Job Displacement and Labor Market Outcomes by Skill Level

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Job Displacement and Labor Market Outcomes by Skill Level

David Seim*

Abstract

This paper investigates the effects of displacement on outcomes such as annual earnings, unemployment, wages and hours worked. It relies on previously unexplored administrative data on all displaced workers in Sweden in 2002, 2003 and 2004 which are linked to employer-employee matched data at the individual level. By linking the data to military enlistment records, the paper assesses the selection into displacement and finds that workers with low cognitive and noncognitive skills are significantly more likely to be displaced than high-skilled workers. The analysis of displacement effects shows evidence of large and long-lasting welfare costs from displacement. Moreover, studying the heterogenous impacts of job displacement in terms of cognitive and noncognitive skills reveals that although workers with high skills fare better than low-skilled workers in absolute terms, there are no significant differences in the recovery rates between skill groups. Finally, by using administrative data on displacements, it is possible to assess quantitatively the bias that results from not being able to separate quits from layoffs in earlier studies.

JEL Code: J60, J63, J65, I21, C23

Keywords: job displacement, cognitive and noncognitive skills, employer-employee data

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The views expressed are those of the author and do not necessarily represent the official views of Eesti Pank.

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Non-technical summary

While technological innovation is supposed to be the source behind economic growth and a well-functioning economy, it involves moving workers from low-productivity jobs into new and more productive ones. This is often accomplished through layoffs. A substantial body of research in economics has shown that this entails large private costs for the displaced. Workers who are laid off do not adjust to the new labor market conditions and instead their earnings drop, their health deteriorates and even mortality rates among displaced workers increase.

This paper analyzes the consequences for all workers who lost their jobs in Sweden during the years 2002, 2003 and 2004. I study the effect on labor earnings and find that one year after they lost their jobs, the displaced workers were on average earning 23.2 percent less than they would have done if they had not suffered the displacement. However, even seven years after the displacement, workers who had been laid off were earning 15 percent less than would have been the case had they not experienced job loss. These results suggest that job loss entails not only a temporary shock to income, but that the income level is permanently shifted to a lower level.

In order to understand why these shocks are so severe and to give recommendations to policy makers about how to counteract the effects, the paper analyses which workers suffer the most. Motivated by the fact that cognitive ability — IQ — has been shown to matter a lot for labor market outcomes, the impact of job loss for workers of high cognitive ability is compared with that for workers of low cognitive ability. All the analyses in this paper use individual level information. Since Sweden had mandatory military enlistment for all men at the age of 18, records on cognitive ability exist for all men. Moreover, the military enlistment procedure consisted of an interview with a psychologist who evaluated the individual's non-cognitive skills. Personality traits that were rewarded in the interview include emotional stability, perseverance and having an outgoing character. By linking the military enlistment data with information on earnings and job displacements, it is found that cognitive and non-cognitive skills matter for the probability of a worker becoming displaced. Individuals with high cognitive and non-cognitive skills have a lower probability of job loss than do low-skilled workers. However, among workers who lose their jobs, ability did not have an impact on the way they recovered from job loss. In percentage terms, both the initial and the long-term effects of job loss are the same for high- and low-skilled workers.

To conclude, no evidence is found that policy makers should treat displaced workers differently depending on their ability. A much more important factor in determining the impact of job loss is age. Young workers recover at a much

faster pace than older workers do. This motivates the targeting of labor market programmes at older and displaced workers.

Contents

1. Introduction

A large body of literature has established that the individual costs of job displacement are large and long-lasting. Earlier analyses cover the impact on labor market outcomes such as earnings and unemployment (Ruhm (1991), Jacobson, Lalonde and Sullivan (1993), Stevens (1997), Kuhn (2002), Farber (2003), Eliason and Storrie (2006), Couch and Plazcek (2009)), health and mortality (Sullivan and von Wachter (2009), Eliason and Storrie (2009)), and the school grades of displaced workers' children (Rege et al. (2010)). Despite extensive research into the consequences of job losses, our understanding of who becomes displaced and of how different individuals deal with displacement is far from complete. In order to identify adequate policy measures for assisting displaced workers, it is of the utmost importance to understand which groups are most vulnerable.

The importance of ability for labor market outcomes is well recognized within labor economics.[1] Measuring ability empirically, however, is not straightforward. While proxies for acquired skills include IQ-tests, education and formal training, more general skills are difficult to gauge. A recent literature distinguishes between cognitive and noncognitive abilities in trying to capture skills. Heckman, Stixrud and Urzua (2006) and Lindqvist and Vestman (2011) show that noncognitive abilities, such as persistence, motivation and trustworthiness are as important as cognitive abilities in explaining wages.

It has also been suggested that the long-term effects of displacements are different for workers with high and low ability. Neal (1998) argues that high-skilled workers recover faster due to their ability to re-accumulate job-specific skills. Previous empirical studies have assessed the importance of education, occupation, age and industry for the impact of displacement, and also for the selection into displacement. But, to the best of my knowledge, nobody has investigated how the impact of displacement varies with cognitive and noncognitive skills.

This paper attempts to add to our understanding of displacement and labor market outcomes by using a large and unique dataset of Swedish workers. The main building block of the dataset comprises longitudinal employer-employee matched data on all Swedish individuals in the labor force in any of the years 2002, 2003 and 2004. This dataset, which contains information on basic characteristics such as age and education, is merged with administrative data on all the displacements that occurred in the same years at the individual level. In addition, the dataset contains information on cognitive and noncognitive skills

[1] See, for instance, Cawley, Heckman, and Vytlacil (2001), Herrnstein and Murray (1994) and the summary provided by Bowles, Gintis, and Osborne (2001).

for almost all men, measured at conscription, i.e. when the men were about 18 years of age.

The contribution to the literature is threefold. First, the institutional setting in Sweden allows me to distinguish between quits and layoffs. In Sweden, employers wishing to lay off five or more workers simultaneously, or 20 or more workers within a 90 day period, must report this to the Public Employment Service (henceforth PES). By linking this data to employer-employee matched records at the individual level, it is possible to separate voluntary quits from layoffs, thereby resolving a problem in previous studies. Second, Sweden had mandatory conscription until 2010 with the implication that all men around the age of 18 had to undergo two days of physical and mental tests. The draft procedure included a test of the conscript's cognitive skills with sub-tests of logical, spatial, verbal and technical ability. Moreover, in a semi-structured interview a psychologist evaluated each conscript's noncognitive skills. Personality traits that yielded high test scores include persistence, an outgoing character and the willingness to assume responsibility.[2] Merging this data with the employer-employee matched data allows me to estimate how the selection into displacement depends on cognitive and noncognitive skills and how the impact of job loss varies with abilities. Third, the dataset is linked to information about wages and hours worked in order to permit study of the intensive-margin responses to displacement. Although the displaced workers who manage to find a job are a selected group, studying the intensive-margin provides some indication of how new job-specific skills are acquired.

With this data, I first investigate how the selection into displacement depends on skills and age. It turns out that workers with high cognitive and noncognitive skills are significantly less likely to experience job loss than workers with low skill levels. An increase of one standard deviation in either skill measure decreases the probability of the worker being displaced by one percent. In the same analysis, age is shown to matter for the displacement decision. Younger workers are significantly more likely be displaced when plants downsize.

In the analysis of displacement effects on economic outcomes, the impact on total income is first estimated as a proxy for the workers' disposable income. Income is then disaggregated into labor earnings, wages, hours worked and dependence on social and unemployment insurance. I find that the estimated initial drop in income is not as large as the impact on labor earnings, which is estimated at 23.2 percent of pre-displacement earnings. Turning to the long-term effects, labor earnings losses are 15 percent lower than the coun-

[2]Lindqvist and Vestman (2011) argues that although the purpose of the interview was to assess conscripts' ability to fit in with the military environment, the traits that were rewarded are also prized by the labor market. See Carlstedt (2000) for a review of the tests.

terfactual seven years after the displacement. Conditional on the worker having a job, the hourly wage is estimated to have dropped by 18.6 percent two years after displacement.

When studying how ability and age matter for post-displacement patterns, the heterogenous effects on labor earnings are investigated and I find that the patterns for labor earnings are equal in percentage terms for high-skilled and low-skilled workers. The strongest differential impact for labor earnings is found across age groups. Young workers recover at a much faster pace than older workers, regardless of the outcome variable under study. To study how skills matter for the willingness to acquire new skills, I look at the heterogenous effects on student transfers received from the government for adult secondary and tertiary studies as well as participation in job-training programmes. I find that cognitively able and young workers are more likely to start studying after displacement, but there is no such pattern for noncognitive skills. Less able workers are more likely to participate in job-training programmes, irrespective of the skill measure used.

The final part of the paper addresses the potential bias that is present in previous studies that are unable to distinguish quits from layoffs. I estimate the effects of displacement, where displacements are either defined as separations in periods of mass separations, or as separations in time windows preceding plant closure. The mass layoff estimates turn out to understate significantly the effects of displacement, while use of a three-year window prior to plant closure overestimates the effects. The most accurate estimates are obtained by defining displacements as separations occurring during a one-year window preceding plant closure.

The paper is organized in the following way. Section 2 presents the institutional background for the administrative data on displacements. Section 3 outlines the data and the sample restrictions imposed. Section 4 studies the selection into displacement. The econometric framework is presented in section 5. Results regarding the main and the heterogenous effects of displacement are discussed in section 6. Section 7 presents the comparison with earlier studies. Section 8 concludes.

2. Institutional Setup

A problem in previous studies of the costs of job loss is the inability to distinguish quits from layoffs. Even with matched employer-employee data, these studies can not differentiate between voluntary and involuntary separations. In order to resolve this, the current convention is to rely on defining displacements as separations occurring within certain windows of mass-

separations. An alternative method is for workers separated during a time window preceding plant closure to be defined as displaced. The advantage of this is that selection into displacement within the plant is smaller compared to mass layoffs, thus making displacement more exogenous with respect to individual characteristics.

Without information regarding actual displacements some bias is inevitable. First of all, workers have notice periods that are negotiated in individual contracts and depend on variables such as tenure and age. An announcement of displacements for all workers will thus lead to heterogenous layoff dates. An even more serious problem, pointed out by Kuhn (2002), is that the actual separation date is endogenous. The observed separation date in the data may be different from the reported layoff date since workers can start searching for new jobs, register as unemployed, or move to some other region before the prescribed displacement date. This means that at the end of a particular time window, the observed separations will consist of displaced workers with long notice times who were not able to find new jobs during the notice period. Expanding the time window will lead to more displaced workers being included, but it will also imply additional mislabelling of voluntary quits as displacements.

Kuhn (2002) further discusses the problem of falsely labelling separations as displacements. Distressed firms may find ways to dispose of workers, other than formally displacing them. Employers and employees may sign a termination contract voluntarily, older workers may enter into early retirement, acquisitions of parts of distressed firms may imply transitions of workers to the new employer, and workers may enter into disability insurance without having been displaced.

To resolve these issues, I use administrative data on displacements in Sweden. Under Sweden's employment protection law, an employer intending to displace five or more workers simultaneously, or 20 or more workers within a 90 day period, must notify the PES in advance.[3] The notification occurs in two steps: (i) first, the intended number of displaced workers is reported by the employer to the PES together with the cause for the displacement and (ii) no later than one month after the first report, a list of the displaced workers and their displacement dates must be submitted.[4]

[3]Displacements are reported separately for each plant.

[4]These lists are determined in negotiations with labor unions but are characterized by the "last-in-first-out" principle, which means that the workers who were employed last must leave the firm first when downsizing occurrs. This principle is set out in the Swedish Employment Protection Act.

3. Data

In order to estimate the effect of job loss on earnings, unemployment, hourly wages and hours worked as a share of full-time employment, I construct a matched dataset with information from various administrative sources. Earnings histories are obtained from employer-employee matched records (*Registerbaserad Arbetsmarknadsstatistik*) stretching from 1999 to 2009 for all Swedish citizens who were part of the labor force in any of the years 2002, 2003 or 2004. This dataset is merged with demographic information on education, occupation, age and gender from the Longitudinal Integration Database for Health Insurance and Labour Market Studies (*LISA*). Unemployment records are obtained from the Unemployment Register (*HÄNDEL*) and merged with the other data.

Every year in November, Statistics Sweden conducts an inquiry aimed at describing the Swedish wage structure. Employers participating in the study are asked to report the wage of their employees along with information about hours worked and prospective bonuses. The study covers all public sector employees and all employers with more than 500 employees in the private sector. In addition, a random sample stratified by industry and firm size is included. The study produces the Wage Register (*Lönestrukturstatistiken*), which contains information on wages and hours worked for the job held in November. This register is also linked to the baseline dataset.

The PES data include nearly all workers displaced during any of the years 2002–2004 along with a record of the date at which the announcement was received by the PES. To avoid recall events, I exclude all workers who are displaced in the same event and are then working together four years after the announcement.

Finally, I merge these data with individual-level data from the Swedish military enlistment.[5] Officially, Sweden had conscription until 2010, with the implication that all men had to undergo two days of tests of their medical status, physical capacity and cognitive and noncognitive skills.[6] The tests were usually taken at the age of 18 or 19. The cognitive test consisted of four subtests each comprising 40 tasks presented in increasing order of difficulty. The four tests measured inductive skills, verbal skills, spatial ability and technical comprehension. After the cognitive test, each conscript's noncognitive ability was evaluated by psychologists. The assessment looked at the conscripts' ca-

[5] See Carlstedt (2000) and Lindqvist and Vestman (2011) for overviews of the enlistment procedure.

[6] In practice, mandatory enlistment was gradually abandoned implying that the individuals taking the tests during the 2000s consisted increasingly of men and women who were motivated to do military service.

pacity to match the psychological requirements of the military and rewarded them for traits such as a willingness to assume responsibility, independence, an outgoing character, persistence, emotional stability and power of initiative (see Lindqvist and Vestman (2010)). Like the cognitive test, the noncognitive measure consisted of the sum of four sub-tests. As the tests changed several times over the years, I rank the individuals' scores by percentile, within each enlistment year. Under the conventional assumption of normally distributed skills, the variables measuring cognitive and noncognitive skills are obtained from the inverse of the normally distributed CDF, producing standard, normally distributed variables. By merging this data with those above, I obtain skill measures for almost all male workers.

Before starting the analysis, I impose certain restrictions on the data. The adequate sample restrictions imposed differ for the study of who becomes displaced and the analysis of the effects of job loss. Beginning with the sample retained for analysis of the effects of job displacement, I follow Jacobson, LaLonde and Sullivan (1993) (henceforth JLS) by studying workers with stable employment relationships. In contrast to JLS, who focus on workers staying with the same employer for six consecutive years, a stable job is defined here as having kept the same job for more than six quarters. Restriction of the workers studied to those between 20 and 51 years of age means that all workers have a potential attachment to the labor force during the sample period. Due to the high turnover in the construction sector, this sector is excluded. When defining the control group, I exclude the self-employed.

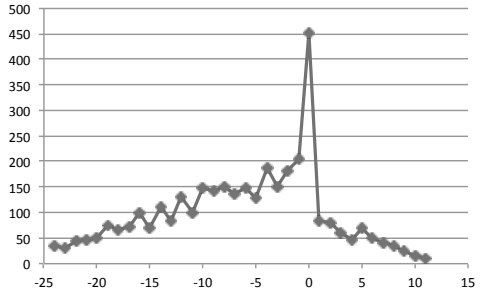
Since the military enlistment data cover men only, there are no women in the sample. However, when the effects on hourly wages and hours worked are analyzed, the sample is further restricted to include only those men who hold a job and are included in the Wage Register.

In the absence of randomly assigned displacement, I restrict the sample to only include displacement events where more than 80 percent of all workers who satisfy the restrictions above received the displacement announcement simultaneously.[7] This is done to avoid selection into displacement within the plant. The potential control group consists of all workers in Sweden during this period who are not included in any of the lists of displaced workers. Tables 4 4 and 5 provide descriptive statistics of the sample and can be found in the Appendix.

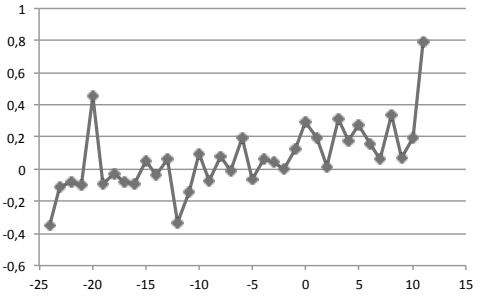
An important concern relates to the timing of the announcement. If workers expect the displacement announcement, they may engage in on-the-job-search

[7]The robustness of this approach was examined by conducting the analysis on a sample with cases where all workers were given the displacement notice simultaneously. The results are qualitatively similar across methods, but the statistical power is stronger with the somewhat more lenient approach.

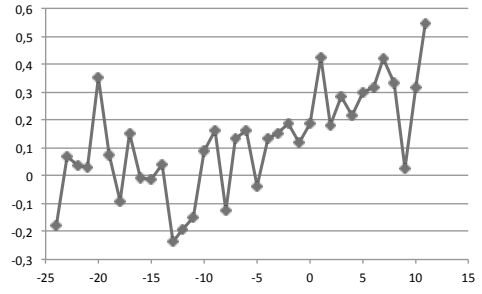
Figure 1: Voluntary Quits (The x-axis denotes normalized difference in months between the announcement and the separations, with the announcement occurring in month 0.)



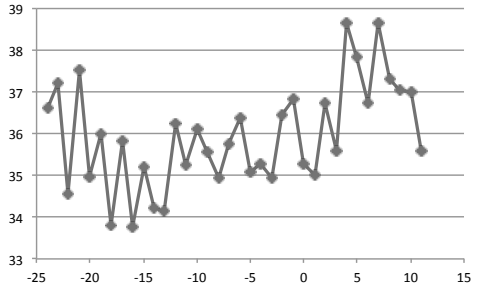
(a) Total Number of Separations



(b) Mean Cognitive Skills of Quitters



(c) Mean Non-cognitive Skills of Quitters



(d) Mean Age of Quitters

before the announcement, leading to an overestimation of the costs of displacement as the group of workers who remain at the firm at the announcement date becomes a selected sample. Figure 6 shows the number of separations in the months surrounding the announcement. Time is normalized so that 0 corresponds to the announcement month. The number of separations increases as the announcement draws nearer and peaks in the month of the announcement. To investigate the characteristics of the workers who separate prior to the announcement, the same figure presents mean cognitive and noncognitive skills by month of separation. Even though there is a selection out of the workplace, it does not seem to be the more generally able workers who separate early. On the contrary, the graphs suggest that there is a selection out of the workplace that is negatively correlated with skills.

4. Selection into Displacement

To motivate the analysis of the heterogenous effects of displacement with respect to age and skill, this section aims at describing how these characteristics matter for the incidence of displacement. The statistical model which estimates the impact of these attributes on displacement is:

$$P_{ijt} = \alpha_j + \eta_t + \mathbf{z_i}\beta + \mathbf{x}'_{ijt}\gamma + \varepsilon_{ijt}, \tag{1}$$

where P_{ijt} is an indicator of worker i being displaced from plant j at time t, $\mathbf{z_i}$ denotes the independent variables including age categories and normalized measures of cognitive and noncognitive skills. Controlling for plant fixed effects, α_j, takes time invariant plant heterogeneity into account and the inclusion of year fixed effects, η_t ensures that results are not driven by macroeconomic developments. Beyond indicator variables for county of residence, the control variables, $\mathbf{x}$, contain information that was determined prior to enlistment and may have affected test scores. These variables are an indicator of education beyond primary level, cohort dummies and enlistment year dummies. When estimating the effect of age on displacement, I create three indicator variables denoting young (aged 25–34), middle aged (aged 35–44) and senior workers (45–54). Equation (1) is estimated using Ordinary Least Squares.

Patterns in the incidence of displacement are presented in Table 1. While both skill measures appear important in predicting displacement, the slope is slightly steeper for noncognitive skills. Since the skill measures are positively correlated, the first and second columns comprise upper bounds on the effect of the skill measures on the probability of displacement occurring.[8] When both ability measures are included, the magnitude of the estimates weakens

[8]The correlation between cognitive and noncognitive skills is 0.3501. Though the source

due to the positive correlation between the two. The addition of interaction and higher-order terms does not affect the results, which suggests that there is indeed a linear relationship between skill levels and the probability of displacement. The point estimates of the last column suggest that an increase of one standard deviation increase in either skill type decreases the probability of displacement occurring by one percent.

Table 1: Selection into Displacement

	(1)	(2)	(3)	(4)	(5)	(6)
Cognitive Skills	–0.013***		–0.010***		–0.010***	–0.010***
	(0.002)		(0.002)		(0.002)	(0.002)
Noncognitive Skills		–0.014***	–0.011***		–0.011***	–0.011***
		(0.002)	(0.002)		(0.002)	(0.002)
Cognitive Squared					0.001	0.001
					(0.001)	(0.001)
Noncognitive Squared					0.000	0.000
					(0.001)	(0.001)
Cognitive x Noncognitive					–0.002	–0.002
					(0.002)	(0.002)
Age 35–44				–0.026***		–0.026***
				(0.003		(0.003)
Age 45–54				–0.052***		–0.052***
				(0.005)		(0.005)
County of Residence Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Enlistment year dummies	Yes	Yes	Yes		Yes	Yes
Observations	62171	62171	62171	62168	62171	62168
R-squared	0.428	0.428	0.429	0.425	0.429	0.428

Notes: The dependent variable is an indicator of displacement occurring. All models are estimated using OLS and include plant and year fixed effects. Standard errors are clustered at the plant level. *: p<0.10, **: p<0.05, ***: p<0.01.

The inclusion of plant fixed effects allows within-plant variations to be exploited and avoids the risk that plants with a high share of low ability workers may be more inclined to displace workers in the first place. For instance, industries such as construction and manufacturing employ relatively high numbers of low-skilled workers and exhibit greater turnover due to their greater cyclical dependence. Such sorting effects are controlled for by the use of firm fixed effects. However, it may well be that turnover rates within a firm are larger among low-skilled workers, implying that tenure is, on average, lower among low-skilled workers. This could imply differential layoff rates under the last-in-first-out rule (henceforth, LIFO) that governs displacement decisions. It could also be explained by the argument in Neal (1998): as

of the covariance is ambiguous (see Lindqvist and Vestman (2010)), estimating equation 1 without each skill variable individually provides an upper bound for the effects while estimating the equation with both measures together yields a lower bound.

high-skilled workers are more capable of learning, they accumulate more job-specific skills and are therefore more highly valued by employers than low-skilled workers. This leads to lower turnover rates among high-skilled workers.

Age is also important in explaining the incidence of displacement. According to column (1), workers aged 35–44 are less likely to be displaced than young workers aged 25–34, with the point estimate suggesting a difference of 2.6 percent.[9] Older workers are even less likely to become displaced. Since estimations are based on within-plant comparisons, the results may to a large extent be driven by the LIFO rule. Tenure and age are positively correlated and firms must abide by the LIFO rule when downsizing, implying that young workers face a higher risk of displacement.

5. Econometric Framework

The previous section covered the selection into displacement, and this part of the paper now concentrates on estimating the effects of displacement. The literature that estimates the effects of job displacement employs the statistical approach taken in the program-evaluation literature by defining a treatment group (displaced) and a control group (nondisplaced).[10] When comparing outcomes before and after displacement across the groups, I exploit the advantage of longitudinal panel data for workers in Sweden. The model to be estimated is:

$$y_{ijt} = \alpha_i + \gamma_{jt} + \sum_{k \geq -5} \delta_j^k D_{ijt}^k + \mathbf{x}_{it}'\beta + \varepsilon_{ijt}, \tag{2}$$

where y_{ijt} denotes the outcome variable of individual i in group j at time t.[11] D_{ijt}^k is an indicator of worker i belonging to group j at time t and being displaced in year $t-k$. The formulation of the dummy variables implies that a worker displaced in 2002 faced the same situation in 2005 as a worker displaced in 2004 did in 2007. δ_j^k measures the effect on the outcome variable of displacement k years ago for a worker in group j. The displaced workers are sometimes reffered to as treated and non-displaced workers as controls for simplicity. By estimating effects prior to actual displacement, I assess the validity of the treatment and control groups as the difference in their outcome variables should not be significantly different from zero in pre-treatment periods. One coefficient for each group and year is obtained and by including

[9] The youngest workers in the sample are 25 in the displacement year.

[10] Examples of studies taking such an approach include Jacobson, Lalonde and Sullivan (1993), Couch and Placzek (2009) and Sullivan and von Wachter (2009).

[11] The only control variable included here, as captured by $\mathbf{x}_{it}'$, is age squared.

group-specific time trends, γ_{jt}, the effect on the outcome variable is measured relative to nondisplaced workers within that group. The inclusion of worker fixed effects, denoted α_i, allows the selection into displacement to depend on time-invariant characteristics for the effects to be unbiased.

While the above approach yields the predicted effects for various characteristics, the estimated parameters do not reveal whether the effects are significantly different from each other. In order to reduce the number of parameters to be estimated, I follow JLS and re-parametrize the above equation in the following way.

$$y_{ijt} = \alpha_i + \sigma_j t_j + \rho_j^{PRE} F_{ijt}^{PRE} + \rho_j^{IMPACT} F_{ijt}^{IMPACT} + \rho_j^{POST} F_{ijt}^{POST} + \mathbf{x}'_{it}\beta + \varepsilon_{ijt}, \tag{3}$$

where σ_j allows for differential group-specific linear trends in the absence of displacement based on different characteristics, and the response to displacement is allowed to vary in the following way:

$$F_{ijt}^{PRE} = \begin{cases} t+k & \text{if } k \leq 0 \text{ and worker } i \text{ in group } j \text{ is displaced in year } k = 0. \\ 0 & \text{otherwise} \end{cases}$$

$$F_{ijt}^{IMPACT} = \begin{cases} 1 & \text{if } k = 1, 2 \text{ and worker } i \text{ in group } j \text{ is displaced in year } k = 0. \\ 0 & \text{otherwise} \end{cases}$$

$$F_{ijt}^{POST} = \begin{cases} t+k & \text{if } k \geq 2 \text{ and worker } i \text{ in group } j \text{ is displaced in year } k = 0. \\ 0 & \text{otherwise} \end{cases}$$

F_{ijt}^{PRE} represents a linear time trend before displacement, F_{ijt}^{IMPACT} captures a jump in the outcome variable immediately following displacement and F_{ijt}^{POST} is a linear time trend representing recovery. As the underlying trend accounts for heterogeneity, all estimates of the effect of job displacement will be relative to non-displaced workers in that same group.[12]

6. Empirical Findings

Equipped with suggestive evidence that younger and less cognitively and non-cognitively able workers are more likely to experience displacement, one

[12]When individuals differ along a continuous dimension, I assume a linear relation in the heterogenous impact. For cognitive and noncognitive skills which are normally distributed variables with mean zero, the baseline effect refers to workers with average skill levels.

purpose of this section is to estimate the heterogenous effects of displacement on different outcome variables with respect to these characteristics. However, the effects are initially investigated for the full sample. It is important to note that the sample retained for analysis of job displacement effects differs from the sample used for analyzing the selection into displacement. In the selection analysis, the sample consists of displacing plants and employees who are employed at the time of the displacement announcement. Here instead, the displaced workers must satisfy the restrictions discussed in section 3. In particular, I only consider displacements in which more than 80 percent of the workforce were displaced simultaneously.

Using the extensive Swedish administrative data, I first analyse how disposable income evolves over time. This is a better measure than labor earnings for estimating the effect of displacement on a worker's welfare. Total income is then decomposed into labor earnings, capital income, and social and unemployment insurance in order to understand the mechanisms behind the effects on welfare. Using data on wages and hours worked, I also investigate intensive margin responses to displacement.

Having studied the aggregate effects of displacement, I then turn to study heterogenous responses based on cognitive and noncognitive skills and age. In order to estimate whether high-skilled workers are better at learning new skills and therefore experience faster rates of recovery, I focus on a subset of variables. In particular, the effects on labor earnings and the propensity to start studying are investigated, measured as the amount of student transfers received from the government, as are job training programmes organized by the Public Employment Service.[13]

6.1. Baseline Estimations

When estimating (2), the outcomes are measured annually. The data stretch from 1999 to 2009 thus encompassing a maximum of five pre-displacement years with seven post-displacement observations for each variable. I first estimate the equation without group specific effects. The estimated coefficients and their associated 95 percent confidence intervals are displayed in Figure 2. The confidence intervals are derived from standard errors clustered at the level of the employer in the displacement year. Time is normalized so that year zero denotes the year that the displacement announcement was reported.

Looking at the figure, the first graph presents the effects on disposable income. As the sum of labor earnings, capital income, and social and un-

[13]Enrolment in adult secondary and tertiary education yields entitlement to student transfers, while job training programmes do not.

employment insurance income at the individual level, this measure does not cover risk sharing and responses within the family, but the estimates provide an upper bound for the impact on welfare. Disaggregating income into different components, the effects on labor earnings are presented secondly in panel (b). In contrast to the results of the study by JLS, there is no estimated drop in labor earnings prior to displacement. If anything, displaced workers' earnings tend to increase slightly. One explanation for this may be that displacement is measured as the date at which the announcement was made, rather than the separation date. The existence of notice periods delays the effects of job loss.

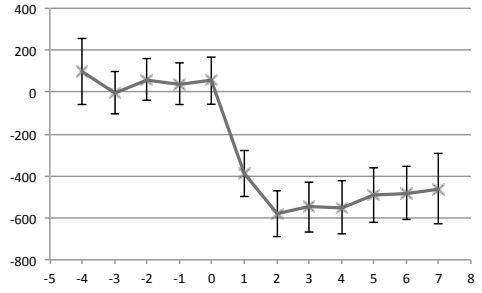
The absence of effects prior to the displacement date ensures the validity of the parallel-trends assumption. However, one year after displacement, the labor earnings of displaced workers have dropped by SEK 67747, or 23.2 percent. There is some recovery over time but even seven years after displacement, income is SEK 43928 lower than the counterfactual. This corresponds to a loss of 15.2 percent of annual earnings. The post-displacement years (2003–2009) were macroeconomically stable apart from 2009. Unlike the analysis in JLS and Eliason and Storrie (2006), my results will thus not be confounded by the additional impact of severe macroeconomic conditions on the effects of job loss.

In order to understand better the mechanisms behind this result and to identify the consequences of displacement in more detail, Figure 2 also presents the effects on capital income. Interestingly, displaced workers experience significant capital income losses over time. With capital income comprising interest earnings on assets such as bank accounts, bonds and other securities, and income from rents, these losses are indicative of displaced workers consuming their assets in order to smooth consumption. The magnitude of the effect grows over time after displacement which may suggest that displaced workers use other means to smooth consumption before consuming their assets.

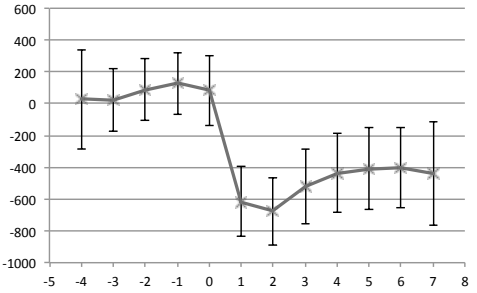
Finally, panels (d) and (e) in Figure 2 present the effects of displacement on hourly wages and on hours worked — measured in November each year — as a share of full-time employment conditional on having a job. The results suggest that the recovery after displacement is also slow on the intensive margin. The hourly wage is estimated to fall by SEK 26 which is equivalent to a drop of 18.6 percent. The post-displacement pattern suggests some recovery on the intensive margin. Hours worked conditional on having a job recovers at a faster pace. The estimated drop in the labor supply of –12.3 percent two years after the announcement amounts to a daily labor supply reduction of one hour.

Figure 3 illustrates dependence on the welfare state by displaying the dynamic effects of displacement on unemployment insurance benefits, student transfers received, labor market training and participation in labor market pro-

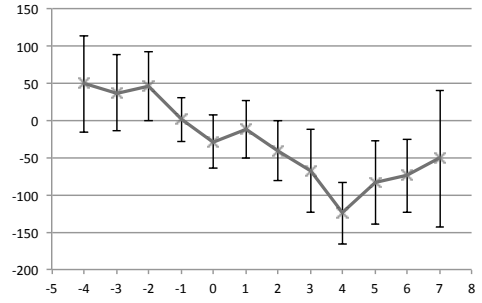
Figure 2: Main Effects of Displacement, I



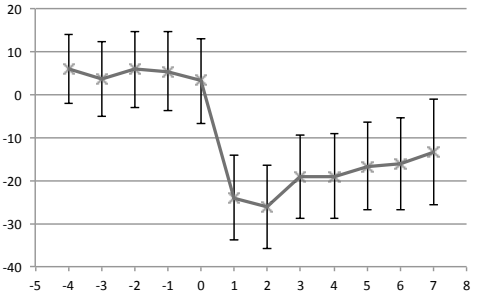
(a) Total Income



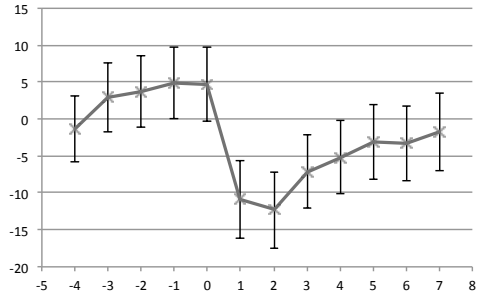
(b) Labor Earnings



(c) Capital Income



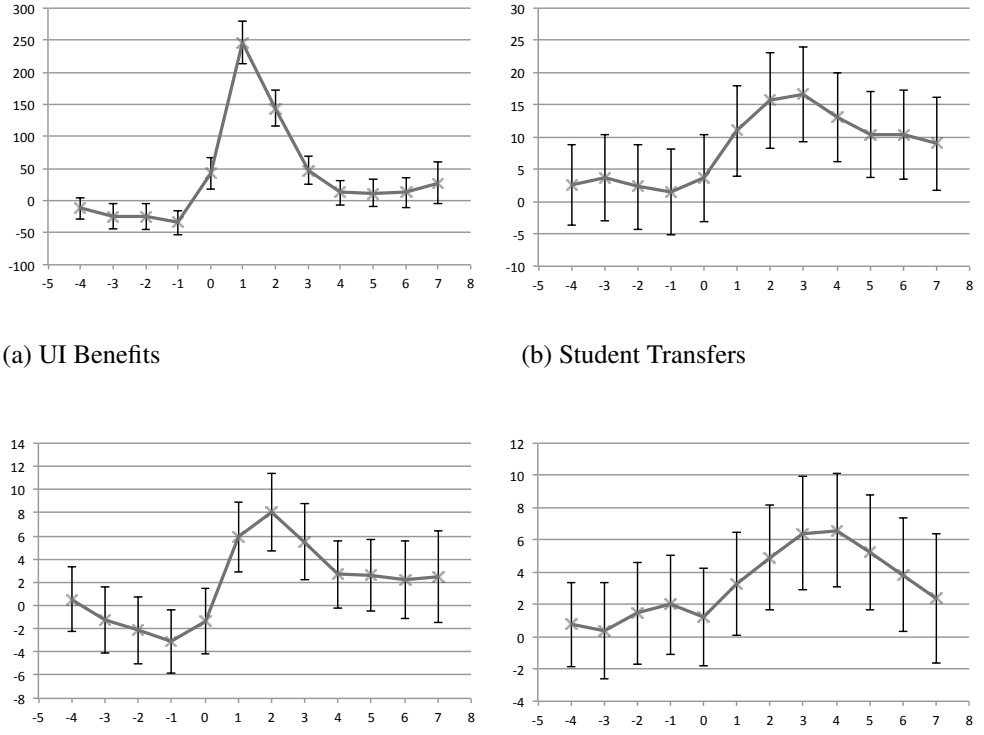
(d) Hourly Wage



(e) Hours Worked

Notes: Estimates of the effects of job displacement on total income, labor earnings, capital income, hourly wage and hours worked before and after displacement. Earnings and capital income are measured in 100 Swedish crowns (SEK). Both earnings and wages are expressed in real terms (denominated in 2002 SEK). The x-axis displays years since displacement. Confidence intervals are defined at the 95 % level and derived from standard errors clustered at the level of the employer in the displacement year.

Figure 3: Main Effects of Displacement, II



(a) UI Benefits

(b) Student Transfers

(c) Labor Market Training

(d) Other Labor Market Programs

Notes: Estimates of the effects of job displacement on UI benefits, student transfers, days in labor market training and days in other labor market programmes before and after displacement. Income variables are measured in 100 Swedish crowns (SEK) and are expressed in real terms (denominated in 2002 SEK). The x-axis displays years since displacement. Confidence intervals are defined at the 95 % level and derived from standard errors clustered at the level of the employer in the displacement year.

grammes. Unemployment benefits provide income for the displaced workers in the short run, but as the effect on UI benefits fades away, dependence on other social insurance programmes remains. In particular, student transfers increase upon displacement implying that displaced workers aim to acquire new skills. Student income comprises government transfers and loans issued by the government and students at universities, colleges, other tertiary educations and adult secondary schools are eligible. Four years after displacement, the increase in student transfers amounts to SEK 1664, or 406 percent, relative to the counterfactual.

To validate the displacement definition, I additionally present a more restrictive approach in which only those displacements where the entire workplace was displaced simultaneously are considered. The results from this exercise are similar in magnitude and are displayed in Table 6 in the Appendix.

Furthermore, focusing on effects for high-tenure workers, defined as those with more than 36 months of tenure with the same employer, yields similar effects.

6.2. Heterogenous Effects

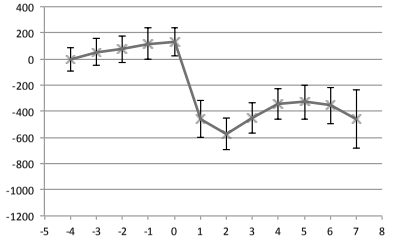
In the analysis of heterogenous effects of displacement based on cognitive and noncognitive skills as well as age, I focus on a subset of the outcome variables above. This restriction is partly imposed to maintain clarity. However, labor earnings and student transfers provide evidence of actual adjustment to new circumstances and the willingness to acquire new skills helping explain how skills and age matter for the ability to adapt to new labor market conditions. Participation in education organized by the PES is separated from studies that qualify for student transfers in order to make clear the distinction between academic studies and labor market oriented studies. The training provided by the PES is aimed at unemployed workers and participants qualify for unemployment benefits but not student transfers.

Figure 4 displays estimates of the impact of job displacement on labor earnings depending on cognitive and noncognitive skills, and age groups. For both cognitive and noncognitive skills, low-skilled refers to the bottom 25 percent of the skill distribution, high-skilled to the top 25 percent and average skills to those in between.

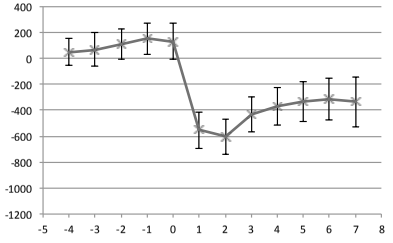
Panels (a)–(f) in Figure 4 suggest that in absolute terms, the losses of high-skilled workers are larger than those of low-skilled workers but that this is simply a consequence of high-skilled pre-displacement workers' earnings being higher.[14] More importantly, neither cognitive or noncognitive skills matter for the pattern of recovery after displacement. The picture suggests abilities are equal in adapting to changing labor market conditions. However, confirming results established in earlier literature, age has a much stronger impact on the capacity to adapt after displacement than cognitive and noncognitive skills, as seen in panels (g)–(i).

[14]The average annual earnings one year before displacement among workers with low cognitive skills is 2449.602 implying earnings losses of about 23 % two years after displacement. For high-skilled workers the corresponding figures are 3610.36 and 22.7 %.

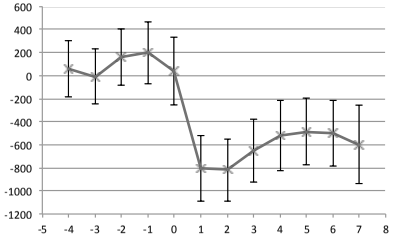
Figure 4: Effects on Labor Earnings by Skill and Age Groups



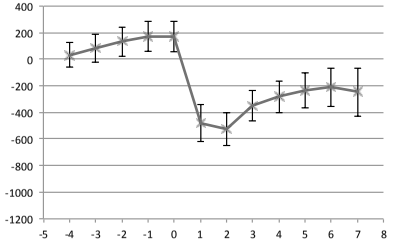
(a) Low Cognitive Skills



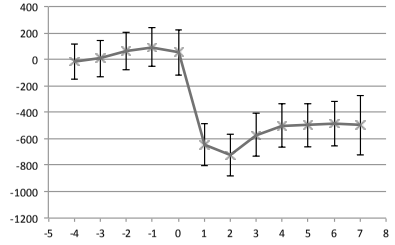
(b) Medium Cognitive Skills



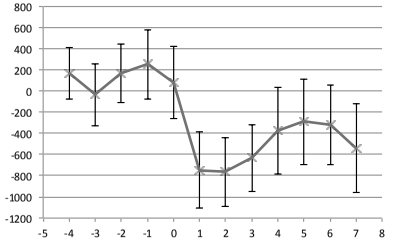
(c) High Cognitive Skills



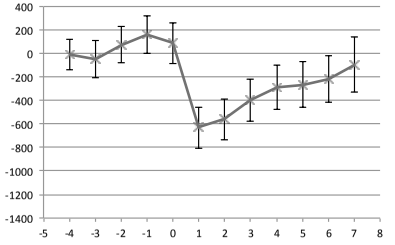
(d) Low Noncognitive Skills



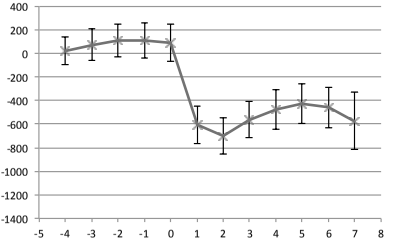
(e) Medium Noncognitive Skills



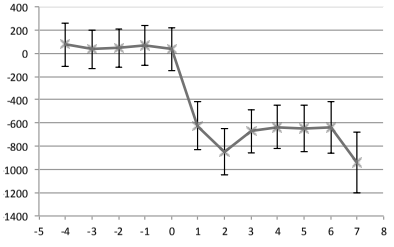
(f) High Noncognitive Skills



(g) Age 25–34



(h) Age 35–44



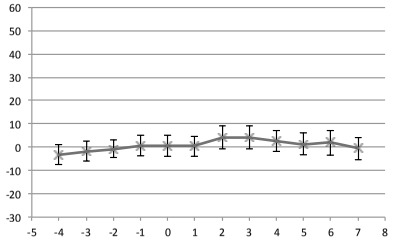
(i) Age 45–54

Notes: Earnings are measured in 100 Swedish crowns (SEK) and expressed in real terms (denominated in 2002 SEK). The x-axis displays years since displacement. Confidence intervals are defined at the 95 % level and derived from standard errors clustered at the level of the employer in the displacement year.

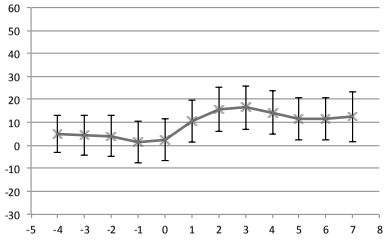
In another attempt to understand how displaced workers adapt to new labor market conditions by acquiring new skills, Figure 5 presents the effects of displacement on student transfers from the government. Panels (a)–(c) and (g)–(i) suggest that workers with high cognitive skills and young workers are more likely to start studying and receiving transfers. Eligibility for student transfers requires admittance to secondary or tertiary schools. Although acceptance in itself often depends on cognitive ability, admission to public adult schools that do not select students based on skills also qualifies for student transfers. Even if the relatively larger impact of displacement on student transfers among workers with high cognitive skills were an outcome of a larger menu of education, it does not translate into a faster recovery of labor earnings. On the other hand, it may be that the larger effect among young workers can, to some extent, explain the differential impact on labor earnings between age groups.

Finally, Figures 6 presents the effects on labor market training provided to unemployed workers. Low-skilled workers are more likely to receive labor market training from the Public Employment Service than secondary or tertiary education. Even seven years after displacement, the average effect of displacement is positive and statistically different from zero at the 10 percent level, irrespective of skill measures. No differential effects across age groups emerge from the figures.

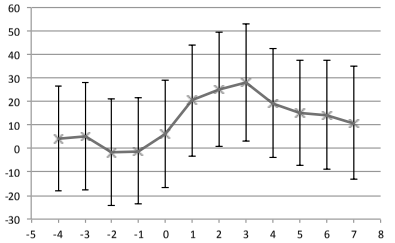
Figure 5: Effects on Student Transfers by Skill and Age Groups



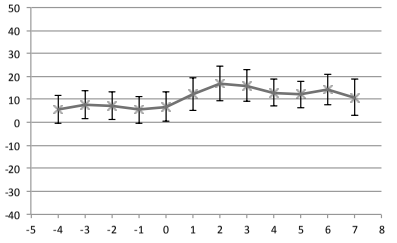
(a) Low Cognitive Skills



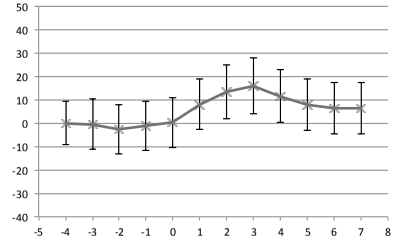
(b) Medium Cognitive Skills



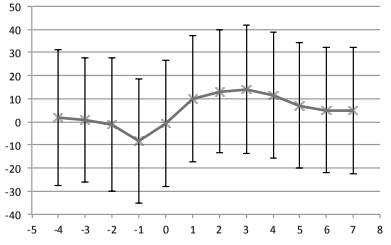
(c) High Cognitive Skills



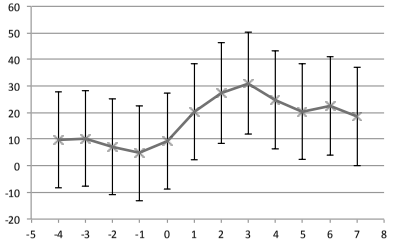
(d) Low Noncognitive Skills



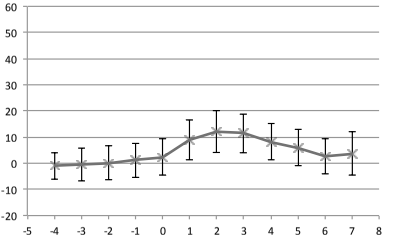
(e) Medium Noncognitive Skills



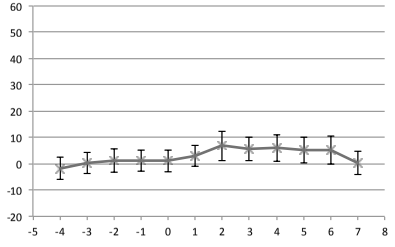
(f) High Noncognitive Skills



(g) Age 25–34



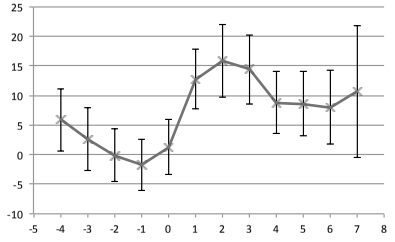
(h) Age 35–44



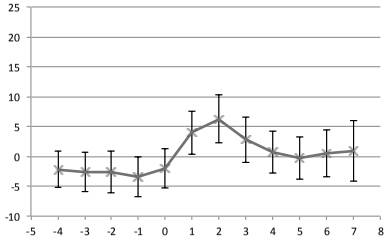
(i) Age 45–54

Notes: Student transfers are measured in 100 Swedish crowns (SEK) and expressed in real terms (denominated in 2002 SEK). The x-axis displays years since displacement. Confidence intervals are defined at the 95 % level and derived from standard errors clustered at the level of the employer in the displacement year.

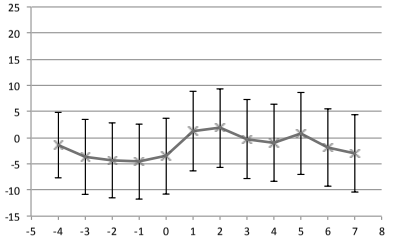
Figure 6: Effects on Days in Job Training by Skill and Age Groups



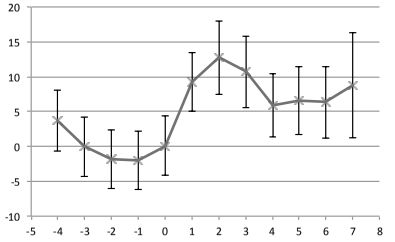
(a) Low Cognitive Skills



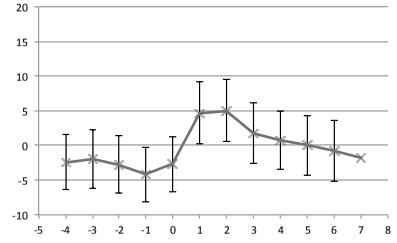
(b) Medium Cognitive Skills



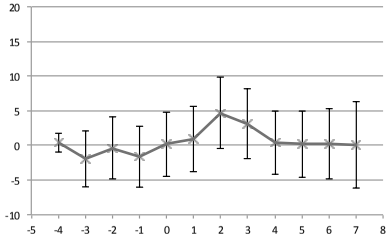
(c) High Cognitive Skills



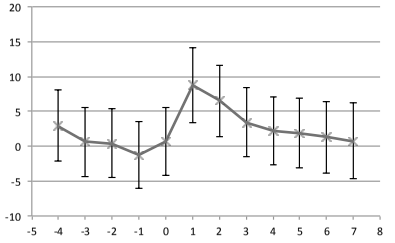
(d) Low Noncognitive Skills



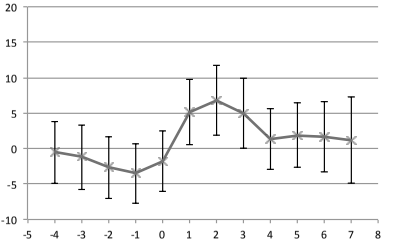
(e) Medium Noncognitive Skills



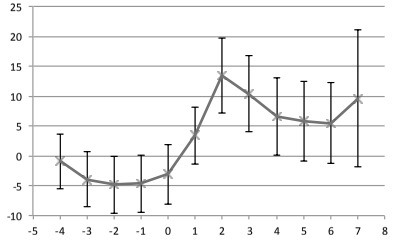
(f) High Noncognitive Skills



(g) Age 25–34



(h) Age 35–44



(i) Age 45–54

Notes: The x-axis displays years since displacement. Confidence intervals are defined at the 95 % level and derived from standard errors clustered at the level of the employer in the displacement year.

Table 2: Labor Earnings Losses by Characteristics, Separate Regressions

	Separate Regressions			One Regression		
	Pre	Drop	Post	Pre	Drop	Post
Main (mean cognitive and noncognitive, young)				135.215***	–240.934*	19.848
				(28.252)	(129.330)	(20.045)
Main (average cogn.)	76.136***	–489.538***	–19.651**			
	(17.409)	(75.054)	(8.480)			
Additional effect, cogn.	18.132	8.499	5.721	–23.296	–51.478	1.338
	(17.136)	(76.707)	(7.695)	(20.820)	(79.905)	(10.727)
Main (average ncog.)	72.142***	–491.728***	–18.096*			
	(19.070)	(83.353)	(9.584)			
Additional effect, ncog.	–6.722	–7.777	9.323	19.080	–17.141	–2.278
	(22.594)	(86.173)	(10.208)	(15.272)	(75.171)	(9.126)
Main (age 25–34)	126.661***	–280.885**	16.215			
	(29.769)	(135.686)	(20.398)			
Additional effect, age 35–44	–93.868***	–393.173**	–65.064**	–90.043***	–349.849**	–56.923**
	(33.079)	(170.335)	(25.523)	(33.806)	(170.609)	(25.813)
Additional effect, age 45–54	–99.775***	–474.131***	–84.425***	–91.233***	–398.492**	–66.007***
	(32.615)	(170.551)	(23.965)	(31.818)	(167.234)	(22.780)

Notes: Coefficients reveal differences across groups. For continuous variables, the main effect corresponds to the effect on an average worker and the impact is assumed to be linear. All specifications control for worker fixed effects and group-specific time trends. Standard errors are clustered on the level of the employer at the time of displacement. *: p<0.10, **: p<0.05, ***: p<0.01.

In order to analyze whether effects are significantly different across groups, I re-parametrize the estimation procedure according to JLS and estimate (3) exploiting the continuous variables measuring skill and the indicator variables for age groups. Table 2 shows the results when earnings regressions are estimated. Workers with high skills experience larger initial drops in earnings but, again, the difference is merely due to high skilled workers earning more when displaced. The recovery is not faster among workers with high-skills, however. The most pronounced differentials in the effects of displacement are found between age groups. Older workers fare significantly worse than younger workers and their recovery is significantly slower.

7. Bias Estimation

A weakness of earlier studies that use administrative data to study displacement effects is the inability to distinguish quits from layoffs. To overcome this issue, separations in times of distress are regarded as involuntary quits initiated by the employer. Such time periods are either defined as windows of mass sep-

arations or time periods preceding plant closure.[15] The alternative approach of using survey-based definitions of displacement has the advantage that it may increase accuracy in identifying displacements by asking workers directly, but the disadvantage of possible measurement error from self-reporting.[16] The PES data permit estimation of the consequences of defining displacements as separations occurring in time windows around mass separations or before plant closures rather than employer-initiated layoffs.

Table 3 presents the results from estimating equation (2) using different definitions of displaced workers. Workers separating in *mass-separation* years, where more than 30 percent of the workforce leave the plant, experience labor earnings losses of a smaller magnitude than the baseline estimates. Although pre-trend estimates indicate a violation of the parallel trends assumption, the estimated coefficients are significantly different from each other in the post-displacement period and the difference fades out over time. The results for workers separating within a three-year window preceding plant closure significantly overstate the effects of displacement. Finally, the estimates for workers separating within a one-year window preceding plant closure are slightly larger than the coefficients for actual displacements, but the difference is never significant.

The estimated differences are not purely due to the use of different definitions. The displacement data include information on the date at which the announcement was received by the PES. This enables restricting the sample of displaced workers to be restricted to include only displaced workers who were part of an announcement in which a majority of the workers were displaced simultaneously, so as to minimize the selection into displacement. Such restrictions are not possible with employer-employee data and consequently, the estimated differences are affected by the inability to distinguish quits from layoffs and the lack of information on the date at which the displacement was announced.

8. Concluding Remarks

This paper uses a unique dataset to analyze, firstly, who becomes displaced and secondly, the effects of displacement. The use of administrative data on

[15] The common problem of false firm deaths, or falsely labeling the disappearance of an organization identity number as a plant closure, is solved by following workers after a potential plant closure. If a majority continues with the same employer, the event is not coded as a plant closure.

[16] Kuhn (2002) discusses a study in which workers and their employers were asked to report the reasons for separations. Interestingly, a lot of asymmetries in responses emerged.

Table 3: Labor Earnings Losses, Different Definitions

	Main	Mass Layoff	T-stat	Plant Cl. – 3y	T-stat	Plant Cl. – 1y	T-stat
–4	26.865	72.209***	0.706	62.606**	0.464	–122.180	0.898
	(46.067)	(18.167)		(30.969)		(119.885)	
–3	22.286	152.912***	1.642	79.883**	0.619	7.642	0.092
	(53.173)	(26.385)		(39.887)		(106.224)	
–2	86.646*	209.108***	1.458	184.555***	1.04	181.884*	0.598
	(52.143)	(31.862)		(41.965)		(107.007)	
–1	125.878**	250.414***	1.427	249.145***	1.224	169.096	0.265
	(55.718)	(31.552)		(44.998)		(107.434)	
0	81.702	165.145***	0.913	9.021	0.653	–110.140	1.131
	(61.135)	(30.292)		(50.089)		(108.468)	
1	–615.854***	–105.948*	4.066***	–756.449***	1.179	–648.950***	0.188
	(64.113)	(61.294)		(55.164)		(112.016)	
2	–677.471***	–184.255**	3.57***	–917.426***	2.065**	–832.725***	0.905
	(59.573)	(78.565)		(56.634)		(111.978)	
3	–522.465***	–183.165***	2.83***	–831.744***	2.723***	–713.833***	1.12
	(58.584)	(61.302)		(55.006)		(112.317)	
4	–436.267***	–148.944***	2.529***	–774.376***	2.939***	–672.774***	1.324
	(62.313)	(51.296)		(52.715)		(116.332)	
5	–410.483***	–138.388**	2.185**	–693.841***	2.363**	–609.809***	1.078
	(64.512)	(60.038)		(55.416)		(120.308)	
6	–402.319***	–262.875***	1.189	–698.127***	2.377***	–629.788***	1.258
	(64.919)	(52.340)		(59.530)		(115.961)	
7	–439.282***	–277.464***	1.063	–719.237***	1.831*	–744.621***	1.515
	(79.622)	(72.607)		(73.251)		(121.896)	

Notes: The table presents the effects of displacement on labor earnings with different definitions of displacement. The column *Main* defines displaced workers as those on the list of displaced workers that the employers submit to the Public Employment Service. In *Mass Layoff*, displaced workers are defined as separations occurring in years when more than 30 % of the workforce separate and *Plant Cl. – 3y* and *Plant Cl. – 1y* provide evidence on the effects when workers who separate within a time window of three years and one years, respectively, are labelled as displaced. Standard errors are clustered at the level of the employer in displacement year. *: p<0.10, **: p<0.05, ***: p<0.01.

displacements at the individual level allows the issue of distinguishing quits from layoffs in linked employer-employee data to be overcome. Using this data, I first analyze the selection into displacement and find that both cognitive and noncognitive skills lower the probability of displacement. Moreover, younger workers are more likely to be displaced than older workers.

The analysis of displacement effects on different income and welfare measures focuses on displacement events in which 80 percent or more of the workforce receive an announcement of an impending displacement simultaneously in order to make the selection into displacement as exogenous as possible within the workplace while maintaining statistical power. The persistent earnings losses found in the earlier literature are confirmed. The estimates are large, considering that the period under study was macroeconomically stable. Separation of the extensive margin responses from the intensive margin shows that even conditional on finding a job, the recovery of hourly wages is slow. Hours worked are also lower than in the counterfactual and recover slowly.

Turning to heterogenous effects, the effects of displacement are not significantly different across groups with high or low cognitive or noncognitive skills and the largest differences are found in the effects of job loss across age groups. The labor earnings of younger workers recover at a faster pace than those of older workers.

The results have direct policy implications. The importance of age suggests that older workers require more assistance in adjusting to new labor market conditions after displacement. However, I find no support for the hypothesis that differential treatment with respect to skill is justified.

Finally, I assess the validity of using employer-employee matched data when analyzing displacement effects. I find that separations occurring within one year before plant closures yield results that are closest to those obtained with actual displacement data.

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Appendix

Table 4: Sample Characteristics, Effects of Displacement

	All	Displaced Workers	Nondisplaced Workers
Labor Earnings, year –1	2802.091	2924.940	2801.424
	(1744.942)	(1544.177)	(1745.948)
Unemployment Inci, year -1	0.051	0.029	0.051
	(0.220)	(0.168)	(0.221)
Unemployment Days, year -1	4.550	2.2	4.563
	(26.44)	(16.937)	(26.486)
Age	37.917	36.857	37.923
	(6.820)	(6.715)	(6.821)
Tenure	87.906	66.286	88.024
	(58.386)	(49.531)	(58.409)
Skills:			
Cognitive Skills	0.056	–0.008	0.056
	(0.949)	(0.993)	(0.949)
Noncognitive Skills	0.005	–0.119	0.005
	(0.937)	(0.915)	(0.937)
Industry:			
Manufacturing	0.341	0.463	0.341
	(0.474)	(0.499)	(0.474)
Service Sector	0.146	0.261	0.145
	(0.353)	(0.439)	(0.352)
Wholesale and Retail trade	0.154	0.122	0.154
	(0.361)	(0.328)	(0.361)
Transport and Communications	0.122	0.113	0.122
	(0.327)	(0.316)	(0.327)
Public Sector	0.169	0.008	0.170
	(0.374)	(0.088)	(0.375)
Intensive Margin:			
Hourly Wage (SEK)	143.250	140.852	143.258
	(64.762)	(54.024)	(64.794)
Hours Worked as Share	95.798	98.623	95.788
of Full-time	(18.844)	(12.978)	(18.860)

Notes: Standard deviations in parentheses. The sample consists of men aged 20 to 51 with more than six quarters of tenure at the time of treatment. Workers qualify as displaced if the job loss event affects more than 80 % of the workers at the plant. Displaced workers who do not satisfy the restrictions are removed altogether.

Table 5: Number of Observations, Effects of Displacement

	Displaced Workers	Nondisplaced Workers
Person level, earnings and unemployment	2703	497287
Person-Year level, earnings and unemployment	29733	5470157
Person level, wages and hours worked	2147	399894
Person-Year level, wages and hours worked	10077	2963754
Observations, selection	10294	51877

Notes: Since the data for wages and hours worked do not cover the entire working population and because not all workers have jobs in all years, the number of observations is lower for those variables. The sample restrictions are different in the analysis of who becomes displaced, see the text for details.

Table 6: Effects on Displacement in Different samples

Year	Baseline	Restrictive	Long Tenure
–4	26.865	–6.486	52.95
	(46.067)	(61.667)	(49.451)
–3	22.286	–29.288	118.606**
	(53.173)	(70.282)	(55.397)
–2	86.646*	74.72	82.525
	(52.143)	(67.442)	(53.571)
–1	125.878**	97.94	56.373
	(55.718)	(75.141)	(54.55)
0	81.702	7.437	67.509
	(61.135)	(84.798)	(60.395)
1	–615.854***	–722.086***	–551.861***
	(64.113)	(83.205)	(70.871)
2	–677.471***	–703.144***	–682.29***
	(59.573)	(76.004)	(60.587)
3	–522.465***	–540.683***	–565.695***
	(58.584)	(74.197)	(59.35)
4	–436.267***	–452.036***	–494.934***
	(62.313)	(81.882)	(62.103)
5	–410.483***	–423.49***	–470.502***
	(64.512)	(84.278)	(65.576)
6	–402.319***	–415.114***	–493.085***
	(64.919)	(84.94)	(66.169)
7	–439.282***	–539.986***	–519.298***
	(79.622)	(104.804)	(83.693)
Observations	5466696	5134501	273,616
R-squared	0.188	0.185	0.171

Notes: Effects of displacement depending on the definition of displacement. Column (1) shows the baseline estimations. In column (2), only displacements in which the entire workplace receive displacement at the same time are considered. Finally, column (3) requires workers to have more than 36 months of tenure before time 0. Effects are measured in 100 SEK and are expressed in real terms. The error terms are clustered at the level of the employer at the onset of displacement. Years are normalized so year 0 denotes the displacement year. All specifications control for year and worker fixed effects. *: $p<0.10$, **: $p<0.05$, ***: $p<0.01$.

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