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The Effect of Labor Market Shocks across the Life Cycle

Abstract

Adverse economic shocks occur frequently and may cause individuals to reevaluate key life decisions in ways that have lasting consequences for themselves and the economy. These life decisions are fundamentally tied to specific periods of an individual's career, and economic shocks may therefore have substantially different impacts on individuals – and the broader economy - depending on when they occur. We exploit mass layoffs and establishment closures to examine the impact of adverse shocks across the life cycle on labor market outcomes and major life decisions: human capital investment, mobility, family structure, and retirement. Our results reveal substantial heterogeneity on labor market effects and life decisions in response to economic shocks across the life cycle. Individuals at the beginning of their careers invest in human capital and relocate to new labor markets, individuals in the middle of their careers reduce fertility and adjust family formation decisions, and individuals at the end of their careers permanently exit the workforce and retire. As a consequence of the differential interactions between economic shocks and life decisions, the very long-term career implications of labor shocks vary considerably depending on when the shock occurs. We conclude that effects of adverse labor shocks are both more varied and more extensive than has previously been recognized, and that focusing on average effects among workers across the life cycle misses a great deal.

JEL-Codes: I200, J630.

Keywords: labor supply, human capital, education, fertility, family formation, mobility, retirement, disability, economic shocks, job displacement.

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1. Introduction

Adverse economic shocks occur frequently and may cause individuals to reevaluate key life decisions in ways that have lasting consequences for themselves and the broader economy. These life decisions are fundamentally tied to specific periods of an individual's career, and economic shocks can therefore impact individuals differently depending on when shocks occur. For example, economic shocks may be more likely to impact human capital investments and mobility decisions of young workers, family formation and fertility decisions of mid-career workers, and labor force exit and retirement decisions of old workers. The effects may also vary significantly across men and women as they differ in terms of career and life choices, the timing of these choices, and the costs and benefits associated with such choices. Because of differential interactions between economic shocks and key life decisions across the life cycle, the very long-term career implications likely also vary considerably depending on the timing of the shock.

In this paper, we exploit mass layoffs and establishment closures to provide the first comprehensive analysis on the impact of adverse shocks across the life cycle and how these shocks impact major life decisions: human capital investment, family structure, and retirement.[1] We then follow individuals up to 15 years after the shock to provide novel insights on the aggregate reduced-form impact of all these effects on the very long-term career outcomes of individuals. Using mass layoffs and establishment closures to explore these questions is ideal, as these events occur often and generate sizable employment and earnings losses (Ruhm 1991, Jacobson *et al.* 1993, Davis and von Wachter, 2011, Ichino *et al.* 2017).[2] We thus have a context in which individuals of all ages face large adverse shocks; this allows us to test if the impact of such shocks differ across the life cycle, if individuals' major life decisions change depending on when in their careers the shocks occur, and what the sum total of all these effects are in the very long term.

The core contribution of this paper is to move beyond the existing literature on adverse labor shocks and demonstrate that focusing on average effects among workers across the life cycle misses a great deal. Our results highlight that the effect of shocks differs greatly depending on the age at which it occurs, and that individuals fundamentally differ in how they respond to shocks depending on their age. These results highlight the importance of establishing more flexible

[1] While human capital investment, fertility, and retirement are not the only important decisions impacted by job loss, they are of high economic interest and the focus of this paper.
[2] A mass layoff is defined as an establishment losing at least 30 percent of its workforce in a given year.

employment protection and support policies that account for the age-varying nature of economic shocks. In addition, our results highlight that males and females may benefit from differential employment protection and support policies given the observed heterogeneity in the interactions between economic shocks and key life decisions. Shedding light on such differential effects is important as males and females have dissimilar access to risk-mitigating institutions and technologies, and face different costs and benefits associated with career and life decisions. We conclude that effects of adverse labor shocks are both more varied and more extensive than has previously been recognized.

To conduct our analysis, we exploit matched employer-employee records on all Norwegian residents aged 20 through 60 between 1986 and 2018. These data allow us to link each worker with her employer and identify whether establishments are downsizing or closing down from one year to the next. A unique personal identifier enables us to combine these data with information from various population-wide administrative registers, such as the central population register, the education register, the tax and income register, the social benefit registers, the wealth register, the military conscription test register, and the residency and workplace location registers. Consequently, we can construct an extensive panel covering the universe of Norwegian workers and much of their demographic, education, labor, wealth, ability, and family information.

In terms of empirical method, we begin by defining a set of base years, 1989 through 2006. We set relative time to equal 0 for all individuals in that base year. We define our treatment group as those who lost their job due to a mass layoff or establishment closing between relative time 0 and relative time 1. We then follow workers over time – from relative time *-3* through relative time *+4* – and use an event study approach to compare changes in outcomes among those who experienced an involuntary job separation relative to those who did not. We perform this analysis for all men and women as well as separately for men and women at different ages, from age 20 to age 60. In every specification, we include individual fixed effects to account for potential time-invariant systematic differences across individuals.[3]

[3] Most papers in the job loss literature condition on set of base year plant, industry and region characteristics when they compare outcomes of displaced and non-displaced workers. Individual fixed effects are more conservative as they account for all of these differences as well as any unobservable time-invariant differences between displaced and non-displaced workers. Provided that individual panel data is available, the use of individual fixed effects is often preferred in the job loss literature. See for example Huttunen, Moen, and Salvanes (2016), Schaller and Zerpa (2019), and Willage and Willén (2020),

We supplement our main analysis method with an alternative identification strategy that relies exclusively on the difference in the timing of treatment. Specifically, we follow Fadlon and Nielson (2019) and deviate from the conventional job loss literature assumption that everybody who satisfy the employment history conditions should be in the control group. Instead, we include only workers in firms where a plant closure or mass layoff happens some time in the future. This alternative approach overcomes the concern that the control group is positively selected. This alternative approach therefore relies on a different set of identifying assumptions that are considerably milder than our baseline estimation approach (that the timing of the shock is conditionally random rather than that the shock in itself is conditionally random), provides strong protection against the concern that individuals may be endogenously selected into the treatment sample, and helps reinforce the causal interpretation of our findings.

We present four sets of results. First, consistent with existing literature, we document substantial earnings and employment effects caused by involuntary job separation. However, in contrast to prior literature, we show that the employment and wage effects vary greatly across the life cycle. While individuals in their twenties suffer relatively small wage effects and manage to almost fully recover within four years of the shock, individuals in their fifties experience much greater effects both in terms of initial impact as well as persistence. The differential effect on employment and earnings across the life cycle is an interesting finding with important implications for employment protection and welfare policies. Specifically, it suggests that displacement harms individuals differently across their careers, and highlights the value of establishing more flexible employment protection and support policies that account for the age-varying nature of economic shocks.

Second, we find substantial heterogeneity in how adverse labor shocks impact key life decisions of individuals across the life cycle. Individuals at the beginning of their careers respond to economic shocks by relocating to other local labor markets and investing in human capital, individuals in the middle of their careers respond by reducing fertility and being more likely to end their marriages, and individuals at the end of their careers respond by permanently exiting the workforce and collecting disability pension. The majority of the human capital effect is driven by displaced workers enrolling in basic post-secondary education.

Third, we provide suggestive evidence of the mechanisms through which the differential impact between economic shocks and major life decisions occurs. We find that the human capital effects among young workers are driven by high-ability and high-wealth individuals, as well as by individuals from high-SES backgrounds; individuals who face lower costs of returning to school, have few financial constraints, and can potentially enjoy greater returns to their investments. With respect to the negative fertility effects, we show that they are driven by high-income and high-educated individuals who experience relatively larger labor earnings drops and potentially face greater career concerns. Regarding the differential family formation effects (divorce), we show that they are driven by individuals in newer relationship who may be less equipped to deal with adverse shocks. Finally, we show that the differential effect on non-labor force participation across the life cycle is primarily driven by mobility preferences and disability pension take-up.[4]

Our final set of results revolve around the very long-term career implications of job loss as measured 15 years after the shock. These results should be interpreted as the sum total of all the differential interactions between economic shocks and key life decisions across the life cycle (including those that we cannot observe). We find that the very long-term career implications vary considerably depending on the timing of the shock. First, individuals in their early twenties display economically meaningful and statistically significant positive labor earnings effects 15 years after the displacement event. These positive effects are driven by individuals who returned to school, and who relocated to new local labor markets, in response to the displacement event. Second, individuals in their late fifties display very modest and not economically meaningful effects 15 years after the displacement event. This is consistent with the majority of individuals in this age group having exited the labor force and entered retirement within 15 years regardless of losing a job. Third, individuals between these two age groups experience persistent adverse labor earnings effects that extend 15 years after the displacement event. These results highlight the importance of accounting for the dynamics of economic shocks and their interactions with major life decisions across the life cycle when evaluating their overall impact on individuals.

We also document sizable effect heterogeneity across males and females. Shedding light on such differential effects is important as males and females have dissimilar access to risk-mitigating institutions and technologies, and face different costs and benefits associated with career and life decisions. Males are much more likely to respond to economic shocks by returning

[4] With respect to the mobility effects among old workers, we show that financially constrained individuals, and individuals with close family in the same geographic location, are significantly less mobile. In addition, we show that high ability individuals, and highly educated individuals, are more mobile due to underlying job preferences.

to school at a young age, while the effect on females is constant across the first twenty years of their careers. Similarly, the effect on permanent labor market exit and retirement take-up is much more pronounced among males at an older age, while it is noticeably smaller among females. Finally, the fertility effects among males are significantly more substantial at the beginning of their careers, while the impact on females is more concentrated around primary childbearing age (around 30 years old in Norway). Due to the differential interaction between life decisions and economic shocks among males and females, the differential impact on individual earnings across sexes – depending on when during the life cycle the shock occurs – also is substantial. Specifically, females suffer considerably larger effects from job loss that occurs early in their careers while males experience much greater impacts from job loss that takes place later in their careers.

The key assumption underlying our empirical method is the parallel trend assumption - that non-displaced individuals, conditional on a rich set of controls and individual-level fixed effects, represent a plausible counterfactual trend of what the outcomes would have been for displaced individuals had they not been displaced. To study this assumption, we present event studies that show no evidence of differential trends between our treatment and control units prior to a mass layoff or establishment closure event. The other main assumption we invoke is that there are no other contemporaneous shocks that occur in relative time *0* and that impact treatment and control individuals differentially with respect to the outcomes we examine. To study the credibility of this assumption, we follow the existing job loss literature and implement a rich set of sensitivity analyses and robustness checks designed explicitly to examine the credibility of this assumption (e.g., Huttunen *et al.* 2011; Del Bono *et al.* 2012; Huttunen *et al.* 2018). Specifically, we ensure that our results are unaffected by focusing only on establishment closures, looking only at very large establishments, accounting for early leavers, performing propensity score matching on individuals in the pre-displacement period, relaxing the employment history restrictions, extending the pre-displacement period to explore the parallel trend assumption further back in time, and systematically eliminating each industry and base year.

To further explore the credibility of our estimates, we supplement our main analysis method with an alternative identification strategy that relies exclusively on differences in the timing of treatment. Specifically, we restrict our control group only to those who will experience an involuntary displacement event in the future. Then we compare individuals who were involuntary displaced in a given year to individuals who will be involuntary displaced in a future year. This alternative approach relies on a different set of identifying assumptions that are considerably milder than our baseline estimation approach (that the timing of the shock is conditionally random rather than that the shock in itself is conditionally random) and helps reinforce the causal interpretation of our findings.

This paper contributes to the literature in several ways. First, by identifying direct channels through which workers' professional and personal lives are impacted by job loss, this paper contributes to the rich and growing literature on the effect of economic shocks on workers (e.g., Davis and von Wachter 2011; Oreopoulos *et al.* 2012; Adda *et al.* 2013). These studies provide important insights into the effects of shocks on workers' careers, but they do not examine the age-varying impact of shocks over workers' lives. In addition, they do not explore how these economic shocks impact other key life decisions across the life cycle, nor do they examine if interactions between economic shocks and key life decisions vary between men and women. Our contribution relative to this literature is to show that economic shocks may have substantially different effects depending on when they occur, and that the timing of shocks may have fundamentally different impacts on men and women across the life cycle. We see this paper as opening up a new avenue of research on heterogeneity of adverse labor shocks across the life cycle and how such shocks interact with major life decisions.[5]

Second, the effect of job loss has been studied extensively in the literature (e.g., Ruhm 1991; Jacobson *et al.* 1993; Huttunen *et al.* 2011; Ichino *et al.* 2017). Most of these studies have focused on the impact of displacement on wages and employment. The results reveal substantial earnings and employment effects both in the short-term and in the long-term. A rich set of studies have also explored the impact of mass layoffs on non-economic outcome, examining variables such as retirement, marital status, school enrollment, and criminality.[6] This literature provides important insights into workers' response to job loss, and have contributed to a deep understanding on the average effect of adverse shock on a range of outcomes. However, this literature offers little

[5] Another great paper that is related to ours is Rinz (*forthcoming*). This paper uses variation in local unemployment rates across commuting zones in the US to study heterogeneity in labor market effects of the Great Recession across different age groups. However, while Rinz (*forthcoming*) studies heterogeneity across age groups, examining the impact of recession-induced local economic conditions (regardless of being displaced) is fundamentally different from examining the impact of involuntary job separations.

[6] Mortality (Eliason and Storrie 2009a; Sullivan and von Wachter 2009; Browning and Heinesen 2011), morbidity (Browning *et al.* 2006; Eliason and Storrie 2010), pension (Rege *et al.* 2009), fertility (Huttunen and Kellokumpu 2016; Del Bono *et al.* 2012), children's school performance (Oreopoulos *et al.* 2008; Mörk *et al.* 2020; Tanndal *et al.* 2020; Coelli 2011; Rege *et al.* 2011; Willage and Willén 2020), mobility (Huttunen *et al.* 2018), marital status (Eliason 2012), school enrollment (Minaya *et al.* 2020), and criminality (Rege *et al.* 2009b).

information about the interaction of job loss and key life decisions across the worker's life cycle and between the different sexes. This knowledge gap is surprising, because economic shocks alter the costs and benefits of key life decisions that are fundamentally tied to specific periods of an individual's career, and that likely differ considerably between men and women. Our study, while supporting the key findings of the above papers, extends this literature by showing that the timing of economic shocks across the life cycle matter for how they impact individual career and life decisions, and that focusing on average effects without taking this dynamic component into account can lead to suboptimal policy recommendations.

More generally, we provide one of very few comprehensive analyses on the effect of adverse shocks on the very long-term career prospects and wellbeing of individuals. These effects are driven not only by the direct impact of job loss on employment, skills, and experience, but also by all the indirect impacts operating through changes in key life choices (including those that we cannot observe). This is an interesting summary measure to study, as it is not clear to what extent these major life decisions help, or hinder, the job prospects of displaced individuals in the very long-run. Specifically, while some of these life decisions may improve the future job market opportunities of individuals (e.g., human capital investments and labor market mobility), others may worsen the future job market outcomes of individuals (e.g., loss of occupational-specific skills, worsened occupational matches, and potential family instability). These results complement the novel findings of von Wachter, Song and Manchester (2013), who examine the effect of displacement on cumulative employment, and find that the average displaced worker experiences a decline of 1.5 years in total years employed. Our study extends this work by showing that the timing of economic shocks – and how they interact with major life decisions across the life cycle - matter for how they impact the very long-term career prospects of individuals. This is an important contribution to existing literatures on adverse shocks and individual labor market outcomes, and provides a useful summary measure of how damaging economic shocks are at different stages of individuals' careers.

Finally, this paper makes a large contribution to our understanding of differences in labor market outcomes between males and females. Existing research has shown that males and females face disparate career patterns due to factors such as family formation, educational investment, mobility preferences, and retirement (e.g., Kleven *et al.* 2019; Manning and Swaffield 2008). Existing research has also shown that men and women differ in career and life choices related to job search (e.g., Cortes *et al.* 2021), commuting (e.g., Le Barbanchon *et al.* 2020), and housework and childcare (e.g., Ellingsæter and Kitterød 2021; Thomas 1994). Finally, prior work suggests that men and women rely on different forms of social support, have access to different types of risk-mitigating technologies, and may be differentially exposed to the same types of shocks (e.g., Sabarwal *et al.* 2010; Rege *et al.* 2011). This paper contributes to all of these literatures by showing that men and women are differentially affected by job loss, and that these differential effects vary over the life cycle. We also show that men and women respond to shocks in different ways, and that the mechanisms through which they respond differ. Ultimately, we show that the very long-run career implications of involuntary job loss – the sum total of all interactions between economic shocks and key life decisions across the life cycle – are fundamentally different between men and women. This advances our understanding of differences in labor market outcomes between males and females and how they may arise.

In terms of policy implications, our results show that focusing on average effects among workers across the life cycle misses a great deal. Our results highlight that the effect of job loss differs greatly depending on the age at which it occurs, and emphasize the importance of establishing more flexible employment protection and support policies that account for the age-varying nature of economic shocks. For example, while a reduction in job search costs could prove effective in encouraging older individuals to remain in the labor force, educational support may be more beneficial to individuals at the beginning of their careers; particularly among financially constrained workers. In addition, our results highlight that males and females may benefit from differential employment protection and support policies given the observed heterogeneity in the interactions between economic shocks and key life decisions. We conclude that effects of job displacement are both more varied and more extensive than has previously been recognized.

The rest of this paper proceeds as follows. In Section 2, we provide information on the Norwegian employment protection policies, discuss key institutional background, and present our conceptual framework; In Section 3, we introduce our data, describe our sample, and present our empirical method; In Section 4, we present evidence on the labor market effect of job displacement across the life cycle; In Section 5, we discuss the interaction between involuntary job loss and key life decisions over the course of individuals' careers; In Section 6, we analyze heterogeneity across sex and education levels; In Section 7, we show results from falsification tests, robustness checks and sensitivity analysis; In Section 8, we conclude and provide policy recommendations.

2. Background

2.1 Institutional Background

The Nordic welfare state is based on universal healthcare, comprehensive social insurance, and free education through college. In terms of employment protection, Norway is considered to have a medium-to-high degree of protection relative to other OECD countries, similar to Sweden and France (Huttunen *et al.* 2011).[7] When establishments decide to downsize, there is no strict rule regarding the order in which workers are dismissed. While seniority is institutionalized in the main union agreements in the country, this does not represent a binding constraint.[8] Termination requires three months' notice, and there are no legal requirements for severance pay.[9]

Unemployment benefits are available to all individuals who have experienced a reduction in work hours of at least 50 percent, are registered as jobseekers at the public employment office, and had an income over a certain amount before becoming unemployed (Johnsen, Vaage and Willén 2021). The replacement rate is approximately 62 percent. The standard entitlement period was 186 weeks up until 2004, at which point it was reduced to 104 weeks. The rules are more generous for older workers, and from the age of 60.5 every individual is effectively entitled to unemployment benefits until the mandatory retirement age of 67.

One common exit route from the labor market is through disability pension (around 20 percent of the Norwegian population aged 55 through 67 receive disability pension). For the vast majority, the route to disability pension goes through one year of sick leave. Access to disability pension is relatively liberal in Norway, and prior research shows that local labor market conditions impact how doctors assess applications (Dahl, Nilsen, and Vaage 2003). Disability pension is equivalent to what the individuals would have received as public pension from age 67 had they continued in employment until that age. The after-tax replacement rate for fully disabled, previously average earners, is around 65 % (Blöndal and Pearson 1995).

[7] However, Norway is by no means an outlier. While it tends to compare favorably to countries such as the U.K., it is considered to have weaker protection policies than countries such as Italy (Huttunen *et al.* 2011).
[8] The reason for this is that the seniority rule only applies in situations in which "all else is equal," a condition that is very difficult to prove.
[9] Workers with less than five years of tenure can legally be dismissed with only one months' notice. However, in practice, the overwhelming majority of young workers receive a three months' notice.

2.2 Conceptual Framework

This paper examines the interaction of adverse economic shocks and major life decisions, and the extent to which the timing of economic shocks across the life cycle matter for people's career prospects in the very long run. In the main analysis, we focus on three key decisions: human capital investment, family structure, and permanent exit from the workforce. We highlight that this does not represent an exhaustive list of life decisions that may be impacted by economic shocks, and in supplemental analyses we extend the set of outcomes to also include outcomes such as mobility and commuting.

We begin by noting that economic shocks alter the costs and benefits of major life decisions such as human capital investment, family structure, and retirement. Individuals who are the margin of deciding whether to return to school, move to a city with better career prospects, have children, or enter retirement, may therefore be induced to do so by an adverse labor shock. Importantly, the costs and benefits of these major life decisions vary considerably over an individual's life. For example, even though job displacement may generate an across-the-board reduction in the cost of returning to school, the net benefit of additional schooling among old individuals is considerably smaller than that of young individuals. Economic shocks will therefore likely have substantially different impacts on individuals depending on when they occur. Because of differential interactions between economic shocks and key life decisions across the life cycle, the very long-term career implications will likely also vary considerably depending on the timing of the shock.

With respect to human capital investment, we hypothesize that the probability of investing in human capital and returning to school as a response to involuntary displacement decreases with age. First, lifetime financial returns of human capital investment decrease as people age, because they have fewer working years remaining. Relatedly, employers might not be interested in hiring an older worker who has just finished higher education. Second, the non-financial costs of education are higher for older people. These costs include more effort to learn in an unfamiliar educational environment, navigating new bureaucratic obstacles, and a lack of peers of similar age.

With respect to fertility, the relationship between age and reproduction is maximized when people are in their 30s; in Norway, the average age of first birth is 31. Since this is the age when most reproduction occurs, we expect the impact of job loss on fertility to be largest at this time.

The hypothesized impact of job loss on fertility is negative, because the decision to have children is a function of current and future resources. Since a job loss lowers current and future earnings, people considering having children (primarily people around age 30) may decide to have fewer children, or delay having children, when they experience job displacement.

With respect to permanent exit of the workforce, we hypothesize that the probability of permanently leaving the labor force for a displaced worker increases with age. First, the benefits of finding new employment decrease with age, because a person has fewer working years remaining. Particularly for workers late in their careers, the wait for pension eligibility is relatively short. Second, search frictions may be higher for older people, due to factors such as a difficulty using new technology to find positions and a lower preference for relocating and moving to other labor markets. In addition, finding a job for people close to retirement age might be hampered by demand-side obstacles, such as age-based discrimination.

One way to quantify the sum total of the differential interactions between economic shocks and key life decisions across the life cycle (including those that we cannot observe) is to examine the very long-term career implications on displaced individuals. Such effects will be driven not only by the direct impact of job loss on employment, skills, and experience, but also by all the indirect effects operating through changes in these key life choices. This is an important contribution to existing literatures on adverse shocks and individual labor market outcomes, and provides the first summary measure of how damaging economic shocks are for individuals at different stages of their careers in the very long term.

A priori, it is not clear to what extent these indirect life decision effects help, or hinder, the job prospects of displaced individuals in the very long-run. On the one hand, there are certain indirect effects that could serve to improve the future job market opportunities and labor market payoffs of individuals. For example, to the extent that displacement events induce individuals to invest in human capital, move across local labor markets, and delay family formation decisions, the adverse shock could even improve labor outcomes compared to what the individuals would have had absent the shock. On the other hand, there are also indirect life decision effects that could worsen the very long-term career prospect of individuals – for example loss of occupational-specific skills, worsened occupational matches, and potential family instability.

Although we cannot decompose the very long-term effect of job loss into that driven by direct employment effects, and that driven by indirect life decision effects, we can provide the sum total effect of all these changes. We do this by taking advantage of the rich Norwegian administrative data and examining the impact on earnings 15 years after the involuntary job separation event. We argue that this provides us with sufficient time to allow for all indirect effects to actualize. When interpreting these results, it is important to highlight that we have not analyzed an exhaustive list of life decisions that may be impacted by economic shocks. Thus, these results should be interpreted as the sum total of all the differential interactions between economic shocks and key life decisions across the life cycle, including indirect effects that we cannot observe.

An important contribution of this paper is to explore differences in the interaction of economic shocks and key life decisions between males and females. Examining such heterogeneity is interesting because the existing literature has documented sizable gender differences in occupational choice (e.g., Cortes and Pan 2018), career wage growth (e.g., Napari 2009), promotions and career progression (e.g., Blau and Devaro 2007) and fertility timing. In addition, a rich and growing literature has documented important gender differences in job search and mobility (e.g., Cortes, Pan, Pilossoph, and Zafar 2021), in the financial return to job mobility (e.g., Del Bono and Vuri 2011), in the willingness to trade off commute against wage (e.g., Barbanchon, Rathelot and Roulet 2021), in early retirement take-up (e.g., Dahl, Nilsen, and Vaage 2003), in housework (e.g., Bertrand *et al.* 2015), and in how males and females are impacted by the arrival of children (e.g., Kleven, Landais, and Sorgaard 2019). Thus, males and females differ significantly in terms of career development and occupational choice, but also in terms of key life choices and the timing of such decisions. Because of differential interactions between economic shocks and key life decisions across the life cycle, the very long-term career implications among males and females likely also vary considerably depending on the timing of the shock.

3. Data and Method

3.1 Data

We leverage rich population-wide administrative data on all Norwegian residents aged 20 through 60 between 1986 and 2018. A unique anonymous personal identifier enables us to follow individuals over time and across registers, such that we can construct a longitudinal panel covering the universe of residents and much of their demographic, education, labor, and family information.

We obtain demographic characteristics from the central population register, we collect education information from the national education register, we use wage and labor earnings information from the tax register, and we obtain information on hours worked, establishment, and employer from the linked employer-employee register.

Crucial to the analysis is our ability to identify whether establishments downsize or close each year, which is made possible through the linked employer-employee data. Following the existing literature, we define a mass layoff as the establishment reducing employment by more than 30 percent from one year to another, and we exclude establishments with fewer than 20 employees to avoid false closures and mass layoffs (Huttunen *et al.* 2018; Willage and Willén 2020). In Section 6, we show that our results are robust to relaxing the restriction on firm size.

Our data provide detailed labor market information on all Norwegian residents. Labor earnings is measured as pre-tax income (income from labor and self-employment) including taxable government transfers (parental leave, sick leave, and unemployment benefits). We also present some results using gross market income which excludes benefits. Employment status (employed, unemployed, and not in the labor force) is defined at the time of the worker-establishment match.[10] An individual is defined as employed if she has an establishment identification number at that time, and as unemployed if she does not have an establishment identification number at that time and is registered with some unemployment benefits during the year. An individual is defined as not in the labor force if she does not have an establishment identification number and is not registered with any unemployment benefits during the year.

In terms of demographic characteristics, our data provide information on sex, age, education, marital status, and family composition. Through an anonymous family identifier we can obtain information on the individual's spouse as well. Local labor markets are defined based on commuting distance and divides Norway into 160 regions (Gundersen and Juvkvam 2013).[11] In Section 6, we explore treatment heterogeneity across different educational levels. To facilitate interpretation of these results we focus on three education levels: less than high school, high school, and at least some college.

[10] In May until 1995 and in November from 1996.
[11] Local labor markets span more than one municipality (the lowest administrative unit consisting of 435 municipalities during our analysis period), but are smaller than counties (the second-lowest administrative unit).

Table 1 provides summary statistics of key variables from our data, stratified by treatment status. On average, those in our analysis sample are 42 years old, 40 percent are female, 60 percent are married and 10 percent have had a divorce. The modal worker has approximately 12 years of education. The average individual has 1.8 children. That our sample is slightly older and majority male is likely an implication of the sample restrictions we impose; we discuss this in Section 3.2. In Section 7, we demonstrate that our results are robust to relaxing these restrictions.

As we exploit a difference-in-differences framework, we do not need our control group to be similar to our treatment group on observable characteristics (only that they would have trended similarly had the treated individuals not been displaced). Nevertheless, it is interesting to note that the treatment and control group are very similar in general.

3.2 Sample Construction

To construct our sample, we first define a set of *base years,* 1989-2006. We then identify all individuals between the ages of 20 and 60 who were employed at least 20 hours a week at a plant with at least 20 employees in one of the base years. We set relative time equal to 0 for each individual in that base year.

The treatment group consist of those who lost their job due to an establishment closure or mass layoff between relative time 0 and relative time 1. For the control group (individuals not subject to mass layoffs and plant closures), we include individuals who were not displaced between relative time 0 and relative time 1. Thus, our analysis consists of comparing the outcomes of individuals subject to an involuntary job loss with the outcomes of individuals not displaced in that same year. Because we consider displacements that occur in several different years, in the analysis we stack the data from all base years and estimate regressions in event time. We always include base year fixed effects.

We follow people for 8 years around each base year – from relative time -3 to relative time +4. Thus, our main analysis period stretches from 1986 through 2010. To ensure that individuals in the control and treatment groups are as similar as possible, we require that individuals have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years. This implies that our analysis sample consists of workers highly attached to the labor market, similar to what other papers in the literature have done (e.g., Huttunen *et al.* 2011). In all specifications, we include individual fixed effects to account for any time-invariant differences across individuals such as base year occupation, industry, education, family composition, and demographics. It should be noted that some of the outcomes we explore are only available from 1993 onwards, and that the sample size underlying the various regressions therefore differ slightly across certain outcomes. However, the results are robust to restricting all analyses to 1993 onwards.

3.3 Method

We use involuntary job loss from mass layoffs and establishment closures to examine the impact of adverse economic shocks on labor market outcomes across the life cycle, and to study how such shocks impact major life decisions: human capital investment, family structure, and retirement. To this end, we divide the sample into 5-year age bins based on the age of individuals in the base year. We then estimate the following model separately for each of these age groups:

$$y_{ibt} = \alpha + \sum_{t=-3}^{4}[\pi_t(Treat_{ib})] + \gamma_{tb} + \lambda_i + \varepsilon_{ibt}, \quad (1)$$

where y_{ibt} is an outcome for individual *i* at relative time *t* and base year *b*. $Treat_{ib}$ is a binary variable taking the value of one if the individual was involuntarily displaced in base year *b* and relative time 0, and zero otherwise. The π_t coefficients trace out relative pre-treatment trends as well as time-varying treatment effects. The parameters of interest in Equation (1) are thus π_1 to π_4, which trace out the labor market effect of job loss across time. All estimates are relative to the year prior to job displacement.

Equation (1) also controls for base year by relative time (γ_{tb}) and individual (λ_i) fixed effects. The base year by relative time fixed effects control for systematic differences across time and base years that may be correlated both with displacement and the outcomes of interest. The individual fixed effects control for time-invariant differences in observed and unobserved characteristics across individuals that may be correlated with displacement and the outcomes of interest.[12] Standard errors are clustered at the individual level.

[12] Most papers in the displacement literature condition on set of base year plant, industry and region characteristics when they compare outcomes of displaced and non-displaced workers. The individual fixed effects we include in our

The main assumption underlying Equation (1) is that non-displaced individuals with a similar work history and of the same age, conditional on base year by relative time and individual fixed effects, represent an accurate counterfactual trend of displaced workers had they not been displaced. Further, we assume that there are no shocks concurrent with the displacement event that differentially affect individuals in the treatment group compared to individuals in the control group. The coefficients π_{-3} to π_0 in Equation (1) explicitly test for pre-treatment relative trends. If these estimates are economically small and statistically indistinguishable from zero, it suggests that there likely is no selection on fixed trends over time that bias our results. In addition, in Section 7 we perform a rich set of robustness checks and sensitivity analyses designed to examine the credibility of the job displacement design. Specifically, we study the sensitivity of our results to only using plant closures, to only focusing on very large firms, to accounting for early leavers, to relaxing the employment history requirement, and to performing propensity score matching on individuals in the pre-displacement period.

To summarize the large set of π coefficients from Equation (1), we complement the event study results with an overall effect obtained from a simplified difference-in-differences model:

$$y_{ibt} = \alpha + \beta_1 Post_{tb} + \beta_2 Treat_{ib} + \tau(Post_{tb} \times Treat_{ib}) + \gamma_{tb} + \lambda_i + \varepsilon_{ibt}, \qquad (2)$$

where *Post* is a dichotomous variable taking the value of one if the observation took place post relative time 0, and zero otherwise. All other variables are defined as before. The coefficient of interest is τ and measures the average impact of the displacement event aggregated over the first four post-displacement years. While the results from Equation (2) hides some of the time varying treatment effects identified through Equation (1), it facilitates comparison of effects across the life cycle and between subgroups.

4. Labor Market Effects of Job Displacement

Panels A and B of Figure 1 provide the full set of π_t estimates obtained from estimating Equation (1) for employment and labor earnings using all individuals aged 20 through 60. Several

analysis are more conservative as they account for all of these differences as well as any unobservable time-invariant differences between displaced and non-displaced workers.

observations are worth noting. First, treated and control workers are trending similarly in the three years leading up to the displacement event, indicating that there are no differential relative pre-treatment trends between individuals in the two groups that could bias our results. While this follows mechanically for employment due to the sample restrictions we impose, this is not the case for labor earnings.

Second, there is an immediate drop in employment following the displacement event, with an effect size of approximately 16 percentage points in the first post-displacement year. A similar effect can be seen with respect to labor earnings, with a reduction of slightly less than 5000 NOK in annual labor earnings in the first post-displacement year.

Third, the magnitude of the employment effect monotonically shrinks over time, and in the fourth post-displacement year the adverse employment effect is approximately 5 percentage points; one-third of the initial first year effect. With respect to labor earnings, the pattern is slightly different, showing a larger negative effect in the second post-displacement year than in the first. This is expected as the second post-displacement year is the first full year with post-displacement earnings for all workers. In addition, the wage effect does not appear to shrink during the first four post-displacement years.[13] Taken together, this figure is consistent with the existing job displacement literature, and confirms the existence of substantial earnings and employment effects both in the short-term as well as the long-term (e.g., Ruhm 1991; Jacobson *et al.* 1993; Huttunen *et al.* 2011; Davis and von Wachter 2011, Ichino *et al.* 2017).

Focusing on average effects among workers across the life cycle likely misses a great deal. In part because displacement may hurt individuals differently across their careers, but also because individuals may respond differently to shocks depending on when in their careers the shocks occur. To explore this possibility, Figure 2 provides the full set of π_t estimates obtained from estimating Equation (1) for employment stratified by 5-year age bins, and Figure 3 provides the full set of π_t estimates obtained from estimating Equation (1) for labor earnings stratified by 5-year age bins.

Figures 2 and 3 demonstrate that effects of job displacement are both more varied and more extensive than has been recognized, and that the timing of economic shocks across the life cycle matters for how they impact individual labor market outcomes. Specifically, the figures demonstrate that the adverse labor market impact of job displacement generally increases with age,

[13] This result is robust to using gross market income (which does not include taxable transfers); see Appendix Figure A1.

both in terms of immediate impact as well as persistence. For individuals below the age of 30, the immediate employment effect is approximately 13 percentage points, and this shrinks to around 3 percentage points four years after displacement. In contrast, for individuals in their late fifties, the effects are much larger: 18 percentage points in the first year and 10 percentage points four years later. With respect to earnings, individuals below the age of 30 experience a relatively modest initial impact, and four years after the displacement events they have recovered relatively well. In contrast, individuals above the age of 50 experience a much larger initial impact, and this effect is much more persistent across the four post-displacement years that we examine. The differential effect on employment and earnings across the life cycle is an important finding, implying that displacement hurt individuals differently across their career. Focusing on average effects among workers across the life cycle therefore misses a great deal.[14]

5. Adverse Labor Shocks and Key Life Choices

The differential labor market impact of job loss across the life cycle may partly be explained by individuals responding differently to shocks depending on when in their careers the shocks occur. In this section, we explore three key life decisions and their interaction with job displacement events: human capital investment, family structure, and permanent labor force exit. We also explore the main mechanisms through which these potential interactions occur. However, it is important to note that we do not examine all possible life decisions that may be impacted by adverse labor shocks; that is not the goal of the paper. Rather, we focus on these three key decisions and their interaction with economic shocks to illustrate how the timing of economic shocks can have substantially different impacts on individuals depending on when in their careers they occur.

5.1 Human Capital Investment

With respect to human capital investment, Figure 4 shows results obtained from estimating Equation (1) using school enrollment as the independent variable. The figure illustrates that young

[14] The pre-treatment trends are statistically significantly non-zero for two of the subgroups when it comes to earnings. However, these trends are very small and in the opposite direction to the effects we find. Thus, if anything, they contribute to a slight attenuation of the estimates among these subgroups. It should also be noted that this only applies to earnings, and not to any of the other outcomes we examine.

people are most likely to respond to involuntary displacement events by returning to school and investing in human capital, with an effect size of approximately 2.5 percentage points in the first few years after displacement. People in the middle of their careers (aged 30 through 40) are significantly less likely to respond by returning to school, with a point estimate of around 0.05 percentage points. Individuals between the ages of 40 and 50 display very small and not economically significant school effects of around 0.02, and the point estimates for individuals above the age of 50 are practically 0. Note that due to the timing of data collection, the education effects appear already in relative time zero.[15]

In terms of the existing literature, Minaya *et al.* (2020) find an increase in postsecondary enrollment following involuntary displacement events in the US. The effects they identify are small and only marginally economically meaningful, suggesting that less than 1 out of every 100 displaced individuals return to school following an involuntary layoff. However, while Minaya *et al.* (2020) provides important insights on average effects across a wide range of age groups, they have not taken the age-varying impact across the life cycle into account. The results from our analysis suggest that the human capital investment response is much greater among young individuals than previously suggested (almost four times larger than the effects identified in Minaya *et al.* (2020)) while it is not significant among older individuals.

That the effect of job displacement on human capital investment is monotonically declining with age is consistent with the conceptual framework outlined in Section 2: lifetime financial returns of human capital investment decrease as people age, and the non-financial costs of education are likely higher for older people. Provided that the return to education among the affected cohorts is positive, this is an important finding, suggesting that displacement among young individuals may lead to skill upgrading. If so, the very long-term effects of displacement among these people may be small or even positive. We study this in greater detail in Section 5.4 below.

To explore the human capital effects more closely, and to better understand the type of education the displaced individuals acquire, we estimate Equation (2) using enrollment in high

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[15] Specifically, individuals are defined as having lost their job if they had a job at the time the employment register was updated in relative time 0, but not at the time the employment register was updated in relative time 1. Thus, a large fraction of individuals subject to involuntary displacement in our sample lost their jobs in the calendar year that encompasses relative time 0, such that they may respond by returning to school already in that year.

school programs, basic university programs (undergraduate), and advanced university programs (postgraduate). We focus on individuals who were subject to involuntary displacements between age 20 and 35 as these are the individuals for whom we find overall enrollment effects. The results from this exercise are shown in Figure 5. The figure reveals that the majority of the human capital effect is driven by displaced workers enrolling in, and supplementing their education with, basic post-secondary education. The effect on high school enrollment is approximately 50 percent smaller, and the effect on postgraduate enrollment is not significantly different from zero. These results suggest that most of the human capital effect is driven by relatively well-educated individuals going back to school rather than by low-educated individuals going back to complete secondary school.

The decision to return to school in response to an involuntary displacement event likely depends on the expected costs and benefits of such human capital investments, as well as on any financial constraints that the individuals face. To better understand which individuals are induced to invest in human capital in response to a job displacement event, we perform two auxiliary analyses. In the first supplemental analysis, we consider heterogeneous effects across the ability distribution of individuals. The hypothesis underlying this exercise is that higher ability individuals have a lower cost of returning to school and learning, and may find it easier to get accepted into university programs. Thus, the cost of human capital investment may be lower for these individuals. To measure ability, we exploit individual performance on mandatory military conscription tests at age 18. Note that conscription did not become mandatory for women until 2014, and we therefore conduct this analysis exclusively for men.[16]

In the second supplemental analysis, we consider heterogeneous treatment effects across the wealth distribution. The hypothesis underlying this analysis is that financially constrained individuals may struggle to return to education in response to a job loss, while financially wealthy individuals may find it easier. We examine this hypothesis in two ways: (1) we link our data to the Norwegian wealth register and examine effects among individuals above and below the median wealth in the relevant age cohort, and (2) we link individuals to their parents and examine effects among individuals whose parents earned above or below median labor earnings when the

[16] This is similar to the AFQT test is the US. Looking at test scores based on the conscription test is particularly helpful for the older cohorts in our analysis as we do not have data on GPA or standardized school exams going this far back in time.

individuals were 18. We focus on individuals who were subject to involuntary displacements between age 20 and 35 as these are the individuals for whom we find overall enrollment effects.

Table 2 shows results from these auxiliary analyses. The results demonstrate that the human capital investment effects are three-times as large among the high ability individuals compared to the low ability individuals. Further, among the youngest individuals in the sample, relatively wealthier individuals, and individuals who grew up with high-income parents, are more likely to return to school compared to individuals of lower socioeconomic status. In terms of contextualizing these findings, it is worth noting that the cost of education in Norway is low from a global perspective (no tuition fees), and that the wealth heterogeneity most likely would be considerably larger in countries such as the US.[17]

Taken together, the heterogeneous effects by ability and wealth documented above are consistent with the conceptual framework outlined in Section 2: individuals who face lower costs of investing in human capital, and individuals who are less financially constrained, are more likely to return to school following an involuntary job loss event. The results highlight that even within age groups, there is substantial heterogeneity in response to job loss as a function of the costs and benefits associated with alternative life decisions. Provided that the return to education among the affected cohorts is positive, this has important policy implications, highlighting the role of the government in facilitating retraining and human capital investments among individuals subject to economic shocks.

5.2 Family Structure

Figures 6 and 7 show results obtained from estimating Equation (1) using fertility (number of children) and marital status (divorce). With respect to fertility, the results demonstrate that involuntary job loss generates a modest reduction in the number of children individuals have around the peak reproduction age in Norway, 30. The event studies suggest that this is a permanent reduction in fertility rather than a delay in the decision to have children; if it simply was a postponement of fertility decisions, we would most likely see a decrease in the first couple of years followed by a positive effect four years later. The point estimates are precisely estimated zeros for

[17] While there is no tuition fee, there are still several expenses associated with pursuing higher education, such as moving and forgoing earnings.

individuals above the age of 40 owing to the fact that very few individuals have children at these ages.[18]

While there are many reasons underlying individual fertility decisions, current economic research has mainly focused on two: household resources and future career concerns. First, the decision to have children is partly a function of current and future resources, and changes to household resources can therefore affect individual fertility decisions (e.g., Black *et al.* 2012; Lovenheim and Mumford 2013). Second, there are substantial career effects associated with fertility (e.g., Kleven *et al.* 2019), and career-driven individuals may therefore strategically plan the timing of fertility decisions to minimize its career impact (e.g., Huttunen and Kellokumpu 2016). As job displacement events affect both household resources and future career concerns, these are likely pathways through which the observed fertility effects operate.

To better understand which individuals are induced to reduce fertility in response to a job displacement event, we therefore perform two auxiliary analyses. First, we stratify the sample by median pre-displacement labor earnings to examine if those who experience relatively larger labor earnings drops are more likely to reduce fertility in the event of job loss. Second, we stratify the sample by the pre-displacement level of education to see if more educated individuals, who are more likely to be classified as career workers who make strategic fertility decisions, exhibit a stronger response on the fertility dimension. We focus on individuals who were subject to involuntary displacements between age 20 and 40 as these are the individuals for whom we find overall fertility and family formation effects.

The results from these exercises are shown in Panels A and B of Table 3. The results show that higher-income individuals who experienced a relatively larger labor earnings shock compared to lower-income individuals, exhibit a stronger fertility response. In addition, we find suggestive evidence that more highly educated individuals, with potentially greater future career concerns, respond more strongly than low-educated individuals. In addition to highlighting heterogeneity in job loss responses among individuals at the same phase of their careers, these results hint at potentially differential effects among children of the affected individuals. While this is beyond the scoop of the current paper, we explore this in detail in Salvanes, Willage and Willén (2021).

[18] Examining fertility decisions among individuals aged 50 and above may also be interpreted as a useful placebo test as these individuals – and in particular women – have exceeded the age at which they can have children. This helps reinforce the causal interpretation of our findings.

In terms of the existing literature, Huttunen and Kellokumpu (2016) find a relatively sizable effect of job loss on fertility, but only following female job loss events. The results we present show that the average effects that they identify hide substantial and important heterogeneity across age groups. As we will show in Section 6, stratification by sex and age at job loss also reveals interesting effects on fertility following male job loss, but at different ages than those relevant for females.

With respect to marital status, Figure 7 provides suggestive evidence of an increase in divorce rates following displacement among individuals at the very beginning of their careers, though the point estimates are small and only marginally statistically significant at the ten percent level. In terms of prior literature, papers such as Keldenich and Luecke (2020) as well as Eliason (2012) have found job loss to raise the risk of marital dissolution. While our results are generally in agreement with those findings, we also show that the relationship between job loss and divorce risk is driven by relatively young individuals at the beginning of their careers. This heterogeneity across the life cycle is consistent with the conceptual framework in Section 2, and of great importance for understanding how the timing of shocks can impact individuals differently depending on when they occur.

What can explain the heterogeneity in divorce effects across the life cycle? While there exists many reasons underlying couples' decisions to file for divorce, prior research suggests that individuals in newer and less stable relationship, as well as individuals in more financially constrained relationships, may be less equipped to deal with adverse shocks. Their probability to file for divorce in the event of unexpected shocks may therefore be higher.

To explore these mechanisms, we stratify the young analysis sample based on marriage duration in the pre-displacement year (more than 3 years versus less than 3 years), as well as based on whether the individual has above or below median wealth in the given age group. The results from these exercises are shown in Table 4. While we fail to identify any differential impact across the wealth distribution, we find large differences by marriage duration. Specifically, the effects load entirely on individuals in relatively new marriages, with a point estimate of 0.02 significant at the 5 percent level. The effect among individuals in more stable marriages is 0.01 and is not statistically significant. While we cannot reject equality of coefficients, these results are suggestive of the divorce effects being driven by individuals in relatively new marriages.

Taken together, the results from Figures 6 and 7 demonstrate that involuntary displacement disrupts both fertility decisions and family formation, but only among individuals at the beginning of their careers when marriages are relatively new and household resources are yet to be stabilized.

5.3 Permanent Exit From Labor Force

With respect to permanent exit from the labor force, Figure 8 shows results obtained from estimating Equation (1) using non-labor force participation as the outcome. The results from this exercise demonstrate that the probability of exiting the labor force in response to a displacement event monotonically increases with age, both with respect to the immediate impact as well as persistence. For individuals in their early twenties, the effect on labor market exist in the first post-displacement year is around 5 percentage points, and this shrinks to 2 percentage points in the fourth post-displacement year. In contrast, for an individual in the late fifties, the initial impact is around 17 percentage points, and this is reduced to 7 percentage points in the fourth post-displacement year.

Some of the differential effect on non-labor force participation across the life cycle may be driven by differences in mobility preferences and disability pension take-up. First, individuals at the end of their careers may be less willing to move in response to economic shocks, such that they are more likely to exit the labor force than to relocate in search for jobs. Second, research has shown that disability pensions are a common labor market exit route among older workers, lowering the cost of permanently exiting the labor force (Johnsen, Vaage and Willén 2021).

To explore these potential channels, Panels A and B of Figure 9 plot the results obtained from estimating Equation (2) using mobility and disability pension as outcomes. The results in Table 8 confirm the above hypotheses, demonstrating that the effect of moving across local labor markets in response to involuntary displacement declines with age, and that the effect of entering disability retirement in response to an involuntary displacement is monotonically increasing with age.[19] It should be noted that the effect on disability pension is consistent with prior work such as

[19] In terms of the disability pension (DP) effect, it is worth noting that the after-tax replacement rate depends on, and is decreasing in, an individual's pre-DP earnings (Johnsen, Vaage and Willén 2021). Thus, the DP option is more financially attractive to individuals who were previously employed in lower paying positions. To this end, we stratify the analysis sample by the highest completed level of education and re-estimate Equation (2). The results demonstrate that low-educated individuals who benefit relatively more from disability pension take-up are driving the DP effects (Figure 13).

Rege *et al.* (2009), and the effect on mobility is consistent with the work of Huttunen *et al.* (2018). However, the differential impact across the life cycle on both disability pension take-up and mobility has not been shown before, and adds important insights to our understanding of the relationship between economic shocks and key life decisions across the career.

In terms of better understanding the mobility effects and which individuals are more or less likely to move, prior research suggests that financially constrained individuals, and individuals with close family in the same geographic location, may be less mobile (e.g., Mulder and Malmberg 2014, Michielin *et al.* 2008, Dawkins 2006, Huttunen *et al.* 2018). In addition, existing research also suggests that high ability individuals, and highly educated individuals, are more mobile due to underlying job preferences. To explore this in greater detail, we perform supplemental analyses in which we stratify the sample based on whether the individual has above or below median wealth in the given age group, whether the individual has an adult child living in the same local labor market, by education level, and by ability level (proxied by score on army conscription test). Note that the stratification based on having an adult child living in the same LLM is restricted to those who lost the job between the age of 40 and 60 since very few people below these ages have adult children.

The results are shown in Appendix Figure A2. While wealth does not appear to differentially impact older individuals' post-displacement mobility decision, the presence of close family in the local labor market does. Specifically, the results suggest that older individuals are less willing to move if they have an adult child living in the same geographic area. With respect to education and ability, high skilled individuals are much more willing to move, though the difference in mobility effect across high- and low-ability individuals shrinks as individuals age.

Rather than moving across local labor markets in search for better job opportunities, individuals can simply choose to commute longer distances. Interestingly, estimating Equation (2) using cross-LLM commuting as the independent variable shows that older workers who are involuntarily displaced are no more likely to commute than older workers who are not displaced (Appendix Figure A3). Among young individuals, on the other hand, there is a sharp drop in commuting probability among displaced relative to non-displaced individuals (conditional on working). This is an interesting result, suggesting that young individuals decide to move across LLMs rather than commute across LLMs in search for better jobs following displacement events.

5.4 The Very Long-run Effects

While the relationship between economic shocks and key life decisions are of independent interest, the differential interactions between economic shocks and life decisions across the life cycle also mean that the very long-term effects of job displacement on individual labor market outcomes likely vary dramatically.

First, young individuals are more likely to respond to displacement events by investing in human capital and reallocating across local labor markets, such that the very long-run effect may be relatively small and could even be positive. Second, mid-career individuals are more likely to delay family formation and fertility decisions, which could reduce the very long-run impact of displacement on earnings due to smaller child penalty effects (e.g., Kleven *et al.* 2019). Third, individuals in their forties are more likely to exit the labor market and take up disability pension, and are less likely to move, such that the very long-run effect may be relatively big. Fourth, the oldest individuals in our sample are all likely to have entered retirement a few years after the displacement event, such that the difference in labor earnings between treatment and control individuals likely is near zero.

To examine this question in detail, Figure 10 provides results obtained from estimating the effect of displacement on labor market outcomes measured 15 years after the event took place. As this outcome is measured only once for each individual, we provide results based on Equation (2). Looking across the figure, several interesting observations are worth highlighting.

First, individuals in their early 20s display small but positive labor earnings effects 15 years after the displacement event. This is consistent with skill upgrading and a positive return to their human capital investment response, and in line with the positive mobility response documented in Figure 8. This is a novel finding that has not been documented before. Second, individuals in their late fifties display very modest and not economically meaningful effects 15 years after the displacement event. This is expected as the majority of individuals in this age group has exited the labor force and entered retirement by the end of the 15 year post-period, regardless of displacement events. Third, abstracting away from the tails of the life cycle career age, all individuals experience persistent adverse labor earnings effects that extend 15 years after the displacement event took place. To the best of our knowledge, this is the first paper in the literature to document such long-run persistence of the effects associated with involuntary displacement. The adverse effects are largest among individuals in their late 40s. This is consistent with these workers being more likely to exit the labor market, less likely to invest in human capital, and less likely to relocate to another labor market (but not more likely to take up disability pension) in response to an economic shock.[20]

To better understand the positive long-run earnings effects among displaced individuals aged 20 through 29, we perform an exploratory exercise in which we stratify the sample based on whether the displaced individuals returned to school or not, and whether they decided to reallocate to a new local labor market or not, in response to the displacement event. It is important to note that the decision to invest in human capital and reallocate across local labor markets in response to job displacement is endogenous. Nevertheless, we believe that these are useful exercises for understanding whether it is the individuals who return to school and move that drive the positive long-run effects. The results from this set of exercises are shown in Appendix Table A1. The results demonstrate that the positive long-run effects among the young individuals are driven exclusively by those who return to school and move across local labor markets in response to job displacement. While this is consistent with a positive return to education, and to moving, it could also be that the individual sorting into education and moving are better able to recover from job loss events.

To explore if the differential very long-run labor market impacts translate into severe differences in wellbeing and health, we have also examined mortality effects 15 years after the displacement event. The results are provided in Appendix Figure A4, and show economically negligible and not statistically significant effects. This result is interesting in light of Sullivan and Von Wachter (2009), who find a 10 percent increase in annual mortality hazard 10-20 years after displacement events. However, they examine displacements that took place in the US in the 70s and 80s, and it is likely that the social protection provided by the Norwegian welfare state mutes these effects. Nevertheless, it should be noted that the lack of mortality effects does not preclude the possibility that there are differential long-term impacts on health and wellbeing; only that such differential effects do not cause mortality rates to differ. This is perhaps expected, especially in a country like Norway where free health care and relatively low levels of inequality ensure easy access to health care services.

[20] As labor earnings naturally varies over the lifecycle, it may also be interesting to examine these long-run effects in terms of relative losses (with respect to the pre-displacement year) rather than in terms of absolute losses. However, the pattern displayed in Table 10 remains unaffected following this adjustment. The full set of results from this supplemental analysis is available from the authors upon request.

6. Heterogeneity by Sex and Education

Do interactions between economic shocks and key life decisions differ across sex and education levels? Existing research has shown that males and females face disparate career patterns and trajectories partly due to factors such as family formation, educational investment, and retirement (e.g., Kleven *et al.* 2019; Manning and Swaffield 2008). Prior research has also shown that the impact of economic shocks differs depending on the education level of the individuals (e.g., Farber 2003; Dodini *et al.* 2021). In this section, we examine if such differences also exist with respect to interactions between economic shocks and key life decisions across the life cycle.

6.1 Heterogeneity by Sex

Figure 11 provides difference-in-differences estimates obtained from estimating Equation (2) separately by sex and age group for each of the outcomes discussed in Section 5. The figure reveals important effect heterogeneity across sex with respect to the main labor market outcomes. Specifically, while the employment effect is relatively similar across sex (Panel A), the labor earnings effect is considerably different (Panel B): While the effect on labor earnings is generally constant across the life cycle among females, it declines dramatically over the life cycle for males.

The differential labor market impact of economic shocks across sexes is likely driven by differences in the interaction between economic shocks and key life decisions between males and females. Specifically, the figure shows that males are much more likely to respond to economic shocks by returning to education at a young age (Panel C), are much more likely to relocate to a different labor market at a young age (Panel G), and are more likely to collect disability pension at an older age (Panel H). Finally, the fertility effects among males are more substantial at the beginning of their careers (Panel D), while the impact on females is more concentrated around primary childbearing age (around 30 years in Norway).

Due to the differential interaction between life decisions and economic shocks among males and females, the very long-run impact on individual earnings across sexes – depending on when during the life cycle the shock occurs – may also be substantial. To this end, Figure 12 provides point estimates obtained from estimating the effect of displacement on earnings 15 years after the event took place, separately by sex. Looking across the figure, the results indicate that males are much more likely to recover from, and overcome, the involuntary displacement events that take place at an early age. At the same time, the figure also illustrates that males who are exposed to negative shocks later in their careers are likely to suffer larger long-run labor earnings effects. This is consistent with the larger interaction effects between job loss and life decisions among men than among women. These results highlight the importance of not only accounting for when in the career individuals are exposed to shocks, but also how the impact of the timing of shocks differ across males and females.

6.2 Heterogeneity by Education Level

With respect to effect heterogeneity across education levels, Figure 13 provides difference-in-differences estimates obtained from estimating Equation (2) separately by education level for each of the outcomes discussed in Section 5. As discussed in Section 3, we focus on three education levels to facilitate the interpretation of our results: less than high school, high school, and at least some college.

Looking across the various panels in Figure 13, the effects are relatively similar across the three education levels both with respect to the main labor market outcomes (Panels A and B) as well as the life decisions (Panels C through H). However, there are some noticeable differences. Specifically, the figure shows that high educated individuals are slightly more likely to relocate to a different labor market at a very young age (Panel G), are less likely to collect disability pension at an older age (Panel F), and have noticeably larger fertility effects at a young age (Panel D). However, the standard errors are very large among the young high educated individuals, and it is therefore not possible to reject equality of coefficients across the education levels. The large standard errors among the young high educated individuals is expected, as few individuals have managed to complete higher education and meet the sample restriction related to work history in Section 3 in their early twenties.

As a consequence of the implied differences in the interactions between life decisions and economic shocks among individuals of different educational levels, the very long-run impact on individual earnings across education levels – depending on when during the life cycle the shock occurs – may be different. We examine this in detail in Figure 14, where we provide point estimates obtained from estimating the effect of displacement on earnings 15 years after the event took place, separately by education level.

Figure 14 demonstrates that the very long-run earnings effects are relatively similar across the three education levels. The exception to this relates to the very young individuals. Specifically, among individuals early in their careers, high-educated individuals appear more likely to recover from, and overcome, the involuntary displacement events.

7. Robustness and Sensitivity

The established job loss literature has developed a rich set of sensitivity checks and robustness analyses designed to examine the credibility of the job loss design (e.g., Huttunen *et al.* 2011; Del Bono *et al.* 2012; Huttunen *et al.* 2018; Willage and Willén 2020). In this section, we implement these exercises to ensure that our results are not biased, not driven by spurious correlations, and not caused by endogenous selection into establishments that are closing down or downsizing. In addition, we also present results from out alternative identification strategy that relies exclusively on the difference in the timing of treatment. Finally, it is worth noting that all specifications in this paper include individual fixed effects to account for all time-invariant systematic differences across individuals.

Most of the results from the robustness and sensitivity analyses are displayed in Table 5. Due to space constraints, we focus on the main labor market outcomes, but the robustness of our results extends to all other variables as well. In Panel A, we provide our baseline estimates to facilitate comparison across the various exercises.

In Panels B though D we examine the sensitivity of our results to restricting the sample to larger firms (sequentially restricting our sample to establishments with more than 30, 40, and 50 employees). Examining robustness to larger firms is important as it reduces the risk of false mass layoffs and establishment closures. The estimates are unaffected by these additional sample restrictions.

In Panel E, we explore the robustness of our results to clustering our standard errors at the more conservative municipality level, in which we allow the error component of Equation (1) to be correlated among individuals within the same municipality. This adjustment has no impact on the statistical significance of our estimates.

In Panel F, we perform propensity score matching on individuals in the pre-displacement period. The rationale underlying this exercise is that we would like to obtain a treatment and control group that are as comparable as possible, in order to ensure a meaningful interpretation of the results. By restricting the sample to the common support of the propensity score matching method (in which we regress the probability of treatment as a function of baseline marriage status, fertility history, sex, market income, and age), we avoid the risk of the estimates being driven by control and treatment units that are very different from one another and have little overlap in terms of background characteristics. The results from this exercise demonstrate that our estimates are not statistically significantly different if we restrict the sample to those in the common support region of the PSM, suggesting that the main effects are not identified off of control and treatment units that are very different from one another on observable dimensions.

In Panel G, we adjust our sample by assigning individuals that leave the plant one year before the closure/layoff, potentially in anticipation of the event, to the treatment group. The idea behind this exercise is that “early leavers” may be positively selected, and failing to include them in the treatment group would bias our estimates. The results in Panel G reveal that assigning early leavers to the treatment group does not change the results.

In Panels H and I, we show that the results are unaffected by relaxing the requirement that individuals must have been full-time employed in the three years leading up to the base year. Our decision to focus on highly-attached workers in the main analysis is based on the prior job loss literature in which this sample restriction is imposed to ensure comparability of the treatment and control groups. However, the drawback of this restriction is that it may lead us to estimate a very specific local average treatment effect. The results in Panels H and I demonstrate that relaxing this restriction has no impact on our estimates, suggesting that similar effects can be observed among less-attached workers as well.

In Panel J, we examine the sensitivity of our results to focusing exclusively on establishment closures. This is an interesting exercise, as establishment closures are arguably more plausibly exogenous than mass layoffs. The results are slightly bigger when focusing exclusively on establishment closures and not mass layoffs. This suggests that our main results are not driven by endogenous selection generated by the mass layoff events.

In Panel K, further assess the credibility of our main estimates, we supplement our main analysis method with an alternative identification strategy that relies exclusively on the difference in the timing of treatment.[21] The results demonstrate that our main estimates are robust to only including workers that experience plant closure or mass layoff at some point in the future in our control group. This exercise, which relies exclusively on the difference in the timing of treatment, provides strong protection against the concern that individuals may be endogenously selected into the treatment sample, and helps reinforce the causal interpretation of our findings.

In addition to the results in Table 5, Figure 15 show the sensitivity of our results to eliminating individuals in specific industries and base years. The figure demonstrates that our main results are robust to dropping specific industries and base years, suggesting that the results are not driven exclusively by specific sectors or time periods.

Finally, in the same spirit of studying the credibility of the parallel trend assumption required for causal inference, Figure 16 provides results from event studies that extent the pre-treatment period from 3 to 5 years for employment. The rationale for performing this exercise is that the pre-trends in the main event study for this outcomes are mechanically flat due to the sample restrictions on employment history. By extending these trends backwards into years in which we impose no restrictions, we are better able to assess the comparability of our treatment and control groups over time. Looking across the various panels in Figure 16, we can conclude that there is no observable evidence on differential pre-treatment trends across the control and treatment groups that could bias our results.

Taken together, this extensive set of robustness checks and sensitivity analyses provides strong evidence in favor of a causal interpretation of our results.

8. Conclusion

Economic shocks alter the costs and benefits of key life decisions. Individuals who are the margin of deciding whether to return to school, move to a city with better career prospects, have children, or enter retirement, may therefore be induced to do so when exposed to an adverse labor shock. The costs and benefits of these major life decisions vary considerably over an individual's life,

[21] Specifically, we follow Fadlon and Nielson (2019) and deviate from the conventional job loss literature assumption that any worker who satisfy the employment history conditions should be in the control group. Instead, we include only workers in firms where a plant closure or mass layoff occurs at some point in the future. We also restrict the sample to before relative time 2, so that the control group has not yet experience a plant closure or mass layoff.

and economic shocks will therefore likely have substantially different impacts on individuals depending on when the shocks occur. Because of differential interactions between economic shocks and key life decisions across the life cycle, the very long-term career implications will likely also vary considerably depending on the timing of the shock.

This paper exploits plausibly exogenous job loss from mass layoffs and establishment closures to trace the impact of adverse shocks across the life cycle and examine how they affect major life decisions: human capital investment, family structure, and retirement. We then follow individuals up to 15 years after the shock took place to provide novel insights on the aggregate reduced-form impact of all effects on the very long-term career outcomes of individuals.

We provide four sets of key results. First, consistent with existing literature, we document substantial earnings and employment effects associated with job loss both in the short-term as well as the long-term. However, in contrast to prior literature, we reveal that the employment and wage effects vary greatly across the life cycle.

Second, we reveal substantial heterogeneity in how individuals respond to involuntary displacement events across the life cycle. Individuals at the beginning of their careers respond by relocating to other local labor markets and investing in human capital, individuals in the middle of their careers respond by reducing fertility and being more likely to end their marriages, and individuals at the end of their careers respond by permanently exiting the workforce and collecting disability pension.

Third, we provide strong suggestive evidence of the mechanisms through which the differential impact between economic shocks and key life decisions occurs across the life cycle: the human capital effects among young workers are driven by high-ability and high-wealth individuals who likely face lower costs of returning to school and have few financial constraints preventing them from doing so; the fertility effects are driven by high-income and high-educated individuals who experience relatively larger labor earnings drops and potentially face greater future career concerns; the family formation effects are driven by individuals in newer and less stable relationship who may be less equipped to deal with adverse shocks; and the differential effect on non-labor force participation are primarily driven by differences in mobility preferences and disability pension take-up across the life cycle.

Fourth, as a consequence of the differential interactions between economic shocks and life decisions across the life cycle, we find that the very long-term effect on individual earnings also varies dramatically: individuals in their early twenties display small but positive labor earnings effects 15 years after the displacement event, individuals in their late fifties display very modest and not economically meaningful effects 15 years after the displacement event, and individuals between these two age groups experience persistent adverse labor earnings effects that extend 15 years after the displacement event. The positive long-run earnings effects among young displaced workers are exclusively driven by those who returned to school, and who relocate to new local labor markets, in response to the displacement event.

To the best of our knowledge, this is the first paper in the literature to trace the impact of adverse labor shocks across the life cycle and examine how they impact major life decisions: human capital investment, family structure, and retirement. It is also one of the first paper in the literature to explore the very long-run career effects of job displacement, examining the impact as long as 15 years after the event took place. While the relationship between economic shocks and key life decisions are of independent interest, it is also of great value to understand the aggregate effect on labor market outcomes in the very long-run.

In terms of policy implications, our results show that focusing on average effects among workers across the life cycle misses a great deal. This study highlights that economic shocks have substantially different effects depending on when in an individual's career they occur, and emphasize the importance of establishing more flexible employment protection and support policies that account for the age-varying nature of economic shocks. We conclude that effects of job displacement are both more varied and more extensive than has been recognized.

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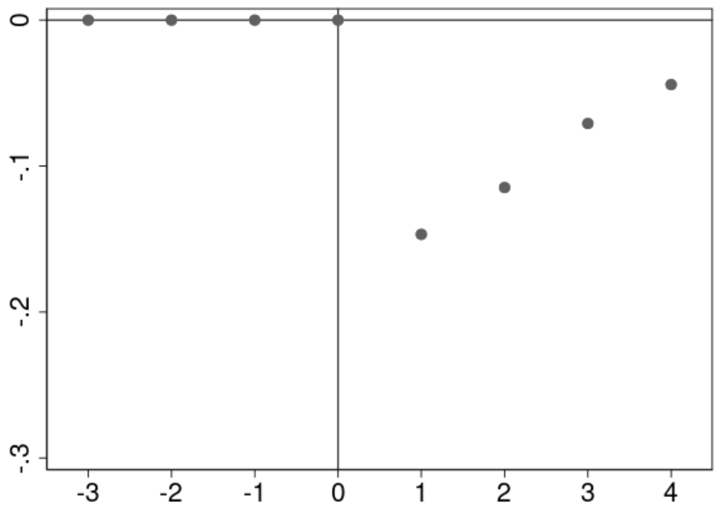
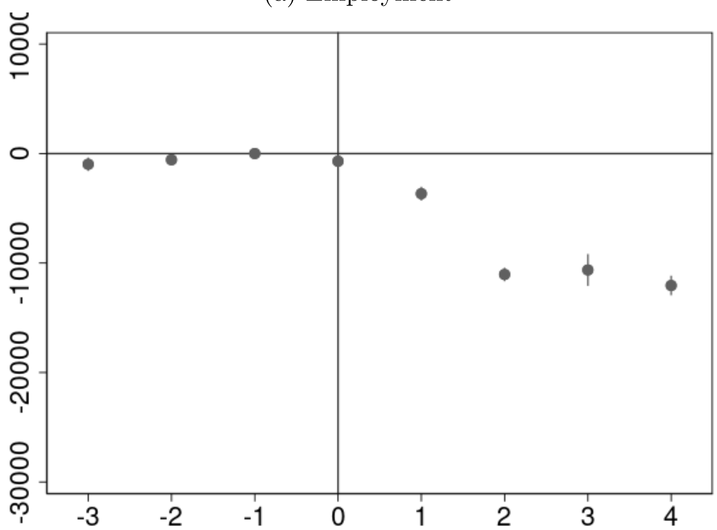


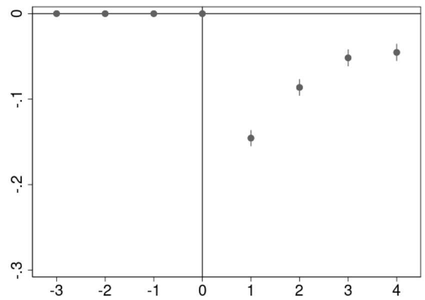
(a) Employment



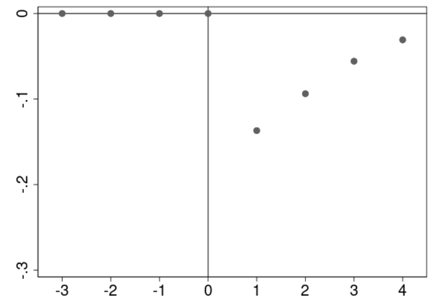
(b) Labor earnings

Figure 1: Pooled sample of men and women aged 20 through 60

Notes: Authors estimation using population-wide register data from Statistics Norway. Dots are point estimates, lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation:
$y_{ibt} = \alpha + \sum_{t=-3}^{4}[\pi_t(Treat_{ib})] + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$,
where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, π_t is the time-varying treatment effect in year t, (π_{-1} omitted so all estimates are relative to $t = -1$). Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Labor earnings is measured in Norwegian kroner (000s). Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.



(a) Aged 20 through 24



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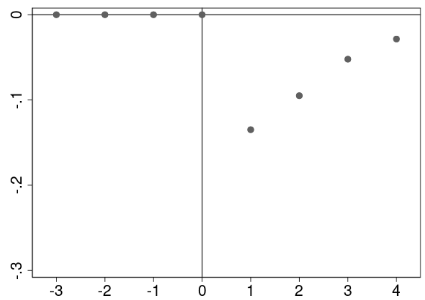
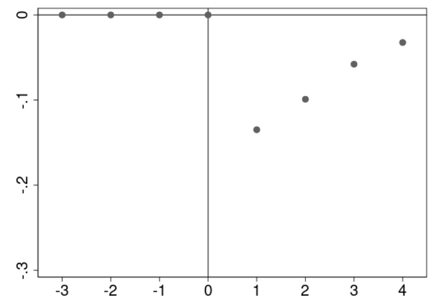
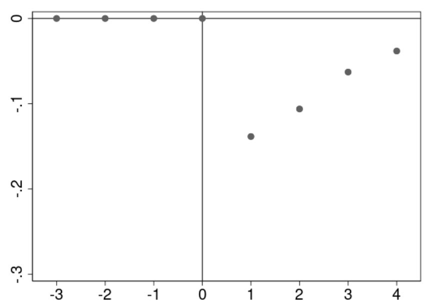


(c) Aged 30 through 34



(d) Aged 35 through 39



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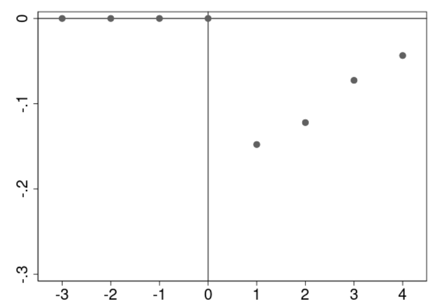
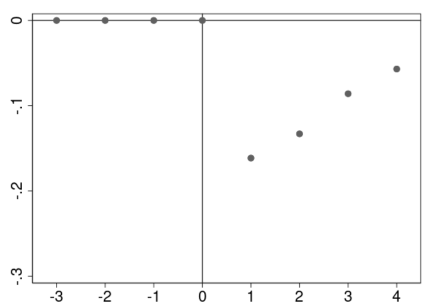
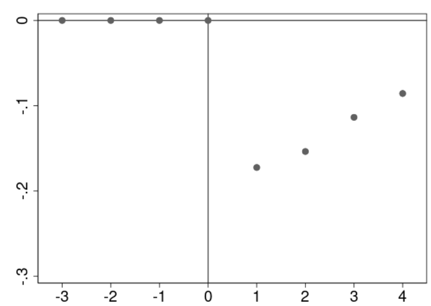


(f) Aged 45 through 49



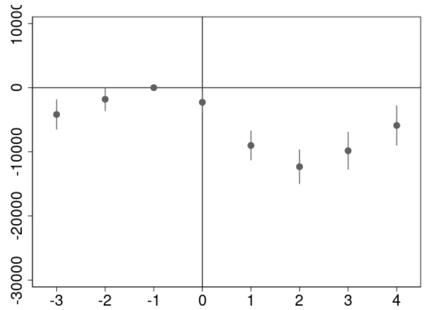
(g) Aged 50 through 54



(h) Aged 55 through 59

Figure 2: Pooled sample of men and women by age of job loss: Employed

Notes: Authors estimation using population-wide register data from Statistics Norway. Each graph represents estimates for a different age group. Dots are point estimates, lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation: $y_{ibt} = \alpha + \sum_{t=-3}^{4}[\pi_t(Treat_{ib})] + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$, where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, π_t is the time-varying treatment effect in year t, (π_{-1} omitted so all estimates are relative to $t = -1$). Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.



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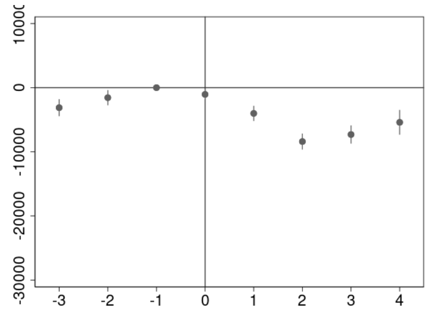

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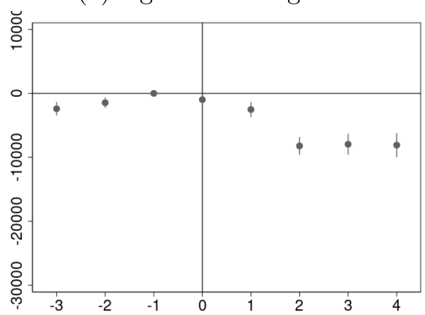

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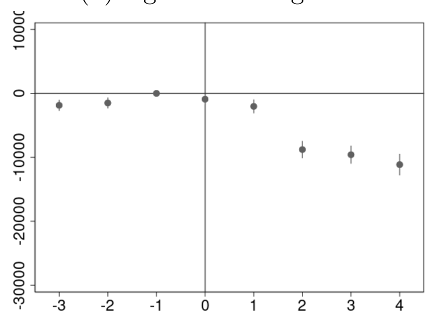

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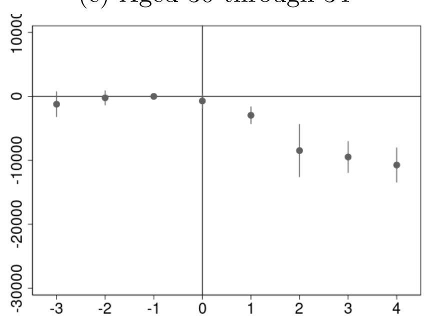

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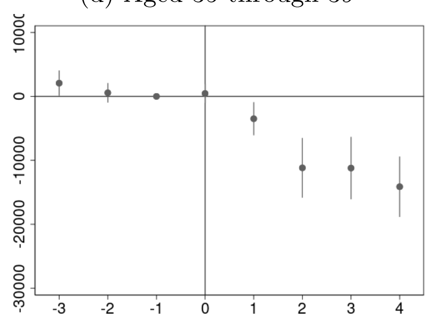

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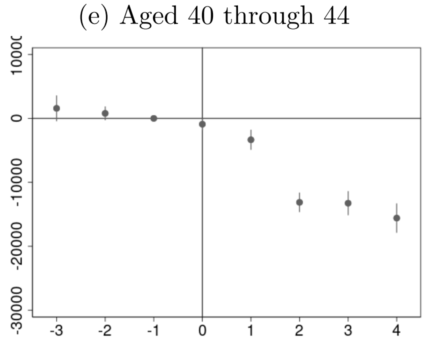

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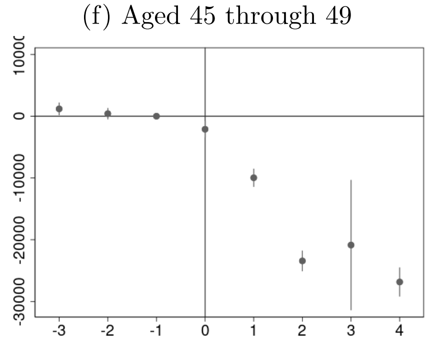
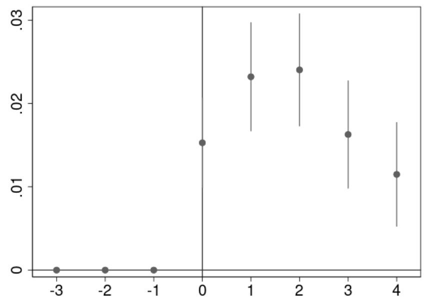


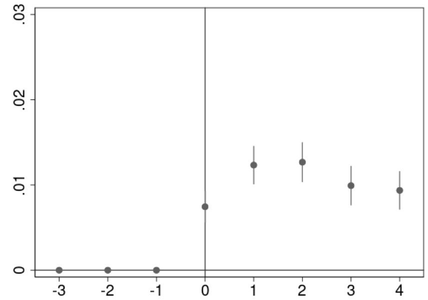
(h) Aged 55 through 59

Figure 3: Pooled sample of men and women by age of job loss: Labor earnings

Notes: Authors estimation using population-wide register data from Statistics Norway. Each graph represents estimates for a different age group. Dots are point estimates, lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation: $y_{ibt} = \alpha + \sum_{t=-3}^{4}[\pi_t(Treat_{ib})] + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$, where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, π_t is the time-varying treatment effect in year t, (π_{-1} omitted so all estimates are relative to $t = -1$). Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Labor earnings is measured in Norwegian kroner (000s). Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.



(a) Aged 20 through 24



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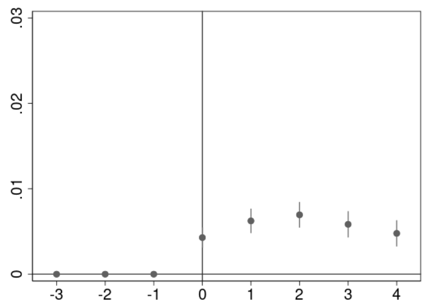

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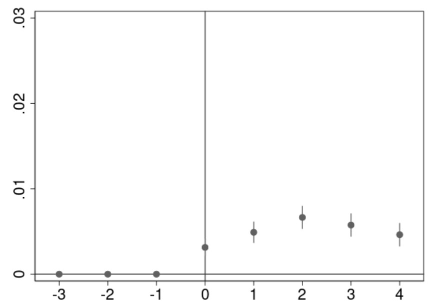

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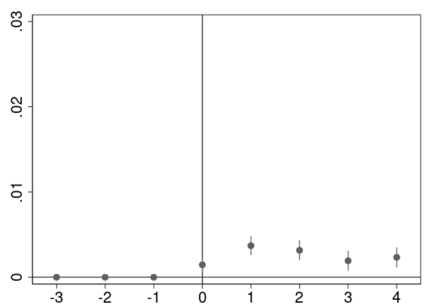

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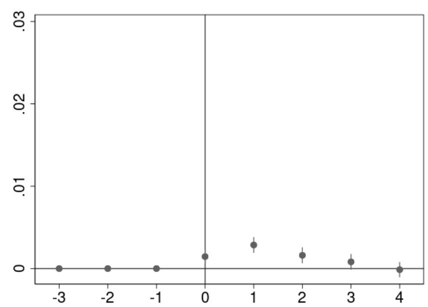
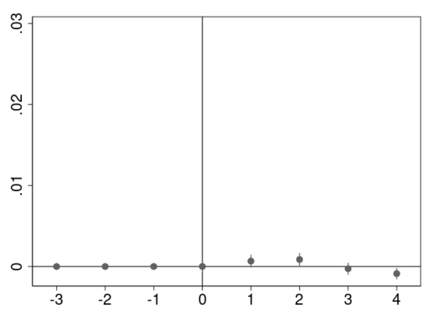
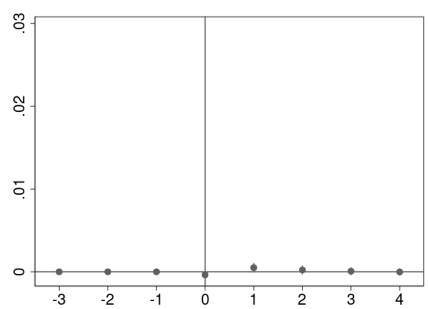


(f) Aged 45 through 49



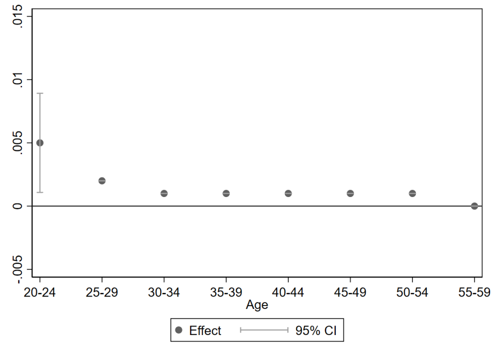
(g) Aged 50 through 54



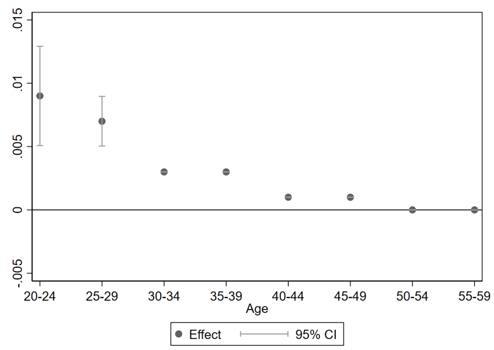
(h) Aged 55 through 59

Figure 4: Pooled sample of men and women by age of job loss: School enrollment

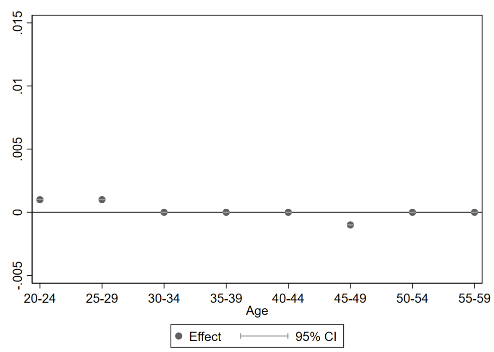
Notes: Authors estimation using population-wide register data from Statistics Norway. Each graph represents estimates for a different age group. Dots are point estimates, lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation: $y_{ibt} = \alpha + \sum_{t=-3}^{4}[\pi_t(Treat_{ib})] + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$, where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, π_t is the time-varying treatment effect in year t, (π_{-1} omitted so all estimates are relative to $t = -1$). Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.



(a) High School



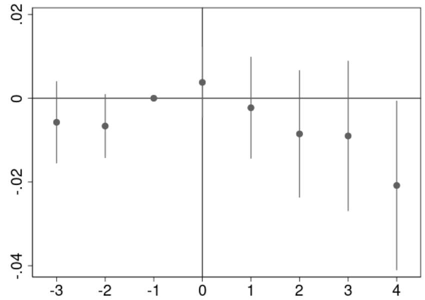
(b) Basic University (Bachelor)



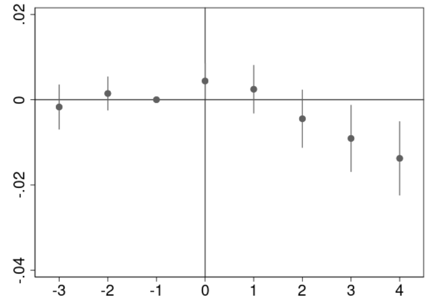
(c) Advanced University (Master+)

Figure 5: Pooled sample of men and women by age of job loss: Detailed school enrollment

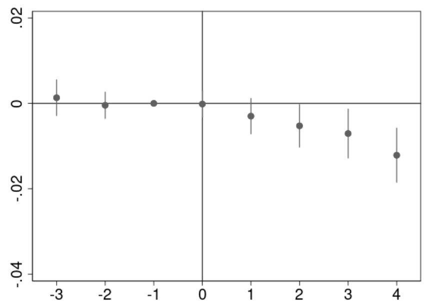
Notes: Authors estimation using population-wide register data from Statistics Norway. Each graph represents estimates for a different education level outcome. Dots are point estimates, lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation:
$y_{ibt} = \alpha + \beta_i Post_{tb} + \beta_2 Treat_{ib} + \rho(Post_{tb} x Treat_{ib}) + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$,
where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, $Post_{tb}$ is a dichotomous variable taking the value of one if the observation took place post relative time 0, and ρ is the treatment effect of interest. Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.



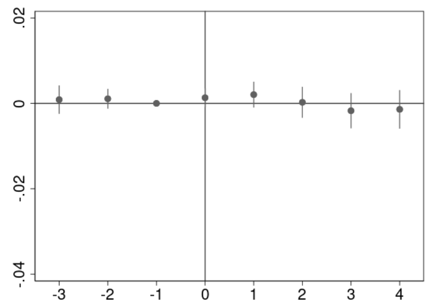
(a) Aged 20 through 24



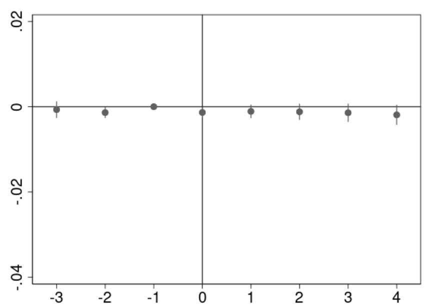
(b) Aged 25 through 29



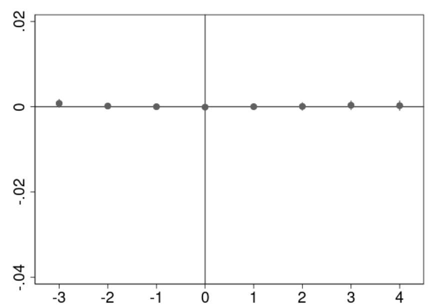
(c) Aged 30 through 34



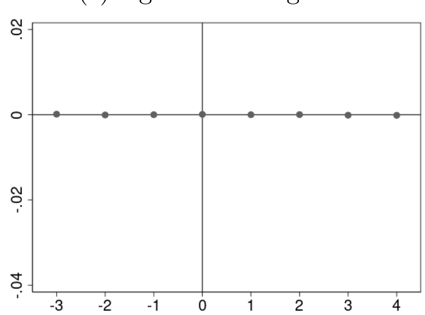
(d) Aged 35 through 39



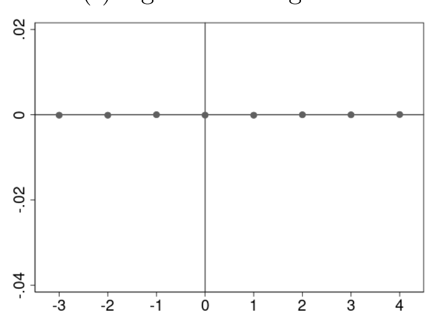
(e) Aged 40 through 44



(f) Aged 45 through 49



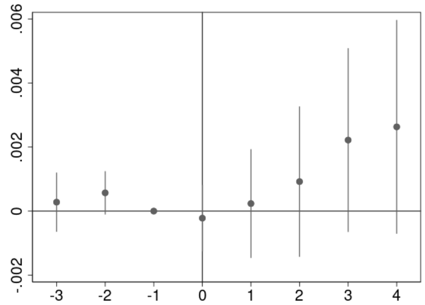
(g) Aged 50 through 54



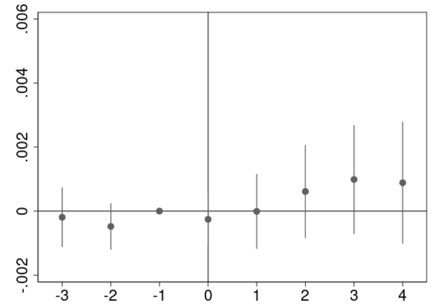
(h) Aged 55 through 59

Figure 6: Pooled sample of men and women by age of job loss: Fertility

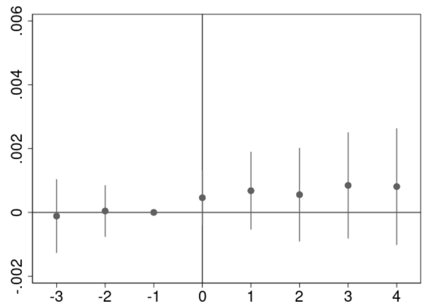
Notes: Authors estimation using population-wide register data from Statistics Norway. Each graph represents estimates for a different age group. Dots are point estimates, lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation: $y_{ibt} = \alpha + \sum_{t=-3}^{4}[\pi_t(Treat_{ib})] + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$, where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, π_t is the time-varying treatment effect in year t, (π_{-1} omitted so all estimates are relative to $t = -1$). Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.



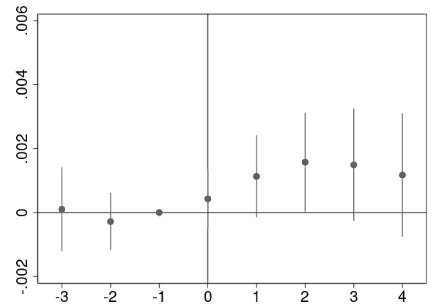
(a) Aged 20 through 24



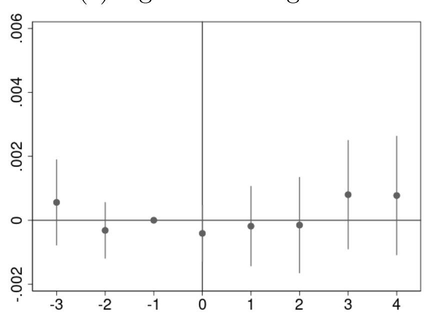
(b) Aged 25 through 29



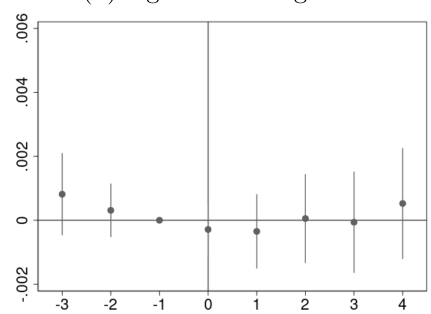
(c) Aged 30 through 34



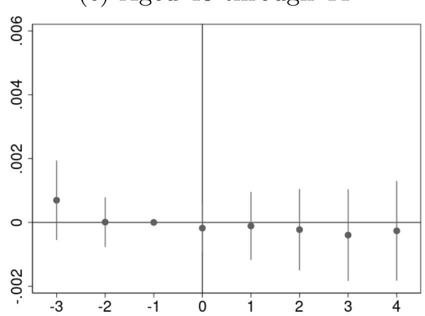
(d) Aged 35 through 39



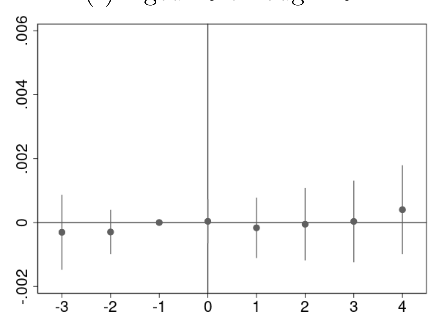
(e) Aged 40 through 44



(f) Aged 45 through 49



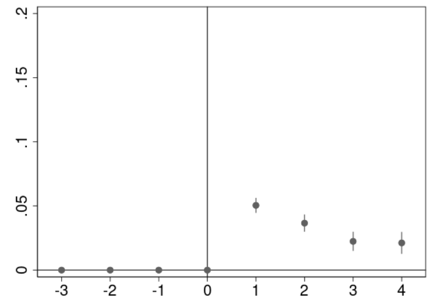
(g) Aged 50 through 54



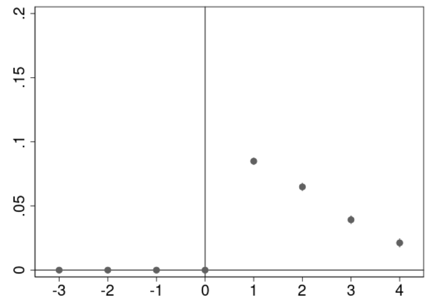
(h) Aged 55 through 59

Figure 7: Pooled sample of men and women by age of job loss: Divorced

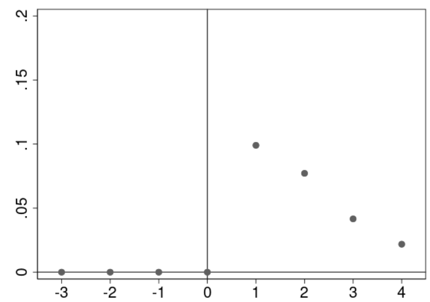
Notes: Authors estimation using population-wide register data from Statistics Norway. Each graph represents estimates for a different age group. Dots are point estimates, lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation: $y_{ibt} = \alpha + \sum_{t=-3}^{4}[\pi_t(Treat_{ib})] + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$, where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, π_t is the time-varying treatment effect in year t, (π_{-1} omitted so all estimates are relative to $t = -1$). Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.



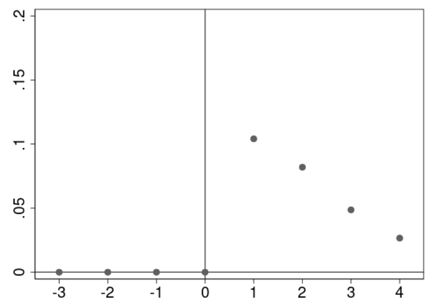
(a) Aged 20 through 24



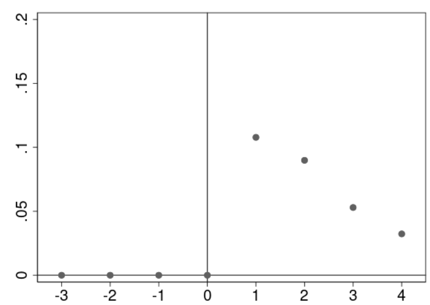
(b) Aged 25 through 29



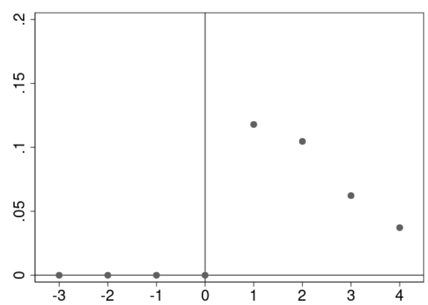
(c) Aged 30 through 34



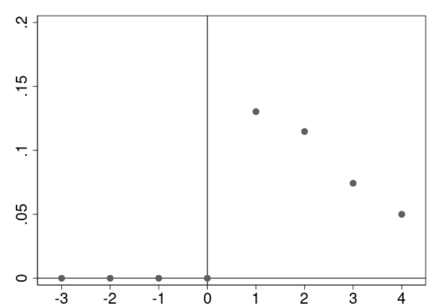
(d) Aged 35 through 39



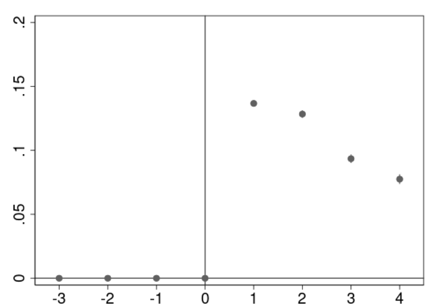
(e) Aged 40 through 44



(f) Aged 45 through 49



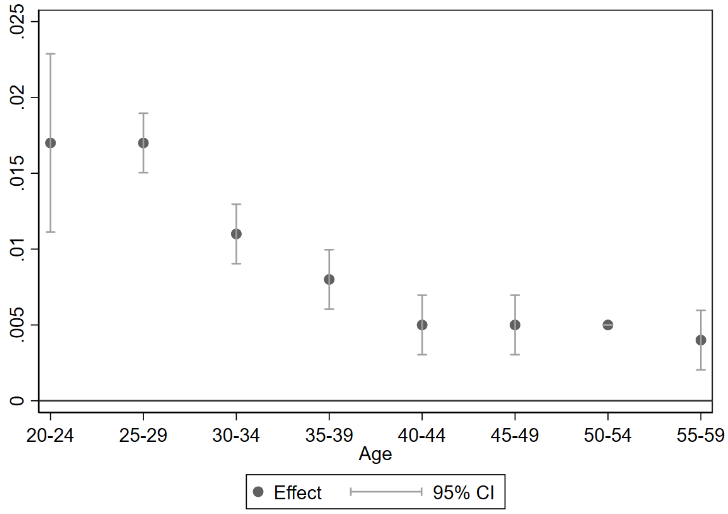
(g) Aged 50 through 54



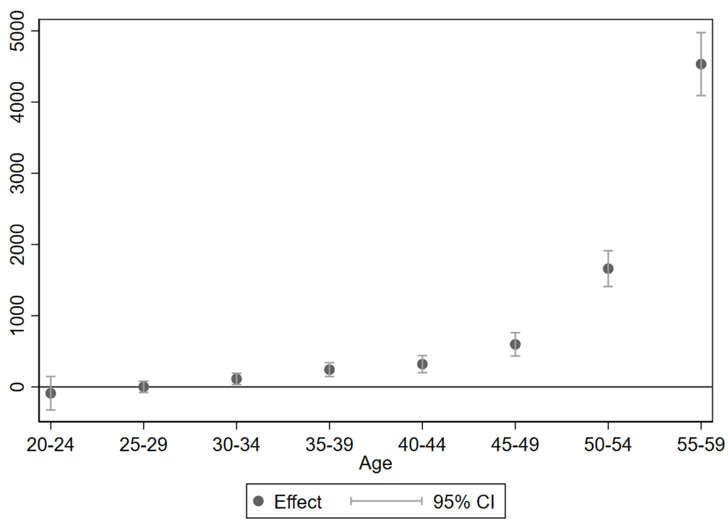
(h) Aged 55 through 59

Figure 8: Pooled sample of men and women by age of job loss: Labor market exit

Notes: Authors estimation using population-wide register data from Statistics Norway. Each graph represents estimates for a different age group. Dots are point estimates, lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation: $y_{ibt} = \alpha + \sum_{t=-3}^{4}[\pi_t(Treat_{ib})] + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$, where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, π_t is the time-varying treatment effect in year t, (π_{-1} omitted so all estimates are relative to $t = -1$). Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Labor market exit is measured by neither employed nor collecting unemployment benefits. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.



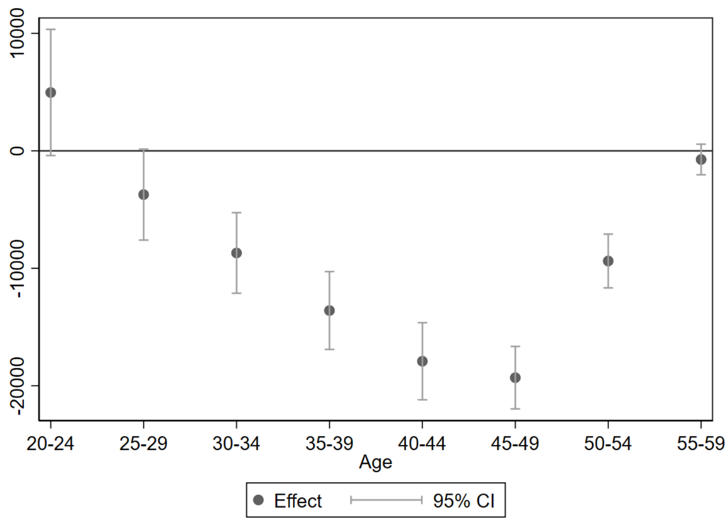
(a) Moving across local labor market



(b) Disability pension

Figure 9: Pooled sample of men and women by age of job loss: Mobility and Disability Pension

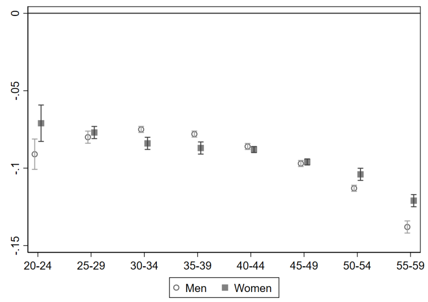
Notes: Authors estimation using population-wide register data from Statistics Norway. Each graph represents estimates for a different outcome. Dots are point estimates, lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation:
$y_{ibt} = \alpha + \beta_i Post_{tb} + \beta_2 Treat_{ib} + \rho(Post_{tb} x Treat_{ib}) + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$,
where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, $Post_{tb}$ is a dichotomous variable taking the value of one if the observation took place post relative time 0, and ρ is the treatment effect of interest. Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Mobility is measured as moving to a different local labor market region compared to relative time $t = -1$. Disability pensions benefits measured in Norwegian kroner. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.



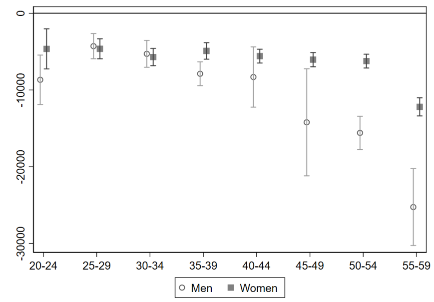
(a) Labor earnings 15 years post base year

Figure 10: Pooled sample of men and women by age of job loss: Earnings 15 years after shock

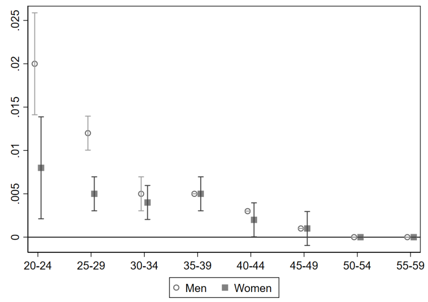
Notes: Authors estimation using population-wide register data from Statistics Norway. Dots are point estimates, lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation:
$y_{ibt} = \alpha + +\beta_1 Treat_{ib} + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$,
where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, and β_1 is the treatment effect of interest. Controls include individual (λ_i) fixed effects. Earnings measured in Norwegian kroner. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.



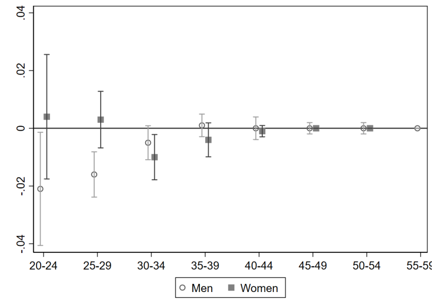
(a) Employed



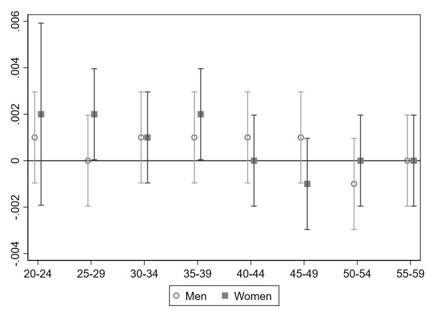
(b) Labor earnings



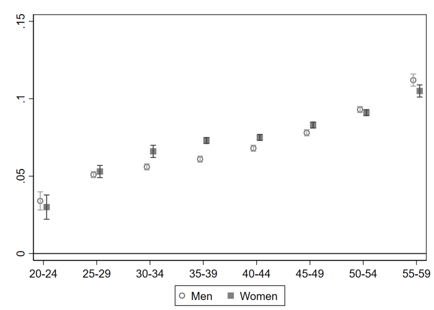
(c) School enrollment



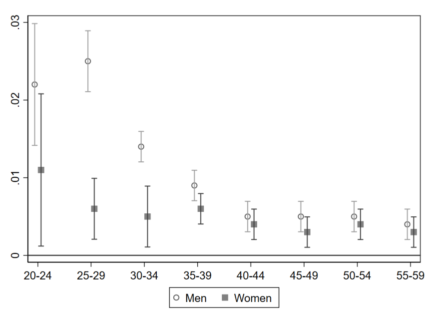
(d) Fertility



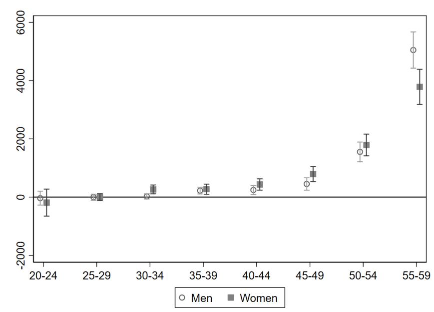
(e) Divorce



(f) Labor market exit



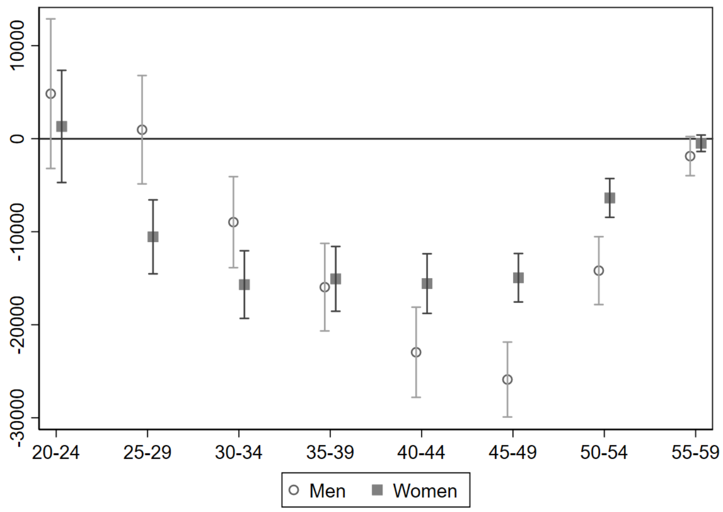
(g) Mobility



(h) Disability Pension

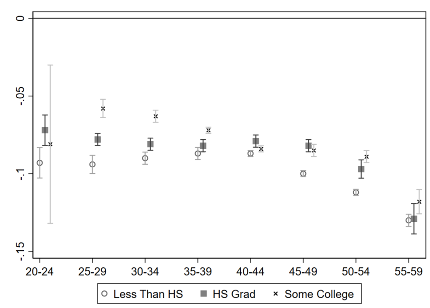
Figure 11: Sample stratified by gender and age of job loss

Notes: Authors estimation using population-wide register data from Statistics Norway. Each graph represents estimates for a different outcome. Hollow circles are point estimates for men, solid squares are point estimates for women, and lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation:
$y_{ibt} = \alpha + \beta_i Post_{tb} + \beta_2 Treat_{ib} + \rho(Post_{tb} x Treat_{ib}) + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$,
where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, $Post_{tb}$ is a dichotomous variable taking the value of one if the observation took place post relative time 0, and ρ is the treatment effect of interest. Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Mobility is measured as moving to a different local labor market region compared to relative time $t = -1$. Disability pensions benefits measured in Norwegian kroner. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.

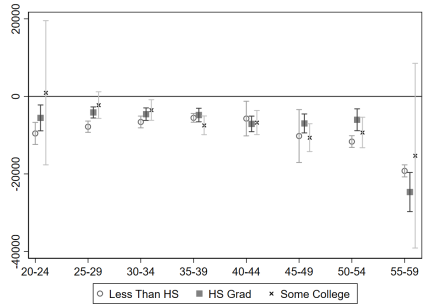


(a) Labor earnings 15 years post base year

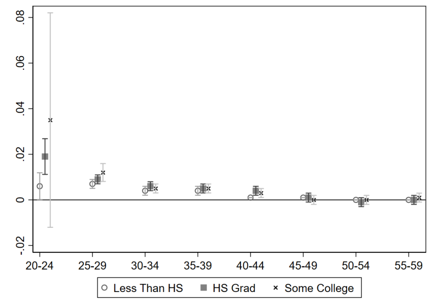
Figure 12: Stratified sample of men and women by age of job loss: Earnings 15 years after shock
Notes: Authors estimation using population-wide register data from Statistics Norway. Hollow circles are point estimates for men, solid squares are point estimates for women, and lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation: $y_{ibt} = \alpha + +\beta_1 Treat_{ib} + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$, where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, and β_1 is the treatment effect of interest. Controls include individual (λ_i) fixed effects. Earnings measured in Norwegian kroner. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.



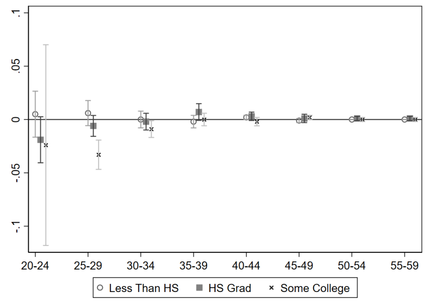
(a) Employed



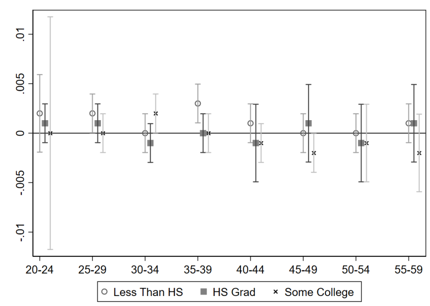
(b) Labor earnings



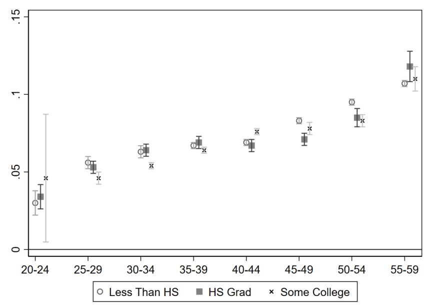
(c) School enrollment



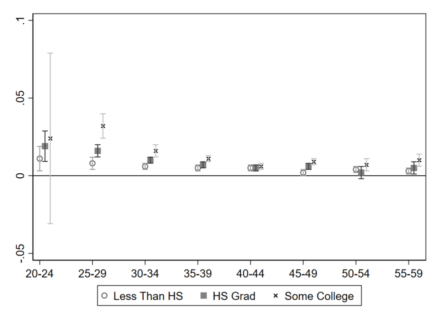
(d) Fertility



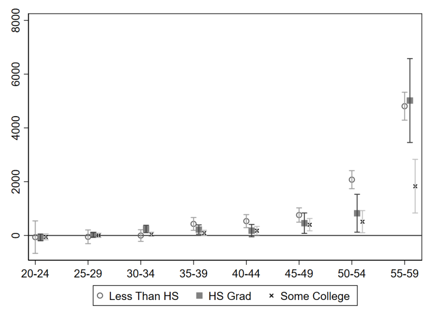
(e) Divorce



(f) Labor market exit



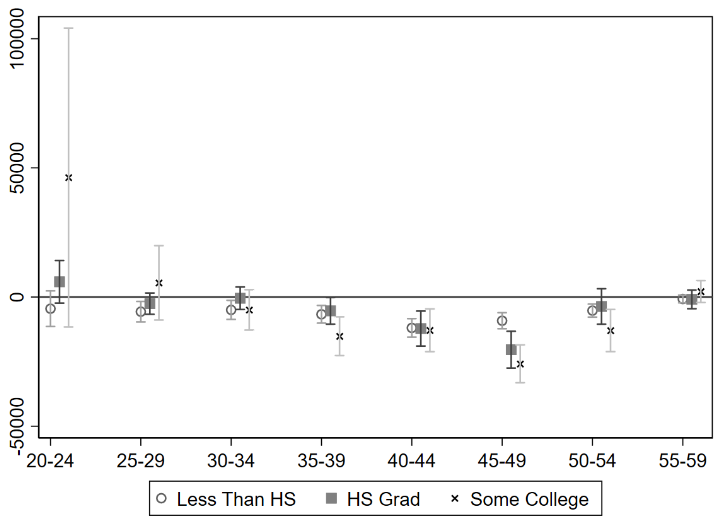
(g) Mobility



(h) Disability Pension

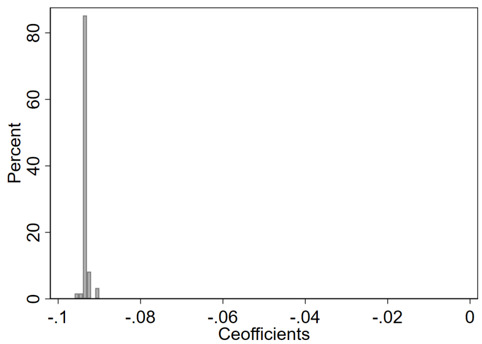
Figure 13: Sample stratified by education and age of job loss

Notes: Authors estimation using population-wide register data from Statistics Norway. Each graph represents estimates for a different outcome. Hollow circles are point estimates for those with less than high school education, solid squares are point estimates for high school graduates, xs are point estimates for those with at least some college, and lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation:
$y_{ibt} = \alpha + \beta_i Post_{tb} + \beta_2 Treat_{ib} + \rho(Post_{tb} x Treat_{ib}) + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$,
where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, $Post_{tb}$ is a dichotomous variable taking the value of one if the observation took place post relative time 0, and ρ is the treatment effect of interest. Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Mobility is measured as moving to a different local labor market region compared to relative time $t = -1$. Disability pensions benefits measured in Norwegian kroner. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.

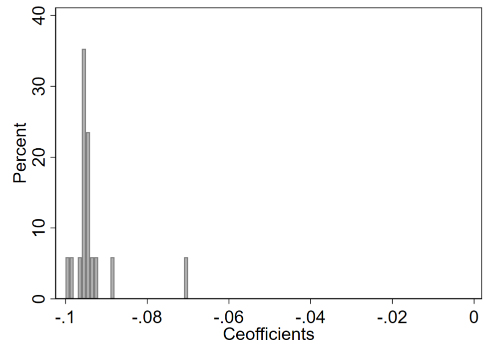


(a) Labor earnings 15 years post base year

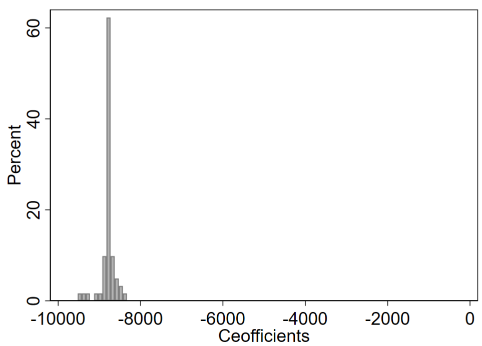
Figure 14: Stratified sample by age of job loss and education level: Earnings 15 years after shock
Notes: Authors estimation using population-wide register data from Statistics Norway. Hollow circles are point estimates for those with less than high school education, solid squares are point estimates for high school graduates, xs are point estimates for those with at least some college, and lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation: $y_{ibt} = \alpha + \beta_1 Treat_{ib} + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$,
where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, and β_1 is the treatment effect of interest. Controls include individual (λ_i) fixed effects. Earnings measured in Norwegian kroner. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.



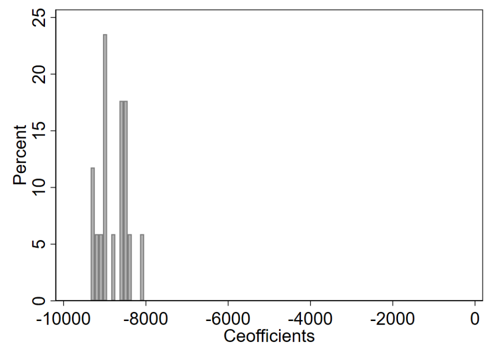
(a) Excluding individual industries, employed



(b) Excluding individual base years, employed



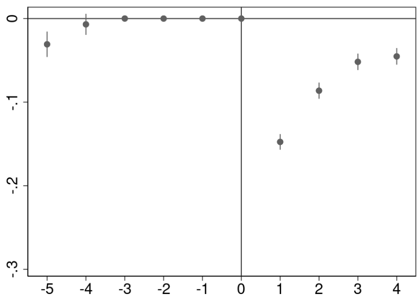
(c) Excluding individual industries, labor earnings



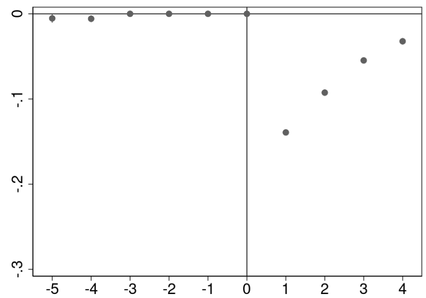
(d) Excluding individual base years, labor earnings

Figure 15: Sensitivity to dropping individual industries and base years

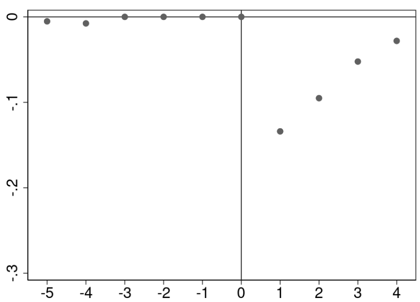
Notes: Authors estimation using population-wide register data from Statistics Norway. Each graph represents estimates for a different outcome-by-sensitivity to sample. Histogram is for all ages, and shows the distribution of the main coefficient of interest to dropping either a single base year or single industry. Estimating equation:
$y_{ibt} = \alpha + \sum_{t=-3}^{4}[\pi_t(Treat_{ib})] + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$,
where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, π_t is the time-varying treatment effect in year t, (π_{-1} omitted so all estimates are relative to $t = -1$). Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Labor market exit is measured by neither employed nor collecting unemployment benefits. Labor market exit is measured by neither employed nor collecting unemployment benefits. Labor earnings is measured in 1000 Norwegian kroner. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.



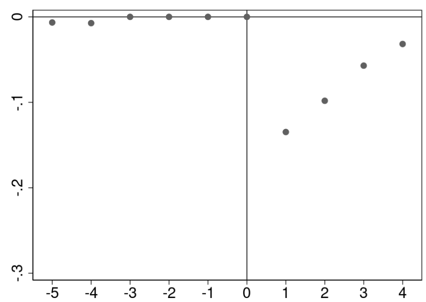
(a) Employed, age 20 through 24



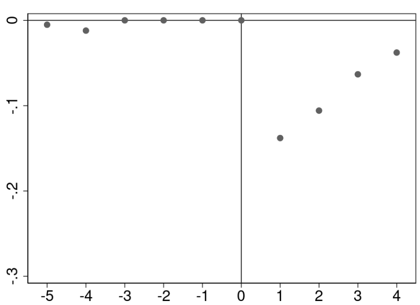
(b) Employed, age 50 through 29



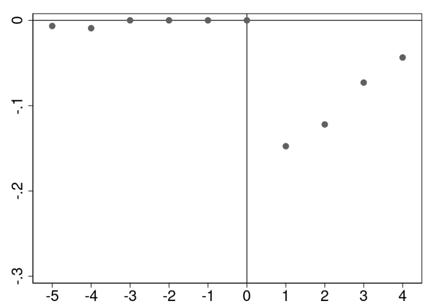
(c) Employed, age 30 through 34



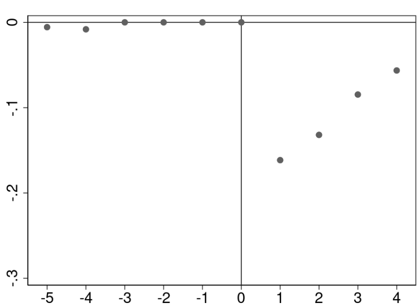
(d) Employed, age 35 through 39



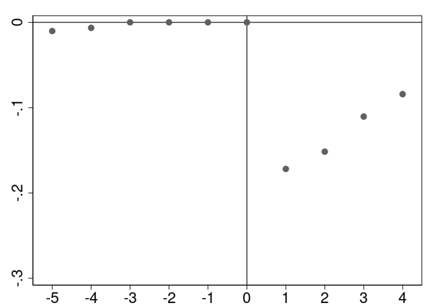
(e) Employed, age 40 through 44



(f) Employed, age 45 through 49



(g) Employed, age 50 through 54



(h) Employed, age 55 through 59

Figure 16: Sample stratified by education and age of job loss

Notes: Authors estimation using population-wide register data from Statistics Norway. Each graph represents estimates for employment for a different age group, and lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation: $y_{ibt} = \alpha + \sum_{t=-5}^{4}[\pi_t(Treat_{ib})] + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$,
where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, π_t is the time-varying treatment effect in year t, (π_{-1} omitted so all estimates are relative to $t = -1$). Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.

Table 1: **Summary Statistics**

	Mean	Standard Deviation
Panel A: Treated		
Female	0.40	0.049
Age	41.66	9.49
Years of Education	11.68	2.84
Market Wage	397.44	208.96
Married	0.63	0.48
Divorced	0.09	0.29
Number of Children	1.77	1.03
Panel B: Control		
Female	0.40	0.049
Age	42.23	9.48
Years of Education	11.87	3.03
Market Wage	430.40	227.81
Married	0.63	0.48
Divorced	0.09	0.29
Number of Children	1.80	1.02

Notes: Authors' estimation based on Norwegian register data from 1986 through 2018. Certain variables are not available for the entire period; see data section for more details. Market wage is measured in 1000 NOK. Means are provided on the left, standard deviations are provided on the right.

Table 2: **Stratified School Enrollment Effects**

	Age 20 through 24	Age 25 through 29	Age 30 through 34
Panel A: Low vs. High Ability			
Low Ability	0.011***	0.007***	0.003***
	(0.004)	(0.001)	(0.001)
High Ability	0.036***	0.016***	0.007***
	(0.009)	(0.002)	(0.001)
Panel B: Low vs. High Wealth			
Low Wealth	0.014**	0.009***	0.006***
	(0.006)	(0.002)	(0.001)
High Wealth	0.025***	0.011***	0.004***
	(0.005)	(0.001)	(0.001)
Panel C: Low vs. High Parental Income			
Low Parental Income	0.018***	0.014***	0.008***
	(0.006)	(0.004)	(0.003)
High Parental Income	0.024***	0.017***	0.007***
	(0.008)	(0.004)	(0.003)

Notes: Authors' estimation based on Norwegian register data from 1986 through 2018. Each column represents estimates for a different age group. Standard errors are clustered at theindividual level. Estimating equation: $y_{ibt} = \alpha + \beta_i Post_{tb} + \beta_2 Treat_{ib} + \rho(Post_{tb} x Treat_{ib}) + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$,
where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, $Post_{tb}$ is a dichotomous variable taking the value of one if the observation took place post relative time 0, and ρ is the treatment effect of interest. Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years. Cut-off for ability is 5 (on a scale from 1-9) on military intelligence exam (men only); cut-off for wealth is median; cut-off for parental income is median at childs age 18. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level and *** denotes significance at the 1 percent level.

Table 3: **Stratified Fertility Effects**

	Age 20 through 24	Age 25 through 29	Age 30 through 34	Age 35 through 39
Panel A: Low vs. High Income				
High Income	-0.022**	-0.013***	-0.009***	0.001
	(0.009)	(0.004)	(0.003)	(0.003)
Low Income	0.001	-0.003	0.000	0.001
	(0.012)	(0.005)	(0.004)	(0.003)
Panel B: Low vs. Education				
Less than high school	0.005	0.006	-0.000	-0.002
	(0.011)	(0.006)	(0.004)	(0.003)
High school	-0.019*	-0.006	-0.002	0.007
	(0.011)	(0.005)	(0.004)	(0.004)
More than high school	-0.024	-0.033***	-0.009**	0.000
	(0.048)	(0.007)	(0.004)	(0.003)

Notes: Authors' estimation based on Norwegian register data from 1986 through 2018. Each column represents estimates for a different age group. Standard errors are clustered at theindividual level. Estimating equation: $y_{ibt} = \alpha + \beta_i Post_{tb} + \beta_2 Treat_{ib} + \rho(Post_{tb} x Treat_{ib}) + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$,
where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, $Post_{tb}$ is a dichotomous variable taking the value of one if the observation took place post relative time 0, and ρ is the treatment effect of interest. Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level and *** denotes significance at the 1 percent level.

Table 4: **Stratified Divorce Effects**

	Age 20 through 24	Age 25 through 29	Age 30 through 34
Panel A: Marriage Duration			
Less than 3 years	0.020**	0.005*	-0.000
	(0.000)	(0.002)	(0.002)
More than 3 years	0.011	0.005	0.003**
	(0.027)	(0.003)	(0.001)
Panel B: Low vs. High Wealth			
Low Wealth	0.000	0.001	-0.001
	(0.002)	(0.001)	(0.001)
High Wealth	-0.000	0.000	0.002
	(0.002)	(0.001)	(0.001)

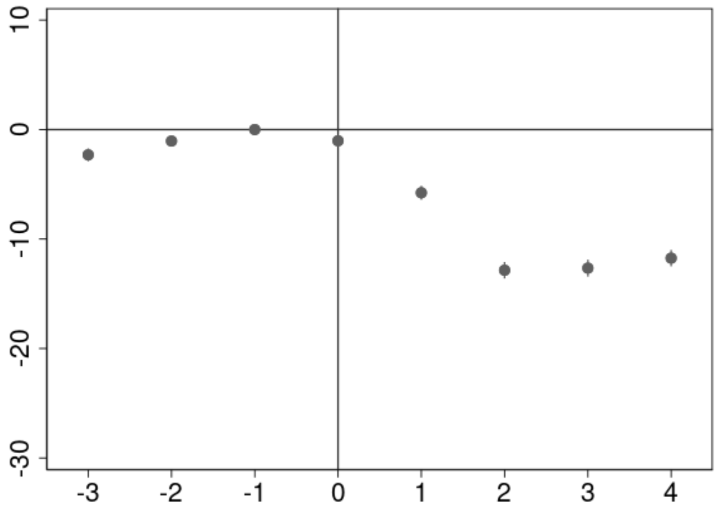
Notes: Authors' estimation based on Norwegian register data from 1986 through 2018. Each column represents estimates for a different age group. Standard errors are clustered at theindividual level. Estimating equation: $y_{ibt} = \alpha + \beta_i Post_{tb} + \beta_2 Treat_{ib} + \rho(Post_{tb} x Treat_{ib}) + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$, where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, $Post_{tb}$ is a dichotomous variable taking the value of one if the observation took place post relative time 0, and ρ is the treatment effect of interest. Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level and *** denotes significance at the 1 percent level.

Table 5: **Sensitivity and Robustness Analysis**

	Employment	Labor earnings
Panel A: Baseline	-0.094***	-8785.996***
	(0.000)	(318.021)
Panel B: Looking at big firms (30+ employees)	-0.091***	-8319.946***
	(0.000)	(336.595)
Panel C: Looking at bigger firms (40+ employees)	-0.088***	-8054.767***
	(0.000)	(356.804)
Panel D: Looking at biggest firms (50+ employees)	-0.086***	-8111.643***
	(0.000)	(316.348)
Panel E: Clustering at municipality level	-0.094***	-8785.996***
	(0.002)	(408.538)
Panel F: Propensity Score Matching	-0.096***	-7477.284***
	(0.000)	(733.891)
Panel G: Accounting for early leavers	-0.157***	-10575.175***
	(0.001)	(1379.927)
Panel H: Requiring only 2 years of work history	-0.120***	-12380.780***
	(0.000)	(278.835)
Panel I: Requiring only 1 year of work history	-0.139***	-13892.51***
	(0.000)	(252.738)
Panel J: Looking only at firm closures	-0.163***	-11107.728***
	(0.001)	(1433.879)
Panel K: Individuals with future mass layoff or firm closure as control group	-0.152***	-10458.960***
	(0.002)	(307.406)

Notes: Authors' estimation based on Norwegian register data from 1986 through 2018. Each column represents estimates for a different outcome, each row represents a different analysis. Standard errors are clustered at the individual level. Estimating equation: $y_{ibt} = \alpha + \beta_i Post_{tb} + \beta_2 Treat_{ib} + \rho(Post_{tb} x Treat_{ib}) + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$, where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, $Post_{tb}$ is a dichotomous variable taking the value of one if the observation took place post relative time 0, and ρ is the treatment effect of interest. Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Panel A is the main baseline model and sample; Panel B increases minimum firm size to 30 employees; Panel C increases minimum firm size to 40 employees; Panel D increases minimum firm size to 50 employees; Panel E clusters standard errors at the municipality level; Panel F restricts the sample to the common support of the propensity score; Panel G includes individuals who leave a firm before a mass layoff or closure in the treated group; Panel H reduces the required work history to 2 years; Panel I reduces the required work history to 1 year; Panel J includes only firm closures in the treated group. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level and *** denotes significance at the 1 percent level.

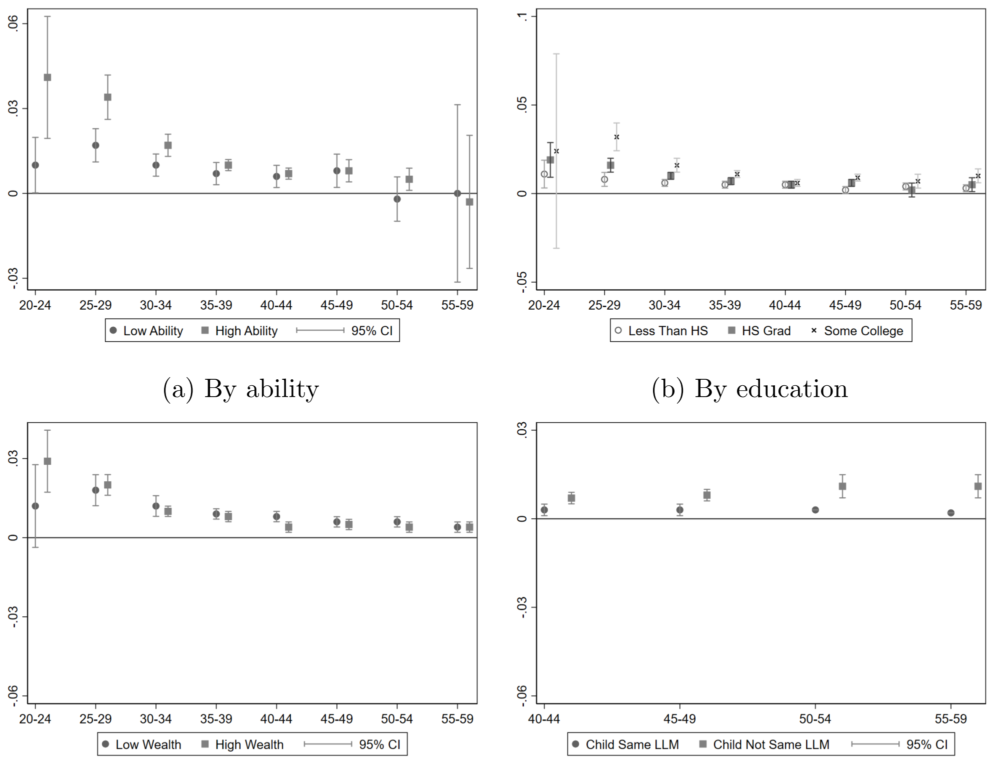
Online Appendix: Not For Publication



(a) Gross market earnings

Figure A1: Pooled sample of men and women aged 20 through 60; robustness to using gross market earnings

Notes: Authors estimation using population-wide register data from Statistics Norway. Dots are point estimates, lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation:
$y_{ibt} = \alpha + \sum_{t=-3}^{4}[\pi_t(Treat_{ib})] + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$,
where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, π_t is the time-varying treatment effect in year t, (π_{-1} omitted so all estimates are relative to $t = -1$). Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Gross market earnings is measured in Norwegian kroner (000s). Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.



(a) By ability

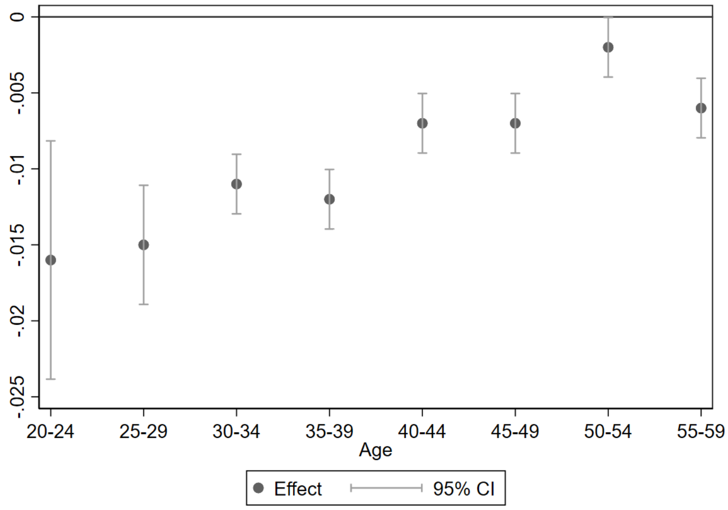
(b) By education

(c) By wealth

(d) By adult child in same LLM

Figure A2: Mobility by subgroups

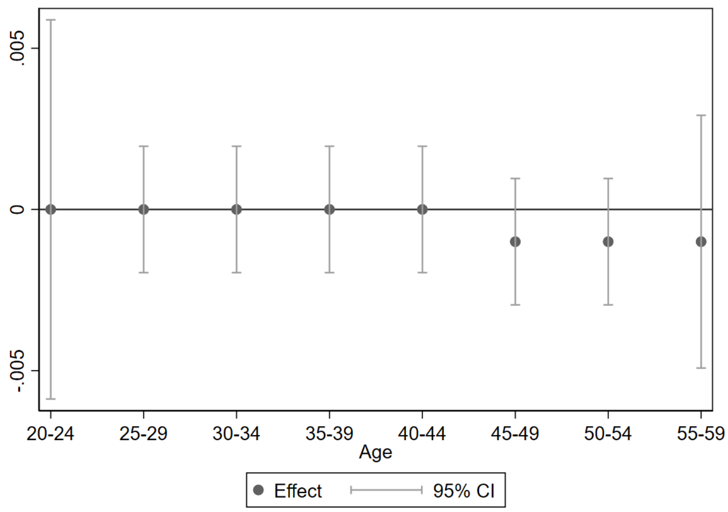
Notes: Authors' estimation based on Norwegian register data from 1986 through 2018. Each graph represents estimates for a different stratification. Dots are point estimates, and lines are 95% confidence intervals. Standard errors are clustered at theindividual level. Estimating equation:
$y_{ibt} = \alpha + \beta_i Post_{tb} + \beta_2 Treat_{ib} + \rho(Post_{tb} x Treat_{ib}) + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$,
where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, $Post_{tb}$ is a dichotomous variable taking the value of one if the observation took place post relative time 0, and ρ is the treatment effect of interest. Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Mobility is measured as moving to a new local labor market region compared to relative time $t = -1$. Cut-off for ability is 5 on military intelligence exam (on a scale from 1-9, men only); cut-off for wealth is median wealth. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.



(a) Cross-LLM Commuting

Figure A3: Pooled sample of men and women aged 20 through 60; commuting

Notes: Authors' estimation based on Norwegian register data from 1986 through 2018. Dots are point estimates, and lines are 95% confidence intervals. Standard errors are clustered at theindividual level. Estimating equation: $y_{ibt} = \alpha + \beta_i Post_{tb} + \beta_2 Treat_{ib} + \rho(Post_{tb} x Treat_{ib}) + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$,
where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, $Post_{tb}$ is a dichotomous variable taking the value of one if the observation took place post relative time 0, and ρ is the treatment effect of interest. Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. Mobility is measured as moving to a new local labor market region compared to relative time $t = -1$. Cut-off for ability is 5 on military intelligence exam (men only); cut-off for wealth is median wealth. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years.



(a) Diceased

Figure A4: Pooled sample of men and women aged 20 through 60: mortality

Notes: Authors estimation using population-wide register data from Statistics Norway. Dots are point estimates and lines are 95% confidence intervals. Standard errors are clustered at the individual level. Estimating equation: $y_{ibt} = \alpha + \beta_1 Treat_{ib} + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$,
where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, and β_1 is the treatment effect of interest. Controls include individual (λ_i) fixed effects. Earnings measured in Norwegian kroner. Sample is restricted to individuals who have been employed for at least 20 hours a week during the three years leading up to the base year, and not enrolled in school during any of those three years. Deceased is a dummy variable taking the value of 1 if the individual has passed away within 15 years of the displacement event.

Table A1: **Stratified Labor Earnings Effects After 15 Years, by Mobility and Human Capital Investment**

	Age 20 through 24	Age 25 through 29
Panel A: Human Capital Investment		
Returned to school	34931.083***	17706.632***
	(5986.252)	(4113.840)
Did not return to school	-1271.175	-6911.812***
	(2879.115)	(2062.869)
Panel B: Cross-LLM Mobility		
Did move	15871.266***	31366.024***
	(5976.485)	(4257.462)
Did not move	2282.548	-10332.850***
	(288.922)	(2037.078)

Notes: Authors estimation using population-wide register data from Statistics Norway. Each column represents estimates for a different age group. Standard errors are clustered at the individual level. Estimating equation: $y_{ibt} = \alpha + \beta_i Post_{tb} + \beta_2 Treat_{ib} + \rho(Post_{tb} x Treat_{ib}) + \gamma_{tb} + \lambda_i + \epsilon_{ibt}$,
where y_{ibt} is a labor market outcome, $Treat_{ib}$ is a binary variable for involuntarily displacement at $t = 0$, $Post_{tb}$ is a dichotomous variable taking the value of one if the observation took place post relative time 0, and ρ is the treatment effect of interest. Controls include relative time (γ_{tb}) and individual (λ_i) fixed effects. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level and *** denotes significance at the 1 percent level.