distribution of the caseworker fixed effects, changes the probability of leaving unemployment within 180 days by 7%, roughly 0.1 standard deviation. This captures differences due to the observed caseworker characteristics studied in this paper as well as differences in other dimensions not captured by our observed caseworker characteristics. Overall, the results suggests that caseworkers matters most early on in the unemployment spell. Nevertheless, the effect seems to persist as the corresponding estimate for earnings after three years is 4.9% or 0.05 of a standard deviation. In summary, it means that there are substantial differences between the worst and the best caseworkers, and that the differences between caseworkers are economically important and long-lasting.

3.9 Conclusions

Remarkably little is known about the individuals responsible for implementing active labor market policies, i.e. caseworkers at employment offices. To get a comprehensive picture on the effects of the resources devoted to active labor market policies, it is important not only to have knowledge on active labor market programs, but also to understand the role of the human resources used.

Due to data limitations and identification difficulties, the evidence on caseworkers is scarce (McCall et al., 2016). In this paper, we provide credible evidence on caseworkers by exploiting that some local public employment offices in Sweden allocate job seekers to caseworkers based on date-of-birth-rules, i.e. as-if random allocation. Coupled with detailed data that include links between caseworkers and job seekers, we can explore mechanisms explaining caseworker performance.

Interestingly, even though the caseworker population contains a substantial degree of heterogeneity, neither cognitive ability, unemployment experience, employment history or educational attainment, can explain caseworker performance. However, we cannot rule out that caseworkers with more experience (tenure) perform better. We find there to be quite substantial gender differences: job seekers assigned a female caseworker have 3.1% shorter unemployment durations than those assigned a male caseworker. Caseworker traits also appear to be important. Specifically, "active" caseworkers, i.e. caseworkers prone to meet job seekers, perform significantly better. A one standard deviation increase in caseworker "activeness", increases the job-finding rate by 2.2% within 6 months.

The results show that caseworker-job seeker similarity matters for job seeker outcomes. As Behncke et al. (2010a) argues, sharing the same social identity can improve understanding, communication and trust, and hence improve job seeker outcomes. Most importantly, and what has never been studied before, we find that sharing similar labor market experiences or level of education improve job seeker outcomes. The effect of such similarity is non-negligible: it increases the 6-months job finding rate by about 0.7 percentage points. We also find that job seekers do better when assigned a caseworker with the same gender. However, for males, this match effect is offset by female caseworkers being more effective than their male counterparts.

Finally, we quantify both observed and unobserved differences between caseworkers by estimating caseworker fixed effects. We show that the overall differences in caseworker performance is economically important and long-lasting. A one standard deviation increase in the distribution of caseworker fixed effects increases the 6-month job finding rate by 7.0%, and earnings after three years by 4.9%.

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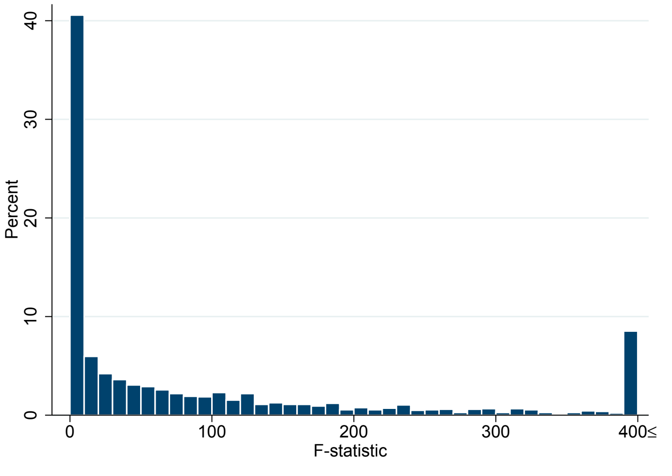
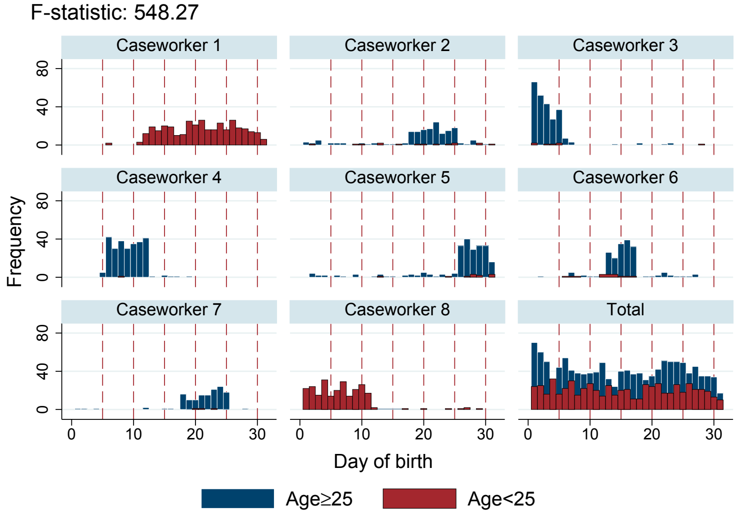
Appendix

FIGURE A-3.1. PREVALENCE OF DATE-OF-BIRTH-RULES



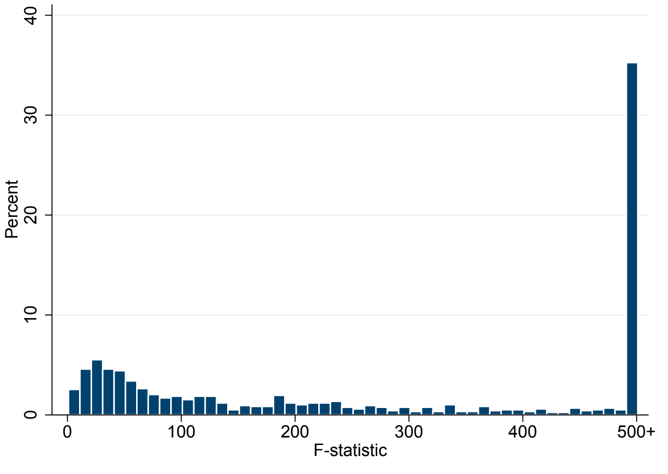
Note: The distribution of F-statistics from regressions of job seekers' date-of-birth (1–31) on caseworker dummies (within office and year). A low F-value indicates no date-of-birth-rule (an even distribution of date-of-birth over caseworkers), and a high F-value indicates a date-of-birth-rule (an un-even distribution of date-of-birth over caseworkers).

FIGURE A-3.2. AN EXAMPLE OF AN OFFICE WITH A SEPARATE DATE-OF-BIRTH-RULE FOR YOUTHS



Notes: Number of job seekers (above/below 25 years of age) born on each day-in-month per caseworker at one office, in 2003.

Figure A-3.3. Strength of predicted caseworker instrument



Notes: The figure show separate first stage F-statistics where a dummy for the actual caseworker has been regressed on a set of dummies of predicted caseworker within and office and year.

Table A-3.1. Descriptive statistics

	Caseworker characteristics			
Panel A	All offices		Date of birth offices	
	Mean	SD	Mean	SD
Age	47.06	10.19	47.19	10.09
Female	0.62	0.49	0.61	0.49
Swedish	0.88	0.32	0.87	0.33
# clients	97.04	107.25	127.19	127.57
Experience	13.51	10.82	13.58	11.03
Primary School	0.03	0.17	0.04	0.20
High School	0.33	0.47	0.31	0.46
College	0.64	0.48	0.65	0.48
# observations (unique)	3,985		478	
# observations	22,988		2,293	
	Job seeker characteristics			
Panel B	All offices		Date of birth offices	
	Mean	SD	Mean	SD
Age	31.83	12.24	31.13	11.70
Female	0.47	0.50	0.48	0.50
Swedish	0.86	0.34	0.88	0.33
Married	0.24	0.43	0.23	0.42
Children	0.36	0.48	0.34	0.48
Earnings (t-1)	95777.59	120087.74	90243.25	114473.39
Days unemployed	281.68	488.12	265.76	480.25
Primary School	0.30	0.46	0.27	0.44
High School	0.53	0.50	0.53	0.50
College	0.17	0.38	0.20	0.40
# observations (unique)	1,601,217		212,049	
# observations	2,220,067		289,713	
	Office characteristics			
Panel C	All offices		Date of birth offices	
	Mean	SD	Mean	SD
# caseworkers	12.62	10.89	16.74	11.58
# job-seekers	1219.15	1283.03	2114.69	1814.19
# observations (unique)	252		32	
# observations	1,821		137	

Notes: The table shows means and standard deviations for job seeker and caseworker characteristics. The sample consists of registered unemployed individuals and caseworkers at all local offices in Sweden 2003–2010.

TABLE A-3.2. FIRST STAGES : CASEWORKER DEMOGRAPHICS AND CASEWORKER EDUCATION

	Dependent variable: Actual caseworker characteristic						
	Age	Female	Swedish	Secondary degree	Univeristy degree	Bussnies degree	Social degree
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Predicted caseworker characteristic							
Age	0.402***	-0.000	-0.000	-0.000	0.000	-0.000	0.000
	(0.008)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female	-0.125	0.378***	0.006	0.002	-0.002	-0.000	-0.002
	(0.119)	(0.007)	(0.004)	(0.006)	(0.006)	(0.006)	(0.004)
Swedish	-0.029	0.020**	0.377***	-0.002	0.001	-0.009	-0.001
	(0.176)	(0.009)	(0.013)	(0.008)	(0.008)	(0.009)	(0.007)
Secondary	-0.284	0.001	-0.003	0.393***	0.022	0.030**	0.009
	(0.452)	(0.019)	(0.012)	(0.017)	(0.025)	(0.015)	(0.009)
Univeristy degree	-0.131	0.001	0.000	0.007	0.408***	0.016	0.007
	(0.450)	(0.019)	(0.012)	(0.015)	(0.024)	(0.014)	(0.009)
Bussnies degree	-0.058	0.001	-0.006	0.015**	-0.014*	0.384***	-0.001
	(0.128)	(0.007)	(0.004)	(0.007)	(0.007)	(0.008)	(0.003)
Social degree	0.134	-0.008	-0.004	0.011	-0.010	0.006	0.377***
	(0.191)	(0.009)	(0.006)	(0.007)	(0.008)	(0.007)	(0.012)
F-statistic	394	423	117	339	374	389	144
# offices	6816	6819	6819	6819	6819	6819	6819
# observations	2,218,758	2,219,275	2,219,275	2,219,275	2,219,275	2,219,275	2,219,275

Notes: The table shows first stage estimates, where we have regressed each actual caseworker characteristic (13 different regressions) on the full set of instruments, i.e. the 13 predicted caseworker characteristics. The most predictive instrument is the one corresponding to the actual caseworker characteristic (see diagonal). All regressions include office, year, and age-group (below/above 25 years of age) fixed effects, as well as the interactions between these fixed effects. Standard errors are clustered at the caseworker level and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

TABLE A-3.3. DATE-OF-BIRTH RULES AND RANDOM ASSIGNMENT OF JOB-SEEKERS TO CASEWORKERS

	Independent variables:			
	Experience actual caseworker		Experience predicted caseworker	
	Coef. Est.	Std. Err.	Coef. Est.	Std. Err.
Demographics				
Male	0.000134	(0.000178)	-0.000020	(0.000063)
Disabaled	0.000436***	(0.000079)	0.000010	(0.000025)
Native	-0.000082	(0.000126)	0.000023	(0.000040)
Age	0.082831***	(0.012987)	0.001858**	(0.000845)
Unemployment and earnings history				
Earnings (t-1)	346.179340***	(71.897102)	-4.442865	(14.680464)
Employed (t-1)	0.001399***	(0.000265)	-0.000046	(0.000055)
Welfare (t-1)	-0.000057	(0.000099)	0.000002	(0.000036)
Level of education				
Primary school $<$ 9 years	0.000219***	(0.000039)	-0.000014	(0.000018)
Primary school 9 years	-0.001745***	(0.000278)	-0.000048	(0.000043)
High-school 2 years	0.000798***	(0.000123)	0.000052	(0.000052)
High-school 3 years	0.000067	(0.000046)	-0.000014	(0.000021)
University $<$ 3 years	0.000624***	(0.000189)	0.000030	(0.000044)
University $\geq$ 3 years	0.000012	(0.000010)	-0.000004	(0.000008)
# observations	2,220,061		2,220,061	

Notes: The table shows separate OLS estimates for each job seeker characteristic on years of experience of the actual (column 1) and the rules-predicted caseworker (column 2). All regressions include office, year, and age-group (below/above 25 years of age) fixed effects, as well as the interactions between these fixed effects. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

TABLE A-3.4. TESTS OF THE MONOTONICITY ASSUMPTION

	Independent variable: Actual caseworker experience			
	Quartile predicted unemployment duration			
Panel A	1^{st}	2^{nd}	3^{rd}	4^{th}
Predicted caseworker experience	0.404^{***} (0.017)	0.362^{***} (0.012)	0.330^{***} (0.012)	0.268^{***} (0.010)
# observations	543,846	542,421	541,794	543,757
	Quartile job seeker age			
Panel B	1^{st}	2^{nd}	3^{rd}	4^{th}
Predicted caseworker experience	0.428^{***} (0.018)	0.334^{***} (0.012)	0.307^{***} (0.013)	0.299^{***} (0.012)
# observations	584,601	532,383	573,793	529,280

Notes: First-stage estimates separately by quartiles of job seekers' predicted unemployment (panel A) and quartiles of job seekers' age (panel B). For details on how predicted caseworker is defined see section 3.5.1. All models also include interacted year fixed effects, office fixed effects, and a dummy for above the age of 25. First-stage F-statistic is a joint test for all instruments. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

TABLE A-3.5. SENSITIVITY ANALYSES

	Single estimate (1)	Immigrant control (2)	Date-of-birth fixed effects (3)	Other caseworker characteristics (4)	Date-of-birth offices (5)
	Panel A: Caseworker demographics				
Female	0.011^{***}	0.010^{***}	0.009^{***}	0.009^{***}	0.013^{***}
	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
	Panel B: Caseworker traits				
Active	0.015^{***}	0.014^{***}	0.014^{***}	0.013^{***}	0.025^{***}
	(0.005)	(0.004)	(0.005)	(0.005)	(0.008)
	Panel C: Caseworker ability				
Non-cognitive	0.001	0.002	0.001	-0.003	0.011
	(0.004)	(0.004)	(0.004)	(0.006)	(0.008)

Notes: Column 1 uses the single instrument estimates. Column 2 controls for immigrant status and column 3 includes date-of-birth fixed effects. In column 4 we include all caseworker characteristics included in Table 4 and 5. Column 5 restricts the analysis to date-of-birth-offices (as defined in Section 3.5.1). All models also include interacted year fixed effects, office fixed effects, and a dummy for above the age of 25. First-stage F-statistic is a joint test for all instruments. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Chapter 4

Extended Unemployment Benefits and the Hazard to Employment*

*This chapter was written within the project "How does a declining sequence of UI replacement rates affect the hazard to employment?" (DNR: 144/2008) at the Institute for Evaluation of Labor Market and Education Policy (IFAU). I would like to thank Niklas Blomqvist, Peter Fredriksson, Martin Olsson, David Seim, Johan Vikström for valuable comments and seminar participants at Stockholm University, Institute for Research on Labor & Employment at UC Berkley and IFAU. All remaining errors are my own. Funding from Handelsbanken is gratefully acknowledged

4.1 Introduction

How does the generosity of unemployment insurance (UI) affect job search behavior? While providing a safety net for unexpected job loss, the provision of UI creates disincentives for job search by lowering the alternative cost to working. The question of how benefit levels and its overall generosity affects time in, and the hazard out of, unemployment has a long tradition in labor economics and been subject to extensive research. The "spike" in the hazard rate out of unemployment coinciding with UI exhaustion is a widely established empirical result since the seminal work by Katz and Meyer (1990*a*,*b*). This result has generally been attributed to shirking behavior among job seekers, holding off finding a new job until approaching benefit exhaustion. However, later work by Card, Chetty and Weber (2007*b*) challenges this view by attributing the lion's share of such spikes to flight out of the labor force. They argue that "[s]pikes are generally smaller when the spell length is measured by the time to next job than when it is defined by the time spent on the unemployment system" (p. 1). Hence, " [...] the size of the spike in re-employment rates at exhaustion in the current U.S. labor market (and many other labor markets) remains an open question. Further work on estimating these hazards using administrative measures of time to next job would be particularly valuable" (p. 16).[1] Indeed, if benefit exhaustion renders job seekers to leave the labor force, the expected cost of extending UI benefits could be exaggerated if transition to work is higher from unemployment than non-employment.

This paper contributes to the debate about the timing of re-employment and UI exhaustion, while additionally adding to the large literature on the effects of UI on job search behavior (see section 4.2 for a short review). In particular, I examine the effect on unemployment duration, and exit to employment, of an exogenous 30-week UI benefit extension in Sweden. For identification, I take advantage of a feature in the Swedish UI system which entitles individuals with a child below the age of 18 to 90 weeks of unemployment benefits instead of the statutory 60 weeks. As assignment to the extended UI benefit is determined by the age of a job seekers' youngest child at the time of regular UI exhaustion (60

[1] The term re-employment refers here to an exit out of unemployment to any new employer whereas a recall is returning to ones previous employer.

weeks), I exploit the quasi-experimental variation generated around the age threshold using a regression discontinuity (RD) design. This allows me to estimate the casual effect of increasing potential duration of UI on actual benefit duration, unemployment duration and hazard to employment. Further, I allow the effects to vary with duration on UI and in unemployment to test whether job seekers time employment to benefit extension.

The main findings are threefold. First, while the increase in potential duration on UI increases actual duration on UI by about 2.7 weeks on average, I find no evidence of it prolonging duration in registered unemployment or negatively effecting the hazard to employment. This suggest that, the 30 week benefit extension did not prolong average unemployment duration as job seekers were on average unemployed as long but with a somewhat higher replacement rate. The absence of negative effects on unemployment duration and future employment is believed to be driven by job seekers access to fairly generous post-UI programs which weakens the disincentive effects of the benefit extension. Second, being eligible to 30 additional weeks of UI does not appear to have affected job search behavior prior to the actual extension period. That is, I find no evidence of job seekers lowering their search effort due to the anticipation of extended benefits. Third, I find distinct spikes in the exit out of UI at benefit exhaustion, but no such spikes are present in the hazard to employment. This therefore speaks in favor of the interpretation made in Card, Chetty and Weber (2007*b*).

The remainder of the paper is organized as follows. Section 4.2 briefly reviews the related literature and section 4.3 describes the Swedish UI system and the institutional details surrounding the benefit extension. In section 4.4, I outline the identification strategy, describe the data and validate the assumptions needed for casual inference. Section 4.5 presents the empirical results while section 4.6 concludes.

4.2 Previous literature

There is an extensive literature on how the generosity of UI affects job search behavior where the results are largely coherent with the theoretical predictions made in Mortensen (1977).[2] For the U.S., Card and Levine (2000) studies a (temporary) program which offered a benefit extension of 13 weeks to the unemployed in New Jersey. While the number of people reaching regular benefit exhaustion appears to have increased by about 1-3 percent, exit-rates and average unemployment duration remained virtually unchanged. In their seminal study, Katz and Meyer (1990*a*) detects sharp increases in the hazard out of unemployment at the time of benefit exhaustion. Moreover, they suggest that extending the potential duration by one week prolongs unemployment duration by about 0.16 to 0.2 weeks. In more recent studies, Card et al. (2015) and Landais (2015) exploit kinks in the US benefit schedule to estimate the effect of increased UI benefits. While one additional week of potential UI is estimated to increase unemployment duration by 0.2-0.4 weeks, the elasticity with respect to the benefit level ranges between 0.2 to 0.7.[3] Moreover, Card et al. (2015) suggests that these elasticities differ substantially with overall macroeconomic conditions. This highlights the problem of policy endogeneity which many early U.S. studies of unemployment behavior have been subject to. A increase in potential duration have been induced by business cycles, estimates on unemployment duration will inevitably be biased.[4]

For Europe, Hunt (1995) evaluates a reform in Germany which resembles the one investigated in this paper. Replacement rates were cut from 63 to 56 percent for unemployed workers without children. While no significant changes in the flow to employment could be detected among parents, the reform seems to have had the adverse effect of increasing the likelihood of leaving the labor force. In a subsequent reform, Hunt (1995) finds that extending benefits for workers above the age of 42 increases their duration of unemployment.[5] However, the impact on the hazard to leaving the labor force appears to be larger than

[2] A strand of literature also looks at the effect of UI generosity on job match quality where the evidence suggests a zero or very small positive effect (see e.g., Nekoei and Weber (2017); Lalive (2007), Card, Chetty and Weber (2007*a*); Caliendo, Tatsiramos and Uhlendorff (2013)) .

[3] For a summary of estimated elasticities in the U.S. across studies see appendix in Card et al. (2012) Table 4.

[4] See Lalive, Van Ours and Zweimüller (2006) for a discussion of the importance of understanding policy endogeneity when estimating the effect of benefit increases.

[5] The magnitude of the effects are, however, somewhat unreliable as significant effects can only be found among 44-48 year olds whereas 49-57 year olds are unaffected.

the hazard to employment among the older workers, thus corroborating the interpretation of hazard spikes at UI exhaustion (Card, Chetty and Weber, 2007*b*). Exploiting similar age thresholds for older workers in Germany, Schmieder, Von wachter and Bender (2012) estimates the effect of extended potential duration on non-employment duration using data covering 20 years. They find that an additional week of UI benefits yields 0.1 weeks of longer non-employment duration on average. The effect on actual UI benefit duration are estimated to be three to four times larger.

Several studies have taken advantage of various benefit discontinuities in the Austrian UI-system, rendering exogenous variation in both potential duration, replacement rates and severance pay (see e.g. Lalive, Van Ours and Zweimüller, 2006, Lalive, 2007, 2008, Card, Chetty and Weber, 2007*a*,*b*, Nekoei and Weber, 2017). The estimates on benefit extension are largely consistent across the studies, ranging from 0.05 to 0.1 additional weeks of unemployment or non-employment duration from one extra week of potential duration.[6] In other words, 10 weeks of increased potential duration tends to prolong non-employment by about 0.5 to 1 weeks (Card, Chetty and Weber, 2007*a*, Lalive, Van Ours and Zweimüller, 2006, Lalive, 2008).[7] An interesting feature in Card, Chetty and Weber (2007*a*,*b*) is also that potential duration lowers hazard rates throughout the entire spell, thus implying that people are forward looking as the benefit extension affects job search behavior in expectation of future benefits. They show that job-finding rates decrease by about 5-9 percent during the first 20 weeks when extending potential duration from 20 to 30 weeks.

A well-established empirical fact is the spike in hazard rates at the time of benefit exhaustion. This has primarily been attributed to shirk-

[6]Lalive (2008) uses data on unemployment duration and finds that the effect of one week increase in potential duration for women is 0.32-0.44. This upper estimate may however biased due to manipulation of the forcing variable among women. The lower estimate, using border identification, which is less likely subject to self selection, the effect is still 4 times larger than for men. This is attributed to special rules for early retirement for women.

[7]The large difference between the Austrian and U.S. estimates (0.05-0.1 vs. 0.16-0.4) warrants some attention. As future benefits will be discounted by the probability of survival and potential duration may exhibit decreasing marginal utility one potential explanation for these results could be differences in baseline potential benefit durations.

ing behavior among the unemployed by seemingly holding off taking a job until benefits run out.[8] Using reductions in potential benefit durations in Slovenia, van Ours and Vodopivec (2006) show that such spikes move, almost one to one, with the timing of exhaustion. While this could represent job seekers both finding jobs and moving to labor market programs or leaving the labor force, Card, Chetty and Weber (2007*b*), in contrast, shows that the spike in Austria is driven by job seekers exiting the labor force and not entering employment. The unemployment exit hazard is 2.4 times larger at exhaustion than at the baseline period while the employment hazard is 1.15. Moreover, recalls to old jobs appear to be more common than starting new ones. In fact, Card, Chetty and Weber (2007*b*) suggests that fewer than one percent of the spells are manipulated in such a way that job finding coincides with the timing of benefit exhaustion. This is also consistent with estimates in Schmieder, Von wachter and Bender (2012) where only 8 percent of unemployed who reach benefit exhaustion return to employment whereas the majority escapes to non-employment.

There are a couple of studies estimating the effect of UI generosity on duration and hazard rates in Sweden. Focusing on the presence of hazard spikes, Carling et al. (1996) estimates the transition to employment and labor market programs. Though imprecise, the estimates give evidence of spikes at exhaustion but, due to the lack of a valid control group, it is not possible to draw firm conclusions about the potential distorting effects of UI. Studying a Swedish reform in 1995, which cut replacement rates by 5 percent, Carling, Holmlund and Vejsiu (2001) finds an increased transition to employment by 10 percent, or elasticity of 1.7 which is substantially larger than any other comparable finding. The interpretation of the results are, however, muttered by accompanying changes in the UI system which increased the incentives for job search. Moreover, treatment and control groups are defined based on previous wages, which could influence the hazard to employment directly and therefore bias the estimates. Both these objections carry over to Bennmarker, Carling and Holmlund (2007) who evaluates two consecutive UI reforms in Sweden in 2001 and 2002, which increased the

[8] Card and Levine (2000) proposes informal contracts between the unemployed and the old employer such that recalls are timed to UI exhaustion thus rendering a spike in the hazard rate at that time.

benefit cap. Here the overall hazard rate appears to be unaffected by the reform. However, a further analysis shows heterogeneous responses across gender with men being largely unaffected while women, in stark contrast to standard theoretical predictions, increase the employment. Bennmarker, Carling and Holmlund (2007) attributes this unexpected effect to a child care reform taking place at the same time.

4.3 Unemployment compensation in Sweden

The provision of UI in Sweden is obtained through voluntary membership in branch-specific union-affiliated UI funds and the national coverage rate is about 70 percent of the labor force.[9] Job losers with sufficient work history are eligible for UI benefits with a base amount of 320 SEK per day as long as they are registered at the public employment service (PES). In order to acquire income-related UI benefits, the unemployed having been working for twelve months also have been a member of a UI fund for the same amount of time. The maximum replacement rate is then 80 percent of the workers' former wage, subject to a benefit cap of 680 SEK per day is implemented on monthly wages above 18,700 SEK.[10,11] Workers who are laid off have a seven day waiting period before receiving their first UI payment whereas voluntary quitters are subject to a 45-day waiting period.[12]

The statutory length of a regular benefit period is 60 weeks (300 working days, 5 days a week). A job seeker may choose how many days a week he or she want to be collect UI benefits, where the maximum is 5 days per week. Therefore the duration on UI may be longer than 60 weeks if a job seeker chooses to collect UI part-time. Hence, there is a difference between the duration on UI and utilization of UI where the former refers to calender time on UI and the latter how many days/weeks are collected. Note that the two are equal if a job seeker utilizes 5 days a week.

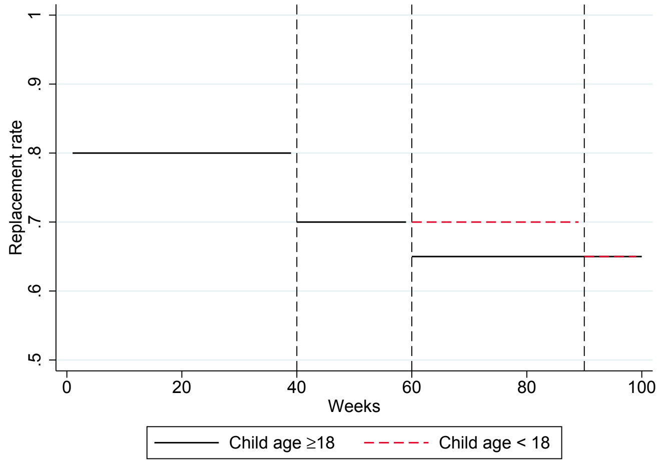
[9] Several reforms enacted in 2007, one of which increased UI-fund membership fees, led to a significant drop in the number of workers eligible for UI.

[10] In September 2015 the cap was raised to 25,025 SEK. As this is outside the sample period this does not affect my estimations.

[11] In April 2019, the SEK/US Dollar conversion rate was 9.2 and SEK/Euro conversion rate was 10.4.

[12] Prior to July 7 2008, there was a 5-day waiting period for involuntary quitters.

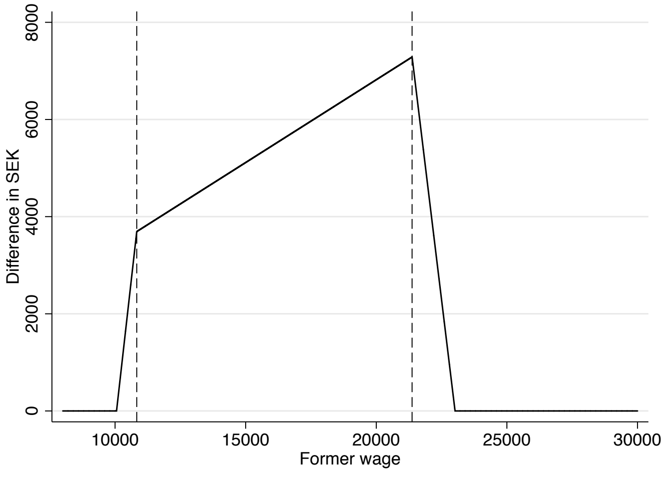
FIGURE 4.1. BENEFIT SCHEDULE



Notes: The figure shows replacement rates by weeks in unemployment (on UI) for job seekers entitled to income related UI with wages above and below the wage floor and cap, respectively. The solid black line depicts replacement rates for workers who at the time of regular benefit exhaustion (60 weeks) have a child above the age of 18. The dashed red show replacement rate for job seekers who are the care taker of a child below the age of 18 at week 60 on UI.

The benefit schedule has a two-tiered structure where replacement rates are cut from 80 to 70 percent, 40 weeks into the benefit period (i.e after the job seeker has utilized 200 days of UI). However, the second tier could be extended by 30 weeks if the unemployed – at the time of regular benefit exhaustion (week 60) – is the caretaker of a child below the age of 18. Figure 4.1 shows the step-wise benefit schedule which, conditional on having a child, is a discontinuous function of the child's age at week 60. The extension is formally awarded at the 300th day on UI and is based on the child's age at that exact time which is checked by a third party. If a child turn 18 during the extended period, the extension has already been granted and hence there is no change in replacement rate until the extended benefit period runs out. Importantly, if individuals are forward looking and the discontinuity is salient enough, future benefits should be discounted to its present value thus making the discontinuity equally present at the start of unemployment as job losers would be able to approximate the age of their child at regular benefit exhaustion

Figure 4.2. Treatment intensity by former wage



Notes: The figure shows the maximum difference in Swedish krona (SEK) as function of the job seekers former wage between job seekers entitled and not entitled to the extended benefit duration. The calculations assumes full discounting and a probability equal to unity of surviving on unemployment for at least 90 weeks.

by adding to it the number of weeks remaining on UI.[13] In that case, job finding rates could be affected prior to the benefit *de facto* being awarded (Card, Chetty and Weber, 2007*b*).

job seekers who exhaust their benefits are offered to enter the Job and Development Guarantee (JDG), an active labor market program targeted towards the long-term unemployed. Participation in the program entitles the job seeker to activity support (a form of unemployment assistance) which corresponds to a replacement rate of 65 percent which is paid out by the Swedish Social Insurance Agency for an, essentially, indefinite period. Participating in the JDG is optional for individuals who are entitled to extended benefits as they can enter after their 60th week on UI but remain at 70 percent replacement rate until week 90 when the extended period ends. After that, the same rules apply.

Due to the benefit cap and the base amount, only workers with monthly wages between $10,057 - 23,015$ SEK are affected by the benefit cut. Treatment intensity thus varies directly both through the individuals' former wage, and indirectly through the probability of staying

[13]In this case, one can, for newly awarded benefit periods of 60 weeks, view the discontinuity as the child being 16 year and 44 ± 1 weeks at the first day of the benefit period rather than ± 18 at the time of benefit exhaustion.

unemployed. To get a sense of the magnitude of the financial incentives one could imagine an individual who intends to uphold UI for as long as possible. In other words, the probability of staying on UI is equal to one. Fully utilizing the 30 weeks of extended benefits with 70 percent versus 65 percent would then render an additional amount of $0 - 7285.5$ SEK ($0 - 48.57$ SEK daily) depending on the former wage. Figure 4.2 shows the financial incentives (i.e. treatment intensity) based on former wages assuming that the job seeker stays on UI throughout. The two dashed lines depict the interval where treatment intensity is largest in percentage terms of the former wage, i.e. a 5 percentage point difference between the control and treatment group.

4.4 Identification strategy

4.4.1 Empirical Strategy

An individual's benefit level is a function of his or her former wage. Therefore, it is likely to be correlated with personal characteristics that could affect the duration of unemployment directly. In order to circumvent this omitted variable bias I take advantage of the institutional setting described in section 4.3 using a RD design. In the limit, close to the threshold, treatment can be thought of as randomly assigned and hence orthogonal to any remaining heterogeneity that might influence the outcome directly (Lee and Lemieux, 2010). I estimate the following baseline model

$$y_i = \alpha + \beta \mathbf{1}[ChildAge_i < 18] + f(ChildAge_i) + X_i' \delta + \varepsilon_i \qquad (4.1)$$

where y_i is the outcome variable which represents either benefit duration (weeks on UI) or unemployment duration (weeks in registered unemployment at PES) of individual i. The forcing variable $ChildAge_i$ is the age of individual i's child in years and months at the time of regular benefit exhaustion, which is normalized to zero and modeled flexibly with a functional form $f(\cdot)$ that allows for different slopes on either side of the threshold. The treatment indicator $\mathbf{1}[ChildAge_i < 18]$ is a dummy variable equal to unity if at the time of regular UI exhaustion the child is below the age of 18.[14,15] X_i' is a vector of individual covariates[16] that I include to increase efficiency and ε_i an error term. I estimate equation (4.1) semi-parametrically using a local linear regression as the forcing variable is discrete which rules out more recent estimation techniques suggested by Calonico, Cattaneo and Titiunik (2014). I use a main bandwidth of ± 18 months and confirm the robustness of the results by varying both the bandwidth and the functional form $f(\cdot)$ as suggested by (Lee and Lemieux, 2010).[17]

Equation (4.1) retrieves the reduced form, intention-to-treat (ITT), estimate of β if individuals are forward looking (as suggested by (Card, Chetty and Weber, 2007*a*,*b*)) as search behavior would be influenced in expectation of future possible benefit extensions. To investigate to what extent individuals react and adjust their search behavior in expectation of a future UI extension and/or time their job finding such that it coincides with benefit exhaustion, I estimate a dynamic version of equation (4.1) as follows:

$$\Pr(y_{it} \mid T \geq t) = \alpha_t + \beta_t \mathbf{1}[ChildAge_i < 18] + f(ChildAge_i) + X_i' \delta_t + \varepsilon_{it} \tag{4.2}$$

where y_{it} is either a dummy variable equal to unity if individual i leaves the UI system or a dummy for being deregistered from unemployment due to getting employed in time t, effectively censoring observations which lacks an end date or where an individual have left the unemployment register for other reasons than employment. Here β_t captures the

[14]The age of a child at benefit exhaustion is approximated by adding the number of weeks remaining in the benefit period until exhaustion to the child's age at the time of the first UI payment in the spell. This assumes the maximum take out of 5 days a week as the sample is restricted to full-time unemployed individuals. It turns out that this is a fairly reasonable assumption, as the hazard out of employment occurs precisely after 60 or 90 weeks for about 83 percent of the sample (see Figure 4.5)

[15]As the sample only consists of fresh UI spells treatment status is effectively based on the child being below the age of 16 year and 8 months at the start of the unemployment spell. If an individual chooses to utilize UI at a slower pace then 5 days a week this bears the consequence of misspecifying some individuals in the control group as being treated. Nevertheless, utilizing fewer than 5 days a week would be suboptimal as it merely adds to the age of the child at regular benefit exhaustion. This does not, however, introduce bias in the estimate of β but renders it to be interpreted as an intention-to-treat (ITT) effect.

[16]The covariates are: gender, age, annual earnings in 2006 and six dummies for level of education.

[17]See the Appendix for robustness.

difference in the hazard rate out of unemployment between the treated and control for those individuals who are still registered as unemployed at time $T \geq t$.

4.4.2 Data

I exploit data from the Swedish Public Employment Service (PES) on the universe of registered unemployment spells in Sweden from mid-2007 to the start of 2014. They contain the start and end date of each unemployment spell together with several personal characteristics such as age, gender, level of education, country of birth. I trace job seekers throughout their unemployment spell, registering different stages via search categories such as on the job search, part time unemployed or taking part in various labor market programs. To a certain extent, I observe the reason for leaving unemployment, e.g. whether a job seeker got full-time, part-time, subsidized employment, died or exited to education. However, in the final sample, about 9 percent of spells end due to a reason that is registered as "unknown" or "lost contact" and around 11 percent are right censored ongoing spells at the end of the observation window (February 18, 2014). Unemployment is defined as being registered as full-time unemployed or part of some program which does not involve subsidized employment. An unemployment spell ends with an individual leaving the unemployment register as long as she does not reappear as unemployed within 30 days. This is considered a temporary break of the unemployment spell and thus being a part of the original one. When estimating differences in hazard rates, a spell ends by the individual either entering full-time or part-time employment while the other reasons for exiting unemployment are censored.

The unemployment data is merged with data from the Swedish Unemployment Insurance Board (IAF) that contain weekly UI payments made to each individual. The register contains the start of each UI benefit spell, previous wages, paid benefit amounts and the number of days left in the UI period in any particular week. The start of an unemployment spell and a benefit period do not always coincide as claiming UI payments can be done with a lag. In order to make sure that the benefit period belongs to a particular unemployment spell, I consider benefit periods that have begun within 8 weeks prior to the start of unemployment. This also excludes voluntary quitters as the number of waiting days for voluntary quitters are 45 (9 working weeks). Finally, I use of the Swedish Multi-Generation Register which links parents to their children and contains the date of birth at the monthly level.

A UI benefit period can consist of several unemployment spells. If a spell is interrupted by e.g. temporary employment or education, reverting back to unemployment implies continuing with the previous benefit period unless the employment spell has lasted for more than 12 months. People re-entering unemployment will therefore in general have different number of weeks left on UI until benefit exhaustion.[18] Although using multiple unemployment spells for the same individual under the same benefit period more than doubles the number of observations, it severely complicates the analysis and identification. I therefore restrict the sample to newly registered benefit-entitled unemployment spells.

I impose some additional restrictions on the data. First, i restrict previous wages for which UI is based upon to $10,057 - 23,015$ SEK, as individuals below and above are only partly or completely unaffected by the treatment (see Figure 4.2). Second, ages are restricted to 25-59, as special rules and programs may apply to younger individuals and early retirement could be an option for older job seekers. Third, In order to not include job seekers having being granted an additional benefit period of 60 weeks but with lower benefits (65 % replacement rate) I exclude job seekers with 65% replacement rates during their benefit period. Finally, I exclude job seekers with a child who turn 18 the same month as regular UI expiration may be reached. The research design is thus a "donut" RD. This is done as I am unable to determine whether the child is exactly above or below 18 in a given month.

Table 4.1 show different moments of observable characteristics of job seekers in the main sample where the age of the child at predicted UI exhaustion is between 16.5 and 19.5 years. i.e a bandwidth of ± 18. The average job seeker is around 47 years old and has a little more than 2 children. No more than 26 percent have a college degree and about 10

[18]For this reason a benefit period can span several years. Prior to 2007 the duration on UI was in practice quasi-fixed as new benefit periods where given on a discretionary basis. Prior to 2001 unemployed could even re-qualify for new round of benefits by participating in labor market programs which in practice enabled indefinite cycling within the UI-system (Sianesi, 2008, Bennmarker, Skans and Vikman, 2013).

Table 4.1. Summary Statistics

	Mean	Standrad deviation	Median	Min	Max
Weeks in unemployment	45.92	55.02	26	0	363
Days used of UI	148.47	120.54	115	0	420
Age	47.73	5.18	30	47	59
Annual Earnings	15.66	10.19	17.2	0	79
# of children	2.34	1.08	2	1	11
Female	0.58	0.49	1	0	1
Disabled	0.10	0.30	0	0	1
Level of education					
< Primary School	0.09	0.28		0	1
Primary School	0.18	0.39		0	1
High school	0.48	0.50		0	1
College < 2 year	0.09	0.28		0	1
College	0.17	0.37		0	1
Ph.D	0.00	0.06		0	1

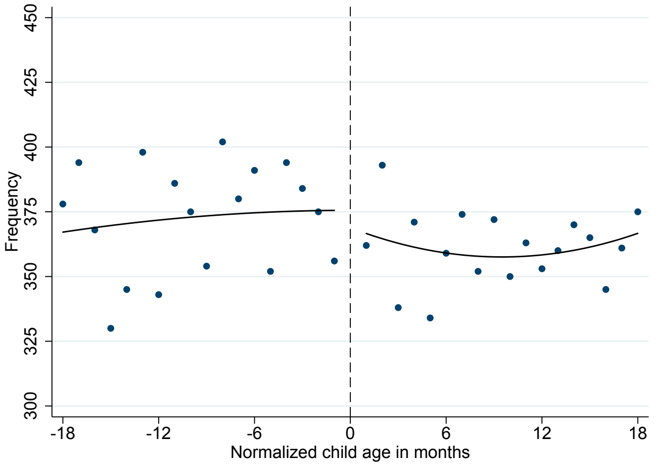
Notes: Table show moments of observable job-seeker characteristics for the main estimation sample used in the analysis with 13,162 observations. Ages of job-seekers children at onset of unemployment is thus restricted to 16.5 to 19.5 years. Annual earnings in the fourth row are presented in 10,000 SEK and refers to earnings in year 2006.

percent will at some point during the spell become registered as having some sort of disability. The average earnings in 2006 is around 155,000 SEK where 10 percent of the sample has zero earnings. The average unemployment duration is about 46 weeks but as usual the distribution of duration is highly skewed to the right leaving the median is 26 weeks. At the median, about a third of the standard UI benefits are used (115 out of 300 days) whereas the average is about 150 days.

4.4.3 Identifying assumptions

The validity of the RD-design hinges upon imperfect control over the forcing variable. As there are economic incentives to extend UI, one concern may be that job seekers can control the assignment variable and sort to the right of the cut-off into treatment. This implies that job seekers manipulate the age of their child at the time of UI exhaustion. Age, as such, is checked by a third party and hence virtually impossible to manipulate, however, a job seeker could time UI benefit entry such that is coincides with their child being just below the age of 18 at UI

FIGURE 4.3. DENSITY AROUND THRESHOLD



Notes: The figure shows the frequency of observations around the threshold. The solid lines are the OLS regression fit which include a second order polynomial polynomial function interacted with the threshold estimated on a bandwidth of ±18. The jump at the threshold is estimated to -6.7 with a standard error of 20.4.

benefit exhaustion. If so, this would invalidate the RD-design as it implies a selection into treatment and thus non-random assignment of prolonged potential duration on UI benefit. Formally, the identifying assumption could be written as,

$$\lim_{\Delta \to 0^-} \mathbb{E}[\varepsilon \mid ChildAge = 18{+}\Delta] = \lim_{\Delta \to 0^+} \mathbb{E}[\varepsilon \mid ChildAge = 18{+}\Delta] \quad (4.3)$$

where ε is the error term of equation (4.1). Approaching the threshold, the distribution of any unobserved heterogeneity correlated with the outcome of interest is the same among those just below and above the cut-off. Although the assumption of continuity of ε can not be fully tested, its validity can be assessed by checking that the frequency of observations and that pre-determined observable characteristics varies smoothly around the threshold (Lee and Lemieux, 2010).

Figure 4.3 shows the frequency of observations within a 18-month bandwidth of the threshold. There is no evidence of bunching on either side of the cut-off and, in the spirit of (McCrary, 2008), regressing the frequency on an indicator for being below the threshold along with the control function renders insignificant estimates with a p-value of .222 and 0.74 using a first and second order polynomial, respectively. This is perhaps not surprising as the margins for timing the start of the unemployment spell such that UI exhaustion occurs just before the child's 18th birthday is virtually non-existent when having dropped voluntary quits and assigning (intention to) treatment based on the maximum 5 day UI take-out (see section 4.4.1).

I further test the continuity assumption by regressing an indicator for being below the threshold on several pre determined covariates along with the control function. Column (1) to (4) in Table 4.2 show results from these regression varying both the bandwidth and the flexibility of the control function. I find no strong evidence of selection into treatment as I am unable to predict treatment at conventional significance levels using individual job seekers' characteristics by joint significance F-test. Using the linear specification (column 1 and 2), I fail to reject the null hypothesis of all coefficients being jointly equal to zero with a p-value of 0.497 and 0.491. However, among specifications allowing for a higher polynomial degree (column 3 and 4), one F-test reject the null-hypothesis at the 5-percent level. This is most likely due to the level of education estimates being highly variable and the quadratic specification overfitting the data. Nevertheless, all estimated coefficients are small in economic terms. E.g. column 1 in Table 4.2 shows a linear specification for the main bandwidth of 18-months on each side of the threshold. The likelihood of treatment decreases by only 0.001–0.02 percent per 10,000 SEK in annual earnings (in year 2006).[19]

As an additional test of the continuity assumption, I plot separately the relation between the outcomes listed in Table 4.2 and the forcing variable in Figure A-4.1. Column (5) and (6) in 4.2 show the results of these relations by regressing individual job seekers' covariates separately on the treatment indicator along with the control function. There appears to be some imbalance at the threshold job seekers just below (in the treatment group) have about a 2 percentage point lower likelihood of having a college degree, significant at the 5 percent level. As higher education is negatively correlated with both unemployment duration and the use of UI, any bias stemming from this potential imbalance should

[19] As I restrict the sample based on the pre-unemployment wage (reported in the IAF data) I choose to balance annual earnings in the year 2006 which is the year before the first spell int he sample. Balancing pre unemployment wages also renders an exact zero.

Table 4.2. Balancing of Covariates

	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.0012	0.0020	0.0022	-0.0003	-0.0001	0.0149
	(0.0041)	(0.0037)	(0.0024)	(0.0024)	(0.0138)	(0.0196)
Age	0.0001	0.0003	0.0004	0.0001	0.0414	0.4771*
	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.1773)	(0.2761)
Annual Earnings	-0.0002	-0.0002	-0.0001	-0.0001	-0.2764	-0.5756
	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.3407)	(0.6638)
Level of education						
Primary School	0.0020	0.0021	0.0009	-0.0006	-0.0056	0.0199
	(0.0090)	(0.0079)	(0.0056)	(0.0053)	(0.0112)	(0.0143)
High school	0.0024	0.0067	-0.0036	-0.0039	-0.0140	-0.0502*
	(0.0084)	(0.0075)	(0.0059)	(0.0050)	(0.0162)	(0.0268)
Some College	0.0063	0.0045	-0.0019	-0.0007	-0.0033	0.0011
	(0.0079)	(0.0080)	(0.0041)	(0.0039)	(0.0099)	(0.0174)
College	0.0113	0.0119	0.0011	0.0010	-0.0213**	0.0241*
	(0.0079)	(0.0080)	(0.0052)	(0.0048)	(0.0089)	(0.0143)
Ph.D	0.0020	-0.0037	-0.0077	-0.0066	-0.0001	-0.0008
	(0.0391)	(0.0360)	(0.0229)	(0.0214)	(0.0018)	(0.0026)
Polynomial degree						
1st order	✓	✓			✓	
2nd order			✓	✓		✓
Bandwidth ±	18	24	18	24	18	18
p-value	.497	.491	.0297	.28	.	.
R^2	0.769	0.763	0.911	0.905	.	.
# clusters	36	48	36	48	36	36
N	13,162	17,355	13,162	17,355	13,162	13,162

Notes: The table show balance tests of baseline covariates at the threshold. Columns (1)-(4) show results from regressing the a dummy for being above the threshold on a set of baseline covariates and a polynomial control function interacted with the threshold. The excluded category for highest attained education is less than primary school. The bottom of the table displays the F-statistic and the corresponding p-value from testing the hypothesis that all coefficients being jointly equal to zero. Columns (5)-(6) report results from balancing tests where each covariate have been regressed separately on the instrument and a polynomial control function in relative ranking interacted with the threshold. Standard errors clusteredon the forcing variable and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

render an underestimation of the treatment effect. Nevertheless, when estimating treatment effects in the next section I control for the level of education to handle this potential imbalance. The results are not sensitive to including these controls thus suggesting that the observed imbalance is of minor importance.

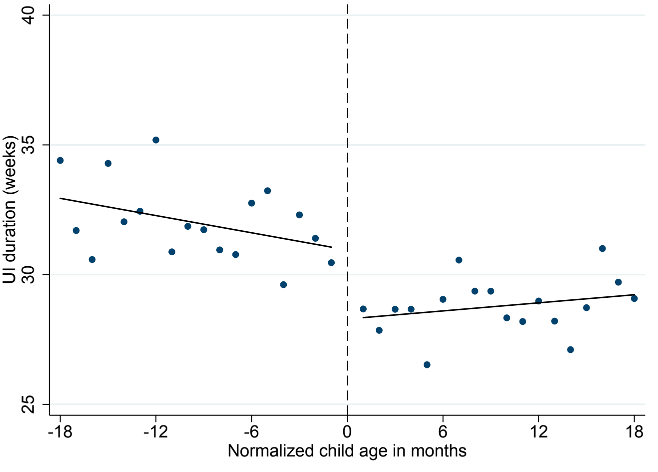
The overall take-away from this exercise is that job seekers have imprecise control of the forcing variable and failure to reject the continuity assumption. This leads me to conclude that treatment can be considered as good as randomly assigned among individuals around the threshold. If, on the other hand, a bias would exist, due to education being slightly unbalanced, it is likely to be minor. I control for all covariates listed in Table 4.2 in my regressions to avoid any potential bias and as can be seen in Table 4.3, this barely changes the estimates, thus confirming the lack of any substantial bias.

4.5 Results

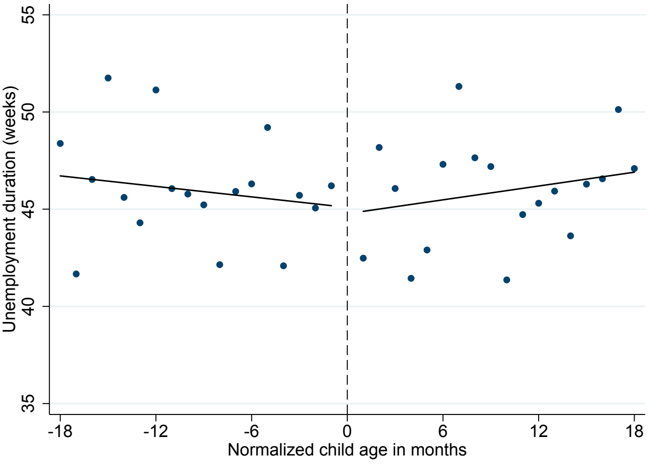
First, I present what I refer to as reduced form estimates. As the benefit extension is granted based on the age of a job seekers' child at the time of UI exhaustion, which by a forward looking individual could be foreseen, these estimates reflect an ITT-effect. I.e how having the possibility of utilizing the 30 week UI extension affect the duration on UI and in unemployment. So I do not condition on job seekers *de facto* utilizing the extension. As these reduced form estimates could be influenced by dynamic selection, I follow job seekers dynamic responses to the potential extension of UI duration by analyzing survival an hazard rates to employment and its timing with respect to UI exhaustion.

Figure 4.4. UI benefits and duration in unemployment by normalized child age

(a) *UI benefit use*



(b) *Unemployment duration*



Note: The figure show a) average utilization of UI and b) unemployment duration, both in weeks and as a function of a job seekers' child age in months at regular UI exhaustion (week 60), normalized to zero for age 18 years. The regressions include a linear polynomial function interacted with the threshold. Bins are discrete and represent 1 month.

4.5.1 Reduced form response to benefit extension

Figure 4.4 (A) plots UI the average utilization of UI by the forcing variable. There exist a clear discontinuous downward jump at the threshold indicating that job seekers who are eligible to the 30 week extension indeed use more weeks of UI. Table 4.3 show estimates of the effect of the 30 week UI extension on actual UI benefit duration and unemployment duration. As can be seen in column (1), the discontinuous in Figure 4.4 (A) is estimated to 2.5 weeks (standard error 0.702) which corresponds approximately to a 9 percent increase on average. This suggests that a 10 week increase of potential UI renders roughly one additional week in actual take up which corresponds to an elasticity of about 0.2 which is somewhat smaller than the UI elasticities found in Germany by (Schmieder, Von wachter and Bender, 2012). It is reassuring that adding covariates or allowing for higher order polynomials in column (2) and (3) hardly changes the estimates, thus bolstering confidence in the identifying assumption that treatment is orthogonal to other characteristics correlated with the outcome. Moreover, results remain stable when varying the bandwidth as can be seen in Figure A-4.4.

While the benefit extension have a clear effect on actual utilization of UI, it need not necessarily affect unemployment duration if job seekers who utilize the extension would have otherwise, in absence of the extension, would continued to be unemployed but without additional benefits. Thus the effect on UI duration can be seen as a "first stage" to the effect on unemployment duration. Figure 4.4 (B) plots the average time registered at the PES as unemployed by the forcing variable. Here there is no evidence of the extension having an effect on average unemployment duration as it appears continuous at the threshold. The estimated jump at the threshold, displayed in columns (4) to (6) of Table 4.3, lies around 0.3-0.4 weeks and allowing for a more flexible functional form the estimated effect even turns negative. Allowing for a larger bandwidth increases the estimated effect on unemployment duration to around 2 to 3 weeks, although never statistically significantly different from zero at conventional levels (see Figure A-4.4).

TABLE 4.3. WEEKS OF UI UTILIZATION AND UNEMPLOYMENT DURATION

	UI utilization			Unemployment duration		
	(1)	(2)	(3)	(4)	(5)	(6)
Below 18	2.512***	2.484***	2.019**	0.323	0.397	-0.939
	(0.702)	(0.714)	(0.935)	(1.755)	(1.854)	(2.808)
Control mean	28.286***	15.687***	15.978***	44.771***	20.227***	21.228***
	(0.410)	(2.818)	(2.743)	(1.403)	(5.979)	(6.235)
Polynomial degree						
1st order	✓	✓	✓	✓	✓	✓
2nd order			✓			✓
Controls		✓	✓		✓	✓
Bandwidth	18	18	18	18	18	18
# clusters	36	36	36	36	36	36
N	13,202	13,162	13,162	13,202	13,162	13,162

Notes: The table show estimates on number of weeks of utilized UI and number of weeks in unemployment. All regressions include n:th order polynomials the running variable and its interaction with the treatment indicator to allow for different slopes on each side of the threshold. When indicated regressions control for: gender, age, annual earnings in year 2006, and 5 dummies for level of education. Standard errors clustered on the forcing variable and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Thus, it appears as if the 30 extension has not caused job seekers to stay in unemployment longer but rather to the same extent but with somewhat higher benefits. Nevertheless, it is important to recall that this analysis is unable to take into account whether differences in potential duration has generated differences in e.g. time to employment. It is possible that treatment and control group have the same average length in unemployment but are leaving unemployment to different states (e.g. regular employment, subsidized employment, non-employment). To address this, in the following section I make use of the richness of the PES data which include cause of exit (see section 4.4.2 for description) which allows me to track job seekers throughout the unemployment spell and see why they leave unemployment.[20]

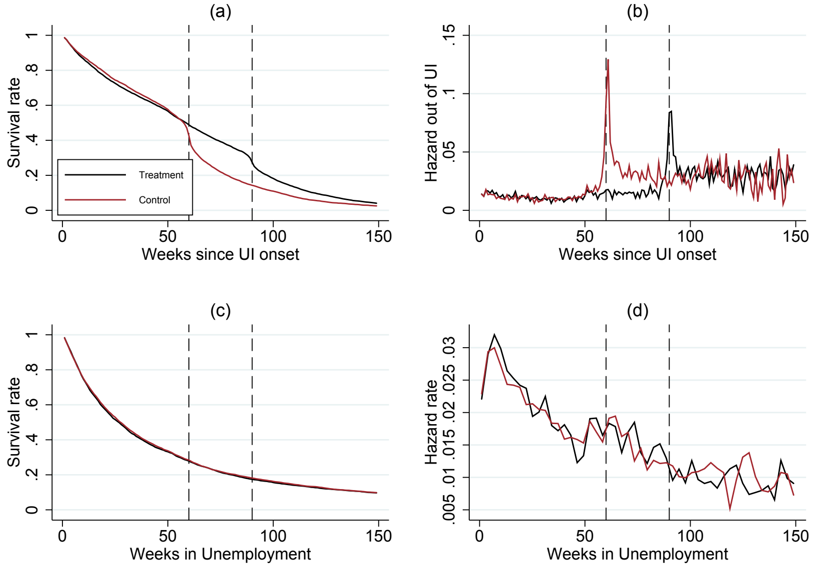
4.5.2 Dynamic response

Graphical analysis

The top panel in Figure 4.5 plots (a) the probability of collecting UI and (b) the probability of leaving the UI scheme, by calender weeks since the start of UI benefits. This is done for workers within the 18 month bandwidth such that the lines corresponds survival and hazard functions for the group of job seekers below (black) and above (red) the threshold, not controlling for the running variable. After 60 weeks, about 50 percent are still collecting UI benefits at some rate. At that time, when UI exhaustion occurs for job seekers in the control group having collected UI 5 days a week, there is a spike in the hazard rate out of UI where job seekers above the threshold are about 12 percent more likely to go off UI. Similarly, there is an equivalent spike at week 90 for job seekers below the threshold who are eligible to the 30 week UI extension. These spikes are to some extent mechanical as UI is exhausted and job seekers are able transfer to the JDG where they would receive activity support from the social insurance agency. Nevertheless, it shows that the treatment and control groups are well defined as there exists a "first stage" in the form of leaving UI.

[20]Unfortunately, the data does not allow me to test attrition to non-employment as it has been shown that about 45 percent of this attrition is due to finding employment while not reporting this to the PES (Bring and Carling, 2000).

FIGURE 4.5. SURVIVAL AND HAZARD RATES OUT OF UI AND UNEMPLOYMENT



Notes: The figure shows in the upper panel a) the probability of survival on UI and b) the hazard rat out of UI as a function of elapsed weeks on UI. In the lower panel c) shows the probability of survival in registered unemployment and d) the hazard out of unemployment as a function of weeks in registered unemployment.

Again, leaving UI need not imply that the job seeker leaves unemployment as he may transfer into e.g. the JDG and receive activity support. This becomes evident when plotting the weekly survival and hazard out of unemployment in Figure 4.5 (d) where the spike at week 60 and 90 virtually non-existent. There is, however, somewhat of an increase or flattening out of the hazard rate when approaching week 60 of unemployment. But equally so for the control and treatment group. Importantly, Figure 4.5 (c) show that the survival functions for the treatment and the control group are literally on top of each other up until week 45 of unemployment.[21] This indicates that dynamic selection out of unemployment is less likely to have occurred as job seekers seem not to act on the possible extension and thus that the absence of effects are unlikely to be driven by compositional changes in the groups. This is also

[21]Note that the likelihood of being on UI is greater than being unemployed. Whereas this can appear counter intuitive as one needs to be unemployed to collect UI benefits, leaving unemployment is defined as also having found part-time employment so job seekers keep collecting UI benefits for days they do not work.

confirmed in Table 4.4 showing non-significant differences in the hazard rate prior to week 50.

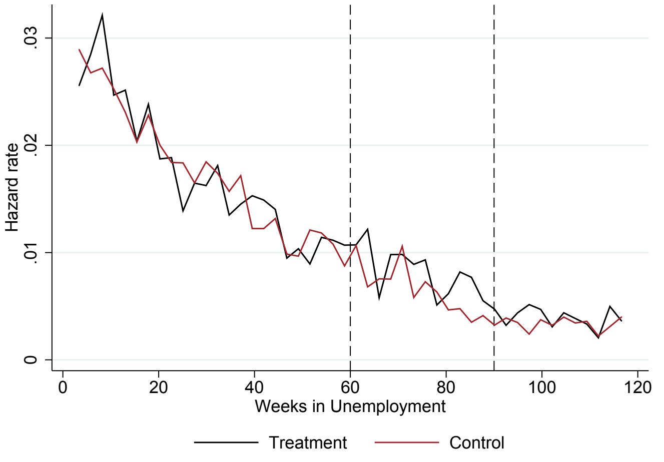
The slight increase in the hazard rate at regular benefit exhaustion (week 60) seems to be in line with previous studies such as (Katz and Meyer, 1990*a*,*b*, Carling, Holmlund and Vejsiu, 2001, Bennmarker, Carling and Holmlund, 2007), although much smaller in size. While there being no visible difference between treatment and control in hazard out of unemployment, there may still exist differences in the reason for exiting. In order to determine whether this small increase in the hazard is due to shrinking behavior or if job seekers become discouraged and leave the labor force I make use of the detailed PES data which provides the reason for exiting unemployment. Although, leaving unemployment for other reasons than regular or subsidized employment is very rare and constitutes about 6 percent of the sample with no significant differences across treatment and control group.[22] Figure 4.6 (a) plots the hazard rate to regular employment by unemployment duration.[23] There is no visible difference in the hazard to employment at week 60 when benefits are exhausted for job seekers in the control group. If anything, it appears as if job seekers entitled to the extended benefit have on average a higher likelihood of leaving for employment during the weeks 60 to 90 of unemployment. On the other hand, Figure 4.6 (b) show that all job seekers have a higher hazard rate to subsidized employment at the time of regular benefit exhaustion. The spike in the hazard starts at week 53 where job seekers become eligible to so-called new start jobs which is a subsidized employment where employers are exempted from paying the general payroll tax of 31.42 percent. While the spike in the hazard rate is present for both groups, it looks like job seekers in the control group have on average a higher likelihood of escaping to subsidized employment.

[22] About 12 percent of the sample are right hand censored.

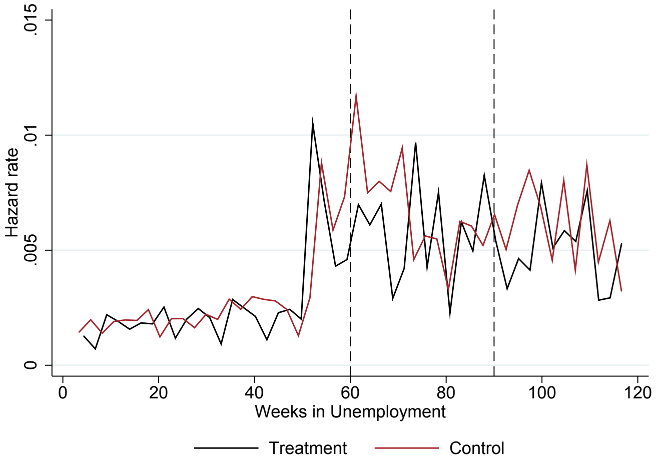
[23] Regular employment is defined as finding a non-government subsidized job wither full-time, part-time or temporary employment.

Figure 4.6. Hazard rate by unemployment duration and reason for leaving unemployment

(a) *Regular employment*



(b) *Subsidized employment*



Note: The Figure plots a) hazard rate to regular employment and b) hazard rate to subsidized employment by weeks in unemployment using a rectangular kernel with a bandwidth of 1. This is plotted separately for job seekers in the treatment group (black) and control group (red). The vertical dashed lines indicate benefit exhaustion for workers utilizing UI 5 days a week in the control group (week 60) and in the treatment group (week 90).

Model estimates

Using the model specified in equation (4.2), Table 4.4 quantifies the difference in hazard rates between control and treatment by unemployment duration. To make the comparison lucid, I have cut the weekly intervals into a pre-exhaustion period and then in blocks of 10 weeks. Column (1) to (4) show the results on the hazard to regular employment whereas column (5) to (8) show exit to subsidized employment. Other reasons for leaving then the ones indicated in the header of the columns are are censored. Column (1) of Table 4.4 show the likelihood of leaving unemployment for regular employment during the first 50 weeks for job seekers in available for regular employment, that is conditional on not having left unemployment for e.g. subsidized employment which is a censored event in this case. The vast majority of job seekers find a job before UI expire and the probability of having left unemployment before week 50, for any reason, is 69.3 percent. In comparison exit to subsidized employment is only 13.5 percent in the pre exhaustion period as seen in column (5).[24] Column (2) in Table 4.4 show the average difference in hazard rates to regular employment between treatment and control group whereas column (3) estimates this difference at the threshold and column (4) adds covariates. In the pre exhaustion period (week 0-50), there are no significant differences in the hazard to either regular nor subsidized employment prior to the extension period. This suggests that, in contrast to e.g. (Card, Chetty and Weber, 2007*b*), that job seekers where unaware of or at least have not acted on the possible UI extension. The absence of such anticipatory behavior may also be due to the rather high replacement rate in the Job and Development Guarantee (JDG). As the difference in replacement rates transitioning from UI to activity support is at maximum a 5 percentage point drop, the optimization cost may exceed the discounted value of the losses, thereby rendering job seekers passive (c.f. Chetty, 2012). This can be be compared to e.g. Germany where the nominal replacement rate is 53 percent while the effective unemployment assistance is about 35 percent and 10

[24]The reason the number of observations in column (1) and (5) in the first row of Table 4.4 adds up to more than the 13,202 used in the main estimation (see Table 4.1) is that ongoing spells (exceeding 50 weeks) are used in both samples.

percent for men and women, respectively, due to a reduction by spousal earnings (Schmieder, Von wachter and Bender, 2012).[25]

The absence of anticipatory behavior enables comparisons of control and treatment groups, conditional on unemployment duration exceeding 50 weeks as dynamic selection is likely a minor issue. Additionally, I test for dynamic selection by balancing of covariates at the threshold among job seekers unemployed at week 60. These results are shown in in Table A-4.1 and display no significant differences of job seekers' characteristics at the threshold and therefore gives creditably to the interpretation that the estimated effects post week 60 of unemployment are indeed a casual effect of the extended UI benefits and not an artifact of dynamic selection.

Column (2) of Table 4.4 show treatment effects during the (possible) benefit extension period. During week 60 to 90 of unemployment, job seekers eligible for the extension seem to in fact have between 1.5 to 2.2 percentage points higher probability of leaving unemployment for regular employment compared to job seekers eligible for the regular 60 week UI benefits. However, this difference turns insignificant in column (3) when estimated at the threshold by including the control function (column 3) and adding controls (column 4). Columns (6) to (8) show estimates of the difference in the probability of leaving unemployment for subsidized employment at different durations in the unemployment spell corresponding to Figure 4.6 (b). There is some suggestive evidence of job seekers in the control group are about 2 percentage points more likely to leave for subsidized employment just as the regular benefit period ends (week 61 to 69). This difference also turns insignificant when controlling for a first order polynomial in the control function and estimating the effect just at the threshold where job seekers should be as good as randomly assigned to the benefit extension. Nevertheless, while this increases the standard error the point estimate it remains about the same size.

[25]In Austria where Nekoei and Weber (2017) and Card, Chetty and Weber (2007*a*,*b*) study the effect of benefit increases on non-employment duration, unemployed job seekers who exhaust their benefits can apply for unemployment assistance, which is 92 percent of UI. However, as unemployment assistance is means-tested on household income, the effective replacement rate is only around 39 percent of UI.

TABLE 4.4. HAZARD RATE TO REGULAR AND SUBSIDIZED EMPLOYMENT

	Regular employment				Subsidized employment			
Unemployment week	Mean hazard rate [# obs.] (1)	Difference in hazard rate (2)	Difference in hazard rate (at threshold) (3)	Difference in hazard rate (at threshold)* (4)	Mean hazard rate [# obs.] (5)	Difference in hazard rate (6)	Difference in hazard rate (at threshold) (7)	Difference in hazard rate (at threshold)* (8)
0-50	0.653	0.005	0.011	0.012	0.135	-0.017	0.002	0.002
	[11,515]	(0.008)	(0.015)	(0.014)	[4,616]	(0.011)	(0.024)	(0.024)
51-60	0.082	0.004	-0.011	-0.012	0.050	0.000	-0.002	-0.005
	[4,490]	(0.007)	(0.012)	(0.012)	[4,340]	(0.007)	(0.016)	(0.016)
61-69	0.071	0.008	0.024	0.023	0.058	-0.021***	-0.018	-0.016
	[3,707]	(0.009)	(0.019)	(0.020)	[3,652]	(0.007)	(0.016)	(0.015)
71-80	0.060	0.015*	-0.005	-0.006	0.046	0.002	-0.015	-0.016
	[3,048]	(0.008)	(0.015)	(0.015)	[3,003]	(0.007)	(0.016)	(0.016)
81-90	0.046	0.022***	0.013	0.013	0.048	-0.005	-0.012	-0.013
	[2,491]	(0.007)	(0.014)	(0.014)	[2,498]	(0.009)	(0.020)	(0.020)
91-100	0.032	0.013*	0.011	0.012	0.051	-0.013	-0.023*	-0.025*
	[2,064]	(0.007)	(0.014)	(0.015)	[2,105]	(0.008)	(0.013)	(0.013)
101-110	0.034	-0.003	0.009	0.007	0.048	-0.002	0.015	0.011
	[1,769]	(0.009)	(0.021)	(0.022)	[1,796]	(0.011)	(0.027)	(0.027)
111-120	0.030	0.014*	0.005	0.005	0.038	-0.003	-0.003	-0.001
	[1,517]	(0.008)	(0.016)	(0.017)	[1,530]	(0.010)	(0.013)	(0.014)
Control function			✓	✓			✓	✓
Covarites				✓				✓

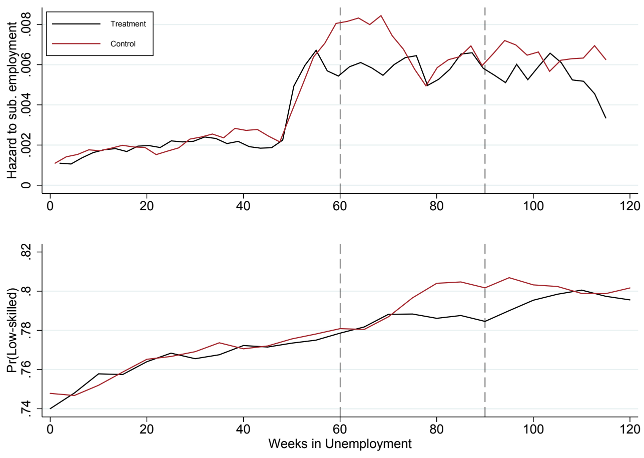
Notes: Table show estimates in column (1) and (5) average hazard rate to regular and subsidized employment, respectivly, with the number of observations in each sample in hard brackets. Column (2) and (6) estiamtes the mean difference in hazard to regular and subsidized employment, respecitvly, between control and treatment group. Column (3) and (7) additionally controls linearly for the forcing variable and its interaction with treatment and column (4) and (8) adds the pre determined covarites gender, age, annual earnings in year 2006, and 5 dummies for level of education. Standard errors clusteredon the forcing variable and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

An interesting, although weak, pattern emerges from Table 4.4. While job seekers eligible to the 30 week extension seem if anything somewhat less likely to exit to subsidized employment during week 61 to 100, they are more likely to exit regular employment. The upper panel of Figure 4.7 reproduces a more smoothed version of Figure 4.6 (b) and plots the hazard rate to subsidized employment for treatment and control group separately by unemployment duration. The lower panel of Figure 4.7 also plots the share of low-skilled job seekers, defined as the share of people with no more than high school education. job seekers with access to extended UI (treatment group) are about 2 percentage points less likely to be low-skilled between week 75-95. This could also explain the significant mean differences in the hazard to regular employment seen in column (2) to (4) in Table 4.4. Thus, I attribute lions share of the hazard difference to the fact that the control group is selected in such a way that it contains less able job seekers post exhaustion. Interestingly, there is no spike in the hazard to subsidized employment for the treated job seekers at extended benefit exhaustion (week 90). Rather, the treatment group seems less likely to leave for subsidized employment. This may seem surprising as the share of low-skilled individuals of the control and treatment group converges around week 110 of unemployment. However, I take this as evidence that the relatively high-skilled in the treatment group are the individuals that find regular employment. This suggests that the high-skilled individuals who entered into subsidized jobs would most likely have gotten regular employment had they remained on UI.

4.6 Conclusions

This paper uses a natural experiment in Sweden where job seekers with children under the age of 18 get 90 instead of 60 weeks of UI benefits, to show that although increasing potential UI duration had a positive effect on actual UI duration (estimated at 2.7 weeks, implying an elasticity of 0.2), it had no significant impact on either unemployment duration nor the hazard to employment. This stands in contrast to the previous literature which has found positive effects on unemployment duration rather consistently (see e.g. Card, Chetty and Weber, 2007*a*,*b*, Lalive, Van Ours and Zweimüller, 2006, Lalive, 2007, 2008, Landais, 2015, Nekoei and We-

Figure 4.7. Hazard rate to subsidized employment and share of low-skilled by weeks in unemployment



Notes: The upper panel of the Figure plots the hazard to subsidized employment using a rectangular kernel with a bandwidth of 4. The lower panel plots the share of low-skilled job seekers by five week intervals of unemployment duration. Low-skilled is defined as having no more than high school education as a function of weeks in registered unemployment. This is done separately for job seekers in the treatment group (black) and control group (red). The vertical dashed lines indicate benefit exhaustion for workers utilizing UI 5 days a week in the control group (week 60) and in the treatment group (week 90).

ber, 2017, Schmieder, Von wachter and Bender, 2012). I attribute this disparity of results to the rather generous replacement rates offered in programs available to job seekers after UI exhaustion. As the disincentive effects of UI depend on the change in replacement rates, which in Sweden is 5 percentage points, this creates minor financial incentives to adjust search behavior. This highlights the importance of taking alternative benefits schemes into consideration, and their potential effects on the incentives of job search, both when designing a UI-system and when estimating its effects on e.g. unemployment duration. While the effects of UI on unemployment duration and the hazard to employment are well researched, I encourage future researchers to look into how different levels of post UI exhaustion benefits (such as unemployment assistance) affect the duration on UI and in unemployment.

The previous literature has found that the probability of leaving unemployment increases sharply at benefit exhaustion (see e.g. Katz and Meyer, 1990*a*,*b*, van Ours and Vodopivec, 2006, Carling et al., 1996) which has mainly been attributed to strategic behavior and shirking among job seekers, thus timing job-finding to benefit exhaustion. However, Card, Chetty and Weber (2007*b*) opposes this view and shows, using Austrian data, that fewer than one percent of unemployment spells are manipulated in such a way. They point out that "[s]tudies that focus on the duration of benefit receipt often find elevated hazards prior to exhaustion. In contrast, most studies that have focused on time to re-employment and used administrative data to measure job starts have found relatively small changes in exit rates at or near benefit exhaustion." (p. 15). The evidence presented in this paper speaks in favor of the interpretation in Card, Chetty and Weber (2007*b*). I find no evidence of job seekers manipulating or postponing employment such that it should coincide with benefit exhaustion. Rather, while there being a sharp increase in the hazard rate out of UI the absence of a corresponding hazard to regular full-time or part-time employment is strikingly absent. Moreover, job seekers do not appear to lower their search intensity during the unemployment spell in anticipation of future UI benefits.

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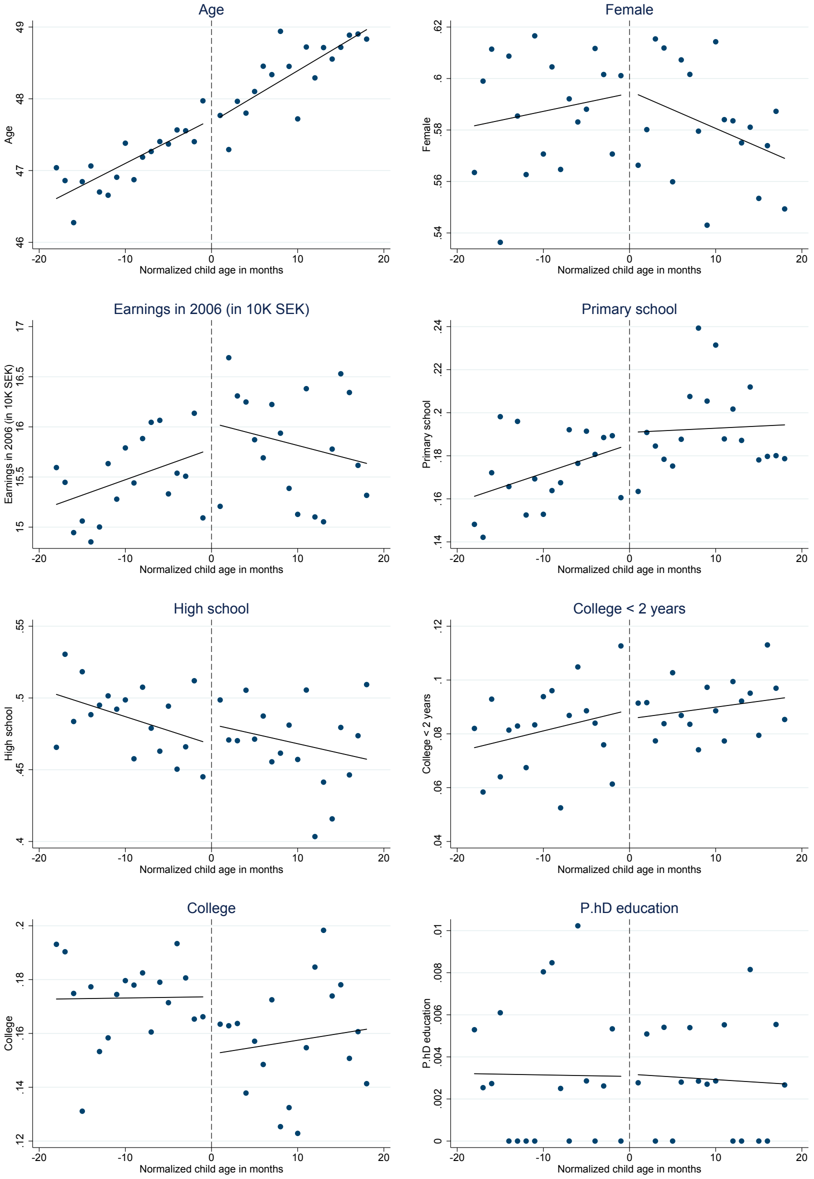
Appendix

TABLE A-4.1. BALANCING OF COVARIATES ON JOB-SEEKERS UNEMPLOYED AT WEEK 60

	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.0102	-0.0062	-0.0025	-0.0035	-0.0041	-0.0312
	(0.0087)	(0.0081)	(0.0047)	(0.0044)	(0.0413)	(0.0631)
Age	-0.0002	-0.0006	0.0005	0.0004	0.0455	0.7356
	(0.0011)	(0.0009)	(0.0007)	(0.0006)	(0.5335)	(0.8421)
Annual Earnings	-0.0002	0.0000	-0.0003	-0.0002	-0.1726	-0.9851
	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.4663)	(0.7023)
Level of education						
Primary School	0.0192	0.0127	0.0035	0.0083	0.0352	0.0001
	(0.0143)	(0.0133)	(0.0072)	(0.0067)	(0.0224)	(0.0341)
High school	0.0046	0.0073	0.0019	0.0004	-0.0463	-0.0457
	(0.0122)	(0.0123)	(0.0056)	(0.0058)	(0.0344)	(0.0461)
College $<$ 2 year	0.0130	0.0106	0.0109	0.0054	0.0023	0.0224
	(0.0232)	(0.0215)	(0.0133)	(0.0119)	(0.0206)	(0.0367)
College	0.0222	0.0167	0.0084	0.0120	0.0289	0.3489
	(0.0174)	(0.0160)	(0.0083)	(0.0080)	(0.0269)	(0.0396)
Ph.D	0.0405	0.0292	0.0376	0.0411	0.0022	0.0066
	(0.0600)	(0.0632)	(0.0344)	(0.0371)	(0.0042)	(0.0067)
Polynomial order						
1st order	✓	✓			✓	
2nd order			✓	✓		✓
Bandwidth ±	18	24	18	24	18	18
p-value	.349	.957	.233	.071	.	.
R^2	0.773	0.767	0.913	0.908	.	.
# clusters	36	48	36	48	36	36
N	3,281	4,319	3,281	4,319	3,281	3,281

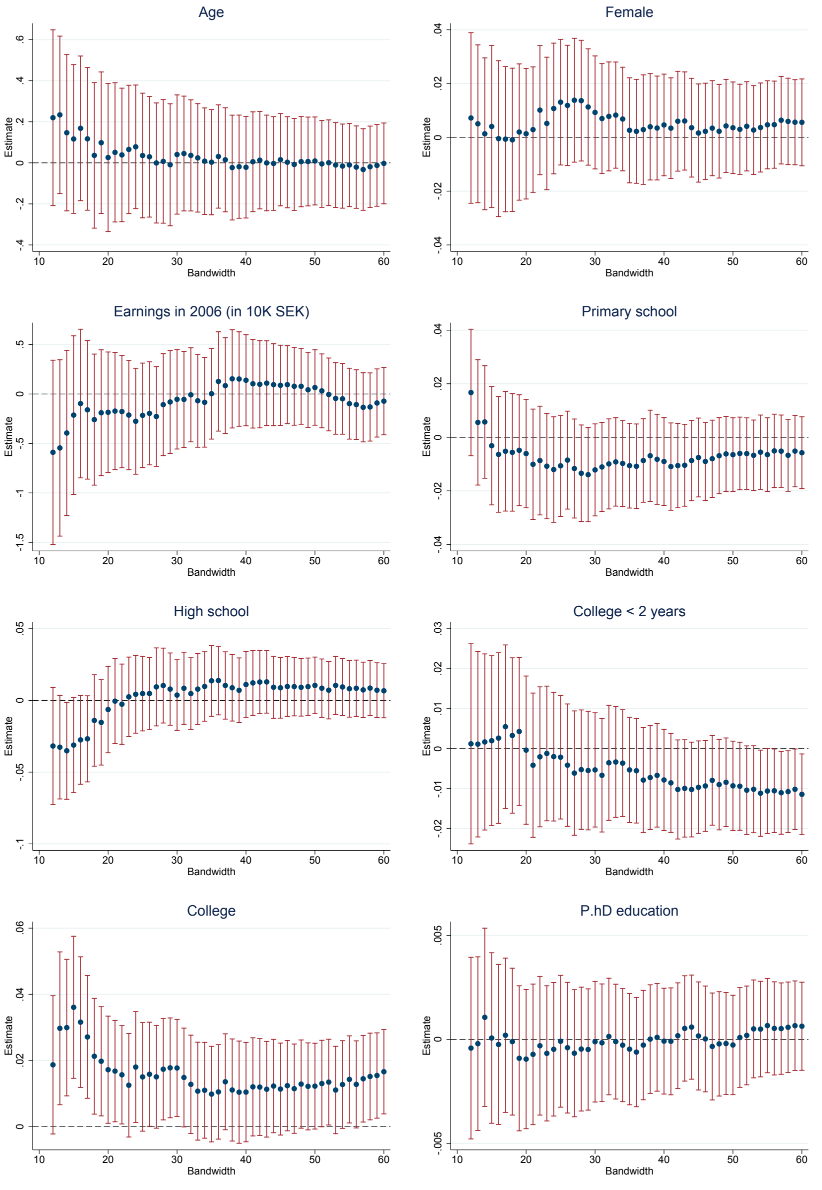
Notes: The table show balance tests of baseline covariates at the threshold for job seekers unemployed after 60 weeks. Columns (1)-(4) show results from regressing the a dummy for being above the threshold on a set of baseline covariates and a polynomial control function interacted with the threshold. The excluded category for highest attained education is less than primary school. The bottom of the table displays the F-statistic and the corresponding p-value from testing the hypothesis that all coefficients being jointly equal to zero. Columns (5)-(6) report results from balancing tests where each covariate have been regressed separately on the instrument and a polynomial control function in relative ranking interacted with the threshold. Standard errors clusteredon the forcing variable and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Figure A-4.1. Balancing of covariates



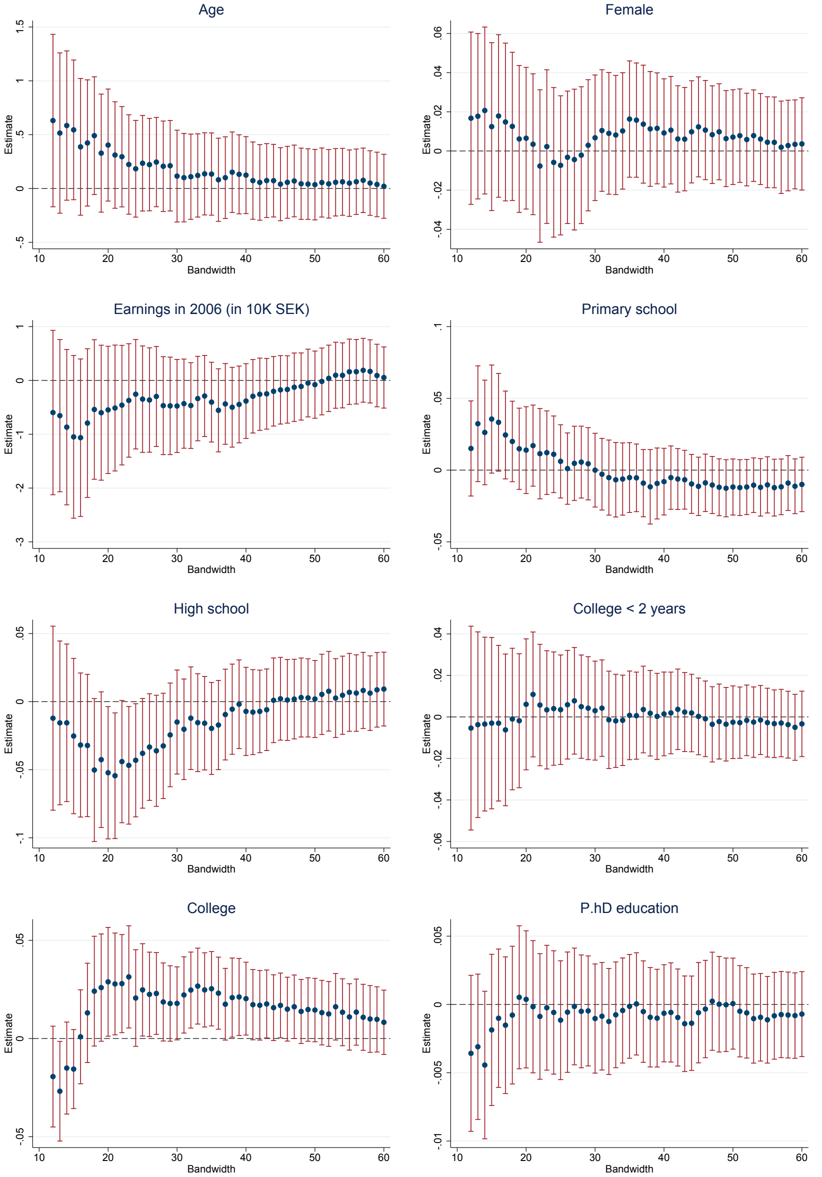
Notes: The figure plots average job seeker characteristics by the age of the job seekers' child in months at approximated benefit exhaustion. Age is normalized to 0 zero at the age of 18 and bins are discrete. Each graph is fitted with a first order polynomial on each side of the threshold and the estimated jump at the threshold can be found in Table 4.2 column (5), separately for each covariate.

Figure A-4.2. Balancing of covariates by bandwidth (1^{st}-order polynomial)



Notes: The figure show results from balancing of job seeker characteristics at the threshold for different bandwidths. Estimates are produced by regressing the specified pre determined covariate on an indicator for being below the threshold and a first order polynomial function interacted with the threshold. Standard errors clustered on the forcing variable and shown in parentheses and the red lines show 95 percent confidence intervals.

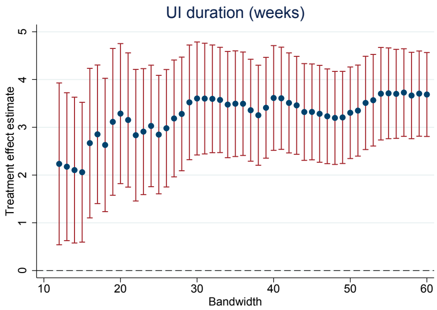
FIGURE A-4.3. BALANCING OF COVARIATES BY BANDWIDTH (2^{nd}-ORDER POLYNOMIAL)



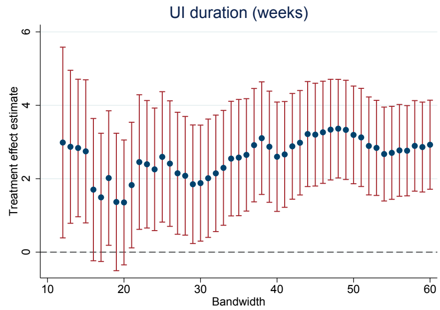
Notes: The figure show results from balancing of job seeker characteristics at the threshold for different bandwidths. Estimates are produced by regressing the specified pre determined covariate on an indicator for being below the threshold and a second order polynomial function interacted with the threshold. Standard errors clustered on the forcing variable and shown in parentheses and the red lines show 95 percent confidence intervals.

FIGURE A-4.4. ESTIMATED TREATMENT EFFECTS BY BANDWIDTH

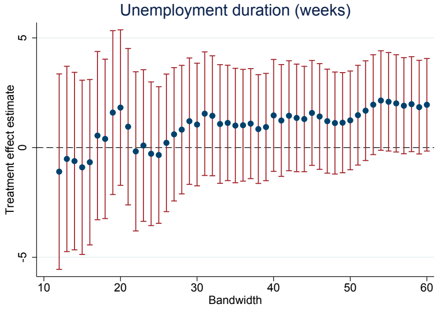
(a) 1^{st}*-order polynomial*



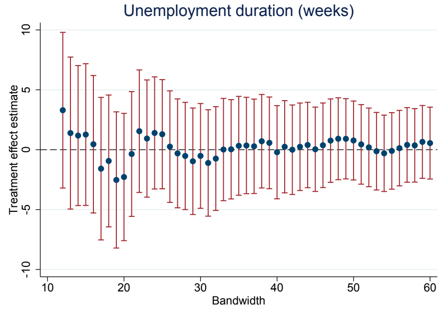
(b) 2^{nd}*-order polynomial*



(c) 1^{st}*-order polynomial*



(d) 2^{nd}*-order polynomial*



Notes: The figure show estimated treatment effects along with 95 percent confidence intervals, estimated by equation (4.1), as a function of bandwidth around the threshold. This is done for duration on UI in weeks and weeks in unemployment as well for the natural log of both variables. The regressions include a linear polynomial function interacted with the threshold and controls for gender, age, annual earnings in 2006 and six dummies for level of education. Standard errors in parentheses which are clustered on the forcing variable.

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