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From distress to recovery

An analysis of involuntary job loss in the Swedish manufacturing industry

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Abstract
With growing disruptive forces affecting employment levels, it is essential to examine the impact of involuntary job loss, also known as job displacement. The purpose of this study is therefore to analyse costs of job displacement incurred by displaced workers. Using longitudinal administrative data, we follow Swedish manufacturing workers from plants experiencing mass layoffs or closures for an 11-year period. This allows us to gauge the effect of job displacement on annual earnings and employment status. Through three reduced form regressions we estimate the difference between displaced workers and a control group of workers surviving mass layoffs. To our knowledge, this comparison approach has not yet been evaluated in previous research. The results indicate that displaced workers experience short-term losses in both earnings and probability of finding employment. Furthermore, an extensive heterogeneity analysis shows that low-educated as well as old, high-tenured workers are particularly vulnerable. In comparison to literature examining other countries, the negative effects appear to be moderate and workers seem to experience a relatively fast recovery, thereby indicating that the Swedish labour market is well-functioning in protecting workers facing involuntary job loss.

Keywords: Involuntary job loss, displacement costs, employment status, earnings losses, human capital, manufacturing industry

JEL: J21, J24, J30, J31, J60, J63, J64, J65

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1. Introduction

The rise of a fourth industrial revolution is changing the landscape in which global industries operate. Disruptive forces driving these changes include increased automation, development of advanced manufacturing technologies as well as demographic and geopolitical changes. Although these inevitable changes hold great promises for increasing overall productivity, there is reason for concern regarding what impact they will have on employment levels as millions of workers are estimated to lose their jobs involuntarily (OECD, 2015; World Economic Forum, 2016). Bearing this in mind, it is important to gain a better understanding of the economic impact that involuntary job loss – also known as job displacement – has on affected workers. Indeed, examining this issue provides important implications for how government institutions can manage the challenges facing workers.

In light of this background, the purpose of this study is to analyse the costs of job displacement incurred by displaced workers. The term cost refers to costs connected to the labour market position of affected workers, namely losses in earnings as well as difficulties in finding employment after being displaced. Moreover, we aim to investigate which is the main channel driving these costs. The main research question of this study is therefore:

What are the effects of job displacement on earnings and employment status?

Broadly defined, a displaced worker refers to an individual who suffers an involuntary job loss due to economic or technological changes leading up to organisational restructuring such as mass layoffs, firm closures and other forms of labour cutbacks (Kletzer, 1998). Through this definition, one can argue that displacement serves as an exogenous shock since the affected workers leave their jobs due to reasons unrelated to their ability or performance. In this study, we look specifically at workers who are displaced in connection with either plant closures or mass layoffs in the Swedish manufacturing industry. By focusing on closures and mass layoffs, we are able to capture the exogeneity of displacement. Moreover, we examine workers who were displaced between 1996 and 2009 and follow these individuals five years before and five years after displacement. This is, to our knowledge, the first Swedish study in the field of job displacement to examine a time period including the Swedish recession in the 1990's, the most recent financial crisis as well as the intermediate years. The chosen years allow us to study a period including various levels of economic stability.

Although it is well documented in previous literature that displaced workers tend to experience significant and persistent earnings losses after being displaced (e.g. Jacobson et al., 1993; Fallick, 1996; Sullivan & von Wachter, 2009), there are various reasons why the particular scope of this study is of interest from a theoretical perspective. To begin with, a clear majority of previous literature was conducted in the US. Literature is sparser and more inconclusive in the European and Swedish context. Existing literature in Europe show, similarly to studies from the US, a negative impact on the earnings of displaced workers. However, estimated effects are smaller in Europe (Eliason & Storrie, 2006; Hijzen et al., 2010; Huttunen et al., 2011). Furthermore, previous analysis regarding the main channel driving estimated losses in earnings have been contradictory. Some studies conclude that losses are attributed to longer periods of unemployment while others find them to be mainly driven by lower wages at the subsequent employer. With these limitations of previous literature in mind, we aim to contribute to existing research by further shedding light on the costs of job displacement in the European setting as well as the main channels driving the costs.

Conducting this study in Sweden does not only contribute to existing research through empirical findings in the European context. It is also particularly interesting since the Swedish labour market structure differs significantly from the US due to stronger employment protection laws, unionisation as well as a more generous welfare system (Eliason & Storrie, 2006). This difference has been found to play a significant role in determining the impact of displacement on workers (Kuhn, 2002). Moreover, Sweden serves as a suitable country for this type of study due to prevailing seniority rules stating that employers cannot choose whom to displace in mass layoffs. Instead, they follow a certain order in which the most recently employed worker is laid off first (SFS, 1982:80). This could reduce the risk of selection bias driven by workers being displaced due to unobservable factors such as lower productivity. Furthermore, the manufacturing industry has been identified to be one of the industries most severely affected in terms of job losses (OECD, 2015; Eliasson & Hansson, 2016) and is therefore particularly important to examine.

A majority of previous literature estimates the effect of displacement by comparing displaced workers to a control group of non-displaced workers from firms or plants not experiencing any significant economic downturn. This is also the case for the two Swedish studies closest to this paper in terms of scope (Eliason & Storrie, 2006; Eliasson & Hansson, 2016). We argue that using this type of control group induces a risk of overestimating the negative effect of displacement since high-performing firms can pay higher wages. Thus, the control group is more likely to enjoy systematically higher wages than the treatment group. In contrast, this study uses a control group consisting of workers who belong to the same plants as the displaced workers but who did not get displaced - so called *survivors*. By doing so, we avoid overestimating the costs of displacement. As far as we are aware, this is one of the first studies comparing displaced workers with survivors of displacement.

Using a longitudinal administrative registry data including an extensive list of worker-, plant- as well as region-level characteristics, we compare the results of an OLS regression, a regression with plant fixed effects and an individual fixed effects model. Doing so, we can assess the effect of displacement on earnings and employment status, in other words the probability of being employed at a given year. By comparing these models, we provide a better understanding regarding their robustness and impact on estimated results. We find evidence that an individual fixed effects model – also known as a generalised difference-in-differences approach – is most suitable when estimating the effect on earnings. This is an econometric method popularised by Jacobson et al. (1993) and has since then been used in various studies within this research area (e.g. Eliasson & Hansson, 2016; Eliason & Storrie, 2006). Meanwhile, the classical OLS regression is found to be more appropriate when estimating the effect on employment status.

We use the aforementioned models to estimate the effects of job displacement for each pre- and post-displacement year. The results indicate that displaced workers experience a significant loss in earnings during the first two years after displacement as well as a significantly lower probability of being employed in the years following displacement. In particular, we find that displaced workers incur a loss in earnings measured up to SEK 22,000 two years after being displaced, which corresponds to a percentage loss of approximately 7.6 percent. Moreover, the probability of being employed within two years after displacement is reduced by 15 percent. Although workers appear to never fully reach their pre-displacement level of earnings during the observed time period, they display a relatively fast recovery rate.

When analysing the channels driving the perceived earnings losses, we find them to be mostly attributed to longer periods of unemployment rather than lower earnings at the subsequent employer. Furthermore, we compare the estimated costs of job displacement across different subgroups and find older, high-tenured workers and workers with low education to be particularly vulnerable to displacement. That being said, the results are generally moderate as most workers manage to find employment relatively quickly without enduring large costs. This becomes apparent especially when comparing to US studies. In conclusion, we find the Swedish labour market with its employment protection laws and substantial support system to be relatively well-functioning in terms of managing workers who are displaced from their jobs.

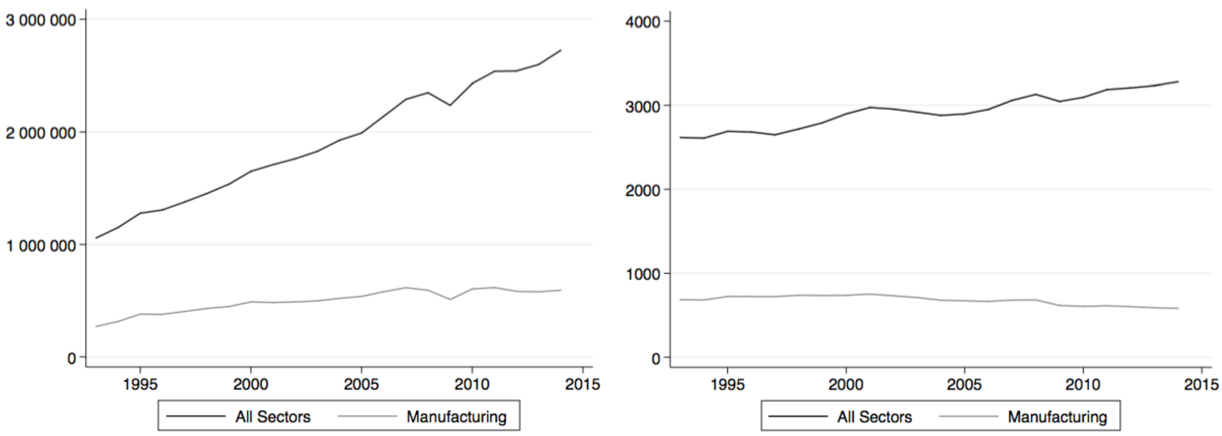
This study is organised as the following: Section 2 provides a background over the Swedish labour market. Section 3 provides a review of previous literature. The data used in the study as well as how the analysed sample is generated are described in section 4. Section 5 presents descriptive results for the sample. Section 6 reviews the empirical strategy used in the study. In section 7 the main results are presented and analysed. A discussion regarding policy implications, limitations as well as the validity of the study is provided in section 8. Lastly, section 9 presents a conclusion of the study.

2. Background

2.1. *The manufacturing industry in Sweden*

The manufacturing industry has always been an important component of the Swedish economy as it still accounts for almost 20 percent of total GDP (Carlgren, 2017). However, as shown in figure 1 (a) illustrating total value added to GDP, there has been a stagnation in the contribution to GDP from the manufacturing industry compared to the growth of all economic sectors combined. Furthermore, looking at the average number of employed workers over time, we see that there has been a decline in the manufacturing industry over the last 20 years. Contemporaneously, the rate of employed workers has been growing for all economic sectors. This confirms the previously discussed employment-related challenges facing the manufacturing industry. As automation processes and advanced manufacturing technologies reshape the industry the demand for labour decreases, making firms downsize their workforce (OECD, 2015). With that in mind, it becomes increasingly important to examine the impact of this trend on workers who lose their jobs.

Figure 1: The economic development of the manufacturing industry



(a) Value added to GDP over time (mn SEK) (b) Average number of employed over time (1000s)

Source: (Statistics Sweden, 2017)

2.2. *Regulations regarding job displacement*

In Sweden, the *Employment Protection Law* (EPL) serves as a protection for workers dismissed from their current employer as well as a tool used to increase job stability among employees (OECD, 2015).

The EPL states two main reasons for dismissals. The first reason is shortage of work, which often results in mass layoffs. The second reason is due to personal factors, for instance if the worker mismanages his or her job. One of the main components of the EPL is the *last-in-first-out* (LIFO) rule that regulates how employers should conduct dismissals linked to shortage of work.

The LIFO rule implies that a firm must negotiate with trade union representatives before executing a mass layoff. The aim of those negotiations is to manage mass layoffs so that they affect as few workers as possible. Furthermore, a firm cannot select whom to displace. Instead, mass layoffs are supposed to be executed in accordance to the LIFO rule which suggests an order of priority. This order favours workers with a longer employment period since they are prioritised to stay before workers with a shorter employment. Displaced workers are, in turn, prioritised for re-employment at the same firm up to nine months after displacement (SFS 1982:80).

There are, however, quite common methods to sidestep the LIFO rules (Eliason & Storrie, 2006). Furthermore, in 2001 some softening of the rules for small companies occurred. This resulted in firms with ten employees or less being allowed to exempt two workers from LIFO considerations (OECD, 2015).

2.3. *Support systems for displaced workers*

2.3.1 *Support in pre-displacement period*

The Swedish support system for displaced workers is mainly characterised by the norm that employers are primarily responsible for the workers whom they displace. One way in which this takes shape in practice is that displaced workers are often given a notice about their dismissal several weeks or months prior to the actual displacement. Hence, this is enabling the provision of support to affected workers already during the pre-displacement period (OECD, 2015).

Pre-displacement support is often provided by non-profit foundations known as *job security councils* (JSCs). JSCs often begin providing support to displaced workers already when they are given a notice of their dismissal. Support measures include planning job-seeking activities, labour market training programs, mental support and counselling as well as financial support. Moreover, advisors working at JSCs enjoy a high degree of freedom in adapting their services to fit the needs of each individual worker (Diedrich & Bergström, 2006).

The magnitude of JSCs' measures is often outlined in *collective bargaining contracts* between employers and labour union representatives and is financed by the employers. Although only available for union members, the support provided by JSCs covers today over 2 million workers and have been found to be successful in helping displaced workers find employment (OECD, 2015; Diedrich & Bergström, 2006).

2.3.2 *Wage bargaining systems*

Another type of support system prevalent in Sweden concerns the wage bargaining structure. Traditionally, firms facing economic downturn may choose to mitigate losses through labour cost reduction. This can be done through cutting wages, reversing previous wage increases as well as freezing future increases. However, since wages in Sweden are mainly regulated through collective bargaining contracts, the wage bargaining system in Sweden is rigid. Consequently, it is uncommon for employers to utilise these forms of interventions (OECD, 2015). In fact, the Swedish National Mediation Office (2015) reports that wages are regulated through collective bargaining contracts for nearly 90 percent of all workers in the labour market. Union organisations are very reluctant to accept wage reductions as it is considered to weaken their bargaining power and have a negative impact on future negotiations with employers. Therefore, firms are often forced to turn to other means to mitigate losses during economic downturn (OECD, 2015). That being said, there have been occasions in which union organisations are forced to accept wage reductions in order to avoid mass layoffs of workers and closures of entire plants. This happened for instance in the manufacturing industry during the financial crisis in 2009. Large firms such as Scania and Volvo came to agreement with union representatives to reduce wages, temporarily freeze wage increases as well as abolish employee bonuses in order to reduce the magnitude of mass layoffs (Lovén, 2009; Mandl et al., 2009).

Another matter related to wage bargaining systems worth highlighting concerns *severance payments* - a form of compensation paid out to employees in connection to displacement. This kind of compensation is often offered in the event of an unjust displacement and is very common in Sweden. In fact, severance payments in Sweden are among the highest across OECD countries. Because of the strong degree of unionisation in Sweden, negotiations in connection with displacements often involve some kind of severance payment agreement that could potentially reduce the monetary costs of job loss in the short run (OECD, 2015).

3. Literature Review

3.1. *Overview of job displacement literature*

The costs of job displacement for workers have mainly been studied through two perspectives. Some studies focus on the *survivors of displacement*, in other words employees who remain at the workplace after displacement (e.g. Clarke & Patrickson, 2001; Devine et al., 2003; Bennett et al., 1995; Maertz et al., 2010; Brockner & Greenberg, 1990). Other studies choose to focus on the displaced workers - also known as *victims*. This is often done through comparison studies in which they are compared to a control group of non-displaced workers (e.g. Jacobson et al., 1993; Hijzen et al., 2010; Eliason & Storrie, 2006; Huttunen et al., 2011; Eliasson & Hansson, 2016).

Research on survivors predominantly examines the phenomenon which scholars refer to as *the survivor syndrome* - a term describing survivors' anger, guilt, perceived unfairness, depression and trust in management after the event of a mass layoff (Baruch & Hind, 2000). The survivor syndrome is found to have a significant negative effect on survivors' overall level of productivity (Wang-Bae, 2003; Cascio, 1993). Studies within this field are usually based on survey data and the reviewed costs of displacement are mostly stress-related rather than monetary (e.g. Maertz et al., 2010; Brockner & Greenberg, 1990; Appelbaum et al., 1997). To our knowledge, only few studies that compare survivors to victims in terms of monetary outcomes have been conducted (Devine et al., 2003).

Considering studies focusing on victims, the vast majority examines outcomes related to workers' labour market status such as earnings and incidence of unemployment (e.g. Jacobson et al., 1993; Eliason & Storrie, 2006; Hijzen et al., 2010; Eliasson & Hansson, 2016). Since it is the perspective relevant for this paper, we choose to dedicate the remaining part of this literature review to examine the findings of these studies. Generally, all literature presented below estimate the effect of displacement by comparing displaced workers to a control group of non-displaced workers from firms or plants not experiencing any significant economic downturn. Thus, our study contributes to the existing body of literature by using survivors as a control group.

3.2. *The characteristics of displaced workers*

In terms of demographic characteristics of displaced workers, it is found both in the US as well as in Europe that workers belonging to the youngest and oldest age groups are most likely to be displaced (OECD, 2015; Kletzer & Farlie, 2003; Huttunen et al., 2011). A stylised fact from job stability literature, which could explain why young workers are more vulnerable to layoffs, is that *new jobs end early*. Furthermore, institutions such as the seniority rules prevailing in Sweden can enhance this mechanism (Eliason & Storrie, 2006). Despite their vulnerability, previous studies show that young workers tend to find employment relatively quickly after displacement compared to older, prime-aged workers (OECD, 2015; Kletzer & Farlie, 2003). Huttunen et al. (2011) explain the large fall in earnings and probability of finding employment for older workers as an effect of them retiring earlier because of displacement. Couch and Placzek (2010) explain the loss in earnings for older workers as a result of them being high-tenured. This is discussed more in detail in the last section of this literature review.

Another established finding is the negative correlation between education level and the likelihood of being displaced. Low-educated workers are more likely to be displaced than high-educated workers. They are also more likely to experience larger long-lasting losses in earnings as a result of displacement (Kletzer, 1998; Eliason & Storrie, 2006; Huttunen et al., 2011; OECD, 2015). Farber (1996) finds that displaced workers holding a university degree enjoy a 16 percentage point higher probability of finding employment than workers with a high school degree. This can be partially explained by the notion that high-educated workers possess more transferable human capital that allows them to find employment at a relatively fast rate (Huttunen et al., 2011).

In terms of gender differences, studies have generally found that men are more likely to be displaced than women (Fallick, 1996). However, this seems to be driven by the types of jobs that men and women hold, with men being more overrepresented in particularly affected sectors such as the manufacturing industry (OECD, 2015; Eliason & Storrie, 2006). Furthermore, men usually lose relatively more in earnings than women (Eliasson & Hansson, 2016; Hijzen et al., 2010). In contrast, women have displayed more difficulty to re-enter employment once they have been displaced (OECD, 2015).

In theory, losses are expected to differ depending on whether the worker is displaced through a closure or a mass layoff. Gibbons and Katz (1991) suggest that workers displaced through closures should experience lower losses in earnings than those displaced through mass layoffs. The explanation behind this is that some workers, expectantly low-performing and unproductive, are likely to get displaced in mass layoffs. However, empirical results vary where some studies find larger losses for workers displaced through mass layoffs (e.g. Gibbons & Katz, 1991; Fallick, 1996; Podgursky & Swaim, 1991) while others find no difference (e.g. Stevens, 1997). Moreover, the findings differ from country to country. For instance in the Swedish labour market context it is found that workers displaced through closures suffer larger losses in earnings (Eliasson & Hansson, 2016).

3.3. *Empirical results across countries*

As noted by Hijzen et al. (2010) and Huttunen et al. (2011), the literature on the costs of job displacement in the US context is vast. The most influential paper is written by Jacobson et al. (1993). Using data from Pennsylvania in the period 1970 to 1980, the authors are able to compare a group of displaced workers with non-displaced workers. Their findings suggest large, persistent decreases in earnings due to job separation. In particular, the authors estimate a loss of 40 percent in earnings for displaced workers. Displaced workers also seem to lose earnings already before job separation – a pattern often referred to as Ashenfelter's dip[1]. Earnings start to recover rapidly after the drastic initial drop. The recovery seems, however, to stagnate and the authors find earnings for displaced workers to still be 25 percent lower than for non-displaced workers six years after displacement. These losses are found to be largely due to "lower earnings for those who work, rather than an increase in the number of workers without quarterly earnings" (Jacobson et al., 1993).

A more recent paper by Couch and Placzek (2010) revisits the study by Jacobson et al. The authors find the estimates of Jacobson et al. (1993) to be relatively large since the data used cover a period of disproportionately high amount of job losses in the manufacturing sector. In their study, Couch and Placzek intend to generalise the findings of Jacobson et al (1993) to a more favourable economic state. They use data between 1993 and 2004 in Connecticut to estimate the impact of job displacement on earnings. The study shows a drop of 33 percent in earnings directly after displacement and six years after the earnings losses range from 13 to 15 percent. Thus, their study demonstrates that under more regular economic states, estimates are smaller yet still negative and persistent. In a different study by Stevens (1997), it is also found that the negative effects of displacement on earnings are persistent. Even six years after displacement the earnings for displaced workers are lower than for non-displaced workers. With these findings in mind as well as many other studies providing similar results, there seems to be a growing consensus among US studies that job displacement leads to a persistent and negative effect on earnings (e.g. Stevens, 1997; Fallick, 1996; Ruhm, 1991; Podgursky & Swaim, 1987).

In contrast, the European supply of displacement literature is substantially smaller, although it has become a growing field in recent years. Studies in Europe often find estimated effects on earnings to be smaller than in US literature (Huttunen et al., 2011; Carneiro & Portugal, 2006; Hijzen et al., 2010). The European studies provide a variety of estimated effects as the perceived losses in earnings range from 1 to 35 percent and the magnitude seems to vary depending on labour market context.

[1]Ashenfelter's dip was found by Ashenfelter (1978). Workers participating in working programs experience a dip in earnings right before entering the program. This can be driven by the notion that workers actively stop applying for jobs knowing that they will enter the program. A similar argument can be made for displaced workers, where they suspect they will get displaced and thus work less.

One of the more recent European studies in this topic was conducted by Huttunen et al. (2011), in which the authors analyse the short- and long-term effects of job displacement in Norwegian manufacturing plants. They find that after seven years, displaced workers have on average 3 percent lower wages than non-displaced workers. The authors also find the effect to decrease with plant size. Moreover, they conclude that the observed costs are mainly driven by the workers who find employment in a different firm - so called between-firm movers. Another study by Hijzen et al. (2010) estimates earnings losses resulting from firm closures and mass layoffs in the United Kingdom. The results show that yearly earnings losses vary between 14 and 35 percent. Further, Couch (2001) studies German workers only displaced through firm closures. The findings show that in the year of displacement annual earnings for displaced workers drop with around 14 percent and two years later the loss in earnings are reduced to 7 percent.

There has also been a few studies of displacement conducted using Swedish data (e.g. Eliason & Storrie, 2006; Eliasson, 2013; Eliasson & Hansson, 2016). The estimated percentage loss in earnings in those studies ranges from 8 to 10 percent. One of the most influential Swedish studies conducted by Eliason and Storrie (2006) uses administrative Swedish registry data to identify workers displaced in 1987 due to firm closures. They find that directly after unemployment the earnings differential amount to approximately 8 percent (SEK 11,500[2]). Furthermore, by the time of the Swedish recession in 1993, the difference in earnings between displaced and non-displaced workers increases. The authors also estimate the effect of displacement on the probability of re-employment and find it to decrease by 8 percentage points. Through their study, Eliason and Storrie conclude that displaced workers are more vulnerable to subsequent macroeconomic shocks.

In another Swedish study, Eliasson (2013) examines the impact of displacement on workers being displaced in the recession of 1993, and compares this with workers displaced in the recent financial crisis of 2008. His findings suggest that the crisis of 1993 had a harder impact on displaced workers compared to the financial crisis. Eliasson finds an immediate drop of SEK 22,000 and 25,000 after the crises. In a study by Eliasson and Hansson (2016), the authors look at how displaced workers are affected in various sectors. They compare the effect on earnings for displaced workers within the manufacturing industry, tradable and non-tradable services. The authors confirm that displaced workers from the manufacturing industry are more severely affected by earnings losses. Moreover, manufacturing workers incur losses in earnings of SEK 30,000 the year after displacement. Eliasson and Hansson further confirm that the losses in earnings are mostly adhered to longer periods of unemployment rather than lower wage levels in the subsequent jobs of the displaced workers.

[2]All losses in this section are expressed in 2009 price level.

3.4. *The drivers of job displacement costs*

Despite differences in magnitude, the drop in displaced workers' earnings after separation is nowadays considered a stylised fact. However, it is often discussed which channels drive the effect. The two possible channels are:

1. Lower wages at subsequent job leading to a drop in earnings after displacement
2. Longer periods of unemployment leading to lower annual earnings which itself leads to a drop in earnings after displacement

Hijzen et al. (2010) and Bender et al. (2002) claim that in Europe the loss in earnings is usually driven by longer periods of unemployment. Meanwhile, in America the loss is foremost driven by lower wages at subsequent jobs (e.g. Kruse, 1988; Ruhm, 1991).

The channels driving the persistent earnings losses can be explained by various mechanisms brought up by Carrington and Fallick (2017). Initially, the authors suggest *job matching* as a mechanism. In a job matching theory developed by Jovanovic (1979) it is advocated that a productivity component is specific to a particular match between employees and firms. However, in opposite to human capital theory this component of productivity is fixed over time. That is, some individuals match better to some jobs than others.

A second mechanism is the *revelation of information or signalling.* After displacement, employers could suspect that displaced workers are of lower quality. Consequently, these workers receive lower wages or have difficulties finding employment (Gibbons & Katz, 1991). Further, the mechanism of *back-loaded compensation* suggests that workers tend to receive a level of earnings that exceeds the value of their productivity level because employers want to attract more stable workers (Carrington & Fallick, 2017). In addition, workers might receive excessively high earnings due to *strong regulations or high levels of unionisation* (Fallick, 1996). The consequence of the latter two mechanisms is that displaced workers receive lower earnings that better match their actual level of productivity at the subsequent employer.

Among others Eliason (2011) suggests the mechanism of *intra-household reallocation of income.* As a worker gets displaced, the spouse could temporarily enter the labour market or take on more work in order to compensate for the reduction in household income. Consequently, the displaced worker seeks employment with less hours of work and thereby receives lower annual earnings since someone else in the household mitigates the costs. Moreover, as a result of displacement the displaced workers often suffer from *mental or physical illness* which might affect their ability to work after displacement and thus also reduce their earnings (Eliason & Storrie, 2009).

Lastly, Carrington and Fallick (2017) present *loss of specific human capital* as one of the main mechanisms behind the persistent drop in earnings after displacement. Loss of specific human capital is the most frequently discussed driving mechanism in previous research (e.g. Jacobson et al., 1993; Fallick, 1996; Kletzer, 1998; Huttunen et al., 2011). The theory of human capital was first brought forward by Becker (1962), who argued that workers possess firm-specific human capital, which is accumulated through on-the-job training and tenure. According to Becker, firm-specific human capital raises the productivity of workers, which in turn leads to higher wages. Upon job loss, workers lose their firm-specific human capital as only their current employer values it, thus resulting in difficulties to find employment as well as reduced wages at subsequent jobs. All in all, firm-specific human capital is said to not be transferable across firms.

Empirically, this has been reflected by the findings that high-tenured displaced workers tend to suffer longer periods of unemployment as well as lower earnings (Jacobson et al., 1993). In fact, it is found that one additional year of tenure is associated with a loss in earnings of 1 to 1.3 percent and around 2 to 5 percent longer periods of unemployment (Farber, 1996; Podgursky & Swaim, 1987). It is thus clear that specific human capital is a determining factor for workers' ability to find employment in other firms. Since first brought up by Becker, the theory has also been expanded to include other forms of specific human capital such as industry-, region- as well as job task-specific human capital (Neal, 1995; Huttunen et al., 2011; Huttunen et al., 2015; Fackler & Rippe, 2017).

4. Data and sample retained for analysis

4.1. *The LISA Database*

This study is conducted using administrative data derived from a longitudinal integration database for health insurance and labour market studies (LISA by Swedish acronym). The database is distributed by Statistics Sweden. LISA contains annual data on all individuals of age 16 and older who are registered in Sweden at least one year during the period 1990 to 2014. Since the database is longitudinal, each individual can be followed through the entire time period in which he or she is included in the population. Furthermore, the database contains unique identity numbers for each individual which can be used to link an individual to their family as well as to the firm and plant they work at. Thus, users of LISA are allowed to incorporate annual data on family, firm and plant characteristics for each individual. The information included in the database is extensive and can be divided into six categories, namely demographic, education, employment[3], income, family and firm- as well as plant-level variables (Werke & Ekmark, 2017).

4.2. *Advantages of administrative data*

The vast majority of literature regarding job displacement conducted in the US as well as some studies in Europe are composed using survey data (e.g. Farber, 1996; Farber, 2017; Neal, 1995; Fackler & Rippe, 2017). Despite surveys being a popular approach to collect displacement data it has numerous shortcomings that can distort empirical results. Some of these weaknesses are discussed in the paper by Jacobson et al. (1993) and include lack of a control group of non-displaced workers and smaller sample sizes. By using administrative data, we can avoid these issues since it in general provides a larger sample size as well as the possibility to acquire suitable control groups.

It is also found that survey data can be contaminated by recall bias (Jacobson et al., 1993)[4]. In the context of displaced workers, it is observed that survey participants are often unable to correctly recall pre-displacement earnings and tend to overstate them (Oyer, 2004). This is not an issue in this study since the reported earnings are based on official data from government agencies such as the Swedish tax agency (Statistics Sweden, 2016). Moreover, it is documented that in survey data workers are less likely to report job losses that were experienced long periods before the survey is conducted. This inaccuracy would result in a possible under-reporting of displacements (Jacobson et al., 1993). The issue is eliminated by

[3]Data for employment variables such as a binary variable indicating employment status, employer identification number etc. refer to workers' status as of November each year.

[4]Recall bias is a term derived from the research area of epidemiology and is defined as a "systematic error due to differences in accuracy or completeness of recall to memory of past events or experiences" (Porta, 2008, p. 240).

using administrative data as we are able to track all displacements experienced by workers in the data set for the studied time period.

Furthermore, survey data can generally suffer from sample attrition as researchers tend to lose observations over time, thus resulting in a smaller sample size. Attrition can also lead to biased inferences if the loss of observations is non-random (Eliason & Storrie, 2006). In contrast, sample attrition can be significantly reduced when using administrative data since the collection of data is not dependent on the willingness of individuals to participate.

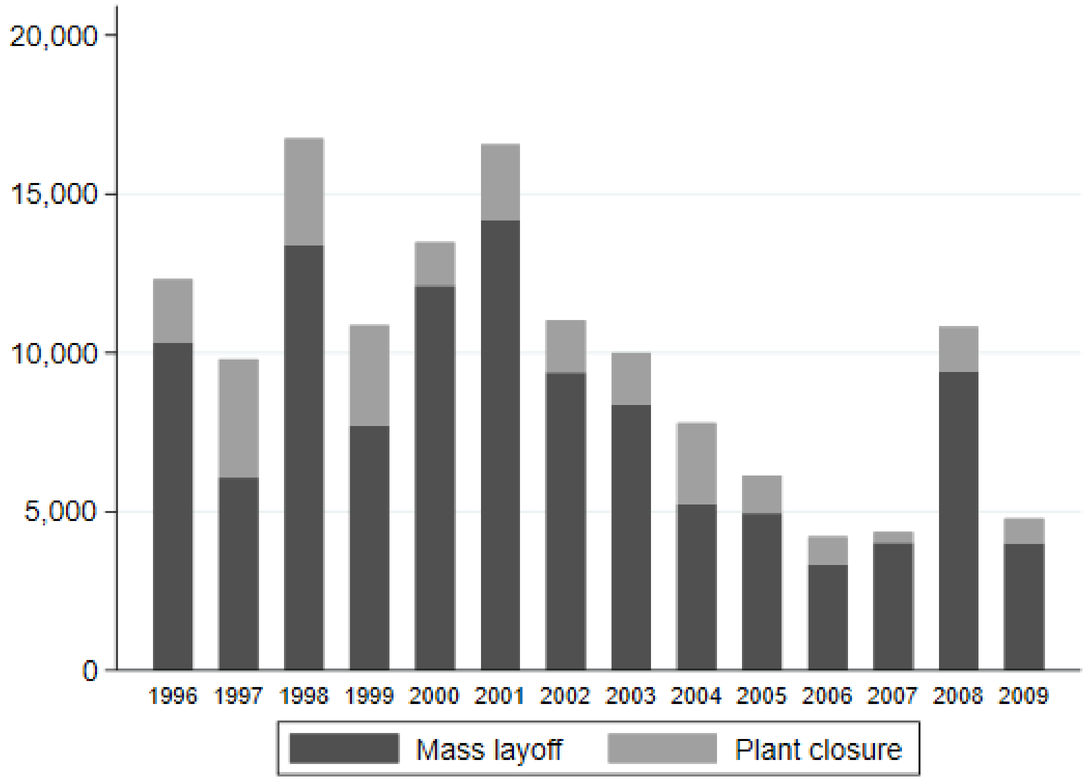
4.3. *Definition of displacement*

As mentioned, a displaced worker is broadly defined as a worker being involuntarily separated from his or her current job due to reasons beyond his or her control (Kletzer, 1998; Huttunen et al., 2011; Eliasson & Hansson, 2016; Fackler & Rippe, 2017). Thus, displacement serves as an exogenous shock since the affected workers leave their jobs due to reasons unrelated to their ability. However, one of the main issues when studying job displacement is the inability to distinguish between a voluntary separation and a true displacement (Fallick, 1996; Kletzer, 1998). This issue is also prevalent in this study since the database LISA does not record whether a job separation is voluntary or a result of an involuntary, employer-initiated separation. There are two established methods to deal with the aforementioned issue and capture the exogeneity of displacement. Either, one can focus solely on workers displaced in connection with plant or firm closures (e.g. Hijzen et al., 2010; Eliason & Storrie, 2006; Eliason, 2011), or one can examine displacements through both closures and sufficiently large mass layoffs (e.g. Huttunen et al. 2011; Boman, 2011; Eliasson, 2013; Sullivan & von Wachter, 2009).

In this study, we choose to follow the latter method and examine displacements both in connection with mass layoffs and closures. We include mass layoffs because it is found that the clear majority of job displacements in the Swedish economy is due to mass layoffs and not due to closures (Ohlsson & Storrie, 2006; Eliasson, 2013). Additionally, this is found in our data. Figure 2 illustrates the number of displaced workers between 1996 and 2009 according to displacement type[5]. As seen, a majority of displaced workers in this time period lose their jobs in connection to mass layoffs rather than closures. In particular, of the 220,855 workers who were displaced during this period, 81 percent were displaced through mass layoffs and 19 percent through plant closures. Thus, only examining closures would limit the study to a small portion of the total amount of displacements in the economy and could potentially reduce the applicability of the results on displacements other than through closures.

[5] In this figure, a worker is considered to be displaced at year t if the worker got separated from his or her job between November year t and November year $t+1$. Consequently, the majority of workers registered as displaced in 2008 were actually displaced during 2009, which explains why the number of displaced workers is much higher in 2008 compared to 2009.

Figure 2: Number of displaced workers according to displacement type



Source: Authors' rendering of Statistics Sweden data (2016)

With this in mind, we use the following definition of a displaced worker:

A displaced worker is an individual separated from a plant between year t and year $t+1$, where the plant had at the time of displacement at least 50 employees and:

- The plant closed down between November year t and November year $t+1$ (plant closures), or
- the plant experienced an absolute reduction of at least 50 workers and a relative reduction of at least 30 percent of the workforce between November year t and November year $t+1$ (mass layoffs)[6].

Note that this definition allows us to include workers who were displaced between year t and $t+1$ from the plant they worked at but who got employed at a different plant between these years. The definition does therefore not require that a worker is unemployed at $t+1$, but only that he or she no longer works at the plant he or she was separated from.

[6]Data for employment variables are registered at November each year. This includes the displacement status of workers, which uses the employer identity number to define if a worker was displaced or not.

4.3.1 *Defining displacements at the plant level*

We restrict the sample to include plants with at least 50 workers. Firstly, plants with ten or less workers are exempted from the LIFO rules, allowing them to conduct layoffs differently than larger plants (see section 2.2). Secondly, by excluding smaller plants we reduce the risk of including voluntary job separations in the sample. Suppose smaller plants would be included – say plants of ten workers. A layoff of 30 percent would in this scenario imply three workers leaving the firm. Understandably, it is hard to motivate a reduction of three workers as a mass layoff and thus an involuntary separation. Furthermore, we choose a 30 percent reduction of workers since this is the conventional definition of a mass layoff (Hijzen et al., 2010; Huttunen et al., 2011; Jacobson et al., 1993).

Moreover, the decision to use layoffs and closures at the plant-level instead of firm-level enables us to control for possible overestimations of the displacement rate. In general, firms' identity numbers are not stable as they tend to change in correspondence to organisational restructurings such as mergers, acquisitions or changes of ownership. Thus, defining displacements at the firm-level creates a considerable risk of misclassifying these changes in firm identity numbers as firm closures. This is a phenomenon also known as *false firm deaths*, which in turn could lead to an overestimation of the true displacement rates (Eliasson, 2013; Eliason & Storrie, 2006). In contrast, plant identity numbers are more stable over time as they rarely change in connection with organisational restructurings of this kind. In addition, Statistics Sweden has taken measures to explicitly create time consistent plant identity numbers, in particular for plants with ten or more employees (Gamerov, 2017; Eliasson & Hansson, 2016). There are, however, some instances in which the identity number of a plant changes such as in connection with mergers of two plants or a split of one plant into two. In those cases, the disappearance of the initial plant identity number could be wrongly classified as a plant closure. Favourably, Statistics Sweden registers whether the disappearance of a plant identity number is due to a merger, split or an actual closure (Gamerov, 2017). Thus, we are able to eliminate all such cases of false plant closures.

4.4. *Further data restrictions*

One potential problem is that some workers registered at the plants have short term contracts with the employer, meaning that they leave the firm voluntarily. Presumably, this is common among younger workers without an established working history. Another issue might be that older workers leave voluntarily in time for retirement which could lead to a misclassification of them as displaced[7]. To address these issues, we restrict the sample to include only workers of age 21 to 60 years old at the year in which the event of displacement occurs. Previous studies from Sweden have a more stringent restriction on age and only include workers between

[7] The standard retirement age in Sweden is 65.

ages 20 and 50 (e.g. Eliason & Storrie, 2006; Eliasson, 2013). The reason for this is that the authors want to exclude older workers leaving the labour force for an early retirement. However, Huttunen et al. (2011) argue that if the sample excludes early retirees the effect of displacement can be severely underestimated. Thus, we find our age restriction to be suitable.

We also impose a restriction considering the pre-displacement employment status of the workers, namely that they must have been employed in years $t-5$ and $t-4$. This restriction is useful for two reasons. To begin with, the restriction makes the pre-displacement characteristics of non-displaced and displaced workers more similar and thus enhances the matching between the two groups, which is necessary when conducting comparisons between a treatment and control group. Moreover, the restriction creates a sample consisting of workers with stronger labour market attachment. Including workers with weaker attachment to the labour market can distort the estimations. Such workers could, for instance, be more prone to leave their job voluntarily in order to study or travel. Thus, only including workers with stronger attachment to the labour market reduces the risk of misclassifying voluntary job separations as job displacements.

We also restrict the sample to only include workers with a registered employment status for each year. Thereby, we eliminate attrition caused by deceased workers or workers who have emigrated. This allows us to reduce the risk of having sample attrition bias without significantly reducing the sample size.

4.5. *Data structure*

Having determined the sample restrictions, we proceed by constructing the sample retained for analysis as following. Firstly, we collect data on workers from each year of 1996 to 2009 who worked at a plant where displacement occurred. Each of these years constitute a base year which is denoted by t. For every base year we separate the sample into a control and a treatment group. The treatment group contains workers displaced in connection with plant closures or layoffs between the base year t and $t+1$. The control group contains workers who were not displaced between the base year t and $t+1$ from the plants that experienced mass layoffs. We follow each worker five years prior to the base year (from $t-5$) as well as five years after (to $t+5$). Thus, the data set is a balanced panel data, containing observations from 1991 to 2014 for a total of fourteen cohorts. For the purpose of estimation, we stack the cohorts so that each base year gets denoted by a t, each year following the base year gets denoted by $t+1$ and so on. Through this, we are able to analyse the pooled effect of displacement. Figure 3 illustrates how each of the cohorts represented in the rows are pooled forming a time variable that takes on the values $t+j$ where $j \in \{-5,-4,...0,...4,5\}$.

Figure 3: Pooling the cohorts

$t-5$	$t-4$	$t-3$	$t-2$	$t-1$	t	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$
1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001
1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002
1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003
...	...	...	...	...	...	...	...	...	...	...
2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014

By constructing the data this way, we acquire a sample structured as exemplified in table 1. That is, we follow each worker from $t-5$ to $t+5$. Note that the variable *displaced* in the table is a binary variable equal to 1 if a worker was displaced during the observed base year. Thus, we can for instance observe the earnings over time for worker 1 who was displaced at base year 1996. We can also observe the earnings over time of worker 2 who was not displaced in base year 1996.

Table 1: The structure of the sample

id	base year	time	displaced	earnings
1	1996	$t-5$	1	63411
1	1996	$t-4$	1	62301
...	...	...	...	...
1	1996	t	1	57086
1	1996	$t+1$	1	200
...	...	...	...	...
1	1996	$t+5$	1	46971
2	1996	$t-5$	0	34976
...	...	...	...	...
2	1996	$t+5$	0	35610
...	...	...	...	...
25002	1999	$t+1$	1	47111

5. Descriptive results

Given the sample restrictions described in the previous section, we end up with a sample containing 2,429,405 observations for a total of 220,855 workers. Of these workers, 138,862 (63 percent) are displaced and 81,993 (37 percent) are non-displaced. Table 2 illustrates the distribution of displaced and non-displaced workers in each base year cohort.

Table 2: Number of displaced workers at each base year cohort

	Non-displaced	Displaced
1996	6857	12315
1997	3182	9801
1998	6737	16754
1999	3700	10863
2000	9578	13472
2001	9824	16558
2002	5841	11026
2003	5197	9995
2004	5157	7788
2005	3745	6137
2006	2183	4208
2007	3071	4357
2008	15046	10810
2009	1875	4778
Total	81993	138862

With the exception of 2008, the treatment group is consistently larger than the control group[8]. The reason for this is that we include plants undergoing layoffs with a minimum of 30 percent reduction of workforce as well as plants which close down entirely. Furthermore, the sample includes a total of 999 plants of various sizes. As presented in table 3, the size of the plants ranges from 50 to 3,890 workers, with an average of approximately 699 workers.

Table 3: Plant characteristics

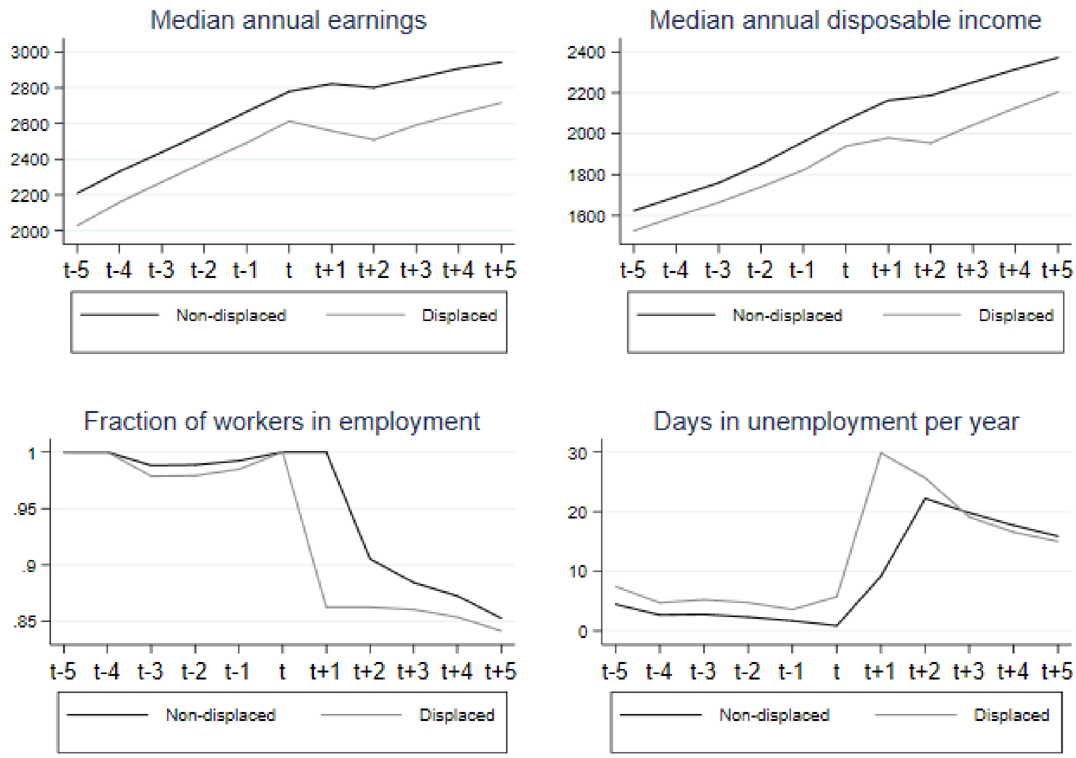
Number of plants	Avg. Plant size	Min. Plant size	Max. Plant size
999	699	50	3890
	(830)		

Mean standard error is given in parenthesis

[8]Due to the financial crisis larger plants are included in the data which increases the proportion of non-displaced workers.

As shown in figure 4, the pattern for earnings and disposable income is very similar over time. Earnings are defined as the annual pre-tax work-related income, while the disposable income is defined as total annual post-tax income including non-work related income such as allowances [9]. To start with, non-displaced workers have consistently higher earnings and disposable income. Still, the two groups seem to have similar growth in earnings over time. The earnings for displaced workers drop at the time of displacement and seem to return to pre-displacement growth rate after $t+2$. Despite the recovery, the gap in earnings between displaced and non-displaced is bigger than during the pre-displacement period. The same can be concluded about the median disposable income for the two groups.

Figure 4: Comparison of labour market outcomes



The fraction of workers in employment at each year and the average days of unemployment follow the same path for both displaced and non-displaced workers during the pre-displacement years. This is partly driven by the previously discussed employment restriction imposed on the data. Furthermore, as expected, the graphs clearly diverge after the event of displacement. It is noteworthy that the fraction of workers in employment among non-displaced workers also decreases after displacement. This is due to the fact that the plants where the workers are employed are experiencing economic downturns and can therefore conduct additional downsizing in subsequent years. The same patterns can be seen for the average days of unemployment per year.

[9]Both earnings and disposable income are deflated by 2009 consumer price index.

Table 4: Sample means of selected pre-displacement characteristics

	Displaced	Non-Displaced	Diff.	Displaced Employed	Displaced Unemployed	Diff.
Age	42.66	43.58	0.92***	42.10	46.18	4.08***
Male	0.70	0.71	0.01***	0.71	0.59	-0.12***
Domestic partner	0.62	0.63	0.01***	0.63	0.56	-0.07***
Foreign	0.13	0.13	0.00	0.11	0.19	0.08***
Children home	0.54	0.53	-0.01	0.56	0.42	-0.14***
Infant	0.14	0.13	-0.01***	0.15	0.09	-0.06***
No high school	0.22	0.22	0.00	0.20	0.36	0.16***
High school	0.50	0.53	0.03***	0.50	0.53	0.03***
University	0.27	0.25	-0.03***	0.30	0.12	-0.18***
Experience	23.49	24.22	0.73***	22.87	27.33	4.46***
Tenure	3.16	3.93	0.77***	3.07	3.72	0.65***
Employed t+2	0.86	0.91	0.05***			
Metropolitan region	0.22	0.27	0.05***	0.22	0.23	0.01***
Urban region	0.66	0.64	-0.02***	0.67	0.65	-0.02***
Rural region	0.12	0.10	-0.02***	0.12	0.12	0.00
Avg Pre-disp earnings	2528.63	2662.62	133.99***	2609.15	2023.06	-586.09***
Earnings t-5	2163.05	2311.97	148.92***	2211.11	1861.24	-349.87***
Earnings t+2	2663.60	2941.33	277.73***	3048.68	245.58	-2803.10***
Disposable income t-5	1649.01	1732.00	82.99***	1670.64	1513.18	-157.47***
Disposable income t+2	2211.44	2376.79	165.35***	2335.918	1429.84	-906.07***
N	138,862	81,993		119,786	19,076	

Unless stated otherwise, all variables refer to mean values for year t. The binary variable *Male* is equal to 1 if a worker is a male. *Domestic partner* is a binary variable equal to 1 if a worker has a domestic partner. *Foreign* is a binary variable equal to 1 if a worker was born in a different country than Sweden. *Children home* is a binary variable equal to 1 if a worker has at least one child living in the same household. *Infant* is a binary variable equal to 1 if a worker has at least one child in the age 0-3. *Experience* is a variable measuring a worker's total amount of work experience, defined in accordance with Heckman et al. (2008) as $age - years\ of\ education - 6$. *Tenure* is defined as the number of years an individual has worked at the plant, measured up to five years prior to displacement. Finally, the binary variables indicating type of region (*metropolitan*, *urban* and *rural region*) use a definition of region type based on the population size and density, with metropolitan regions being the largest and most dense while rural regions are the smallest and least dense (Pichler, 2011).
* indicates that the estimate is significant at a 10% significance level.
** indicates that the estimate is significant at a 5% significance level.
*** indicates that the estimate is significant at a 1% significance level.

Table 4 shows the means for a selected number of pre-displacement characteristics. Reviewing the sample distributions in terms of gender, we find that a clear majority, approximately 70 percent, are men. This is expected given that we examine the manufacturing industry which is largely dominated by men. Statistics from 2010 show that only 23 percent of the workforce within the Swedish manufacturing industry were women (Svenskt näringsliv, 2011). Furthermore, the average ages presented in the table range between 42-46 years, thereby indicating that the sample consists mostly of middle-aged workers. This is expected given the restriction that workers need to be employed between $t-5$ and $t-4$ which might exclude younger workers. Finally, in terms of education levels, the sample is dominated by workers who only hold a high school degree. Almost 51 percent of the workers belong to this group while only 27 percent hold a university degree.

We further compare and test the mean differences between displaced and non-displaced workers using t-tests. Likewise, we compare the mean values for displaced workers who are employed in $t+2$ with workers who are unemployed at the same point in time. For displaced and non-displaced workers, we find that all mean differences between the two groups are statistically significant at the 1 percent significance level with the exception of the variables *Foreign*, *Children home* and *No high school.* Non-displaced workers are somewhat older, more experienced, and have higher tenure than displaced workers. This is in line with previous studies and no surprise given that the choice of whom to displace is mainly determined by the seniority rules. Note, however, that the tenure variable is truncated in the sense that tenure can only be observed up to five years prior to the event of displacement. Hence, the mean values of the tenure variable are very low. This issue is due to lack of data and has been a problem in previous studies as well (e.g. Eliason & Storrie, 2006; Hijzen et al., 2010). Further, a larger fraction of non-displaced workers live in metropolitan regions, while a larger fraction of displaced workers live in the more remote regions. Lastly, displaced workers have approximately SEK 13,340 lower pre-displacement average earnings than non-displaced workers, which is in line with the outcome in figure 4.

Comparing displaced workers employed at year $t+2$ to those who remain unemployed at that time, we note that all mean differences are statistically significant at the 1 percent level except the rural region indicator. Unemployed workers are on average four years older and have higher tenure than employed workers. This is in line with previous studies suggesting that older, high-tenured workers are unsuccessful in finding employment after displacement. The displaced, employed workers have higher levels of education than the unemployed workers which could be explained by the notion that more education implies a more transferable human capital. In addition, there are more women in the group of displaced, unemployed workers. Lastly, at year $t+2$ the earnings of displaced, employed workers are slightly greater than those of non-displaced workers indicating that the large difference in earnings between displaced and non-displaced could be mainly driven by the displaced, unemployed workers.

6. Empirical strategy

We choose to conduct two main estimations in order to examine the costs borne by displaced workers. Our first estimation concerns the effect of displacement on earnings. Secondly, we estimate the effect of displacement on employment status, in other words the probability of being employed.

Note that when comparing a treatment and control group over time, the main identifying assumption is the *common trends assumption* (Angrist & Pischke, 2009, pp. 230-231; Lechner, 2011). Mathematically, the assumption can be written as:

$$\begin{aligned} &E(Y_{t+}|X_t = x, D_t = 1) - E(Y_{t-}|X_t = x, D_t = 1) \\ &= E(Y_{t+}|X_t = x, D_t = 0) - E(Y_{t-}|X_t = x, D_t = 0) \\ &= E(Y_{t+}|X_t = x) - E(Y_{t-}|X_t = x) \end{aligned} \tag{Common trends}$$

Y_{t+} represents the outcome of interest in a post-displacement period, for example earnings at year $t+2$. X_t represents a set of control variables which can be both time-invariant and time-varying. Y_{t-} represents the outcome of interest in a pre-displacement period. D_t is a binary variable ($d \in \{0,1\}$) equal to 1 if an individual is a displaced worker and equal to 0 if an individual is a non-displaced worker. The assumption states that the treatment and control group have common trends, implying that the trends in the outcome of interest would have been identical for displaced and non-displaced workers in the absence of displacement. By constructing a model that fulfils this assumption, we can properly identify the causal effect of displacement. Thus, we conduct the estimations on earnings and employment status using the following three types of models:

1. Classical ordinary least squares (OLS) regression
2. Regression with plant fixed effects
3. Regression with individual fixed effects

Using these three models, we intend to estimate the difference between the actual outcome of a displaced worker and the expected outcome of the worker had he or she not been displaced. The latter is approximated by the outcomes of non-displaced workers. We describe and motivate each of these three models in detail below.

6.1. *Classical OLS Regression*

Our first regression using classical OLS has the following specification:

$$Y_{ib,t+j} = X_{ib}\beta + Z_{ib}\Upsilon + \sum D_{ib,t+j}\delta_{t+j} + \gamma_{b,t+j} + \epsilon_{ib,t+j} \tag{1}$$

$Y_{ib,t+j}$ represents the following two dependent variables:

- *Employment status:* A binary variable equal to 1 if worker i in base year cohort b is employed at time $t+j$, and 0 otherwise.
- *Earnings:* Annual pre-tax earnings measured in SEK 100 of a worker i in base year cohort b at time $t+j$. Annual earnings are deflated by the consumer price index of year 2009. The measurement of earnings refer to cash gross earnings and includes gross wage paid by the employer, compensation for travel expenses, sickness (and rehabilitation) insurance, severance payments and vacation payments.

$\sum D_{ib,t+j}$ is a set of variables including the main variable of interest $D_{ib,t+j}$ for each observed year. $D_{ib,t+j}$ is a binary variable equal to 1 if worker i in base year cohort b was displaced j years ago or, if j is negative, will be displaced in the future. Thus, j can take on the values $j \in \{-5, -4, ...0, ...4, 5\}$. Note that each of these binary variables estimates the effect of being displaced on the outcome of interest at a given point in time. For example, the binary variable $D_{i,b,t+4}$ is equal to 1 if a worker i from base year cohort b was displaced 4 years prior to $t+4$. If the outcome of interest is earnings then the coefficient of this variable provides us with the estimated effect of being displaced on the level of earnings at year $t+4$.

X_{ib} and Z_{ib} are vectors of pre-displacement worker and plant characteristics respectively. The content of these vectors is described in detail further below (see section 6.4). $\gamma_{b,t+j}$ are base year-specific time dummies. They are included to ensure that we compare earnings of displaced and non-displaced workers from the same base year cohort at a certain point in time relative to the year of displacement. Lastly, the error term $\epsilon_{ib,t+j}$ is assumed to fulfil the *zero conditional mean assumption*, namely that:

$$E(\epsilon_{ib,t+j}|X_{ib}, Z_{ib}, \sum D_{ib,t+j}) = 0 \tag{Zero conditional mean}$$

The assumption implies that the error term is uncorrelated with the independent variables, which is a key assumption when conducting an OLS regression in order to generate unbiased estimates (Wooldridge, 2013, p.86).

Using the classical OLS regression, we estimate the difference between displaced workers and non-displaced workers at a given point in time. In this regression, we are not controlling for unobservable permanent differences in outcomes between plants. For instance, workers from high-performing plants may on average earn higher levels of earnings than workers from low-performing plants, thereby violating the identifying assumption of common trends. Nevertheless, since our sample only includes plants that are experiencing economic downturn, it is possible that the average level of earnings across these plants are relatively similar. In fact, they could have more similar trends in comparison to previous studies that use control groups consisting of non-displaced workers from plants that experience no economic downturn. Furthermore, according to Huttunen et al. (2011), earnings tend to decrease with plant size. Since we restrict the data to only include larger plants we can argue that the earnings across plants are more similar. Thus, we hypothesise that there are no significant permanent differences in earnings and employment status between workers from different plants. To evaluate if this is indeed the case, we compare the results of the classical OLS regression with the results from a plant fixed effects regression.

Another issue concerning the classical OLS regression is that it does not control for unobservable permanent differences in earnings and employment status between displaced and non-displaced workers. Displaced workers may, for instance, on average earn less than non-displaced workers due to lower levels of productivity. This would violate the identifying assumption of common trends between the treatment and control group. On the other hand, one can argue that the seniority rule precludes this type of violation since the rule does not allow employers to base dismissals on productivity levels. Therefore, we hypothesise that the prevalence of seniority rules eliminates unobservable permanent differences in earnings and employment status between workers. In order to evaluate if this is true, we compare the results of the classical OLS regression with the results of a regression including individual fixed effects.

6.2. *Plant fixed effects*

The second regression model including plant fixed effects has the following specification:

$$Y_{ib,t+j} = X_{ib}\beta + \sum D_{ib,t+j}\delta_{t+j} + \gamma_{b,t+j} + \lambda_{pb} + \epsilon_{ib,t+j} \tag{2}$$

All components of the specification are identical to those in regression (1). Note that the error term $\epsilon_{ib,t+j}$ is still assumed to fulfil the zero conditional mean assumption. In addition, the specification includes plant fixed effects λ_{pb} which vary between plants p and base year cohorts b. By including plant fixed effects, we control for any unobservable permanent differences in outcomes between workers from different plants. Thus, in this estimation we estimate the difference in outcomes between displaced and non-displaced workers in a given year who worked at the same plant at the year of displacement t. This type of regression model with plant fixed effects has, to our knowledge, not been tested in previous literature on job displacement.

The variation allowed in this regression is lower than in regression (1) as between-plant variation is removed. However, we still allow for the presence of any unobservable permanent differences in outcomes between displaced and non-displaced workers at the same plant.

6.3. *Individual fixed effects*

As mentioned earlier, regressions (1) and (2) do not control for all unobservable permanent differences in outcomes between displaced and non-displaced workers. One way to solve for this issue is to eliminate any time-independent variation between workers by conducting a regression including individual fixed effects. This approach was first popularised in this research area through the study by Jacobson et al. (1993). Since then it has been used in multiple studies on job displacement (e.g. Hijzen et al., 2010; Eliason, 2014; Fackler & Rippe, 2017). The individual fixed effects regression has the following specification:

$$Y_{ib,t+j} = X_{ib,t+j}\beta + \sum D_{ib,t+j}\delta_{t+j} + \gamma_{b,t+j} + \alpha_{ib} + \epsilon_{ib,t+j} \tag{3}$$

The components $Y_{ib,t+j}$, $\sum D_{ib,t+j}$, and $\gamma_{b,t+j}$ are identical to the corresponding components in the previous two models. $X_{ib,t+j}$ is a vector of time-varying polynomials of worker experience level, which is described in more detail further below (see section 6.4). α_{ib} are the individual fixed effects. Furthermore, a *strict exogeneity* assumption is made for the independent variables which implies that the error term $\epsilon_{i,t+j}$ should not only be uncorrelated with the independent variables in a given year but also across different years. In addition, the model requires that the error term is uncorrelated across individuals and time, in other words the assumption of *serial uncorrelation* should hold (Wooldridge, 2013, pp. 350-353). These key assumptions provide unbiased estimates with correctly measured standard errors. The mathematical definitions of the two assumptions are written below:

$$E(\epsilon_{ib,t+j}|X_{ib,t+s}, \gamma_{b,t+s}, \sum D_{ib,t+s}) = 0 \ for \ all \ s \tag{Strict exogeneity}$$

$$Corr(\epsilon_{ib,t+j}, \epsilon_{ib,t+s}) = 0 \ for \ all \ j \neq s \tag{Serial uncorrelation}$$

Since the data consist of multiple overlapping cohorts, the same worker may appear in several control groups which could result in serial correlation. In order to adjust for this issue, we use clustered standard errors at the individual level (Hijzen et al., 2010)[10].

The regression including individual fixed effects is essentially a generalisation of a difference-in-differences approach (Jacobson et al., 1993). To understand why, let us consider the following mathematical definition of the main estimated coefficients of interest, δ_{t+j}, when the outcome is annual earnings [11]:

[10]We also use clustered standard errors at the individual level for the classical OLS regression (1) as well as regression (2) with plant fixed effects.

[11]Note, however, that the same mechanism applies when the outcome of interest is employment status.

$$\Delta Earnings_T : E\{(Earnings_{t+j} - Earnings_{t-5})|X_{ib,t+j}, D_{ib,t+j} = 1\} \tag{4}$$

$$\Delta Earnings_C : E\{(Earnings_{t+j} - Earnings_{t-5})|X_{ib,t+j}, D_{ib,t+j} = 0\} \tag{5}$$

$$\Delta Earnings_T - \Delta Earnings_C = \delta_{t+j} \tag{6}$$

Note that $Earnings_T$ refers to earnings for workers in the treatment group of displaced workers and $Earnings_C$ refers to earnings for workers in the control group of non-displaced workers. Expressions (4) and (5) show that by including individual fixed effects, we are calculating the within-variation of workers. That is, for each displaced and non-displaced worker, we estimate the change in earnings at each observed year $t + j$ relative to the earnings at the selected pre-displacement year of comparison, $t - 5$. Using this, we estimate the average difference in earnings between year $t - 5$ and each of the other subsequent years for displaced (shown in expression 4) and non-displaced workers (shown in expression 5). We choose $t - 5$ as the year of comparison to capture any potential decline in earnings during the pre-displacement years. The decline might occur as a result of economic downturn at the plant or due to Ashenfelter's dip (see section 3.3).

Since we are always comparing $t - 5$ to another point in time, the estimations become two-period comparisons. When conducting two-period comparisons using fixed effects, the estimated model becomes a version of the classical difference-in-differences estimation (Angrist & Pischke, 2013, pp. 227-233). Thus, as shown in expression (6), The estimator calculates the difference between average change in earnings for displaced workers from expression (4) and average change in earnings for non-displaced workers from expression (5), which gives us the estimated difference-in-differences coefficient for each year $t + j$.

By using the difference-in-differences approach we control for any time-independent variation in the outcomes of interest between displaced and non-displaced workers. However, this leaves us with less variation in the data, thus reducing the efficiency of the estimates (Wooldridge, 2013, p. 511). Therefore, there is a trade-off between enhancing the internal validity of the study through reducing unobserved variation included in the estimations and increasing the efficiency of the estimations.

6.4. *Selection of control variables and further model choices*

6.4.1 *Control variables*

The choice of control variables is based on previous studies where the selected variables have proven to be essential (e.g. Huttunen et al., 2011; Hijzen et al., 2010; Eliason & Storrie, 2006). They are also selected based on the sample mean analysis showing significant differences between displaced and non-displaced workers (see table 4).

In the classical OLS regression (1) and the plant fixed effects regression (2), X_{ib} is a vector of pre-displacement worker characteristics observed at year t including:

- A binary variable indicating if a worker is a male.
- A binary variable indicating domestic partnership.
- Binary variables for education levels. The variables indicate whether the workers hold a university degree, a high school degree or does not hold any high school degree, where the last category is excluded in the model.
- A binary variable for having at least one child between ages 0 to 3.
- A variable measuring experience level. This variable is a proxy for a worker's total amount of work experience, defined in accordance with Heckman et al. (2008) as $age - years\ of\ education - 6$.
- A set of binary variables indicating tenure. Tenure is defined as the number of years a worker has been in the same plant. This can be observed in our data for up to five years prior to displacement. Using this information, we construct a set of binary variables for the following categories of tenure: 0 to 1 years, 1 to 2 years, 2 to 3 years, 3 to 4 years, 4 to 5 years and at least 5 years.

The vector also includes region binary variables for the regions where workers in the sample reside. We have chosen to use *Functional analysis regions*, henceforth FA regions. FA regions was brought forward by the Swedish Agency for Economic and Regional Growth (SAERG) and is defined as the local labour market of workers (Jernström & Pichler, 2017). SAERG defines a local labour market of an individual by including all municipalities that are within feasible commuting distance. This results in a total of 72 FA regions. Including region binary variables using FA regions instead of municipalities allows for more variation in the data, thus increasing the efficiency of the estimations.

Z_{ib} in the classical OLS regression (1) is a vector of plant characteristics including industry code and plant size. Lastly, in the individual fixed effects regression (3), $X_{ib,t+j}$ is a vector of time-varying polynomials of worker experience level (experience^2, experience^3, experience^4), which has been shown to be important to include when conducting estimations involving earnings and experience (Lemieux, 2006).

6.4.2 *An alternative to control variables*

An alternative method to including control variables in the regressions is to use a propensity score matching (PSM) estimator, which has been utilised in some previous literature (e.g. Huttunen et al., 2011; Eliasson & Hansson, 2016; Eliason & Storrie, 2006). The method was first brought forward by Rosenbaum and Rubin (1983). PSM uses observed covariates to estimate the propensity of getting displaced for each worker in the sample. Workers from the treatment and control group are then matched based on their estimated propensity score. The two alternative methods achieve essentially the same objective, namely to control for the effect of observable pre-treatment differences. However, previous studies comparing the two methods have failed to find any significant improvement in the estimates by using PSM matching (Eliasson & Hansson, 2016; Hijzen et al., 2010; Smith & Todd, 2005). In addition, as brought up by Angrist and Pischke (2009, pp. 86-91), PSM has proven to be complex to use as there are many details to be considered such as how to model the propensity score. Furthermore, it puts a relatively high demand on the control group size in order to achieve better matching. Since we compare displaced workers with a control group consisting of the survivors of mass layoffs, we have a smaller control group than many previous studies. Given these arguments, we choose to conduct regressions with covariates.

6.4.3 *Choice of binary regression model*

The estimations on employment status in this study are performed using the linear probability model (LPM). LPM is convenient for interpretations since it immediately reports the magnitude of the estimated effects. Some previous studies have used the probit model (e.g. Huttunen et al., 2011; Eliasson, 2013), which requires an adjustment of the estimated coefficients before interpreting the magnitude of the effect. Moreover, it has been seen in previous literature that probit and LPM often generate identical results (Angrist & Pischke, 2009, pp. 104-107).

7. Results and analysis

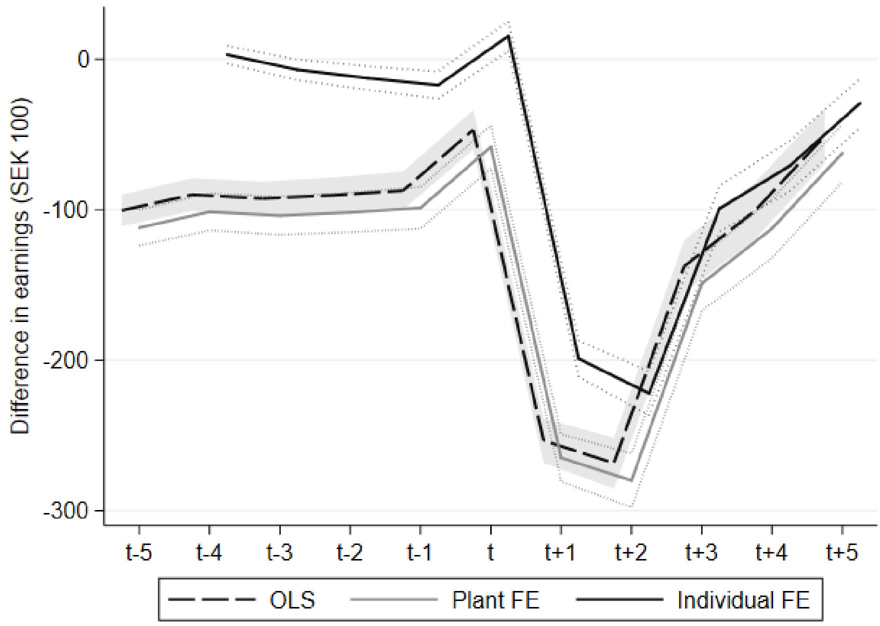
7.1. *Estimating the costs of job displacement*

Using the aforementioned regressions, we present and analyse the effects of displacement on annual earnings and employment status. Further, we compare the three regression models used in order to determine which is most suitable for analysis.

7.1.1 *The effect on earnings*

Figure 5 plots the coefficients δ_{t+j} of the displacement variables from regression models (1) to (3) for each year[12]. Recall that the coefficients δ_{t+j} measure the difference between actual earnings for displaced workers and their expected earnings had they not been displaced, where the latter is estimated using the earnings of non-displaced workers. Furthermore, the dependent variable is annual gross earnings for worker i from base year cohort b at each year $t + j$ where $j \in \{-5, -4, ..., 0..., 4, 5\}$.

Figure 5: The estimated effect of job displacement on annual earnings



Recall that the classical OLS regression (1) estimates the difference between displaced and non-displaced workers for a given point in time. In the plant fixed effects regression (2), we estimate the differences between displaced and non-displaced workers who worked at the same plant in year t for a given point in time. Lastly, the individual fixed effects regression (3) estimates the difference in earnings between $t - 5$ and each of the following years for displaced and non-displaced workers.

[12]See table 5 on page 35 for estimated coefficients

Note that only reporting the absolute change in earnings does not fully provide insight regarding the relative loss in earnings between different groups of workers. Thus, we also calculate the percentage loss in earnings at a given point in time relative to the average earnings for the control group at that time.

In general, the three regressions show that during the year immediately after displacement $t+1$, displaced workers earn approximately SEK 20,000 to 26,500 less than their expected earnings had they not been displaced. After $t+2$ workers begin to recover from the drop in earnings. However, there seem to be mixed evidence between the three estimated regressions on whether displaced workers reach their pre-displacement level of earnings within the observed five years following displacement. While this appears to be the case when looking at the classical OLS regression and the regression including plant fixed effects, the individual fixed effects regression does not show an equally strong recovery.

Next, we discuss the estimated effects of the control variables[13]. As mentioned earlier, the classical OLS includes time-invariant controls for plant and individual characteristics and the plant fixed effects regression includes time-invariant controls for individual characteristics. Lastly, the individual fixed effects regression includes time-variant polynomials for experience level. Looking at the classical OLS regression and the plant fixed effects regression, the control variables yield justifiable outcomes in terms of size and direction. Being a male worker and holding a degree has a strong positive effect on earnings. Additionally, being in a domestic partnership seems to have a positive effect on earnings. However, married individuals are often older than non-married individuals which explains the direction of the coefficient. Furthermore, having a child has a negative effect on earnings. This could be explained by the fact that people having children go on parental leave which decreases their earnings. Plant size have a positive correlation with earnings which seems intuitive since larger plants have more resources and therefore are able to pay higher wages. Finally, experience have a positive effect on earnings.

When testing for significance of fixed effects in regressions (2) and (3), we find the fixed effects for both models to be statistically significant[14]. However, a comparison of the results from the classical OLS regression (1) and regression (2) including plant fixed effects shows the models to yield similar results. This indicates that including plant fixed effects and thereby eliminating between-plant variation in earnings does not have a large impact on the estimated effects. Hence, we confirm the previously mentioned hypothesis that only using plants experiencing economic downturn reduces permanent differences in earnings at the plant-level.

[13]See table 14 in appendix.

[14]We conduct F-tests to test for joint significance of the fixed effects. The results can be found in table 16 in the appendix.

When comparing the classical OLS regression (1) and the regression including individual fixed effects (3), we note several differences between the two estimations. Firstly, the estimated differences in earnings for the pre-displacement years are smaller in regression (3), which indicates that the treatment and control group are better matched during the pre-displacement period in this model. Thus, including individual fixed effects enhances the robustness of the estimations as it reduces pre-treatment differences between displaced and non-displaced workers. Secondly, the estimated negative effects from regression (3) are generally smaller in magnitude than in regression (1). This implies a negative selection into displacement due to unobservable permanent differences in earnings between the treatment and control group. In other words, displaced workers earn on average less than non-displaced workers which could be a result of different productivity levels. As brought up in the literature review, productivity is closely related to the earnings of a worker (Becker, 1962). Consequently, non-displaced workers could be more productive than displaced workers, indicating that workers might, to some degree, be displaced based on their productivity rather than tenure. Presumably, the LIFO rule could be sidesteped among some employers as touched upon in the background section. For instance, the rule can be negotiated away through collective bargaining agreements between employers and union organisations. Such a deviation would allow employers to dismiss workers in mass layoffs based on other criteria than the tenure-based order that the LIFO-rule demands. Therefore, there might be a negative selection into the group of displaced workers, which rejects the previously mentioned hypothesis that the prevalence of seniority rules eliminates unobservable permanent differences in earnings.

Considering the arguments above, we find regression (3) including individual fixed effects to be most suitable for estimating the effect on earnings since it controls for unobservable permanent differences in average earnings between displaced and non-displaced workers. Therefore, we use regression (3) in future analyses of the estimated effect on annual earnings. When examining the results of this model, we conclude that earnings losses for workers are estimated to be SEK 20,000 one year after displacement. Furthermore, annual earnings continue to fall two years after displacement and reaches at most a loss of approximately SEK 22,200 at year $t+2$. Note that average earnings for non-displaced workers at that year is SEK 294,132. Thus, the estimated effect corresponds to an earnings loss of approximately 7.6 percent. After year $t+2$ the trend turns and earnings increase steadily through the remaining observed years. However, during the studied time span, the level of earnings never returns to the initial pre-displacement level. Indeed, at year $t+5$, the level of annual earnings for displaced workers is still approximately SEK 3,000, or roughly 1 percent lower than the expected annual earnings they would have received in the absence of displacement.

Turning to the pre-displacement years, we observe an upward slope in earnings right before the event of displacement at year t. This is also observed in previous studies (e.g. Eliasson

& Hansson, 2016; Eliasson, 2013) and can be explained by a number of factors. First, the definition of displacement requires all workers to be employed at year t, thereby increasing the estimated level of earnings at that particular year. Second, it can also be explained by the previously discussed notion of severance payments (see section 2.2). Displaced workers can acquire such payments if they get laid off due to shortage of work. Note that severance payments are included in our measurement of annual earnings and that Sweden has one of the highest rates of severance payments among OECD countries (OECD, 2015). Finally, one reason might be that our data could still contain voluntary leavers. These are workers that leave for a new employer, often paying a higher salary. However, they are still considered to be displaced workers in the sample due to the inability to distinguish between voluntary and involuntary leavers.

Table 5: Estimated effect of job displacement on earnings

	Earnings		
	OLS	Plant FE	Individual FE
$t-5$	-100.47***	-111.89***	
	(5.29)	(6.07)	
$t-4$	-89.99***	-101.41***	3.04
	(5.60)	(6.27)	(3.02)
$t-3$	-92.50***	-103.92***	-6.99*
	(5.76)	(6.45)	(3.46)
$t-2$	-90.36***	-101.78***	-12.49**
	(6.03)	(6.70)	(3.99)
$t-1$	-87.32***	-98.74***	-17.18***
	(6.46)	(7.03)	(4.60)
t	-46.83***	-58.26***	15.48393**
	(6.72)	(7.27)	(5.00)
$t+1$	-253.38***	-264.80***	-198.99***
	(7.54)	(8.02)	(6.17)
$t+2$	-268.38***	-279.80***	-221.98***
	(8.66)	(9.16)	(7.43)
$t+3$	-137.57***	-148.99***	-99.29***
	(8.87)	(9.29)	(7.77)
$t+4$	-100.95***	-112.37***	-70.87***
	(9.61)	(9.99)	(8.54)
$t+5$	-50.98***	-62.40***	-29.20***
	(9.33)	(9.72)	(8.28)
N	2,429,405	2,429,405	2,429,405

* indicates that the estimate is significant at a 5% significance level. ** indicates that the estimate is significant at a 1% significance level. *** indicates that the estimate is significant at a 0.1% significance level. Clustered standard errors by individuals are provided in parenthesis.

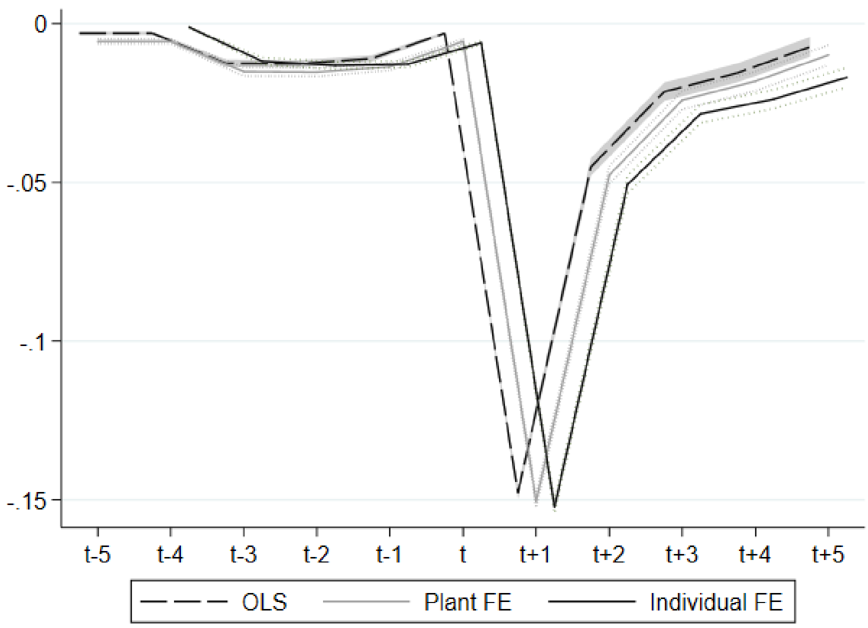
7.1.2 *The effect on employment status*

Figure 6 plots the coefficients δ_{t+j} from estimating the effect of displacement on employment status at each observed year using the three aforementioned regression models[15]. Recall that the dependent variable is a binary variable $Y_{ib,t+j}$ equal to 1 if a worker i from base year cohort b is employed at time $t + j$ where $j \in \{-5, -4, ..., 0..., 4, 5\}$.

As shown in figure 6, the three regression models generate similar results for the effect of displacement on employment status. The results show that the probability of being employed at year $t + 1$ decreases by approximately 15 percent[16]. This is however followed by a fast recovery. Five years after displacement we do not find any large losses in the probability of being employed for displaced workers.

Looking at the estimates of the control variables included in the three regression models[17], we find the effects to be similar to those in the regressions on earnings. The only exception is the estimated effect of experience in regressions (1) and (2). The results show that experience has a significant negative effect on employment status. This is, however, expected since experience is strongly associated with age and tenure. As mentioned in the literature review, these two factors tend to correlate negatively with the probability of finding employment after displacement.

Figure 6: The estimated effect of job displacement on employment status



[15]See table 6 on page 37 for estimated coefficients

[16]We can interpret the estimated effects directly as a relative percentage change in probability of employment compared to the probability of employment for non-displaced workers at year $t + 1$, since that probability is equal to 1 due to the definition of displacement.

[17]See table 15 in appendix.

The estimated effects on employment status for regressions (1) and (2) are similar. Presumably, this indicates that there is no bias stemming from unobservable permanent differences between plants in the estimated effect of workers' probability of being employed. Likewise, the fact that regression (1) and (3) are similar indicate that there is no bias from unobservable permanent differences between displaced and non-displaced workers that affect the estimates. When conducting a test for joint significance of the fixed effects in regression (2) and (3), we see that neither of the two types of fixed effects are significant [18]. One explanation to why we find no differences in outcomes could be that employers often recruit through observable factors and since we control for these in regression (1) we cannot see negative nor positive selection into the treatment group. Consequently, neither of the two fixed effects regressions provide any additional information to the estimation. Moreover, the OLS regression includes more variation and thus has higher efficiency. Therefore, we choose regression (1) to interpret the results and conduct further estimations of the effects on employment status.

Table 6: Estimated effect of job displacement on employment status

	Employment status		
	OLS	Plant FE	Individual FE
$t-5$	-.003***	-.006***	
	(.0002)	(.0004)	
$t-4$	-.003***	-.006***	-.001***
	(.0002)	(.0004)	(.0001)
$t-3$	-.012***	-.015***	-.012***
	(.0006)	(.0007)	(.0007)
$t-2$	-.013***	-.015***	-.013***
	(.0006)	(.0007)	(.0006)
$t-1$	-.011***	-.014***	-.013***
	(.0005)	(.0006)	(.0005)
t	-.003***	-.006***	-.006***
	(.0002)	(.0004)	(.0003)
$t+1$	-.148***	-.150***	-.152***
	(.0010)	(.0011)	(.0010)
$t+2$	-.045***	-.048***	-.051***
	(.0014)	(.0014)	(.0014)
$t+3$	-.021***	-.024***	-.028***
	(.0015)	(.0015)	(.0015)
$t+4$	-.016***	-.018***	-.023***
	(.0015)	(.0015)	(.0015)
$t+5$	-.007***	-.009***	-.017***
	(.0016)	(.0016)	(.0016)
N	2,429,405	2,429,405	2,429,405

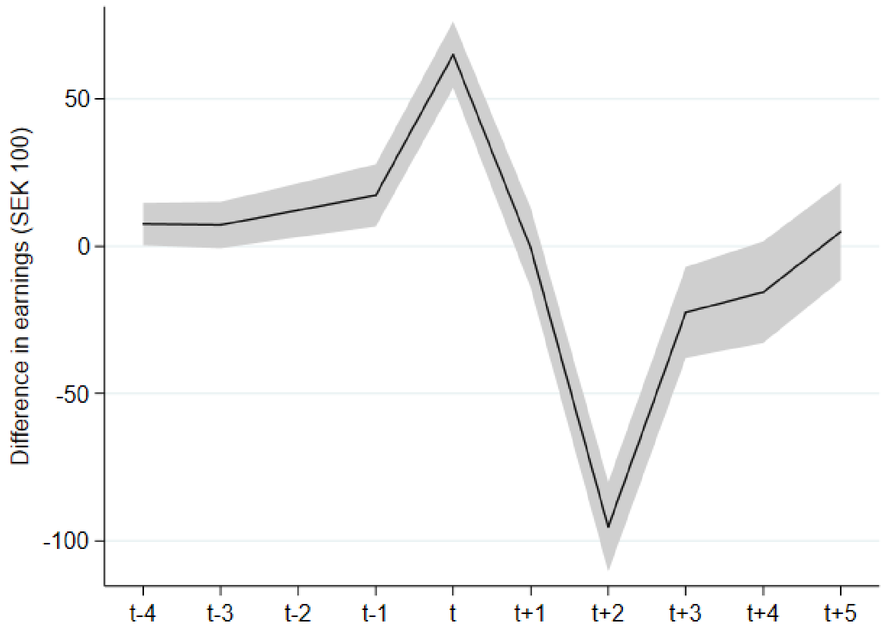
* indicates that the estimate is significant at a 5% significance level. ** indicates that the estimate is significant at a 1% significance level. *** indicates that the estimate is significant at a 0.1% significance level. Clustered standard errors by individuals are provided in parenthesis.

[18]See table 16 in the appendix.

7.2. *Determining the channels driving earnings loss*

Recall that average earnings for displaced workers who get employed within two years after displacement is much higher than for displaced workers who remain unemployed after two years (see table 4). This indicates that estimated earnings losses for displaced workers could be mainly driven by longer periods of unemployment. To assess if this is the case, we estimate a regression with individual fixed effects on earnings. This time, however, a more stringent restriction is imposed on the sample which only includes workers who are employed in the years $t+2$, $t+3$, $t+4$ and $t+5$. Thus, we are able to assess how much of the previously estimated loss in earnings is driven by lower level of earnings at the subsequent employment, rather than periods of unemployment. The results are presented in figure 7[19].

Figure 7: The estimated effect of job displacement on earnings (including employment restriction)



The results suggest that earnings losses for displaced workers reaches at most SEK 9,500 at year $t+2$, which equals a percentage loss of only 2.89 percent. The estimated loss in earnings is thus much lower for this subsample, indicating that losses might be mainly driven by longer periods of unemployment. The results are in line with previous outcomes from studies conducted in the European context (e.g. Hijzen et al., 2010; Eliasson & Hansson, 2016; Bender et al., 2002).

Longer periods of unemployment can be explained by several mechanisms as brought up in the literature review. To begin with, the mechanism of job matching implies that some workers match better to a job than others. Thus, apart from affecting the level of earnings received at the new workplace, job matching might also affect the amount of time displaced workers spend in unemployment before acquiring a new job. A second mechanism that could

[19]The coefficients from the regressions are presented in table 7 in appendix

also affect time spent in unemployment and level of earnings is the revelation of information, or signalling. There could be a perception among employers that displaced workers are of lesser quality, creating more difficulty for workers to find a job after displacement. However, this mechanism is arguably not as significant in the Swedish context given that displacements most likely occur in accordance with seniority rules. Thirdly, as found by Eliason and Storrie (2009), displacement tends to have a negative effect on the mental and physical health of displaced workers, making them unable to work for some period of time. This could partly explain the fact that estimated losses in earnings are mainly driven by longer periods of unemployment. Finally, as several previous studies presented in the literature review have brought up, loss of industry-, firm- or task-specific capital might also be a driving mechanism behind these results. Loss of specific human capital does not only explain periods of unemployment for displaced workers, but is also likely to be a significant determinant for overall loss in earnings. Further discussion of the impact of specific human capital loss on the estimated effects of job displacement is provided in the heterogeneity analysis below.

7.3. *Heterogeneity analysis*

In order to check for heterogeneity in our results we analyse the effect of job displacement on annual earnings and employment status across various subgroups. In particular, we estimate the effect across gender, age groups, level of education, level of tenure and displacement type (mass layoff or closure)[20] [21]. Based on the discussion regarding choice of model, we estimate the effects on annual earnings using regression (3) including individual fixed effects. Meanwhile, the effects on employment status are estimated using the classical OLS regression (1). Further, percentage losses in earnings relative to average earnings for non-displaced workers are calculated. For each heterogeneity test we conduct a chow test which is presented in tables 17 to 20 in appendix. The results of these tests show that all differences across subgroups are statistically significant[22].

7.3.1 *Gender and displacement*

Figure 8 presents the estimated effect on earnings and employment status for men and women[23]. Although we find evidence of a significant difference between the two groups, the difference is not markedly large. We find women to experience a percentage loss in earnings of approximately 8.7 percent at year $t + 2$ while men lose 7.2 percent. Furthermore, women endure a significantly larger loss in the probability of being employed one year after

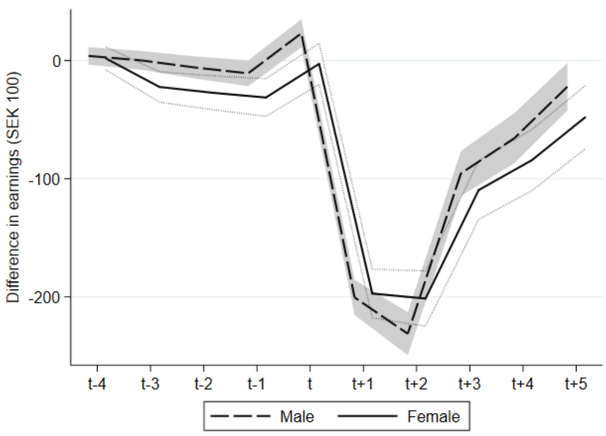
[20]Each subgroup within the treatment group is compared to the equivalent subgroup within the control group. This implies that we for instance compare displaced women with non-displaced women.

[21]The categorisation of age, education and tenure refer to the status of these variables at the pre-displacement year t.

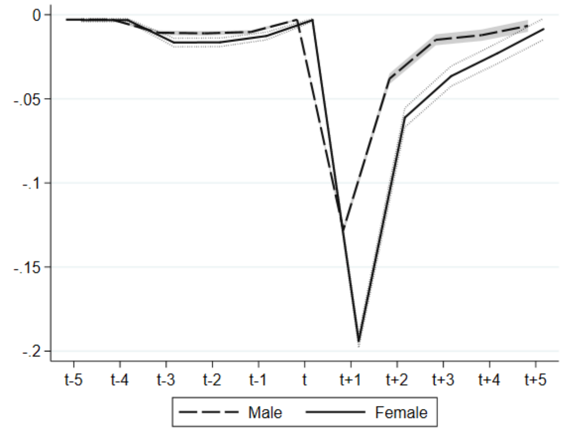
[22]Note that we are unable to conduct a chow test for differences across displacement type since the same control group is used for the restricted and unrestricted models.

[23]The coefficients from the regressions are presented in table 8 in appendix.

Figure 8: The effect of job displacement across gender



(a) Displacement on earnings

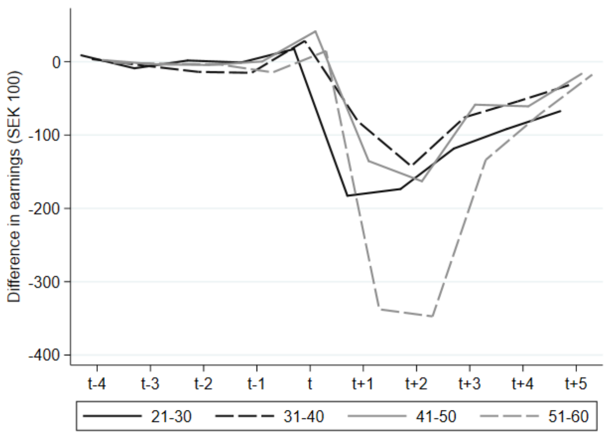


(b) Displacement on employment status

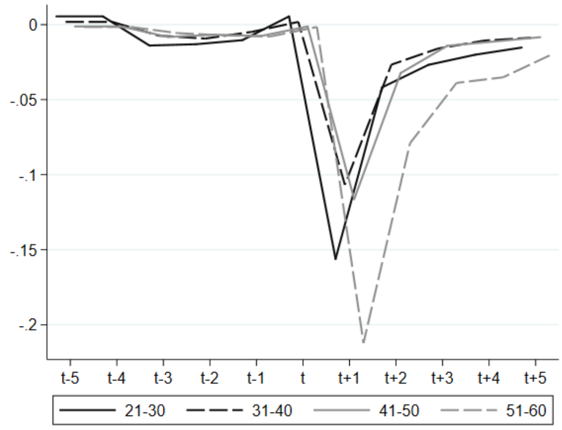
displacement. Specifically, they experience a 19.4 percent reduction in the probability of being employed at year $t+1$ while men experience a loss of 12.8 percent. This is in line with previous findings presented in the literature review (e.g. OECD, 2015). However, women seem to recover from this loss five years after displacement.

7.3.2 *Age and displacement*

Figure 9: The effect of job displacement across age groups



(a) Displacement on earnings



(b) Displacement on employment status

The estimated effect on earnings and employment status for different age groups is presented in figure 9[24]. The results show that one age group stands out in terms of the magnitude of earnings losses, namely workers aged 51 to 60. This group seems to experience the largest loss in earnings by approximately SEK 34,700 at year $t+2$. Measured in percentage loss, this corresponds to a reduction in earnings of 12.1 percent. In contrast, other age groups experience a

[24]The coefficients from the regressions are presented in table 9 in appendix.

loss in earnings between SEK 14,200 and 17,350 (or between 4 and 7 percent). Earnings seem, however, to recover for the oldest workers as the loss at year $t+5$ is not statistically significant.

The oldest age group also experiences the largest reduction in probability of being employed. Specifically, they experience a reduction of 21.2 percent, while the effect for other age groups ranges between 11 and 16 percent. This is in line with previously discussed research which often find older workers to be most severely affected by involuntary job loss (e.g. OECD, 2015; Kletzer & Farlie, 2003). There are three possible explanations here. One concerns tenure and firm-specific human capital. Naturally, human capital accumulates as workers age. As discussed in the literature review, firm-specific human capital reduces the mobility of workers. Consequently, workers with a high amount of firm-specific human capital may find it difficult switching to another firm or industry as employers find them less suitable for working at their firm. In turn, this reduces their chances of acquiring a new job with similar earnings. A second explanation is the mechanism of signalling. If older, high-tenured workers get displaced despite the LIFO rule this might signal to employers that workers are of low quality, thus resulting in longer spells of unemployment and lower earnings. Lastly, as brought up by Huttunen et al. (2011) older people are likely to retire earlier if displaced, which also affect their earnings and employment status.

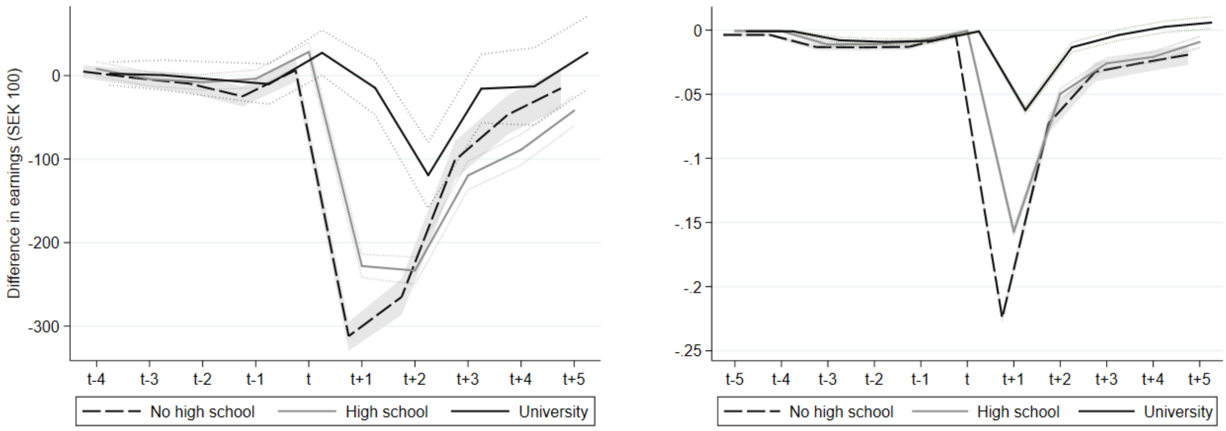
Notably, the youngest age group is the second-most severely affected both in terms of earnings losses and employment status. These workers display a percentage loss in earnings of 7 percent two years after displacement. One interpretation of this finding is that young workers are still relatively new to the labour market and therefore more likely to change occupation status. Furthermore, the LIFO-rule can also be an explanatory factor. As these workers find employment in a new firm, they are the workers with lowest amount of tenure and thus first in line to be displaced in the case of layoffs. Presumably, this could result in a negative circle in which young workers find difficulties maintaining a job, particularly if the industry in which the workers operate within is experiencing economic downturn. This analysis is in line with the notion of new jobs ending early. (e.g. Eliason & Storrie, 2006)

7.3.3 *Education and displacement*

The estimated effect on earnings and employment status for levels of education are presented in figure 10[25]. The results confirm the negative correlation between displacement costs and education level found in previously discussed literature (e.g. Kletzer, 1998; Eliason & Storrie, 2006; Huttunen et al., 2011). Workers with no high school degree experience the highest losses in earnings and probability of being employed. These workers experience at most an earnings loss of SEK 31,200 in year $t+1$ (or 12.7 percent). In contrast, workers with a university degree experience at most a loss in earnings of SEK 11,900, or 2.9 percent, in year $t+2$.

[25]The coefficients from the regressions are presented in table 10 in appendix.

Figure 10: The effect of job displacement across education groups

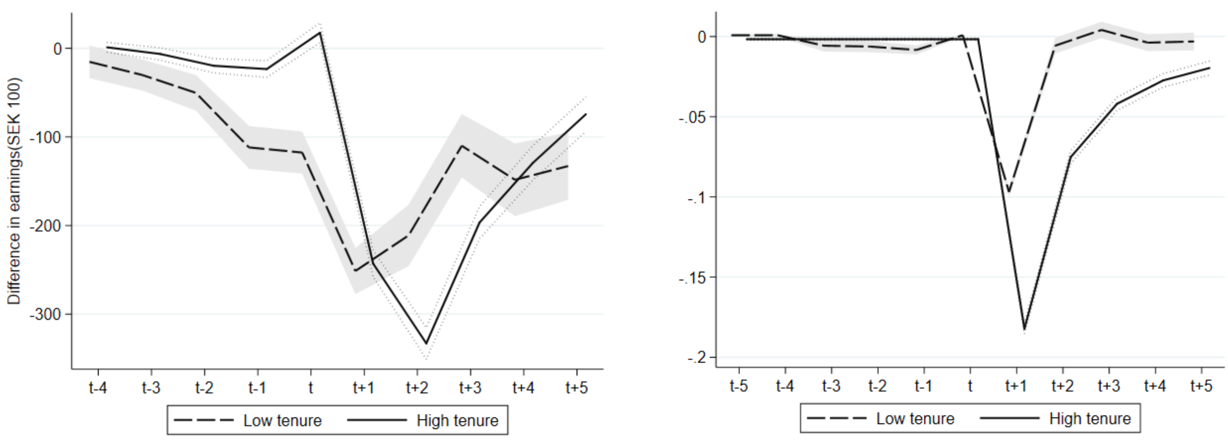


(a) Displacement on earnings (b) Displacement on employment status

In terms of employment status, low-educated workers experience a reduction in the probability of being employed in year $t+1$ by 22 percent, which is significantly larger than the effect for workers with a high school or university degree, who only experience a reduction of 15.7 and 6.2 percent, respectively. Thus, it is evident that workers with a higher degree seem to be only marginally affected by displacement. This can be explained by the relationship between labour mobility and education (see section 3.2). Education increases the mobility of workers as it enables them to acquire skills that are more transferable across firms, industries and job tasks. Therefore, high-educated workers are not as affected by loss in specific human capital when displaced.

7.3.4 *Tenure and displacement*

Figure 11: The effect of job displacement across levels of tenure



(a) Displacement on earnings (b) Displacement on employment status

The estimated effect on earnings and employment status for high- and low-tenured workers are presented in figure 11[26]. Arbitrarily, a low-tenured worker is defined as having at most two years of tenure and a high-tenured worker is a worker with at least five years of tenure.

The results indicate that high-tenured workers endure larger costs of displacement. This is particularly apparent when looking at displaced workers' probability of being employed within one year of displacement. The probability is reduced by 18.2 percent for high-tenured workers and 9.6 percent for low-tenured workers. Furthermore, high-tenured workers experience a loss in earnings of SEK 33,294 at year $t + 2$ (or 11.9 percent). In comparison, we estimate a loss of SEK 21,114 for low-tenured workers, which corresponds to a percentage loss of 6.4 percent. We also find that low-tenured workers experience losses in earnings already during the pre-displacement period. It is likely that the control group of low-tenured workers consists of individuals with relatively high productivity levels or key positions and are therefore valued by the employer. This could explain why the low-tenured workers do not get displaced despite their low tenure and the LIFO-rule. Therefore, the downward trend in pre-displacement years could hypothetically be prevalent because the low-tenured, displaced workers are being compared to workers who are particularly productive and have higher growth in earnings.

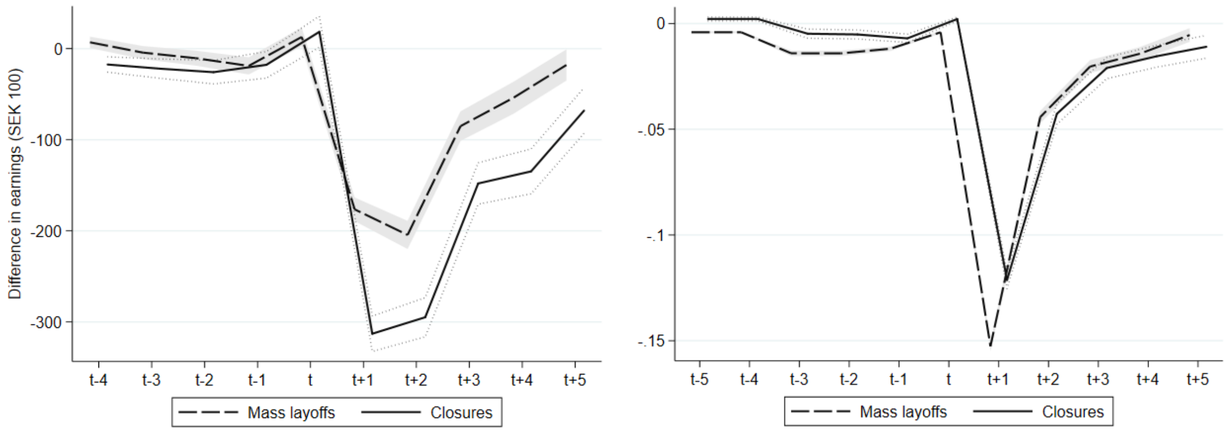
Similar to previously discussed literature, these findings indicate that loss of specific human capital contributes to lower future earnings and a difficulty to find employment after displacement (e.g. Carrington & Fallick, 2017; Fallick, 1996; Kletzer, 1998). Furthermore, it is likely that employers had to circumvent the LIFO-rule in order to displace the high-tenured workers. This indicates that these workers are possibly negatively selected into treatment and less productive than the low-tenured workers whose displacement is more likely to be a result of the LIFO-rule. Thus, displaced high-tenured workers could signal that they are of low quality to potential employers. Moreover, since high-tenured workers tend to be older, it is possible that some of them choose to retire, thereby driving up the magnitude of estimated losses.

There are some issues with the estimation above worth highlighting. One concern is that the size of the control group of low-tenured workers is only 17,539 workers which is relatively low. Thus, some care should be taken when drawing conclusions from these estimations. Moreover, the tenure variable is truncated which might lead to further issues described more thoroughly in section 8.3 under which potential limitations of the study are discussed.

[26]The coefficients from the regressions are presented in table 11 in appendix.

7.3.5 *Displacements through mass layoffs and closures*

Figure 12: The effect of job displacement - mass layoff and closures



(a) Displacement on earnings (b) Displacement on employment status

In figure 12, we compare the estimated effects of displacement for workers displaced through mass layoffs and closures[27]. We find mixed evidence in regard to which group is most affected. On the one hand, workers displaced through closures experience higher loss in earnings. The loss reaches at most approximately SEK 31,300, compared to SEK 20,450 for workers displaced through mass layoffs (or 10.2 and 7 percent respectively). The closure subgroup also seems to experience a slower recovery. This relationship between the two groups was also found in the Swedish study by Eliasson (2013). On the other hand, workers displaced through closures do not seem to be as affected as workers displaced through mass layoffs in terms of employment status. The probability of being employed within one year after displacement is reduced by approximately 12.1 percent for the closure subgroup and 15.3 percent for the mass layoff subgroup.

Theory argues that workers displaced through closures face lower losses in earnings than workers displaced through layoffs due to signalling (Gibbons & Katz, 1991). Closures require all workers to be displaced regardless of their level of productivity and human capital. As for mass layoffs, not all workers are displaced, which can send a signal that the displaced workers are less productive. Consequently, it is more difficult for them to find employment with similar earnings after displacement. However, this does not seem to be entirely true when looking at the estimated results. Although we see that the mass layoff group experience a higher loss in terms of employment status, the results indicate that workers displaced through closures are not better off in terms of earnings. Moreover, we are careful when drawing any definite conclusions from the results since, first of all, the subgroup of closures is small and, second of all, the theory does not suggest any explanations for this kind of mixed evidence.

[27]The coefficients from the regressions are presented in table 12 and 13 in appendix.

8. Discussion

8.1. *Comparison to previous findings*

The estimated percentage loss in earnings of 7.6 percent is moderate in size compared to previous European studies, whose estimated effects vary between 1 and 35 percent. Nevertheless, compared to the two previously discussed Swedish studies considered most similar to this study (Eliason & Storrie, 2006; Eliasson & Hansson, 2016), we find the results to be relatively similar. In particular, the two studies find a percentage loss in earnings ranging between 8 and 10 percent[28]. However, when looking at the estimated absolute earnings losses we find their results to differ from this study. Eliason and Storrie (2006) find an absolute loss in earnings of approximately SEK 11,500 while Eliasson and Hansson (2016) find a loss of SEK 30,000 (both losses are deflated to 2009 price level). Our estimated loss of SEK 22,200 is thus in between the two previous Swedish findings. With that in mind, we precede by discussing potential explanations for why the results regarding absolute loss in earnings differ between the studies.

In order to understand why the absolute loss in earnings estimated by Eliasson and Hansson (2016) is greater, we compare the definition of the control group used. While the two authors use non-displaced workers from plants not undergoing economic downturn as a control group, we use survivors from plants undergoing mass layoffs. Thus, one possible explanation for the difference in findings between the two studies is that survivors also experience losses in earnings due to the general economic downturn at the plant. Moreover, Eliasson and Hansson include plants with ten or more employees, while this study uses a minimum size of 50 employees or more. As mentioned previously, the estimated effect of displacement on earnings tends to decrease with plant size (see section 3.3). We bring forward two interpretations to explain this. First and foremost, it is likely that larger plants belong to major, multi-plant firms which increases the probability of a displaced worker to be re-employed within the same firm and thus not suffer large losses in earnings. In addition, since larger firms have more resources, they are more capable of providing support for displaced workers through programs to help them find employment. Therefore, the differences in plant size could also explain the difference in the estimated effects between the two studies.

Moving on, there are som explanations to why the other Swedish study by Eliason and Storrie (2006) finds a smaller absolute effect on earnings. To begin with, we are only examining the effect of displacement within the manufacturing sector, which often entails larger earnings losses compared to other sectors. Eliason and Storrie examine all sectors and therefore estimate a lower effect on earnings. Furthermore, the sample in our study includes an older

[28]These studies use pre-displacement average losses for displaced workers when calculating the percentage earnings losses. If using the same method, we do not find notably different percentage losses either.

age group than the sample used by Eliason and Storrie, which only includes workers between ages 21 and 50. As both the analysis in this study as well as previously discussed research indicate, older workers tend to suffer from more severe earnings losses when displaced. Hence, including this group of older workers could explain the difference in estimated effects between the two studies. Moreover, as argued by Huttunen et al. (2011), the effect of displacement can be underestimated by not including older workers, who have been found to display a higher probability of leaving the labour force after displacement. This is shown in the heterogeneity analysis of this study where older workers experience much larger losses. With this in mind, it is thus likely that Eliason and Storrie would have estimated larger effects had they included the older age group.

It is also worth highlighting that we examine a different time period than both of the previous studies. Eliason and Storrie look at workers displaced in 1986 while Eliasson and Hansson examine displacements occurring between 2000 and 2009. This study, on the other hand, covers a longer time period of displacements. As shown by Couch and Placzek (2010), different results are obtained depending on the time periods analysed (see section 3.3). Therefore, it is likely that the choice of time period could partly explain the differences in the magnitude of estimated effects between the three studies.

With regard to the estimated recovery of annual earnings, we find displaced workers to recover at a relatively fast rate in comparison to previous Swedish studies. This could be a consequence of the survivor syndrome which can reduce productivity of survivors remaining at the downsizing plants (Wang-Bae, 2003), thereby reducing their earnings as well. Moreover, it is likely that survivors are displaced in the years following the initial event of displacement as employers continue cutting back on expenses through layoffs (see figure 5). Therefore, the recovery observed for displaced workers could be partly driven by growing unemployment rates for the control group. However, as seen in the descriptive analysis, 86 percent of displaced workers find employment within two years after displacement. Hence, it is more likely that the observed recovery for displaced workers is mainly driven by them getting employed relatively fast rather than the control group suffering from earnings losses and periods of unemployment.

Finally, in contrast to previous studies conducted in other places than Sweden (e.g. Jacobson et al., 1993; Hijzen et al., 2010), we do not find any trace of Ashenfelter's dip. As Eliason and Storrie (2006) point out in their study, Ashenfelter's dip should not occur in context of the Swedish labour market due to rigid wage bargaining systems. This has also been confirmed by other Swedish studies (e.g. Eliasson, 2013; Eliasson & Hansson, 2016).

8.2. *Policy implications*

The results indicate that the Swedish labour market is well-functioning in the sense that costs of displacement are relatively low compared to estimates from other countries. Displaced workers lose up to 7.6 percent of average expected earnings and display a relatively fast recovery both in terms of earnings and employment status. Further, we find that almost 86 percent find employment within two years after displacement. Since the aim of this study is to estimate the effect of displacement, we can only speculate what implications the prevailing regulations and protection system have on the costs of displacement.

First of all, the prevailing norm in Sweden is that employers are responsible for their displaced workers (see section 2.3.1). In other words, employers need to ensure that displaced workers are provided with support in finding a new job. As previously mentioned, this kind of re-employment support is mainly provided during the pre-displacement period and is different from standard conduct in other OECD countries. Moreover, employers have a responsibility to, long time in advance, give notice to a worker facing displacement. Consequently, there is great opportunity in Sweden to provide support to displaced workers already during the pre-displacement period. These responsibilities might be reflected in our estimates by the relatively high amount of workers that find employment within the first two years after displacement.

Another implication of labour market policies that could possibly explain the estimated results concerns JSCs. Recall that JSCs offer support to displaced workers in their job-seeking process with the objective to help them find employment before the pre-displacement period ends (see section 2.3.1). The presence of JSCs in Sweden thus reduces the burden on displaced workers that are union members. This could also explain the observed fast recovery that displaced workers experience. Lastly, rigid wage bargaining systems through collective agreements might explain why workers are able to maintain relatively high earnings levels at the subsequent employer as well as not experience reduced earnings in the pre-displacement period (see section 2.3.2).

Apart from indicating that Swedish labour market policies are well-functioning, our results could also shed light on the importance of evaluating the impact of different programs and policies on vulnerable subgroups. Besides older and high-tenured workers, the results indicate that low-educated workers endure significantly larger costs than high-educated workers. As lower education is correlated with lower labour mobility across firms and industries, there is a need to evaluate measures with the purpose of enhancing the ability of this group to re-enter employment after displacement. Such measures could include labour market participation programs and vocational training. Similarly, measures of this kind could also help older, high-tenured workers by facilitating transfers from one firm or industry to another.

8.3. *Limitations and internal validity*

The main concern of studying the effects of job displacement is whether a selection bias between displaced and non-displaced workers, distorting the causality of the observed effects, prevails. However, we believe our estimations to infer causality for two reasons. Firstly, displacement can be thought of as an exogenous shock which is captured through the use of mass layoffs and closures as an instrument for identifying displaced workers. Secondly, the two main estimations on earnings and employment status presented in section 7.1 show that the treatment and control group have parallel trends in the pre-displacement period. The parallel trends imply that the common trends assumption required to infer causality is fulfilled. There are, however, other concerns with the internal validity put forward in the following paragraphs.

8.3.1 *Is job loss really involuntary?*

One issue concerning the data is that there is no distinction between involuntary and voluntary job separations. In other words, the treatment group may include workers leaving their job voluntarily or workers who would have left regardless of the event of displacement. Including these workers could result in an upward bias, thereby underestimating the negative effect on earnings and employment status for displaced workers.

To evaluate if job loss is involuntary we revisit the estimated difference between workers displaced through closures and mass layoffs (see section 7.3.5). As discussed, closures are considered to be more appropriate for capturing pure involuntary job loss since all workers are forced to leave the plant they work at. Therefore, if the closure subgroup displays larger losses as a result of displacement, it would imply that mass layoff subgroup could include voluntary job separations. However, as previously discussed, the results indicate mixed evidence for which sample is most severely affected. It is therefore not possible to conclude whether we observe this issue in our study.

8.3.2 *Survivors as control group*

As brought up, it is possible that the rapid recovery observed in this study is partly driven by survivors losing their jobs in the years following displacement. This could produce an upward bias of the estimates in the sense that the perceived costs of displacement could be underestimated. However, one argument for using survivors as a control group is that they work at the same plants as displaced workers. The experience of survivors may therefore be a better representation of the actual path that displaced workers would have followed had they not been displaced. In fact, one can argue that previous studies overestimate the expected outcome for the treatment group when using a control group consisting of workers from plants not experiencing economic downturn.

8.3.3 *Displacement at the plant level*

One shortcoming of defining displacement at the plant level is that it is complicated to distinguish displaced workers who get rehired within the same firm (within-firm movers) from workers finding employment at a new firm (between-firm movers). The estimated effects on earnings for within-firm movers would most likely differ from that of between-firm movers since their firm-specific human capital and wage premiums would stay intact. Consequently, it is desirable to analyse this group separately from other displaced workers. Some studies, such as the Norwegian study by Huttunen et al., (2011) have conducted such analysis and found that earnings losses are mainly driven by between-firm movers, while the earnings of within-firm movers tend to stay more intact (see section 3.3). Consequently, a sample consisting of a disproportionately high share of within-firm movers may reduce the magnitude of estimated effects on earnings.

Several studies have chosen to focus on displacement at the firm level, thereby excluding within-firm movers from the treatment group (e.g. Jacobson et al., 1993; Hijzen et al., 2010; Sullivan & von Wachter, 2009). This results in underreporting the number of displaced workers at each firm. Moreover, as discussed in section 4, the decision to define displacement at the plant level provides us with a more consistent and accurate identification of workplaces over time.

8.3.4 *Shortcomings of the tenure variable*

As pointed out, we are unable to follow workers' entire working history due to a truncated tenure variable. When estimating the effect of displacement on earnings this should not create any issues since we use individual fixed effects and thereby eliminate any time-invariant differences. However, it may be problematic when estimating the effects on employment status since we use a classical OLS regression in which we control for tenure, thereby comparing workers with the same level of tenure. Tenure cannot be observed for longer than five years, meaning that we are bundling workers with five or more years of tenure with each other. It is therefore likely that the control group consists of more high-tenured workers than the treatment group. This issue may lead to overestimating the negative impact on employment status in our estimations.

8.4. *External validity*

As shown in the literature review, the results vary significantly across countries. Kuhn (2002) and Kletzer (1998) suggest that cross-country differences could be attributed to institutional differences such as variation in employment protection laws, unemployment benefits and wage bargaining systems. Given this, we believe that our results are especially applicable within the Swedish context. However, there are reasons to believe that similar results can be found in countries with similar labour market institutions as Sweden.

Moreover, outcomes are likely to differ across sectors. Fallick (1996) argues that most studies find a structural cause for displacement. Workers have to leave their plants because of changes in international trade conditions, demand as well as due to changes in technological development within their sector. Therefore, costs of displacement vary depending on how a sector is influenced by contemporary economic, technological and political factors. Thus, we believe that our results can be somewhat generalised to sectors facing similar challenges as the manufacturing sector. The results are arguably not applicable to smaller plants since we restrict the sample to plants with 50 or more workers. As brought up earlier, smaller firms are exempted from some paragraphs in the EPL (see section 2.2). Furthermore, workers displaced from smaller firms are found to display larger losses in earnings (Huttunen et al., 2011).

Finally, the heterogeneity analysis show the magnitude of estimated costs to vary across demographic groups. With that in mind, we suspect that displacement costs may vary across labour markets depending on the demographic features of the labour force. Moreover, it is likely that the observed magnitude of displacement costs for each demographic group of workers varies across economies depending on the level of support measures available for different groups of workers.

9. Conclusion

The purpose of this study is to assess the costs of job displacement incurred by displaced workers. Specifically, we examine workers from fourteen different cohorts displaced through plant closures or mass layoffs in the Swedish manufacturing industry. Using longitudinal administrative registry data, we are able to follow displaced and non-displaced workers up to five years before and five years after the event of displacement. The aim is to answer the question: *What are the effects of job displacement on earnings and employment status?*

To do so, we evaluate the choice between three regression models which include lags and leads for the effect of displacement on earnings and employment status. Using these regressions, we estimate the yearly difference between actual outcomes for displaced workers and their expected outcomes had they not been displaced. The latter is approximated using a control group of workers, also addressed as survivors, who were not displaced from the plants conducting mass layoffs. After evaluating the three models, we find that the generalised difference-in-differences approach is most suitable when estimating the effect on annual earnings. Meanwhile, a classical multivariate OLS regression is found to be sufficient when estimating the effect on employment status.

The results indicate that displaced workers experience a significant loss in annual earnings of approximately 7.6 percent and display some difficulty in finding employment within a year after displacement. However, the estimated costs appear to be relatively moderate when put in an international context as they are significantly lower than previous estimates from other European countries as well as the US. This indicates that the Swedish labour market is well-functioning in the sense that displaced workers endure low costs and recover relatively fast. Moreover, it is clear that losses in earnings are mainly driven by longer periods of unemployment. Ultimately, a heterogeneity analysis of the estimated effects identifies a number of subgroups as particularly vulnerable to displacement. In specific, low-educated workers, young workers as well as older, high-tenured workers incur relatively large costs of being displaced as they experience larger losses in earnings and an increased difficulty in finding re-employment. In summary, despite a few groups of workers being particularly affected by displacement, we conclude that the protection laws and support systems available for Swedish workers seem to be relatively efficient.

As the development and implementation of advanced manufacturing technologies, increased automation processes and other disruptive forces continue to reshape global industries, there could be major challenges awaiting the labour market. As seen in this study, some subgroups are particularly vulnerable to job loss both in terms of lower earnings and periods of unemployment. It is therefore essential for government institutions and policy regulators to understand which groups are most vulnerable to displacement as well as how to mitigate their costs. We therefore suggest future studies to analyse these vulnerable groups thoroughly. One example is to examine younger, low-tenured as well as older, high-tenured workers in order to evaluate the efficiency of seniority rules in Sweden and to which extent it impacts these groups. In addition, it is necessary to study the underlying reasons for the estimates found in this study. Evaluating the different components in the protection system such as the JCSs or the collective bargaining contracts could provide further explanations for the rapid recovery in employment status and earnings.

Furthermore, it is essential to understand the underlying mechanisms of the costs borne by displaced workers. In particular, we recommend future research to examine labour mobility. For instance, it is of interest to compare displaced workers who acquire a job within the same firm or industry with workers who move to a different firm or industry, thereby providing a more thorough analysis of human capital. In addition, the evidence is mixed in terms of the difference between workers displaced through mass layoffs and closures. Thus, it would contribute to the existing body of literature as well as current signalling theory to conduct a more accurate comparison between the two displacement types.

In conclusion, we find it essential for future studies to continue shedding light on the topic of displacement in order to fill existing gaps in literature and provide a framework for how to mitigate the upcoming challenges for workers.

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10.3. *Legislations*

SFS 1982:80. *Lag om anställningsskydd*. Stockholm: Arbetsmarknadsdepartementet

11. Appendix

Table 7: Estimated effect of displacement on annual earnings (including employment restriction)

	Earnings
$t-4$	7.53*
	(3.68)
$t-3$	7.23
	(4.02)
$t-2$	12.24**
	(4.66)
$t-1$	17.28**
	(5.41)
t	65.09***
	(5.76)
$t+1$	-.62
	(6.93)
$t+2$	-95.25***
	(7.76)
$t+3$	-22.46**
	(7.89)
$t+4$	-15.58
	(8.79)
$t+5$	4.94
	(8.43)
N	1,867,954

* indicates that the estimate is significant at a 5% significance level.
** indicates that the estimate is significant at a 1% significance level.
*** indicates that the estimate is significant at a 0.1% significance level.
Clustered standard errors by individuals are provided in parenthesis.

Table 8: Estimated effect of displacement on annual earnings and employment status across gender

	Earnings		Employment status	
	Male	Female	Male	Female
$t-5$			-.003***	-.003***
			(.0002)	(.0005)
$t-4$	3.97	1.68	-.003***	-.003***
	(3.77)	(4.93)	(.0002)	(.0005)
$t-3$	.11	-22.29***	-.011***	-.017***
	(4.06)	(6.56)	(.0007)	(.0013)
$t-2$	-5.83	-27.23***	-.011***	-.016***
	(4.76)	(7.32)	(.0006)	(.0012)
$t-1$	-10.79	-31.27***	-.010***	-.013***
	(5.55)	(8.13)	(.0005)	(.0012)
t	23.12***	-2.79	-.003***	-.003***
	(6.04)	(8.88)	(.0002)	(.0005)
$t+1$	-200.40***	-197.11***	-.128***	-.194***
	(7.57)	(10.49)	(.0011)	(.0019)
$t+2$	-231.12***	-201.36***	-.038***	-.061***
	(9.30)	(11.93)	(.0016)	(.0029)
$t+3$	-95.41***	-109.53***	-.015***	-.037***
	(9.66)	(12.63)	(.0017)	(.0031)
$t+4$	-65.81***	-84.26***	-.012***	-.023***
	(10.78)	(13.22)	(.0017)	(.0031)
$t+5$	-22.07*	-48.09***	-.006***	-.009**
	(10.26)	(13.61)	(.0018)	(.0032)
N	1,711,545	723,646	1,711,545	723,646

* indicates that the estimate is significant at a 5% significance level.
** indicates that the estimate is significant at a 1% significance level.
*** indicates that the estimate is significant at a 0.1% significance level.
Clustered standard errors by individuals are provided in parenthesis.

Table 9: Estimated effect of displacement on annual earnings and employment status across age groups

	Earnings				Employment status			
	21-30 years	31-40 years	41-50 years	51-60 years	21-30 years	31-40 years	41-50 years	51-60 years
$t-5$					.006***	.002***	-.001***	-.002*
					(.0006)	(.0003)	(.0003)	(.0006)
$t-4$	8.86	3.66	2.40	-1.25	.006***	.002***	-.001***	-.002*
	(8.12)	(5.10)	(4.70)	(6.59)	(.0006)	(.0003)	(.0003)	(.0006)
$t-3$	-8.88	-5.27	-3.55	-2.58	-.014***	-.007***	-.008***	-.006***
	(10.89)	(6.67)	(6.02)	(6.09)	(.0028)	(.0011)	(.0008)	(.0009)
$t-2$	1.76	-13.79	-4.29	-3.71	-.013***	-.009***	-.007***	-.007***
	(12.33)	(7.61)	(7.20)	(6.84)	(.0026)	(.0011)	(.0008)	(.0009)
$t-1$	-1.01	-15.04	.36	-14.45	-.010***	-.005***	-.008***	-.008***
	(13.22)	(8.40)	(8.16)	(8.45)	(.0021)	(.0009)	(.0007)	(.0010)
t	17.40	28.44**	41.47***	14.69	.006***	.002***	-.001***	-.002*
	(13.94)	(9.16)	(9.09)	(9.14)	(.0006)	(.0003)	(.0003)	(.0006)
$t+1$	-182.73***	-81.35***	-135.42***	-337.67***	-.156***	-.107***	-.116***	-.212***
	(16.00)	(10.96)	(11.80)	(11.19)	(.0029)	(.0016)	(.0017)	(.0022)
$t+2$	-173.53***	-142.42***	-162.99***	-347.31***	-.042***	-.027***	-.032***	-.079***
	(18.28)	(12.59)	(14.07)	(14.63)	(.0040)	(.0022)	(.0023)	(.0032)
$t+3$	-118.36***	-75.80***	-58.54***	-133.78***	-.027***	-.016***	-.014***	-.039***
	(18.90)	(13.26)	(14.30)	(15.61)	(.0040)	(.0022)	(.0024)	(.0035)
$t+4$	-91.52***	-54.07***	-60.78***	-72.30***	-.020***	-.011***	-.011***	-.035***
	(19.46)	(13.80)	(16.77)	(16.82)	(.0039)	(.0021)	(.0024)	(.0036)
$t+5$	-67.39***	-31.05*	-16.37	-17.53	-.015***	-.009***	-.008***	-.020***
	(19.60)	(14.59)	(14.63)	(16.70)	(.0037)	(.0021)	(.0025)	(.0038)
N	284,691	748,165	736,604	665,731	284,691	748,165	736,604	665,731

* indicates that the estimate is significant at a 5% significance level. ** indicates that the estimate is significant at a 1% significance level.
*** indicates that the estimate is significant at a 0.1% significance level. Clustered standard errors by individuals are provided in parenthesis.

Table 10: Estimated effect of displacement on annual earnings and employment status across different levels of education

	Earnings			Employment status		
	No high school	High school	University	No high school	High school	University
$t-5$				-.004***	-.001	-.001***
				(.0007)	(.0003)	(.0003)
$t-4$	4.82	8.04	2.28	-.004***	-.001	-.001***
	(3.84)	(4.27)	(7.05)	(.0007)	(.0003)	(.0003)
$t-3$	-2.93	-4.17	.60	-.013***	-.012***	-.009***
	(5.05)	(4.24)	(9.14)	(.0012)	(.0009)	(.0012)
$t-2$	-9.59	-7.83	-4.77	-.013***	-.011***	-.009***
	(5.83)	(4.72)	(10.76)	(.0013)	(.0008)	(.0012)
$t-1$	-24.87***	-3.81	-10.14	-.013***	-.008***	-.008***
	(6.18)	(5.48)	(12.35)	(.0012)	(.0007)	(.0010)
t	6.44	28.37***	27.32*	-.004***	-.001	-.001***
	(6.99)	(5.62)	(13.78)	(.0007)	(.0003)	(.0002)
$t+1$	-312.00***	-227.98***	-14.73	-.224***	-.157***	-.062***
	(8.85)	(7.12)	(16.53)	(.0025)	(.0015)	(.0013)
$t+2$	-264.87***	-233.46***	-119.32***	-.073***	-.050***	-.013***
	(11.06)	(8.27)	(20.10)	(.0036)	(.0020)	(.0020)
$t+3$	-101.56***	-119.55***	-15.69	-.032***	-.026***	-.004
	(11.64)	(8.68)	(20.95)	(.0038)	(.0021)	(.0021)
$t+4$	-46.78***	-88.76***	-12.95	-.025***	-.021***	.003
	(11.99)	(9.34)	(23.69)	(.0039)	(.0021)	(.0023)
$t+5$	-15.34	-41.93***	27.39	-.019***	-.009***	.006*
	(12.07)	(9.22)	(22.34)	(.0040)	(.0022)	(.0024)
N	540,342	1,249,281	645,568	540,342	1,249,281	645,568

* indicates that the estimate is significant at a 5% significance level.
** indicates that the estimate is significant at a 1% significance level.
*** indicates that the estimate is significant at a 0.1% significance level.
Clustered standard errors by individuals are provided in parenthesis.

Table 11: Estimated effect of displacement on annual earnings and employment status across different levels of tenure

	Earnings		Employment status	
	Low tenure	High tenure	Low tenure	High tenure
$t-5$			.001*	-.002***
			(.0004)	(.0003)
$t-4$	-15.22	1.14	.001*	-.002***
	(9.35)	(2.82)	(.0004)	(.0003)
$t-3$	-30.19***	-6.39	-.006**	-.002***
	(8.92)	(3.56)	(.0019)	(.0003)
$t-2$	-50.19***	-19.64***	-.006***	-.002***
	(10.24)	(4.09)	(.0018)	(.0003)
$t-1$	-111.80***	-23.42***	-.008***	-.002***
	(12.33)	(4.81)	(.0014)	(.0003)
t	-117.61***	17.52**	.001*	-.002***
	(12.06)	(5.78)	(.0004)	(.0003)
$t+1$	-251.28***	-242.84***	-.097***	-.182***
	(13.27)	(7.63)	(.0014)	(.0016)
$t+2$	-211.14***	-332.94***	-.006*	-.075***
	(17.67)	(9.07)	(.0025)	(.0020)
$t+3$	-109.96***	-196.64***	.004	-.042***
	(18.23)	(9.24)	(.0026)	(.0021)
$t+4$	-148.36***	-129.50***	-.004	-.027***
	(20.88)	(9.93)	(.0027)	(.0021)
$t+5$	-132.86***	-74.37***	-.003	-.020***
	(19.42)	(9.90)	(.0028)	(.0022)
N	776,259	1,298,836	776,259	1,298,836

* indicates that the estimate is significant at a 5% significance level.
** indicates that the estimate is significant at a 1% significance level.
*** indicates that the estimate is significant at a 0.1% significance level.
Clustered standard errors by individuals are provided in parenthesis.

Table 12: Estimated effect of displacement on annual earnings for closure and mass layoff subgroup

	Earnings	
	Mass Layoffs	Closures
$t-4$	6.92*	-17.42***
	(3.15)	(4.29)
$t-3$	-4.37	-22.03***
	(3.63)	(5.43)
$t-2$	-10.50*	-25.86***
	(4.18)	(6.56)
$t-1$	-18.95***	-17.77*
	(4.79)	(7.43)
t	12.82*	18.46*
	(5.23)	(8.74)
$t+1$	-176.38***	-312.96***
	(6.59)	(9.91)
$t+2$	-204.48***	-294.83***
	(7.84)	(10.97)
$t+3$	-85.07***	-148.01***
	(8.24)	(11.58)
$t+4$	-53.93***	-134.71***
	(8.95)	(12.56)
$t+5$	-17.95*	-68.05***
	(8.70)	(12.77)
N	2,139,885	1,197,537

* indicates that the estimate is significant at a 5% significance level.
** indicates that the estimate is significant at a 1% significance level.
*** indicates that the estimate is significant at a 0.1% significance level.
Clustered standard errors by individuals are provided in parenthesis.

Table 13: Estimated effect of displacement on employment status for closure and mass-layoff subgroups

	Employment status	
	Mass Layoffs	Closures
$t-5$	-.004***	.002***
	(.0002)	(.0005)
$t-4$	-.004***	.002***
	(.0002)	(.0005)
$t-3$	-.014***	-.005***
	(.0006)	(.0011)
$t-2$	-.014***	-.005***
	(.0006)	(.0011)
$t-1$	-.012***	-.007***
	(.0005)	(.0010)
t	-.004***	.002***
	(.0002)	(.0005)
$t+1$	-.153***	-.121***
	(.0011)	(.0021)
$t+2$	-.044***	-.043***
	(.0015)	(.0025)
$t+3$	-.020***	-.021***
	(.0015)	(.0026)
$t+4$	-.014***	-.015***
	(.0016)	(.0026)
$t+5$	-.005***	-.011***
	(.0016)	(.0027)
N	2,139,885	1,197,537

* indicates that the estimate is significant at a 5% significance level.
** indicates that the estimate is significant at a 1% significance level.
*** indicates that the estimate is significant at a 0.1% significance level.
Clustered standard errors by individuals are provided in parenthesis.

Table 14: Control variables: Estimated effect of job displacement on earnings

	Earnings		
	OLS	Plant FE	Individual FE
Time-invariant variables			
Male	811.4397***	802.8055***	
	(5.436782)	(5.746235)	
No high school degree	-351.8976***	-257.7498***	
	(5.262093)	(5.00806)	
University degree	1242.665***	1031.719***	
	(9.060671)	(8.924248)	
Domestic partnership	331.8819***	292.881***	
	(5.736871)	(5.500894)	
Child 0-3 years	-317.1197***	-319.1931***	
	(8.311732)	(8.037423)	
Plant size	.0446101***		
	(.0041087)		
Experience	9.490463***	8.270294***	
	(.280358)	(.2785455)	
Experience^2	.0085533***	.0076303***	
	(.000302)	(.0002982)	
Time-variant variables			
Experience^2			-2.528073***
			(.0447785)
Experience^3			-.0122123***
			(.0005165)
Experience^4			-8.10e-06***
			(4.25e-07)
N	2,429,405	2,429,405	2,429,405

Dummy variables of industry codes, FA-regions and base-year specific time dummies are excluded from the table. The time invariant variables are observed in year t.

* indicates that the estimate is significant at a 5% significance level.

** indicates that the estimate is significant at a 1% significance level.

*** indicates that the estimate is significant at a 0.1% significance level.

Clustered standard errors by individuals are provided in parenthesis.

Table 15: Control variables: Estimated effect of job displacement on employment status

	Employment status		
	OLS	Plant FE	Individual FE
Time-invariant variables			
Male	.0257423***	.0233468***	
	(.0005855)	(.0005765)	
No high school degree	-.0114403***	-.0073096***	
	(.0007671)	(.0007425)	
University degree	.0160687***	.008375***	
	(.0005471)	(.0005694)	
Domestic partnership	.0206068***	.0202808***	
	(.0005696)	(.0005498)	
Child 0-3 years	-.0133234***	-.0134964***	
	(.0006809)	(.0006609)	
Plant size	6.21e-06***		
	(3.59e-07)		
Experience	-.0025557***	-.0027457***	
	(.0000316)	(.0000311)	
Experience^2	-2.78e-06***	-2.97e-06***	
	(3.45e-08)	(3.39e-08)	
Time-variant variables			
Experience^2			-.0003262***
			(.0000131)
Experience^3			-3.06e-06***
			(1.60e-07)
Experience^4			-2.20e-09***
			(1.31e-10)
N	2,429,405	2,429,405	2,429,405

Dummy variables of industry codes, FA-regions and base-year specific time dummies are excluded from the table. The time invariant variables are observed in year t.
* indicates that the estimate is significant at a 5% significance level.
** indicates that the estimate is significant at a 1% significance level.
*** indicates that the estimate is significant at a 0.1% significance level.
Clustered standard errors by individuals are provided in parenthesis.

Table 16: F-test for joint significance of fixed effects

	F-statistics	
	Earnings	Employment status
Individual FE	2.69***	0.39
Plant FE	33.84***	1.05

*** indicates that the fixed effects are jointly significant at a 1% significance level. Critical values: 1.25 at 1% significance level and 1.17 at 5% significance level.

Table 17: Chow test for significance in difference between gender subgroups

F-statistics	
Earnings	Employment status
72.68***	45.80***

*** indicates that coefficients between two subgroups are significantly different at 1% significance level.Critical values: 1.25 at 1% significance level and 1.17 at 5% significance level.

Table 18: Chow test for significance in difference between age subgroups

	F-statistics	
	Earnings	Employment status
21-30 vs 31-40	142.59***	15.56***
21-30 vs 41-50	159.78***	27.08***
21-30 vs 51-60	160.63***	258.02***
31-40 vs 41-50	199.05***	15.49***
31-40 vs 51-60	157.42***	403.82***
41-50 vs 51-60	69.69***	263.90***

*** indicates that coefficients between two subgroups are significantly different at 1% significance level. Critical values: 1.25 at 1% significance level and 1.17 at 5% significance level.

Table 19: Chow test for significance in difference between education subgroups

	F-statistics	
	Earnings	Employment status
No highschool vs Highschool	87.21***	69.57***
No highschool vs Higher edu	191.11***	269.28***
Highschool vs Higher edu	492.67***	165.23***

*** indicates that coefficients between two subgroups are significantly different at 1% significance level. Critical values: 1.25 at 1% significance level and 1.17 at 5% significance level.

Table 20: Chow test for significance in difference between tenure subgroups

F-statistics	
Earnings	Employment status
149.33***	114.09***

*** indicates that coefficients between two subgroups are significantly different at 1% significance level. Critical values: 1.25 at 1% significance level and 1.17 at 5% significance level.