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DISCUSSION PAPER SERIES

IZA DP No. 15634

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ABSTRACT

The Effect of Labor Market Shocks across the Life Cycle*

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 Classification: I20, J63

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. 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 (e.g., Jacobson *et al.* 1993 and Davis and von Wachter 2011).[1] 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.

To perform our analysis, we use administrative employer-employee matched data on all Norwegian residents aged 20 through 60 between 1986 and 2018. We begin by defining a set of base years (1989-2006), and follow workers over time and use event studies to compare changes in outcomes among those who experienced an involuntary job loss relative to those who did not. We perform analyses for the full sample as well as separately by sex at different ages, from 20 to 60. We supplement this analysis with an alternative identification strategy that relies exclusively on the difference in the timing of treatment, using a control group composed of only workers in

[1] A mass layoff is defined as an establishment losing at least 30 percent of its workforce in a given year.

firms where a plant closure or mass layoff happens in the future. This approach relies on a milder assumption (that the timing of the shock is conditionally random rather than that the shock in itself is conditionally random), and addresses concerns that workers endogenously selected into the treatment sample.

We present four main 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.

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 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 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.

Our final set of results focus on the very long-term career implications of job loss measured 15 years after the shock. These results represent 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 small but statistically significant positive labor earnings effects 15 years after the displacement event. These effects are driven by individuals who returned to school or relocated to new local labor markets, in response to the displacement event. Second, individuals in their late fifties display very modest effects 15 years after displacement. This is consistent with the majority of this age group exiting the labor force and retiring within 15 years regardless of losing a job. Third, individuals between these two age groups experience persistent adverse labor earnings effects 15 years after displacement. These results highlight the importance of accounting for the dynamics of 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 by sex. First, males are much more likely to respond to economic shocks by returning to school at a young age, while the effect on females is constant across the first twenty years of their careers. Second, the effect on permanent labor market exit and retirement take-up is much more pronounced among males at an older age, while it is smaller among females. Finally, the fertility effects among males are 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 is also 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.

This paper makes three important contributions to the existing literature. First, while prior research on the labor market effects of job loss tends to focus on all workers (e.g., Ruhm 1991; Jacobson *et al.* 1993; Huttunen *et al.* 2011; Ichino *et al.* 2017; von Watcher et al. 2013), or on specific age segments of the workforce (e.g., Chan and Stevens 1999; Kletzer and Fairlie 2003), we are able to trace out both the short- and the long-run effects of job loss across the entire lifecycle of individuals through one unified framework and single institutional setting. This provides us with a unique opportunity to isolate the age-specific job loss effects in a systematic way across the life course of workers. This allows us to expand on the existing literature by illustrating how the

age-specific component interacts with job loss conditional on institutional setting and context, and provides a much more comprehensive understanding of the dynamics of job loss among workers.[2]

Second, prior research on the effect of job loss on non-labor market outcomes tends to focus on one particular channel, such as mobility, children, criminality, pension, or fertility.[3] While this literature provides important insights into workers' response to job loss, it offers little information about the relative importance of these different mechanisms in explaining the long-run labor market effects of job loss on individuals. By directly investigating the link between effects of job loss on a rich and varied set of life choices and effects of job loss on long-term labor market outcomes, the paper helps us better understand the mechanisms through which job loss can be so detrimental; and the relative importance of these channels.

Finally, and most importantly, the existing literature offers little 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.[4] By directly investigating the link between effects of job loss on life choices and effects on long-term labor market outcomes – across both genders and ages -- the paper helps us better understand the mechanisms through which job loss can be so detrimental and why effects differ across ages and gender. In particular, by accounting for the differential effects that job loss has on key life decisions, we can explain more than 30 percent of the variation in job loss effects across genders and age. We see this paper as

[2] In addition to the job loss literature, this paper thus also helps advance 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).

[3] Specifically, the non-economic outcomes that have been studied include: Mortality (Eliason and Storrie 2009; 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).

[4] For example, 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.* 2019b; Manning and Swaffield 2008), that men and women differ in 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), and that men and women rely on different forms of social support and have access to different types of risk-mitigating technologies (e.g., Sabarwal *et al.* 2010; Rege *et al.* 2011).

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]

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, present our empirical method, and describe evidence supporting our identifying assumptions; In Section 4, we present evidence on the labor market effect of job displacement; In Section 5, we discuss the interaction between involuntary job loss and key life decisions; In Section 6, we analyze heterogeneity across sex and education levels; In Section 7, we show results from falsification tests and robustness checks; In Section 8, we conclude.

2. Background

2.1 Institutional Background

Norway's welfare state is based on universal healthcare, comprehensive social insurance, and free education through college. In terms of employment protection, Norway has a medium-to-high degree of protection relative to other OECD countries, similar to Sweden and France (Huttunen *et al.* 2011). When establishments downsize, there is no strict rule regarding the order in which workers are dismissed. While seniority is institutionalized in the main union agreements, this does not represent a binding constraint.[6] Termination requires three months' notice, and there are no legal requirements for severance pay.[7]

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

[5] Another 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] 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.

[7] 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.

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 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).

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. This does not represent an exhaustive list of life decisions that may be impacted by shocks, and in supplemental analyses we extend the set of outcomes to also include outcomes such as mobility and commuting.[8]

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 at the margin of deciding whether to return to school, relocate for a job, have children, or retire, may be induced to do so by a labor shock. Importantly, the costs and benefits of these 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.

With respect to human capital investment, we hypothesize that the probability of investing in human capital 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. 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.

[8] It is also interesting to consider how job loss events may differentially impact individuals who just experienced major life changes, such as having a child (see, for example, Willage and Willen 2020). We see it as an extremely interesting area for future research.

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 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 and a job loss event lowers current and future earnings.

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. 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 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 differential interactions between economic shocks and key life decisions across the life cycle (including unobservable interactions) is to examine the very long-term career impact on displaced workers. Such effects will be driven not only by direct impacts of job loss on employment, skills, and experience, but also by all indirect effects operating through changes in these key life choices (such as skill upgrading, mobility, and family formation). 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 be realized. 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. In a later section in the paper, we perform a mediation analysis in which we explore how much of the variation in the long-run earnings effects is driven by the life choice effects we find in the paper.

An important contribution of this paper is to explore differences in the interaction of economic shocks and key life decisions between sexes. This heterogeneity is interesting because the existing literature has documented sizable gender differences in occupational choice (e.g., Cortes and Pan 2018), wage growth (e.g., Napari 2009), career progression (e.g., Blau and Devaro 2007) and fertility. In addition, a growing literature has documented important gender differences

in job search and mobility (e.g., Cortes, Pan, Pilossoph, and Zafar 2021; Butikofer, Loken and Willen 2022), financial return to job mobility (e.g., Del Bono and Vuri 2011), willingness to trade off commute against wage (e.g., Le Barbanchon, Rathelot and Roulet 2021), early retirement (e.g., Dahl, Nilsen, and Vaage 2003), housework (e.g., Bertrand *et al.* 2015), and in how males and females are impacted by fertility (e.g., Kleven *et al.* 2019b). Thus, males and females differ significantly not only in terms of career development and occupational choice, but also in terms of key life choices and the timing of such decisions.

For example, females face a shorter reproduction window than men, and we expect that any effect of job loss on fertility will be concentrated around the prime fertility age for women. Further, the differences in the financial return to mobility and the willingness to trade off commute against wage may imply that men are more likely to respond to job loss by changing local labor markets, particularly at young ages. In addition, because of differences in wage growth and promotions, we expect that the difference of job loss among men and women are greater later in their careers. Finally, because of differences in the return to education and occupational sorting, we expect men to be more likely to return to school following a job loss than females.

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. In particular, if men are more likely to return to school and move across regions at an early age, we may expect that the long-term career implications of job loss are more detrimental to women than men at an early age. As the same time, because of the differences in career wage growth, promotions, and career progression, we may expect that the long-term career implications of job loss are more detrimental to men relative to women later in their careers.

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.

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 7, we show that our results are robust to relaxing the restriction on firm size.

Our data provide detailed labor market information. 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.[9] An individual is defined as employed if she has an establishment identification number at that time, and as unemployed if she does not 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).[10] 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.

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.

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.

[9] In May until 1995 and in November from 1996.

[10] 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).

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, 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.[11] 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.

Mergers and acquisitions potentially represent a concern in our setting, as they could give the appearance of a firm closure or mass layoff when in actually it has no impact on the employment of the worker. To overcome this issue, we exclude individuals from the treatment group if more than 75 percent of their coworkers who experienced the same firm-specific layoff event ended up at the same employer in the year after the displacement event took place.

We follow people for 8 years around each base year – from relative time -3 to relative time +4. 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). 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 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:

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

where y_{ibt} is an outcome for individual i at relative time t and base year b. $Treat_{ib}$ is a binary variable equal to one if the individual was involuntarily displaced in base year b and relative time

[11] Importantly, this empirical design does not impose any restriction on the job loss behavior of treated or control workers in future years. For example, whether an individual in our treatment or control group gets exposed to a job loss event between relative time 1 and 2 has no impact on if we include them in the analysis or the way in which we assign that worker to the control or treatment group, This is important because, as discussed by Krolikowski (2017), conditioning on future job loss events is likely to introduce selection bias into the regressions and complicate the interpretation of the results."

0, and zero otherwise. The π_τ coefficients trace out relative pre-treatment trends as well as time-varying treatment effects. The parameters of interest are π_1 to π_4, which are the labor market effect of job loss across time. All estimates are relative to the year prior to job displacement.

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

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, have an accurate counterfactual trend of displaced workers had they not been displaced. The coefficients π_{-3} to π_0 in Equation (1) test for pre-treatment relative trends. If these estimates are economically small and statistically indistinguishable from zero, it suggests that there is no selection on trends that bias our results. In addition, in Section 7 we perform a rich set of robustness checks designed to examine the credibility of the job displacement design. Specifically, we test 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.

Finally, in Section 7 we also show results from an alternative specification strategy in which we restrict the control group to only those workers who did not experience a firm closure or a mass layoff at relative time zero, but who did experience a mass layoff or firm closure in the future. This exercise mirrors the empirical framework in Fadlon and Nielson (2019), and is implemented as a means to ensure that the effects we identify are not driven by systematic differences between individuals in our control group and treatment group that are correlated both with the probability of experiencing a mass layoff and with the outcomes of interest.

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 binary variable taking the value of one if the observation took place post relative time 0, and zero otherwise. 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 shows the full set of π_τ obtained from estimating Equation (1) for employment and labor earnings using all individuals aged 20 through 60. The first column in Panels C and D of Figure 1 summarizes the large number of π_t by showing the immediate treatment effect in year one, and the longer-term treatment effect in year four, obtained from estimating that same equation. The number of observations underlying each of our results can be found in Appendix Table A-1. Several observations are worth noting. First, 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.

Second, the magnitude of the employment effect shrinks over time, and in the fourth post-displacement year the adverse employment effect is approximately 5 percentage points; one-third of the first year effect. With respect to labor earnings, the pattern is slightly different, as the effect does not shrink during the first four post-displacement years.[12] Overall, this figure is consistent with the existing job displacement literature, and confirms the existence of substantial earnings and employment effects both in the short-and 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 many dimensions, including when in their careers the shocks occur. To preview many of these sources of heterogeneity, Panels C and D of Figure 1 stratify the pooled analysis by males and females, low- and high-educated workers, and younger and older workers. These panels demonstrate that focusing on average effects among workers across the life cycle misses a great deal, something which we explore much more closely below.

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

To more fully exam heterogeneity by age, Appendix Figure A-1 provides the full set of π_t estimates obtained from estimating Equation (1) for employment stratified by 5-year age bins, and Appendix Figure A-2 provides the full set of π_τ estimates obtained from estimating Equation (1) for labor earnings stratified by 5-year age bins. Panels A and B of Figure 2 summarize the large number of π_τ by showing the immediate treatment effect in year one, and the longer-term treatment effect in year four, obtained from estimating that same equation.

Figure 2 shows that the effects of job displacement are more varied and more extensive than has been recognized, and the timing of shocks across the life cycle matters for how they impact labor market outcomes. Specifically, the figures demonstrate that adverse labor market impacts generally increase with age, 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.[13] The differential effect on employment and earnings across the life cycle is an important finding, implying that displacement hurt individuals differently across their career.

5. Adverse Labor Shocks and Key Life Choices

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.

5.1 Human Capital Investment

Figure 3(a) shows the immediate treatment effect in year one, and the longer-term treatment effect in year four, obtained from estimating Equation (1) using school enrollment as the outcome (Appendix Figure A-5 shows the full set of π_τ). We find that young people are most likely to return

[13] To see where in the distribution within age group these displacement effects are coming from, Appendix Figure A-4 show a histogram plotting changes in earnings among displaced workers and control group workers in the six age groups. Comparing the control and treatment distributions, the figure reveals that the displacement events shift the entire earnings distribution leftwards, thickening the left tail and moving the median as well as the mean down."

to school and investing in human capital following displacement, with an effect size of approximately 2.5 percentage points in the first few years after displacement. Mid-career workers (aged 30 through 40) are 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, 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.[14]

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 marginally economically meaningful, suggesting that less than 1 percent of displaced individuals return to school following a layoff. While Minaya *et al.* (2020) provides important insights on average effects across a wide range of age groups, they do not consider the age-varying impact across the life cycle. The results from our analysis suggest that the human capital investment response is much greater among young individuals (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 may be small or even positive. We study this in greater detail in Section 5.4 below.

To better understand the type of education the displaced individuals acquire, we estimate Equation (2) using enrollment in high 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

[14] Specifically, an individual is defined as experiencing a job loss if the individual held a job in relative time zero but did not hold a job in relative time one. For the majority of the base years we use, the date to determine employment was May. This means that a sizable fraction of individuals in our sample experienced the job loss in the calendar year that makes up relative time zero (2011). These individuals, should they choose to return to school as a consequence of the job loss, may therefore do so already in the calendar year that makes up relative time zero (2011), such that we observe a human capital response already in that year.

overall enrollment effects. The results from this exercise are shown in Appendix Figure A-6.[15] The figure reveals that the majority of the human capital effect is driven by displaced workers enrolling in basic post-secondary education. These results suggest that most of the human capital effect is driven by relatively well-educated individuals going back to 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, subject to 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 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 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 individuals were 18.

Appendix Table A-2 shows results from these analyses. We find 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

[15] The point estimates in Figure 5 are obtained through the difference-in-differences specification described in Equation (2). As such, the treatment effect provided in the figure measures the average impact of the displacement event aggregated over the first four post-displacement years.

[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 going this far back in time.

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.

Overall, heterogeneous effects by ability and wealth shows that 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.

5.2 Family Structure

Figures 3(b) and (c) show the immediate treatment effect in year one, and the longer-term treatment effect in year four, obtained from estimating Equation (1) using fertility and marital status (Appendix Figures A-7 and A-8 shows the full set of π_τ). With respect to fertility, the results show 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.[17]

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.* 2013; Lovenheim and Mumford 2013). Second, there are substantial career effects associated with fertility (e.g., Kleven *et al.* 2019a), 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 understand who reduces fertility in response to a job displacement, we 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

[17] 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.

fertility after a 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 and make strategic fertility decisions, exhibit a stronger response. 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.

These results are in Panels A and B of Appendix Table A-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 scope of the current paper, we explore this in detail in Salvanes, Willage and Willén (2020).

In terms of the existing literature, Huttunen and Kellokumpu (2016) find a relatively sizable effect of job loss on fertility, but only for females. 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 3(c) 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.

Taken together, the results from Figures 3 (c) and (d) 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

Figure 3(d) shows the immediate treatment effect in year one, and the longer-term treatment effect in year four, obtained from estimating Equation (1) using non-labor force participation as the outcome (Appendix Figures A-9 shows the full set of π_τ). results obtained from estimating

Equation (1) using. 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 Appendix Figure A-10 plot the results obtained from estimating Equation (2) using mobility and disability pension as outcomes. We find 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 increases with age.[18] It should be noted that the effect on disability pension is consistent with prior work such as 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, married individuals, and individuals with 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). Existing

[18] 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 7).

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, whether the individual is the primary household earner, whether the individual has a spouse, 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, and that married individuals are less likely to move in response to job loss events across all ages. 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

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 by investing in human capital and relocating across labor markets, so the very long-run effect may be small and even be positive. Second, mid-career individuals are more likely to delay fertility decisions, which could

reduce the very long-run impact on earnings due to smaller child penalty effects (e.g., Kleven *et al.* 2019a). Third, individuals in their forties are more likely to exit the labor market and take up disability pension, and are less likely to move, so the very long-run effect may be larger. Fourth, the oldest individuals are all likely to enter retirement shortly after the displacement event, such that the difference in labor earnings between treatment and control individuals likely is near zero.

Figure 4 provides results obtained from estimating the effect of displacement on labor market outcomes measured 15 years after the event took place. Looking across the figure, several interesting observations are worth highlighting.[19]

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. 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 in response to an economic shock.

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

[19] It is important to highlight that we focus on a key set of life decisions, and that job loss may have differential impacts on other aspects of a workers professional or personal life across the life cycle as well. For example, job loss may lead workers to change careers, fall down the job ladder, or lose access to top employers in a way that differs over the life cycle. While space constraints prevent us from conducting a detailed analysis on this dimension in the current paper, we provide some preliminary results on this question in the appendix. Specifically, in Appendix Figure A-22 we show results from estimating our main model using firm rank (based on average wage) as well as the probability of switching industries, as dependent variables. The results demonstrate that workers are equally likely to switch industries after a job loss across the life cycle, but that the ranking of the firm that they are able to switch to varies greatly. Specifically, the older the worker is at the time of displacement, the lower is the rank of the firm that the worker manages to get hired by following displacement (conditional on finding a job). We believe these are interesting results highlighting the career change dynamics as a function of job loss.

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 A-4. 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 differences in wellbeing and health, we have also examined mortality effects 15 years after the displacement event. The results are in Appendix Figure A-13, 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 Norwegian welfare state mutes these effects.

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.* 2019b; 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.* 2020). 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 5 provides results from estimating Equation (2) separately by sex and age group. There is 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 6 provides point estimates obtained from estimating the effect of displacement on earnings 15 years after the event took place, separately by sex. 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.

To what extent can the differential effects of job loss on life choices across ages and genders help explain the differences in the long-term labor market effects of job loss among these individuals? Addressing this question would help us better understand the mechanisms through which job loss can be so detrimental and why effects differ across ages and between gender.

The most natural way through which one can explore this link is by sequentially dropping individuals who respond to job loss through one of these channels, in a regression that estimates the very long-term labor market effects of job loss. One can the study how much the inclusion of these life choices reduces the variation in labor market effects across genders and ages. Such an analysis would provide a suggestive estimate of the differential effect of job loss on long-run labor market outcomes across genders and ages that cannot be explained by the life choices we identify. The main challenge with this approach, however, is that all life choices are endogenous to the job loss event, such that this approach would risk introducing selection bias into the equation and complicate the interpretation of the results.

Having said that, we still believe that such an exercise is worthwhile. To this end, we have performed the analysis in which we sequentially drop individuals who respond to job loss through one of these channels, and we have included the results in Table 2. The results suggest that the life choices we examine can explain almost 35 percent of the variance in labor market effects across genders and ages. This is a substantial effect, showing that the life choices we examine in the paper are directly linked to the variation in long term outcomes across genders and ages. However, we encourage caution when interpreting these results due to potential endogeneity concerns discussed above.

6.2 Heterogeneity by Education Level

With respect to effect heterogeneity across education levels, Figure 7 provides difference-in-differences estimates obtained from estimating Equation (2) separately by education level for each of the outcomes discussed in Section 5. We focus on three education levels to facilitate the interpretation of our results: less than high school, high school, and at least some college.

The effects are relatively similar across education levels both with respect to the main labor market outcomes (Panels A and B) and the life decisions (Panels C through H). However, there are some noticeable differences. Specifically, 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.

Due to the implied differences in the interactions between life decisions and economic shocks among individuals of different educational levels, the very long-run impact on earnings across education levels – depending on when during the life cycle the shock occurs – may be different. We examine this in Figure 8, which provides results from estimating the effect of displacement on earnings 15 years after the event took place, separately by education level. Figure 14 shows that the very long-run earnings effects are relatively similar across education levels, with the exception of very young individuals. Specifically, early-career high-educated individuals appear more likely to recover from, and overcome, the involuntary displacement events.

7. Robustness and Alternative Explanations

7.1 Robustness

The existing 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. In addition, we present results from an alternative identification strategy that relies exclusively on the difference in the timing of treatment.

Most of the results from the robustness and sensitivity analyses are displayed in Table 3. Due to space constraints, we focus on the main labor market outcomes. In Panel A, we provide our baseline estimates to facilitate comparison across the various exercises.

In Panels B though D we examine sensitivity to restricting the sample to larger firms (more than 30, 40, and 50 employees). Altering the firm size restriction does not impact the magnitude of the effects in an economically meaningful way. This is an important result, because the risk of misclassifying mass layoffs and firm closures is likely inversely related to the size of the firm (see, for example, Black, Devereux, and Salvanes 2015), implying that our original results could have suffered from an attenuation bias. The fact the we do not find this to be the case, is consistent with the notion that our sample construction decisions have successfully helped us eliminate such attenuation issues.

In Panels E and F, we explore the robustness of our results to clustering our standard errors at the more conservative municipality level, and at the base year plant level, in which we allow the error component of Equation (1) to be correlated among individuals within the same municipality or within the same base year plant. These adjustments have no impact on the statistical significance of our estimates.

In Panel G, we perform propensity score matching on individuals in the pre-displacement period. The rationale for this exercise is that we would like the treatment and control groups to be as comparable as possible, in order to ensure a correct interpretation of the results. By restricting the sample to the common support of the propensity score (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. The results show 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.

In Panel H, 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. This analysis generates one of the largest differences in coefficients relative to our main analysis, and has important implications. Specifically, early leavers are assumed to switch in the pre-displacement year because they are aware of the upcoming job loss event at the firm. While these workers may be positively selected from the treatment group, they are still responding to the job loss event by taking a new job to avoid unemployment in relative time zero. By assigning these individuals to the control group, we are generating an attenuation bias. In terms of interpreting our findings, this implies that our baseline results may represent a lower bound of the potential employment and earnings effects associated with job loss.

In Panels I and J, we show that relaxing the employment history condition generates slightly larger employment and earnings effects. These results are consistent with the idea that individuals with fewer years of work history are less attached to the labor force, such that it may take longer for them to find a new job and reenter employment. The effect of job loss on workers with lower labor market attachment may therefore be larger than the main estimates. However, we also note that the employment history condition of three years is based on the existing job loss literature, and is meant to ensure that the treatment and control group are similar.

In Panel K, we examine the sensitivity of our results to focusing exclusively on establishment closures. Focusing on firm closures only (rather than on mass layoffs and firm closures) generates larger employment and earnings results (Panel J). This is consistent firm closures being less anticipated by workers than mass layoffs, and the workers have less time to prepare for the post-displacement labor market if they were subject to a firm closure. Specifically, in the event of mass layoffs, workers are generally guaranteed a 3-months grace period before they must leave the firm. In contrast, firm closures may occur unpredictably and lack such guarantees, making the event more abrupt.

Panel L contains results from an alternative identification strategy that relies exclusively on the difference in the timing of treatment.[20] This alternative strategy generates larger employment and earnings effects than our baseline model. This is an interesting finding, because

[20] 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.

the alternative strategy rests on a milder assumption (the timing of treatment is exogenous, rather than that treatment is exogenous), arguably less prone to measurement errors and attenuation concerns than the baseline specification. This implies that our baseline results are relatively conservative, and represent a lower bound of employment and earnings effects.

In addition to the results in Table 3, Appendix Figure A-18 shows 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 industries or time periods. Further, we have examined the difference in probability of being exposed to a job loss within each industry (using 2-digit industry codes). The goal of this exercise is to examine if workers from particular industries are more likely to be exposed to firm closures and mass layoffs than workers from other industries. This exercise helps us understand whether the work being done by workers who lost jobs was similar to the work being done by control workers. The results are in Appendix Figure A-19, and reveal that the estimated difference in treatment status within each industry is centered and tightly clustered around 0; the control and treatment group are not coming from different industries.

Finally, in the same spirit of studying the credibility of the parallel trend assumption required for causal inference, Appendix Figure A-20 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 employment mechanically flat due to the sample restrictions on employment history. By extending pre-period, we are better able to assess the comparability of our treatment and control groups over time. There is no observable evidence on differential pre-treatment trends across the control and treatment groups that could bias our results.

7.2 Alternative Explanations

How much of the age effect that we identify is actually a firm-specific job tenure effect? This question is important for understanding and interpreting our results, in particular in light of recent literature on the role of firm-specific human capital on the earnings effects associated with job loss (e.g., Krolikowski 2017; Huckfeldt 2018; Burdett, Carrillo-Tudela, and Coles 2020; Jarosch 2021).

To explore this question, we perform two supplemental analyses. First, we study the correlation between age and firm-specific tenure. These results are in Appendix Table A-6, and there is a significant but small relationship between age and firm-specific tenure. Specifically, for

each additional year of life, there is an additional 0.1 years of firm-specific tenure. This implies that the difference in firm-specific tenure between a 50-year-old and a 30-year-old is, on average, 2 years. While this correlation is statistically significant, it is relatively small, suggesting that firm-specific tenure effects are unlikely to explain much of the age-varying patterns that we document.

Second, we estimate our main regressions stratified by firm-specific tenure, but splitting the sample by above and below the median firm-specific tenure in our sample. These results, in Appendix Figure A-21, suggest that firm-specific tenure can explain a very small share of the difference in labor market and life decision effects of job loss across the life cycle. Specifically, while high-tenured workers suffer a larger employment effect and are slightly more likely to respond to a job loss event by adjusting their key life decisions (more likely to move at young ages, more likely to return to education at young ages, and more likely to pursue disability pension at an older age), these differences are small. Consequently, the difference in the earnings effect among high-and low-tenure workers is small and often not statistically significant. This is an important set of findings, highlighting that most of the age effect we identify in the paper, in particular with respect to the effect of job loss on key life decisions, appears to be independent of firm-specific tenure effects.

8. Conclusion

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 find substantial earnings and employment effects caused by job loss, both in the short- and long-term. We show these employment and wage effects vary greatly across the life cycle. Second, we reveal substantial heterogeneity in how individuals respond to displacement events across the life cycle. Individuals at the beginning of their careers relocate to other local labor markets and invest in human capital, individuals in the middle of their careers reduce fertility and divorce, and individuals at the end of their careers permanently exit the workforce and collect disability pension. Third, due to differential interactions between economic shocks and life decisions across the life cycle, we find that the very long-term effects on individual earnings also vary: young individuals

display small but positive labor earnings effects 15 years after the displacement event, older individuals display very modest and not economically meaningful effects, and individuals between these two age groups experience persistent adverse labor earnings effects that extend for 15 years.

Through a mediation analysis, we show that the life choices we examine can explain more than 30 percent of the variance in labor market effects across genders and ages. This is a substantial amount, suggesting that the life choices examined in the paper are directly linked to the variation in long-term outcomes across genders and ages. However, we encourage caution when interpreting these results due to potential endogeneity concerns discussed

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