Employment and Earnings Effects of Displaced Workers: Evidence from Unexpected Firm Closures due to Sudden Death of Firm Owners*

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Abstract

This paper exploit a natural experiment to investigate the employment, earnings, and wage effects of displaced workers. Unexpected firm closures due to sudden death of the firm owners results in exogenous variation in worker displacement and allows me to examine the effects of worker displacement. This identification strategy enables me to overcome the identification problems that has plagued the prior literature, namely the endogeneity problems associated with individual displacements and firm closures.

I compare the performance in employment, annual earnings, and wages of the displaced workers to that of a matched sample of individuals using matched worker-firm data from Denmark in a difference-in-differences approach. This enables me to identify the effects of being displaced on employment, earnings, and wages.

I find moderate short-term displacement effects on both employment and annual earnings. The short-term effect on employment is 7.7%, and the average short-term annual earnings loss is around 10.6%. The employment and annual earnings effects diminishes over time. I do not find significant effects on hourly wages, and thus it cannot be concluded that displacements are associated with lower individual productivity.

The findings are in line with the results found in the prior literature, albeit the magnitude of the effects is somewhat smaller. I do not, however, find evidence of an Ashenfelter's Dip, which is in contrast with findings in prior studies. This finding can be attributed to my identification strategy.

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1 Introduction

The Great Recession has resulted in the destruction of millions of jobs across the world, as a consequence of mass-layoffs and firms shutting down [OECD (2013)]. Moreover, the labor market transition in most advanced economies is well-documented [Munch (2010)], and has resulted in the outsourcing of jobs to low-wage countries leading to massive job losses. Thus, both structural and cyclical factors have in recent years led to the displacement of workers in the advanced economies (including Denmark).

There are a number of reasons why economists are interested in measuring displacement effects. First of all, there is a genuine interest in the economic difficulties facing workers when they lose their jobs due to reasons beyond their control. Moreover, evidence from long-term earnings losses after displacement is indicative of the contribution of firm-specific factors to wage determination. Workers displaced involuntarily often face long unemployment spells, during which time their skills may depreciate. Furthermore, theory suggests several reasons why displaced workers might experience losses beyond a period of unemployment, following their job losses. Three reasons are mentioned in Jacobson et al. (1993). First, workers might possess skills that were especially productive in their old position, but not so in their subsequent job. Second, workers losing jobs that paid wage premiums may earn less if their subsequent job pays standard wages. Third, displaced workers' long-term earnings will be lower if workers on their previous jobs had accepted wages below their productivity in return for higher wages later in their careers. As such, the costs of job displacement can be large at the individual level as well as at the social level.[1] The aim of this paper is to measure these costs. Understanding the displacement effects is crucial in designing effective labor market policies in order to guide the displaced workers back into employment.

The empirical literature on displaced worker effects is substantial and very active. Literature reviews conclude that job displacement results in sustained earnings losses [Fallick, B. (1996); Kletzer, L. (1998)]. However, the estimates vary with the type of data used in the analysis, the industry within which the displacement occurs, and business cycle conditions. The displacement effects are also found to vary substantially across countries [OECD (2013)]. The largest effects on earnings are found in Germany, Italy, Great Britain, and The United States, whereas the effects in the Scandinavian countries, Belgium, and Japan are modest.[2,3] Furthermore, prior studies have shown that earnings and wage losses tend to decline over time, but generally persist for a number of years. Finally, some studies also find that earnings decline modestly in the years prior to the displacement, which likely can be attributed to an Ashenfelter's Dip, suggesting that the displacing firms are in trouble in the years leading up to the displacement. This finding is concerning, as it is questioning whether the measured effects are biased by individual or firm-specific characteristics.

The most influential study in the literature is arguably Jacobson et al. (1993). This paper estimate the earnings losses due to displacement, adapting analytical techniques developed for program evaluation to the context of job displacement. Couch and Placzek (2010) use the same techniques (i.e.

[1]Note that some of the reasons mentioned do not necessarily imply social losses.

[2]Accurate comparisons across countries are very difficult to make because of differences in the definition of displacement, measures of earnings/wages, cyclical conditions, and the group of workers whom is in focus.

[3]The differences in the magnitude of the effects could obviously reflect differences in structures in the relevant labor markets. However, a discussion of these differences is beyond the scope of this paper due to page restrictions.

fixed-effects and time trend estimators), but extend their analytical framework by including matching estimators. In these and many other studies in the literature, plant and firm closures are used as a natural experiment, enabling the use of program evaluation methodologies to measure the costs of being displaced. In doing so, one makes the implicit assumption that plant or firm closures are random events, and thus uncorrelated with the firm's productivity or current macroeconomic conditions. If this is not the case, then the estimated displacement costs are likely to be biased. Many studies have therefore tried to take this source of endogeneity into account by comparing the relative performance of workers displaced in a mass-layoff with workers from the same firms that are not displaced. This obviously raises a new concern, since one now has to assume that it is random who is displaced in a mass-layoff. As no source of exogenous variation has yet been found, these endogeneity problems have plagued the prior literature.

In this paper, I use a natural experiment to examine the displacement effects. Unexpected firm closures due to the sudden death of the owner result in exogenous variation in worker displacement, allowing me to measure the costs of being displaced. This identification of worker displacement enables me to overcome the identification problems that have plagued the prior literature. A similar identification strategy has been exploited by Andersen and Meisner (2011; 2012), but has not yet been applied to the field of labor economics. By using matched worker-firm data from Denmark, I am able to follow the displaced workers before and after the displacement event and use a difference-in-differences matching approach to identify the employment, earnings, and wage effects of displacement.

This paper contributes to the literature along three lines. First, to the best of my knowledge, this study is the first to use a natural experiment to measure the displacement effects, which enables me to infer much tighter causality than in prior studies, as it effectively eliminates the endogeneity problems. Second, this paper includes a wider range of explanatory variables than has previously been done in the literature, enabling me to further strengthen causality and to infer group-specific displacement effects. Third, the results provide evidence from the Danish labor market and thereby contribute to the ongoing policy debate about the outsourcing of low-skilled jobs from the Danish labor market. Note however, that this paper has a strict descriptive focus on measuring the displacement effects, and therefore a discussion of the implications of the results in terms of designing labor market policies will not be undertaken in this paper. For such a discussion, see for instance OECD (2013) and Kletzer (1998). Moreover, this paper will only focus on measuring the *economic costs* of being displaced, and not social, psychological or other sorts of potential displacement costs.

My main findings are the following. First, considering a sample of workers displaced between 1994 to 2008, I present compelling graphical evidence of the displacement effects. The evolution of the employment rates and annual wage earnings in the displaced worker sample and a matched control group are completely parallel in the pre-displacement years and then diverge sharply in the years after the displacement. Second, a difference-in-differences approach, based on the graphical analysis, produces estimates of the displacement losses in terms of employment, annual wage earnings and hourly wages. One year after displacement the estimated relative drop in the employment rate is 7.7% and the relative decline in annual earnings is 10.6%. Five years after the displacement the employment rate has converged back, whereas the relative decline in annual earnings remain 7.0%, suggesting that the earnings losses are persistent. This is possibly driven by lower hourly wages. The findings are in line with the results found by Albæk et al. (2002), albeit the magnitude of the effects are smaller. Third, I find evidence for larger group-specific displacement effects for highly experienced persons, even when controlling for age and education. This suggest that people with the most accumulated human capital suffer the most following displacement, which is in line with theory.

Prior studies have investigated the effects of displacement in the Scandinavian countries. Albæk et al. (2002) measures the displacement effects on the Danish labor market using firm-based data linked to individual records from 1980 to 1991. The paper finds that the short-term hourly wage loss is 4.0% and the long-term loss is 6.4% (long-term is defined as 3 years after displacement in all the mentioned studies). The short-term losses in annual earnings for those reemployed are found to be 4.7%, and the long-term losses are 6.8%. Eliason and Storrie (2006) find that the short-term annual earnings loss in Sweden is 8,394SEK and the long-term loss is 5,584SEK using linked employer-employee data from 1986 to 1987. Huttenen et al. (2005) find that the short-term annual earnings loss is 2.0% in Norway and that the long-term loss is 5.0% using employer-employee survey data covering the period 1988 to 2000. These results, and in particular the Albæk et al. results, provides a framework for assessing my results. Table A1 in Appendix A gives a thorough review of the prior literature.

The remainder of the paper proceeds as follows. Section 2 discusses definitions of worker displacement and describes the empirical strategy. Section 3 outlines the data used and presents summary statistics. Section 4 presents the empirical findings, and section 5 briefly concludes.

2 Empirical Strategy

2.1 Defining Displaced Workers

In the literature, several approaches have been used to identify job displacements. The two main approaches use administrative databases and survey data, respectively. The approach based on administrative data usually defines job displacements as separations in conjunction with a firm closure. The reason for this definition is that it is usually not possible to identify the reason for separation in this kind of data. Since it is reasonable to assume that job separations following firm closures are involuntary, firm closures are a suitable way to identify job displacements. The approach based on survey data defines job displacements as self-reported "layoffs".

Both approaches have their advantages and disadvantages. One of the advantages of the administrative data-based approach is the notion that a plant or firm closure constitute a "natural experiment" in which the displacements are more likely to be uncorrelated with individual-specific unobserved characteristics (e.g. ability). This is not the case for individual displacements in the survey data, as these will not be adjusted for this selection bias. A disadvantage of the administrative data-approach is that a lot of displacements are on an individual basis, and thus a study of firm closure displacements alone will not be representative. For instance, one might think that a firm closure-based sample would overestimate the costs of being displaced, as "congestion effects" in local labor markets might make it harder for each individual worker to become reemployed.[4]

In this paper I will exploit the rich Danish register data and use the administrative data-based

[4]See Kuhn (2002) for a further discussion of the advantages and disadvantages of the two approaches.

approach. With the outlined disadvantages of this type of approach in mind, I will describe my identification strategy below.

2.2 Identification of Displacement Effects

The fundamental identification problem in generating unbiased estimates of the displacement effects arises from the likelihood that it is not a random draw who is displaced. Therefore, when measuring the effects the challenge is to identify a population in which the only factor that distinguishes the displaced workers from the non-displaced workers, is the displacement event. In every other observable and non-observable aspect the individuals should be identical. Ideally, one would therefore want a random selection of workers to be displaced, but since this is clearly not feasible in practice, the aim is to find a method that approximates a random draw.

Identifying exogenous variation in an individual's employment status poses a major empirical challenge. The fundamental challenge is to avoid two apparent endogeneity problems. First, as profit-maximizing firms are most likely to choose the least productive workers when they decide who to displace, displacements are not random, and identifying individual displacements will thus tend to overestimate the costs of being displaced. Identifying displacements due to firm closures eliminates this endogeneity problem since a firm closure implies the displacement of all workers independent of the individual worker's productivity. This strategy, however, leads to a second selection problem. Identifying displacements due to firm closures does not take into account that it is not random which firms close. Thus, measuring displacement effects using displacements due to firm closures will also tend to overestimate the effects, given it is more likely that the least profitable and productive firms will shut down (all other things being equal).

Previous studies have attempted to take these selection problems into account when estimating the displacement effects. However, a common problem for these studies is that they are only partly able to control for these identification problems. The seminal paper by Jacobson et al. (1993) tries to control for the endogeneity problem related to the firm closure identification by using a mass-layoff sample where the displacement effects are estimated relative to workers in the same firm that have not been displaced. While eliminating the firm closure-related problem, this approach gives rise to the selection bias related to the individual displacement identification. Therefore, given it is not draw who is displaced in a mass-layoff, this strategy would still tend to overestimate the displacement effects.

In this paper, I exploit exogenous variation in displacement to examine the effects of worker displacement. Exogenous variation in displacement is derived from unexpected firm closures due to sudden death of the firm owner. This natural experiment induces no selection of individuals, except from the death event itself. Thus, for this identification strategy to work, the death has to be unexpected and sudden. Sudden deaths are medically defined as an unexpected death that occurs instantaneously or within a few hours of an abrupt change in the person's previous clinical state.[5] As sudden death is a random draw by nature, displacement due to sudden death of the firm owner is a natural experiment that induces exogenous variation in individuals' employment status. To this end, I have constructed a unique data set from Danish data that allows me to identify the displacement effects from unexpected

[5]See for instance *Medscape.com*.

firm closures. The advantage of this identification strategy is twofold. First, as firm closures due to sudden death of the firm owner are unanticipated, the worker displacements are unrelated to current economic conditions, technological changes, and the profitability of the firm, and it is therefore reasonable to attribute a negative effect on employment, earnings, and wages to the displacement event. Second, the strategy allows me to control for unobserved individual characteristics (e.g. ability) that are likely to correlate with employment status and wages.

There are two potential pitfalls one needs to be aware of, when using this identification strategy. First, the possibility that not all sudden deaths of firm owners lead to firm closures could appear to weaken my identification strategy, as it could be due to some selection bias. Second, if certain firm owners exhibit "risky behavior" (e.g. lead more unhealthy lives), this could induce a selection on the sudden deaths, and thereby the displacements. I approach these prospects by using a matched sample to measure the displacement effects, thereby taking the selection into account. It is important to note that by doing this, the displacement effects I measure will apply to a certain kind of workers, and will not necessarily generalize to all kinds of workers.

2.3 The Statistical Model of Earnings Losses

The simplest way to measure the displacement effects would be to compare workers' earnings in a given period immediately before displacement with some given period after displacement and then compute the difference. This naïve difference approach was used in displaced worker studies in the early literature [c.f. Jacobson et al. (1993)]. However, there are several reasons why this measure might not capture the true displacement effects. First of all, this measure does not control for macroeconomic factors that affects workers' earnings regardless of whether they are displaced. Second, this measure does not account for the growth in earnings that would have occurred in the absence of job loss. E.g. if a given worker would have received a pay rise if the event that led to the shutdown had not occurred, then this will bias the estimates downward. Third, given that the displaced workers are identified through firm closures, these firms' potentially ailing performance might adversely affect workers' earnings several years prior to displacement. E.g. if the firm is about to close down, it might force its workers to work only part-time instead of full-time before finally shutting down.

In the program evaluation literature this effect is called an Ashenfelter's Dip [Ashenfelter (1978)]. The problem arises if the lower wages before the firm closure are *due to* the firm closure. If this is the case, then comparing the year before the firm closure with, say, the year after the firm closure would bias the estimates downward. Taken together, these effects will tend to underestimate the displacement effects, and the naïve difference-approach is therefore not the best way to measure the effects.

Instead, I employ an approach used in the program evaluation literature in order to obtain more reliable estimates of the costs of displacement. To measure the displacement effects, I estimate the difference in wages between a given pre-displacement period and a given post-displacement period, and to control for the aforementioned effects, I compare this difference to the equivalent difference of a control group, to obtain a difference-in-differences estimate. I thereby exploit that my data set contains observations for both displaced and non-displaced workers and that it contains observations across time. The statistical method therefore uses both the cross-sectional dimension and the time-series dimension of the data set. This approach is attractive because it effectively controls for time-invariant individual characteristics that are likely to affect the displacement effects as well as controlling for underlying time trends in wage growth.

There are two identifying assumptions behind the difference-in-differences estimator. The first is the common trend-assumption which states that the time effects are identical across groups. Whether this assumption is fulfilled can be analyzed graphically (see below). The second assumption states that there must be no composition changes within groups over time. This is fulfilled per construction using the sudden death-identification, since this produces exogenous variation with no possibilities of self-selection into the treatment group.[6]

My statistical specification is shown in (1) below, and it is this equation I estimate to capture the displacement effects on earnings and wages:

$$E_{it} = \alpha + \delta_t year_t + \beta \mathbf{x_{it}} + \gamma_0 Treat_i + \gamma_1 Post_t + \gamma_2 \left(Treat \times Post\right)_{it} + \varepsilon_{it} \tag{1}$$

where $Treat_i = 1$ for workers in the treatment group (i.e. displaced workers) and $Treat_i = 0$ for workers in the control group (i.e. non-displaced workers). $Post_t = 1$ after displacement and $Post_t = 0$ before displacement. $(Treat \times Post)_{it} = 1$ if the worker is displaced and time is after displacement, and 0 otherwise. Thus, the effect of the displacement is captured in γ_2. γ_0 is a time-invariant fixed effect summarizing the impact of permanent differences in observed and unobserved characteristics between the treatment and the control group allowing the two groups to have different earnings and wages. γ_1 captures the time-varying fixed effect allowing the two groups' earnings and wages to vary over time, but only due to forces other than the displacement event (as long as the two groups are affected in the same way). The vector $\mathbf{x_{it}}$ consists of a range of observed (time-varying) characteristics of the worker. The δ_t's are the coefficients of a set of dummy variables for each year in the sample period that capture the general time pattern of earnings in the economy. These dummy variables also serve to alleviate the potential problem of Ashenfelter's Dip.[7] $year_t = 1$ if displacement year is t, and 0 otherwise.

I estimate equation (1) using three different response variables. The first outcome variable I use is employment status, which takes the value 1 if the person is in employment, and zero if the person is unemployed. Estimating equation (1) using this outcome variable will produce estimates that measure the employment effects. The second outcome variable I use is annual wage earnings. Estimating the equation using this outcome variable will produce estimates that measure the total earnings costs of being displaced. The total displacement costs can be decomposed into employment effects (i.e. the effects from working less hours) and productivity effects (i.e. the effects from being less productive, e.g. as a result of not being able to exploit the firm-specific capital from a subsequent job or depreciation of human capital). The third outcome variable I use is hourly wages. Estimating the equation using this outcome variable will produce estimates that measure the productivity costs of being displaced.[8]

[6] For a further discussion of the identifying assumptions see Bertrand et al. (2004).

[7] Note that Ashenfelter's Dip is unlikely to occur using the identification strategy I do, in that the firm closures are due to the sudden death of the owner and not some cyclical or structural changes.

[8] Note that hourly wages are not necessarily equal to productivity for the reasons mentioned in the introduction (wage premia and upward sloping wage contracts). They do, however, constitute an indirect measure of productivity.

3 Data

To construct the sample of displaced workers analyzed in this paper, I first identified the firm owners who died suddenly and unexpectedly. Second, I identified the firm closures following the death of the owner and the workers displaced due to these closures. Third, I restricted the sample to workers aged between 18 and 64, thereby reducing the probability of retirement following the displacement. Fourth, to reduce biases due to sample attrition, I required that every worker receive positive wages prior to the displacement. This restriction ensures that the losses I observe result from wage changes instead of missing wage data. Finally, I removed secondary jobs as displacement from these kind of jobs will not serve to shed light on the displacement effects. This identification leaves me with a sample of 602 displaced workers.[9] Appendix B describes in detail the data sources used in the analysis. Appendix B also contains a thorough description of the construction of the sample.

In order to carry out the statistical analysis, I need a control group to measure the relative output performance of the displaced workers. The challenge in choosing a control group is to select a group of workers that is similar to the displaced workers in all observable and non-observable characteristics, and only differs by not being subjected to displacement. Prior studies have simply used the group of workers in firms that have not closed as their control group [see for instance Jacobson et al. (1993); Albæk et al. (2002)]. By doing this, one implicitly assume that firm closures are a random draw. This seems to be a questionable assumption, c.f. the discussion above. This implicit assumption is not necessary in this study, where the firm closures truly are a random draw, as they result from the sudden and unexpected death of the owner. However, the fact that not all sudden and unexpected death of firm owners lead to firm closures (see table B3, Appendix B) suggests that there might be some selection bias. Comparing the differences in sample means between the sample of displaced workers (column 1) and the employed workers (column 4) shows that there are significant differences in characteristics between the two groups (see table C1 in Appendix C).

To take these differences into account, I therefore use a matched sample of workers as control group. The control group is matched using a propensity score[10] matching on gender, civil status, education, experience, tenure, working position, and industry. By using this method, I am maximizing the probability that the only thing that separates the displaced workers from the non-displaced workers is the displacement event. I use the nearest-neighbor matching to obtain one match per observation in my sample of displaced workers.[11]

Now, comparing sample means between the sample of displaced workers and the matched sample of non-displaced workers (column 2) shows that there is no longer a significant difference (see table C1 in Appendix C), which means that I am now able to compare the outcomes between the two groups.[12]

Let me conclude this section by noting that some prior studies have eliminated firms with less than five (or more) employees [see Jacobson et al. (1993); Bonikowska and Morissette (2012)]. This is

[9] The sample size in Albæk et al. is 547 displaced workers.

[10] The propensity score is the probability that a subject will be displaced, based on predetermined characteristics.

[11] In a few cases no suitable match was found, and therefore the control group consist of 567 observations.

[12] Please note that I have also included sample means of the workers employed in firms not closing due to the death of the owner (column 3). These workers have a longer tenure, but besides from this, there are no significant differences in individual characteristics between this group and the sample of displaced workers. This is reassuring, as this indicates that the reason for these firms not closing is not because of more qualified workers.

done in order to make the construction of the mass-layoff samples more reasonable. Given that this study focuses only on firm closures, this restriction is not necessary in this paper, and does therefore not apply. Furthermore, prior studies have restricted on the displaced workers' tenure in a given firm, because high-tenure workers are the ones most likely to have accumulated substantial amounts of firm-specific capital prior to their job losses. This is not done in this study due to sample size restrictions, which will, all other things being equal, tend to give smaller estimates compared to studies restricting on tenure. Note, however, that the average tenure for the displaced workers in this study is quite long (5.8 years, which is the exact same as in Albæk et al. (2002)), which enhances my confidence in leaving out this restriction will not affect the magnitude or the comparability of the results.

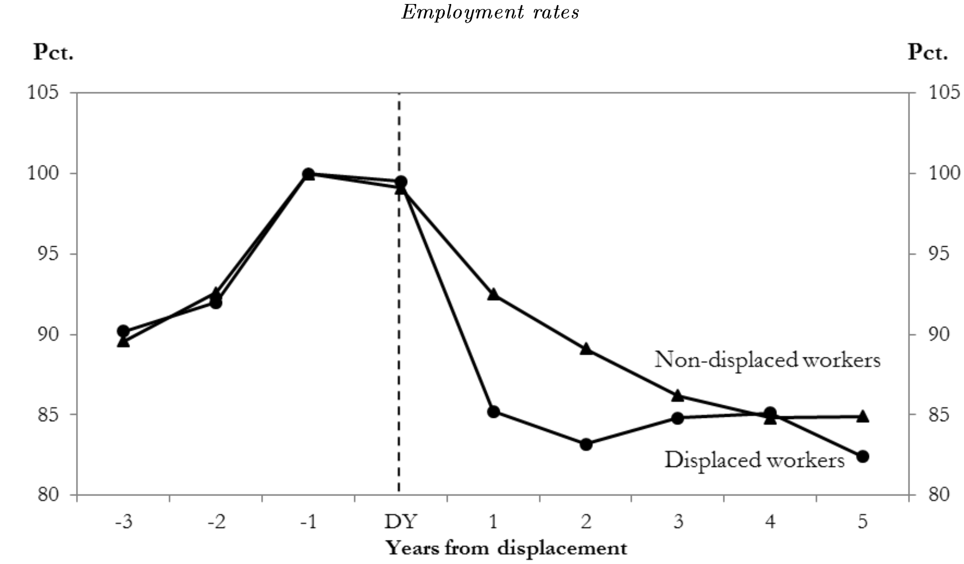
4 Empirical Findings

4.1 Graphical Evidence

Figure 1 shows the development in employment rates in the three years leading up to the displacement event and five years after. The figure has three obvious features. First, the employment rates are completely parallel until the displacement year. This indicates that the underlying trend in the control group is the same as that of the treatment group, thus fulfilling the common trend-assumption (c.f. the identifying assumptions). Second, and not surprising, after the displacement year there is a sharp decline in the employment rate of the displaced workers relative to that of the non-displaced workers. The relative decline one year after the displacement is 7.3%. Third, the employment rate of the displaced workers gradually converge back towards the pre-displacement level. However, even five years after displacement the relative decline in the employment rate is 2.5%, suggesting that some persistence might exist in the displacement effects. This pattern is in line with prior findings in the literature, and is also in line with the results found by Albæk et al. (2002).

Figure 1: Employment Before and After Displacement

Employment rates



Note: DY=Displacement Year.

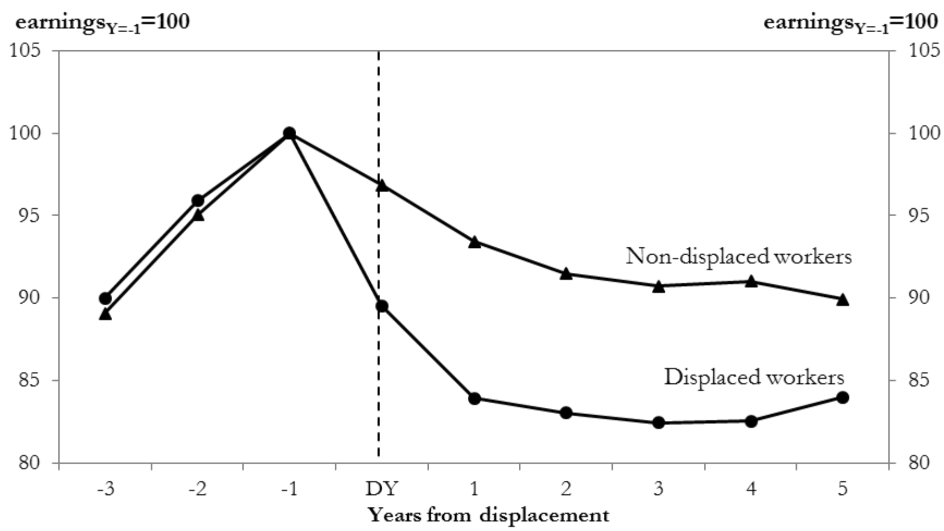
Source: Statistics Denmark and own calculations.

Figure 2 shows the development in average annual earnings in the three years leading up to the displacement event and five years after. The figure has four features similar to those in figure 1. First, the average annual earnings of the two groups seem to follow the same trajectory until the displacement year, again suggesting that the underlying trend in the control group is the same as that of the treatment group, and thus fulfilling the common trend-assumption. Second, in the displacement year there is a decline in average annual earnings among the displaced workers relative to that of the non-displaced workers.[13] The relative decline one year after the displacement is 9.5% (22,964DKK). Given that the two groups follow the same earnings trajectory, and therefore must have had nearly the same earnings-related characteristics, one can interpret the difference in earnings as losses due to displacement. The 9.5%-difference narrows to 5.9% (12,493DKK) five years after the displacement. Third, as the difference in earnings persist even five years after displacement indicates that the displacement effects might constitute a structural loss for the displaced workers.

Finally, there is no sign of earnings beginning to decline before the displacement year, which means that there is no sign of an Ashenfelter's Dip. This is interesting since prior studies have found that earnings start to decline in the years leading up to the displacement event as a result of firms cutting wages and weekly hours. This feature enhances my confidence in the identification strategy, as it indicates that the firm closures are truly unexpected.

Figure 2: Annual Earnings Changes Before and After Displacement

Percentage of pre-displacement annual earnings



Note: DY=Displacement Year. Annual earnings are average annual wage earnings. Earnings are in 2012 Danish Kroner.

Source: Statistics Denmark and own calculations.

The decline in annual earnings can emerge as a result of a lower number of hours worked (employment effect) and lower hourly wages (productivity effect). In this section the displacement effects on hourly wages are examined. Figure 3 shows the development in average hourly wages in the three years leading up to the displacement event and five years after using a balanced panel.[14] As well as the

[13] The drop in earnings happens already in the displacement year, whereas the drop in employment is first seen in the year after (c.f. figure 1). This can be explained by the fact that employment information is registered at the end of November, whereas income information is registered at the end of the year.

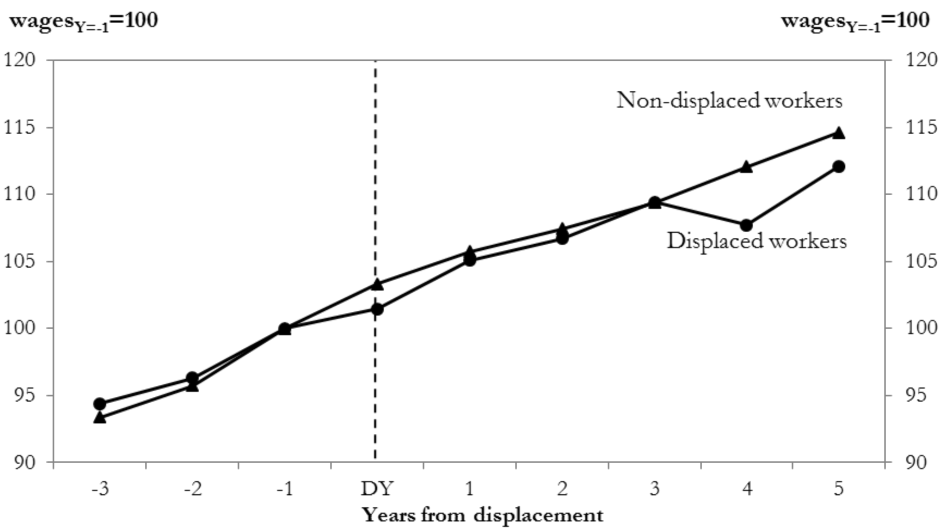
[14] Figure 3 is somewhat different from the figures presented in Albæk et al. (2002). In their study, they present average

figures above, figure 3 confirm that the common trend-assumption is fulfilled as the two groups' hourly wages are completely paralleled until the displacement year. The productivity effects immediately after the displacement event are modest. One year after the displacement the relative decline is 0.7%. However, four years after the displacement there is a 4.4% dip in hourly wages narrowing to 2.5% after five years. This pattern is not easily explained, but it indicates that at least some part of the negative long-term impact on earnings is driven by wage losses, suggesting that the costs of being displaced are not just in terms of labor market attachment, but also carry costs in terms of lower productivity.

Finally, the figure shows that there appears to be an upward trend in hourly wages for both groups. This might be interpreted as real wage increases.

Figure 3: Wage Changes Before and After Displacement

Percentage of pre-displacement hourly wages



Note: DY=Displacement Year. Wages are average hourly wages. Wages are in 2012 Danish Kroner.

Source: Statistics Denmark and own calculations.

Measuring the effects relative to the workers employed in firms not closing following the death of the owner provides similar results, thereby indicating that the potential selection effect in firms closing due to the owners death is not a threat to the identification.[15]

To conclude, the graphical analysis in section 4 provides evidence of displacement effects, both in terms of aggregate earnings effects and in terms of productivity effects. The story seems to be the following: The short-term losses in annual earnings are driven by lower employment rates following displacement. After a few years the employment rates climbs back, but still some earnings effects persists, which seems to be driven by lower hourly wages (i.e. productivity).

earnings and wages conditional on employment at the end of the relevant year. This means that averages are not across the same people in each year, and therefore changes in employment, wage, and selection are all intertwined. The figure in this section is based on a balanced panel of individuals who are observed throughout the period (i.e. conditioning on being in employment at the end of each year). The consequence of doing this is that I remove some observations from my sample, but by doing this, I am able to remove the selection effects from the displacement effects. This approach is in accordance with Jacobson et al. (1993). Note that this will most likely tend to underestimate the effects as the selected individuals are the ones who managed to find a new job immediately after displacement.

[15]Figures not shown, but available upon request.

The findings seems to be in line with those found by Albæk et al. (2002) for the Danish labor market, and the persistence in the effects are also in line with prior findings in the literature. I do not, however, find evidence of an Ashenfelter's Dip, which is in contrast with the findings in prior studies and is a feature that can be attributed to my identification strategy.

4.2 Regression Results

So far I have presented graphical evidence of the displacement effects, but in order to make sure that the effects are actually significantly greater than zero, one has to make a numerical analysis. Therefore, this section presents regression evidence using my sample of displaced workers between 1994 and 2008. The results are based on OLS estimations of equation (1). I present separate estimations of the short-term and long-term displacement effects (i.e. 1 year and 5 years after the displacement year) on employment, annual earnings, and hourly wages. The full details of the regressions are provided in table D1-D4 in Appendix D.

The first set of results is presented in column 1 in table D1 and D2, and shows the short-term and long-term displacement effects on employment. The table reports the difference-in-differences estimates in the gray-colored row controlling for a wide range of observable characteristics. This is the estimates capturing the displacement costs. The regression results show that the employment rate has dropped 7.7% one year after the displacement relative to that of the non-displaced workers, which is significantly different from zero ($p < 0.01$). Thus, there is a clear effect of being displaced on employment. However, five years after the displacement the employment rate has converged back and there is no longer a significant effect. Hence, the estimated effects confirms the pattern in figure 1.

The *post*-estimate is significant and negative, which means that there is a downward trend in the employment rates. There can be several explanations for this. One explanation could be a negative cyclical effect in the analyzed period. Another explanation could be the labor market transition documented in Munch (2010). The coefficients on the explanatory variables captures level-effects across individual characteristics (e.g. the significant and positive estimate on gender shows that men have a 2.9% higher employment rate than women).

The second set of results is presented in column 2 in table D1 and D2, and shows the short-term and long-term displacement effects on annual earnings. The regression results show that the short-term displacement effects on annual earnings are significantly different from zero ($p < 0.05$). The estimated short-term annual earnings loss is around 25,000DKK, corresponding to a loss of 10.6%. Five years after displacement the annual earnings decline has fallen to around 14,000DKK, corresponding to a 7.0% loss, suggesting some persistence in the displacement effects. However, the long-term estimate is insignificant at the 10%-level which is most likely due to the large standard errors resulting from the relatively small sample size. I am therefore putting myself on the line when suggesting that the estimate would have been significant had the sample been larger (see also the graphical evidence above which clearly suggest a significant effect).

Column 3 and 4 in table D3 shows group-specific estimates of the displacement effects on annual earnings. These effects are estimated by interacting the $post \times treat$-dummy with the explanatory variables. The estimates shows that highly experienced workers suffer the largest earnings loss following displacement. Moreover, the same holds for long-tenured workers (though estimates are insignificant) and white-collared workers. These findings are in line with the theory suggesting that these workers will endure the largest losses, as these workers have accumulated most firm-specific capital.

Column 3 in table D1 and D2 shows the estimated effects on annual earnings for the reemployed displaced workers. Here, both the short-term and long-term estimates are insignificant. This is in contrast to Albæk et al. (2002), as they find a 4.7% loss after one year and a 6,8% loss after 3 years. This could, as previously discussed, be due to the differences in identification strategy, as their results might overestimate the effects due to endogeneity problems. The difference in the short-term estimates in column 2 and 3 suggest that the annual earnings losses are driven by employment effects.

The third set of results is presented in column 4 in table D1 and D2, and shows the short-term and long-term displacement effects on hourly wages. Again, the gray-colored row provides the difference-in-differences estimates controlling for observable characteristics. As the regression results shows, the short-term displacement effects on hourly wages are insignificant. Five years after the displacement the relative loss in hourly wages is 8.7DKK corresponding to a 2.6% loss, which is insignificant at the 10%-level. This could, again, be a result of large standard errors. The estimated effects on hourly wages are in the lower end of what is found by Albæk et al. (2002). The explanation for this finding could be the same as the one offered above.

The *treat*- and *post*-estimates are both significant indicating that mean hourly wages are lower for the displaced workers when controlling for the displacement effect, and that there is a common upward trend in hourly wages. These differences are not conflicting the identifying assumptions.

Finally, in table D4 I report the results for a regression using the non-displaced workers in firms not closing following the owners death as the treatment group. The effect estimates are all insignificant, indicating that it is truly random which firms close following the death of the owner.

5 Conclusion

The literature on displacement is very extensive. Many studies have tried to examine the effects of being displaced, but two identification problems have plagued the literature. This paper has estimated the displacement effects using a natural experiment to generate exogenous variation in displacements. By identifying sudden deaths of firm owners, I am able to identify unexpected firm closures which led to displacement of the workers employed in those firms. This strategy provides ceteris paribus-variation, enabling me to overcome the endogeneity problems that have plagued the literature.

Comparing the performance of the displaced workers in my 1994-2008 sample with a matched sample of non-displaced workers I find clear evidence of moderate short-term employment effects of 7.7% and short-term losses in annual earnings of 10.6%. I do not find significant effects on hourly wages, and against this background it cannot be concluded that displacements causes harmful productivity effects. The long-term effects are insignificant at the 10%-level, but this could be due to large standard errors. At least the graphical evidence suggest some persistence in the earnings losses.

The findings are consistent with those found in prior literature, including Albæk et al. (2002) and Jacobson et al. (1993), albeit the magnitude of the effects is somewhat smaller. I do not find evidence of an Ashenfelter's Dip in earnings or wages which can be attributed to my identification strategy.

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Appendix A: Prior Literature

Table A1: Studies of displacement effects across countries

Country	Author	Data	Period	Short-term earnings	Long-term earnings	Earnings definition	Long-term definition	Displacement definition
				A. Scandinavian Regime				
Denmark	Albæk et al. (2002)	Firm-based data linked to individual records	1980-91	4.0%	6.4%	Hourly wages	3 years	Firms w. 5+ employees; 30%+ layoffs; 3+ years of tenure
Finland	Appelqvist (2007)	Finnish Longitudinal Employer Employee Data (FLEED)	1992 and 1997	1992: 42% 1997: 23%	1992: 9.2% 1997: 4.0%	Annual earnings	5 years	21-52-year olds; 3+ years of tenure; 50%+ layoffs
Norway	Huttenen et al. (2005)	Norwegian employer-employee survey	1988-2000	2.0%	5.0%	Annual earnings	3 years	Plant closure; full time male workers aged 25-55
Sweden	Eliason and Storrie (2006)	Linked employer-employee data	1986-87	Earnings differential = SEK 8.394	SEK 5.584	Annual earnings	3 years	Firm closure; workers aged 21-50
				B. Anglo-Saxon Regime				
Canada	Bonikowska and Morissette (2013)	Statistics Canada Longitudinal Worker File (10% random sample of all Canadian workers)	1984-2008		Men: 13-20% Women: 2-7%	Annual earnings	5 years	Permanent layoffs with stable prior labor market attachement
New Zealand	Dixon and Stillman (2009)	Statistics New Zealand's Linked Employer-Employee Data (LEED)	2001-2004	22.0%	16.0%	Monthly earnings	4 years	Firm closures only; 25-64-year olds with 2+ months of tenure
United Kingdom	Hijzen et al. (2010)	New Earnings Survey, Inter-Departmental Business Register, and Annual Business Inquiry	1994-2003	35.0%		Annual income		Firm closures and mass-layoffs
United States	Couch and Placzek (2010)	Administrative files from Connecticut	1993-2004	32.0%	12.0%	Quarterly earnings	6 years	Both quitters and workers fired for cause
United States	Jacobson et al. (1993)	Administrative files from Pennsylvania	1982	50.0%	25.0%	Quarterly earnings	5 years	Firm closures and mass-layoffs; workers with 6+ years of tenure
				C. Central-European Regime				
Belgium	Albæk et al. (2002)	Firm-based data linked to individual records	1980-91	2.0%	3.7%	Hourly wages	3 years	Firms w. 5+ employees; 30%+ layoffs; 3+ years of tenure
Germany	Couch (2001)	German Socio-Economic Panel	1991-96	16.5%	3.5%	Annual earnings	2 years	Firm closures and mass-layoffs
				D. Southern-European Regime				
France	Bender et al. (2002)	Annual social data reports, Permanent Dynamic Sample, Unified System of Enterprise Statistics	1976-95	32.0%	20.0%	Real annual earnings	5 years	Firm closures; men aged 26-60; 4+ years of tenure
Italy	Rosolia (2002)	Random sample of social security individual records	1974-97	11.0%	10.0%	Weekly earnings	4 years	All job separations; 16+ quarters of tenure

Note: The table shows a selection of some of the most influential papers in the literature for each labor market regime.
Source: OECD (2013).

Appendix B: Data Sources and Construction of the Dataset

Data Sources

I construct a data set with 602 workers displaced due to the sudden death of their firm owner. The data covers the entire Danish population in the period between 1990 and 2012. My analysis, however, will focus on individuals who are displaced in the period from 1990 to 2008, leaving a four-year evaluation period after firm closure to quantify the displacement effects.

The data set contains economic, educational, and demographic information about the individuals from relevant official registers. The data set was constructed based on six different sources made available from Statistics Denmark, as explained below.[16]

1. Individual demographic data from the official Danish Central Population Register (CPR Registret). These records include individuals' personal identification number (CPR), gender, date of birth, marital status, and origin.

2. Causes of deaths from the Danish Cause-of-Death Register at the Danish National Board of Health (Sundhedsstyrelsen). This data set classifies the cause of death according to international guidelines specified by the World Health Organization's International Classification of Diseases (ICD-10) system. The source of this data is the official death certificates that are issued by a doctor immediately after the death of Danish citizens. The death certificate details the cause of death based on post-mortem examination reports and information on social and psychiatric history provided by family members and associates. Because the death certificate and the post-mortem examination reports are carried out by a doctor, the classification conveys a medically qualified opinion on the cause of death. I have obtained the cause of death from all Danish citizens who passed away between 1990 and 2008. I use this data set to construct a sample of individuals who died suddenly and unexpectedly. Sundhedsstyrelsen compiles these data and makes them available through Statistics Denmark.

3. Firm data and wage data from Statistics Denmark's Integrated Database for Labour Market Research (IDA). The purpose of IDA is to provide access to coherent data about persons and establishments at the level of individual persons and individual establishments. The firm-level information in this data set is registered at the end of November each year. This data set enables me to identify firm closures, and furthermore, this data set contains information on hourly wages which is used in the regression analysis. Hourly wages in IDA are calculated using the wage income during the year in the firm in the numerator and the estimated number of hours worked in the firm in the denominator. The estimated number of hours worked is based on weekly contributions to a pension scheme, where the size of the contribution depends on the number of working hours. There are some measurement errors in the calculation of this variable, and I therefore only use observations of usable quality. Moreover, this data set enables me to identify firm owners. Firm owners comprise all persons who own (or are co-owners of) an individual firm, i.e. a sole proprietorship, a partnership, or a limited partnership, in which there is at least one employee at the end of November in a given year. The data covers the period from 1990 to 2010.

[16] Collectively, these data are made available through Statistics Denmark, but one has to obtain permission to use them. My permission was granted by Kraka Foundation.

4. Education records from the Danish Ministry of Education. This data set contains information on the educational background of the Danish population. The data is registered on a yearly basis by the Danish Ministry of Education and made available through Statistics Denmark. I use this data set to measure individuals' education levels. I have obtained access to this data from 1990 to 2012.

5. Income information from the official records at the Danish Tax and Customs Administration (SKAT). This data set contains information on the personal incomes of the Danish population. SKAT receives this information directly from the relevant sources, i.e. employers provide statements of wages paid to their employees. I have obtained access to personal income data from 1990 to 2011.

6. Employment data from the Register-based Labour Force Statistics (RAS). This data set provides a description of the Danish population's attachment to the labor market at a given time in the year (end of November). To do so the variable *socio-economic status* is used. This follows international guidelines set by the International Labour Organisation (ILO). I have obtained access to this data from 1990 to 2011.

Combining these data sources allows me to identify the employment and wage effects of displaced workers by using the exogenous variation from unexpected firm closures due to sudden death of firm owners.

Construction of the Data set

To identify firm closures due to sudden death of the firm owner, I merge the data on cause of death to the firm-data. The starting point of my analysis is to identify persons who die suddenly and unexpectedly. To identify sudden and unexpected deaths I rely on medical literature. The medical literature distinguishes between natural deaths (due to disease) and unnatural deaths (accidents and violence). Among natural causes sudden death is defined as unexpected and non-traumatic deaths that occur instantaneously or within a few hours of an abrupt change in the person's previous clinical state. The identification of relevant ICD-10 codes relies on related medical literature as well as a thorough inspection of WHO's detailed classification system.[17] This definition also follows the one used in Andersen & Nielsen (2011) and Andersen & Nielsen (2012). Thus, among natural deaths, I consider acute myocardial infarction (IDC-10: I22-I23), cardiac arrest (I46), congestive heart failure (I50), stroke (I60-I69), and sudden deaths by unknown causes (R95-R97) as sudden deaths. Among causes of unnatural deaths are traffic accidents (V00-V89) and other accidents and violence (V90-V99, W00-X59, Y40-Y69, Y70-Y86, and Y88), which are all unanticipated. Table B1 shows the distribution of causes of death across time using WHO's ICD-10 codes. In total I have identified 150.114 persons who died suddenly and unexpectedly between 1994 and 2008 among 721.295 deaths in total.

The second step in my sample selection is to identify the firm owners among the persons who died suddenly and unexpectedly. This is done by merging the IDA data, in which I identify the firm owners, with the causes-of-death data. Table B2 shows the distribution of persons who died suddenly and unexpectedly between 1994 and 2008 on their type of employment in the year before their death. In total I have identified 594 firm owners who died suddenly and unexpectedly between 1994 and 2008.

[17] See www.who.int/classifications/icd/en.

Table B1: Cause of death, 1994-2008

	ICD-10	Year															Total
		1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	
A. Deaths	***A00-Z98***	***48.317***	***49.806***	***49.457***	***48.892***	***47.916***	***48.658***	***47.634***	***48.157***	***48.669***	***47.902***	***46.907***	***46.261***	***46.939***	***48.086***	***47.694***	***721.295***
B. Deceased's cause of death																	
Natural deaths	**A00-R99**	**10.406**	**10.860**	**9.346**	**8.583**	**8.176**	**8.072**	**7.992**	**8.062**	**7.998**	**7.737**	**7.110**	**7.073**	**6.809**	**5.897**	**5.272**	***119.393***
Acute myocardial infarction (a)	I22-I23	941	995	631	600	459	465	450	473	83	8	9	28	13	10	-	***5.165***
Cardiac arrest (b)	I46	614	817	730	482	463	454	488	536	575	613	458	538	479	174	38	***7.459***
Congestive heart failure (c)	I50	1.672	1.767	1.816	1.664	1.729	1.756	1.576	1.601	1.854	1.801	1.574	1.352	1.351	1.296	1.225	***24.034***
Stroke (d)	I60-I69	5.384	5.543	5.464	5.149	4.995	5.000	4.994	5.070	5.384	5.266	5.025	4.768	4.646	4.196	3.942	***74.826***
Sudden death by unknown cause (e)	R95-R97	1.795	1.738	705	688	530	397	484	382	102	49	44	387	320	221	67	***7.909***
Unnatural deaths	**V00-Z98**	**2.376**	**2.460**	**2.240**	**2.421**	**2.352**	**2.446**	**2.364**	**2.120**	**1.729**	**1.739**	**1.620**	**1.697**	**1.804**	**1.683**	**1.670**	***30.721***
Traffic accidents (f)	V00-V89	609	639	555	521	540	534	512	468	465	453	383	358	316	390	398	***7.141***
Other accidents and violence (g)	W00-X59 Y40-Y69 Y70-Y86 Y88	1.767	1.821	1.685	1.900	1.812	1.912	1.852	1.652	1.264	1.286	1.237	1.339	1.488	1.293	1.272	***23.580***
C. Sudden deaths (a)+(b)+(c)+(d)+(e)+(f)+(g)		***12.782***	***13.320***	***11.586***	***11.004***	***10.528***	***10.518***	***10.356***	***10.182***	***9.727***	***9.476***	***8.730***	***8.770***	***8.613***	***7.580***	***6.942***	***150.114***

Note: This table tabulates the cause of death using ICD-10 classifications. ICD-10 is the World Health Organization's International Classification of Diseases. Panel B shows the number of sudden and unexpected deaths. Note that "Other accidents and violence" does not include suicides and homicides. Panel A shows the number of total deaths in a given year, whereas Panel B and C shows the number of sudden deaths.

Source: Statistics Denmark and own calculations.

Table B2: Type of employment in the year before sudden death, 1994-2008

	Year															Total
	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	
Self-employed	307	244	229	164	191	192	172	157	177	150	122	116	95	109	105	2.530
Assisting Spouse	12	11	9	8	8	2	10	5	5	2	1	4	1	3	4	85
Firm owner	85	72	62	39	37	49	38	19	34	39	30	38	15	22	15	594
Employee	1.178	1.127	1.087	1.006	995	988	1.047	932	896	749	777	857	858	770	907	14.174
Total	1.582	1.454	1.387	1.217	1.231	1.231	1.267	1.113	1.112	940	930	1.015	969	904	1.031	17.383

Note: The total in the table does not sum up to the number of sudden deaths since not all the deceased due to sudden deaths are in employment in the year before they die.

Source: Statistics Denmark and own calculations.

The third step in my sample selection is to identify the firm closures among the firms of which the firm owner dies suddenly and unexpectedly. The identification of these firm closures relies on one of two restrictions: *(1)* Firms that cease to exist in the year or the year after the death of the owner, are assumed to close due to the death of the owner. *(2)* Firms in which all employees except from one are displaced in the year after the death of the owner, are assumed to close due to the death of the owner. This definition is used to construct table B3 that shows the number of firms closing due to sudden death of the firm owner between 1994 and 2008. Among the 594 firms in which the owner dies suddenly and unexpectedly, 445 firms close between 1994 and 2008. Among these, 345 firms close in the year or the year after the owner dies.

Table B3: Unexpected firm closures, 1994-2008

	Year															Total
	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	
Closed due to sudden death of owner	22	40	45	25	21	25	24	17	17	20	21	24	14	16	14	345
Closed for other reason	0	0	1	5	6	8	11	10	4	7	11	8	10	14	5	100
Total	22	40	46	30	27	33	35	27	21	27	32	32	24	30	19	445

Note: The table shows the number of firms that closes due to the unexpected death of the owner for each year. This is defined as firms that ceases to exist in the year or the year after the owner dies or firms in which all employees except from one is displaced following the death of the owner.

Source: Statistics Denmark and own calculations.

The last step in my sample selection is to identify the workers displaced. Table B4 shows the number of workers displaced from unexpected firm closures between 1994 and 2008. Removing secondary jobs and restricting the workers to be aged between 18 to 64 and to receive positive wages prior to displacement leaves me with a sample of 602 workers displaced due to sudden death of the firm owner. This sample is my treatment group.

Table B4: Workers displaced from unexpected firm closures, 1994-2008

	Year															Total
	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	
Displaced workers	116	131	153	75	91	93	101	39	58	84	74	75	38	81	80	1.289
- Under 18 years old	8	10	22	6	10	17	7	5	2	6	8	5	4	14	16	140
- Above 64 years old	13	20	13	8	9	8	7	3	8	10	8	15	6	5	4	137
- Non-positive hourly wage	28	26	32	15	18	20	26	8	17	18	21	23	17	22	21	312
- Secondary job	4	10	10	4	9	6	9	4	6	9	8	6	2	6	5	98
= Final sample	**63**	**65**	**76**	**42**	**45**	**42**	**52**	**19**	**25**	**41**	**29**	**26**	**9**	**34**	**34**	**602**

Note: Secondary job also includes assisting spouses and self-employed.

Source: Statistics Denmark and own calculations.

Appendix C: Summary Statistics

Table C1: Characteristics of displaced and non-displaced matched workers

	Treatment group:			*Control group:*			*Sensitivity:*											
	Displaced workers due to sudden death of owner			**Non-displaced matched workers**			**Non-displaced workers in non-closed firms with sudden death of owner**			**Total 2008-Employment**			**Difference**					
	(1)			**(2)**			**(3)**			**(4)**			**(1)-(2)**		**(1)-(3)**		**(1)-(4)**	
	Mean	*Std*	*Median*	*Mean*	*Std*	*Median*	*Mean*	*Std*	*Median*	*Mean*	*Std*	*Median*	*Mean*	*t-value*	*Mean*	*t-value*	*Mean*	*t-value*
A. Individual characteristics																		
Demographic and socioeconomic																		
Age (years)	38.7	12.1	39	38.2	12.0	38	37.2	12.8	37	39.9	12.1	40	0.5	0.7	1.5	1.0	-1.2	-2.4
Gender (% male)	66.3	47.3	1	67.2	47.0	1	63.1	48.3	1	51.1	50.0	1	-0.9	-0.3	3.2	-0.1	15.2	7.9
Immigrant (%)	3.7	18.8	0	4.6	20.9	0	6.4	24.4	0	7.1	25.7	0	-0.9	-0.8	-2.7	-0.6	-3.4	-4.4
Married (%)	42.7	49.5	0	42.3	49.5	0	49.7	50.0	0	51.6	50.0	1	0.4	0.1	-7.0	0.0	-8.9	-4.4
Education																		
a. Primary education (%)	43.5	49.6	0	43.9	49.7	0	37.5	48.5	0	24.1	42.7	0	-0.4	-0.1	6.0	0.0	19.4	9.6
b. Secondary education (%)	49.0	50.0	0	49.4	50.0	0	53.1	49.9	1	46.3	49.9	0	-0.4	-0.1	-4.1	0.0	2.7	1.3
c. Tertiary education (%)	7.5	26.3	0	6.7	25.0	0	9.4	29.2	0	29.7	45.7	0	0.8	0.5	-1.9	0.3	-22.2	-20.7
Work-related																		
Hourly wage (mean)	208.3	188.1	180	192.5	75.7	186	188.1	95.9	179	225.6	158.6	205	15.8	1.9	20.2	0.2	-17.3	-2.3
Experience (years)	14.6	9.5	14	14.8	9.7	14	14.0	9.9	13	16.6	11.4	15	-0.2	-0.4	0.5	-0.7	-2.0	-5.2
Tenure (years)	5.8	5.5	4	5.6	5.3	3	7.0	5.9	5	6.1	6.2	4	0.3	0.9	-1.2	2.6	-0.3	-1.1
Working position																		
a. Director or employer (%)	0.8	9.1	0	0.7	8.4	0	1.2	10.8	0	2.9	16.9	0	0.1	0.2	-0.4	0.3	-2.1	-5.7
b. White-collar worker (%)	15.6	36.3	0	15.5	36.2	0	18.4	38.8	0	33.7	47.3	0	0.1	0.0	-2.8	0.0	-18.1	-12.2
c. Skilled blue-collar worker (%)	40.7	49.2	0	41.6	49.3	0	48.2	50.0	0	40.6	49.1	0	-0.9	-0.3	-7.5	-0.1	0.1	0.0
d. Unskilled blue-collar worker (%)	18.4	38.8	0	18.5	38.9	0	11.7	32.2	0	8.2	27.4	0	-0.1	0.0	6.7	0.0	10.2	6.4
e. Unspecified and other (%)	24.4	43.0	0	23.6	42.5	0	20.4	40.4	0	14.6	35.3	0	0.8	0.3	4.0	0.1	9.8	5.6
Number of individuals		*602*			*567*			*597*			*2,449,389*		*35*		*5*			
B. Firm characteristics																		
Number of employees (mean)	2.8	3.2	2	110.6	280.3	21	7.0	7.7	5	358.9	980.0	56	-107.8	-9.2	-4.2	-26.8	-356.1	-556.5
Sole proprietorship (%)	77.6	41.8	1	21.2	40.9	0	44.0	49.9	0	5.9	23.5	0	56.4	23.3	33.6	8.8	71.7	42.1
Industry																		
a. Agriculture, forestry, fishing (%)	19.2	39.5	0	13.4	34.1	0	13.8	34.6	0	1.6	12.4	0	5.8	2.7	5.4	1.3	17.6	10.9
b. Manufacturing (%)	7.7	26.7	0	8.1	27.3	0	11.0	31.4	0	14.6	35.3	0	-0.4	-0.3	-3.3	-0.2	-6.9	-6.3
c. Construction (%)	11.5	32.0	0	12.9	33.5	0	11.0	31.4	0	6.9	31.5	0	-1.4	-0.7	0.5	-0.4	4.6	3.5
d. Trade, hotels, restaurants (%)	19.9	40.0	0	20.8	40.6	0	23.9	42.8	0	16.2	36.8	0	-0.9	-0.4	-4.0	-0.2	3.7	2.3
e. Transport, information, comm. (%	11.2	31.6	0	16.4	37.1	0	7.3	26.2	0	6.2	24.1	0	-5.2	-2.6	3.9	-1.5	5.0	3.9
f. Financial, insurance, oth. serv. (%)	12.2	32.8	0	11.3	31.7	0	17.4	38.1	0	13.2	33.8	0	0.9	0.5	-5.2	0.2	-1.0	-0.7
g. Public and personal services (%)	7.3	26.1	0	10.6	30.8	0	13.8	34.6	0	35.0	47.7	0	-3.3	-2.0	-6.5	-1.1	-27.7	-26.0
i. Unspecified (%)	10.5	30.7	0	6.5	24.7	0	1.8	13.5	0	6.3	24.4	0	4.0	2.5	8.7	1.8	4.2	3.4

Note: Tenure is number of years employed at a given firm since 1980. Hourly wages are calculated using the wage income during the year in the firm in the numerator and the estimated number of hours worked in the firm in the denominator. For binary variables (e.g. gender) the median will take the value '1' if the median person is in the category. Characteristics are reported for two years prior to the death of the owner, thereby alleviating potential effects from an Ashenfelter's Dip. The wage information is 2012-level.

Source: Statistics Denmark and own calculations.

Appendix D: Regression Results

Table D1: Regression Analysis of Short-Term Displacement effects

Response variable:	Employment		Annual Earnings				Hourly Wages	
	(1)		(2)		(3)		(4)	
Population:	All		All		Reemployed		Reemployed	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
Treat *(d)*	0.00174	(0.013)	-26,819.2***	(8027.294)	-24631.2***	(7,474.114)	-14.85***	(4.463)
Post *(d)*	-0.0743***	(0.013)	-19,328.2**	(8054.020)	-4,213.1	(7,601.550)	9.517**	(4.370)
Treat x Post *(d)*	-0.0770***	(0.019)	-24,995.3**	(11375.499)	-4,337.1	(10,894.870)	1.196	(6.262)
Explanatory variables:								
Age	0.000525	(0.003)	4,260.0**	(2,097.522)	4,498.5**	(2,037.128)	3.831**	(1.195)
Age^2	-0.0000408	(0.000)	-91.21***	(25.625)	-88.08***	(24.969)	-0.0509***	(0.015)
Male *(d)*	0.0194*	(0.011)	6,4617.3***	(6,590.991)	6,5411.7***	(6,305.682)	37.19***	(3.673)
Immigrant *(d)*	0.0379	(0.025)	-1,656.8	(1,5147.641)	12,000.9	(14,766.060)	-13.98	(8.728)
Primary education *(d)*	-0.0211**	(0.010)	-30,680.0***	(5,910.973)	-29,688.0***	(5,659.870)	-23.19***	(3.259)
White-collar *(d)*	0.00731	(0.014)	57,670.7***	(8,599.802)	60,450.9***	(8,230.688)	31.89***	(4.736)
Experience	0.00371	(0.002)	9,823.3***	(1,444.094)	9,988.6***	(1,391.890)	1.672**	(0.832)
$Experience^2$	-0.0000432	(0.000)	-139.2***	(38.253)	-143.9***	(36.744)	-0.0182	(0.022)
Tenure	0.000627	(0.003)	3,828.5**	(1,803.751)	3,261.4*	(1,738.117)	-0.965	(0.979)
$Tenure^2$	0.0000114	(0.000)	-193.8**	(81.876)	-159.1**	(79.427)	0.0192	(0.044)
Constant	1.023***	(0.063)	144,200.1***	(38,649.302)	13,1873.6***	(36,985.136)	106.1***	(21.924)
Year of displacement dummies	Yes		Yes		Yes		Yes	
R^2	0.1045		0.2189		0.2286		0.1520	
Sample size	2219		2222		2102		2029	

Note: Estimated using OLS. Standard error in parentheses. *(d)* indicates discrete changes in dummy variables from 0 to 1. Pre is one year before displacement and post is one year after. Only observations with data of usable quality in wage regressions. * p<0.1, ** p<0.05, *** p<0.01.

Source: Statistics Denmark and own calculations.

Table D2: Regression Analysis of Long-Term Displacement effects

Response variable:	Employment		Annual Earnings				Hourly Wages	
	(1)		(2)		(3)		(4)	
Population:	All		All		Reemployed		Reemployed	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
Treat *(d)*	0.00131	(0.014)	-27,412.9**	(8,476.049)	-25,365.3**	(7,779.648)	-14.97**	(4.869)
Post *(d)*	-0.149***	(0.015)	-21,454.1**	(9,114.046)	13,797.1	(8,684.240)	37.05***	(5.217)
Treat x Post *(d)*	-0.0273	(0.021)	-14,128.3	(12,645.048)	595.2	(12,220.405)	-8.747	(7.265)
Explanatory variables:								
Age	0.0140***	(0.004)	4,911.4**	(2,354.385)	3,065.5	(2,329.226)	4.602**	(1.408)
Age^2	-0.000244***	(0.000)	-106.2***	(28.870)	-68.40**	(28.849)	-0.0634***	(0.017)
Male *(d)*	0.0285**	(0.012)	69,283.3***	(7,264.657)	70,683.7***	(6,940.065)	44.44***	(4.231)
Immigrant *(d)*	0.0191	(0.030)	-31,314.5*	(17,592.243)	-12,050.6	(17,170.986)	-31.44**	(10.537)
Primary education *(d)*	-0.0198*	(0.011)	-34,315.2***	(6,549.707)	-34,025.3***	(6,269.976)	-25.19***	(3.757)
White-collar *(d)*	0.0222	(0.016)	65,236.9***	(9,251.579)	6,4266.2***	(8,810.138)	34.02***	(5.304)
Experience	0.000305	(0.003)	7,581.7***	(1,624.341)	7,916.4***	(1,593.543)	0.211	(0.979)
$Experience^2$	0.0000744	(0.000)	-98.19**	(43.303)	-111.9**	(42.729)	0.0109	(0.026)
Tenure	0.00337	(0.003)	3,790.2*	(2,020.100)	2,597.8	(1,954.821)	-0.528	(1.186)
$Tenure^2$	-0.0000935	(0.000)	-180.4*	(92.243)	-118.7	(90.530)	0.00927	(0.055)
Constant	0.795***	(0.077)	16,7151.2***	(45,588.251)	17,5366.6***	(43,736.684)	115.0***	(24.809)
Year of displacement dummies	Yes		Yes		Yes		Yes	
R^2	0.1682		0.2013		0.1947		0.1821	
Sample size	2022		2027		1871		1815	

Note: See table D1. Post is five years after displacement.

Source: Statistics Denmark and own calculations.

Table D3: Group-specific displacement costs

Response variable:	Employment				Annual Earnings			
	(1)		(2)		(3)		(4)	
Population:	All		All		All		All	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
Treat *(d)*	0.01000	(0.110)	0.0160	(0.158)	-19,0161.6**	(70,331.623)	3,577.2	(97,299.623)
Post *(d)*	0.161	(0.114)	0.166	(0.161)	-23,0820.2**	(73,130.592)	-33,342.5	(99,040.501)
Treat x Post *(d)*	-0.565**	(0.193)	-0.570**	(0.224)	25,5650.7**	(123,586.378)	57,970.0	(138,001.315)
Interactions:								
Age	0.0372**	(0.012)	0.0377**	(0.013)	-6,186.1	(7,419.286)	-2,814.2	(8,306.403)
Age^2	-0.000476***	(0.000)	-0.000480**	(0.000)	83.14	(90.524)	21.19	(101.217)
Male *(d)*	0.0181	(0.036)	0.0144	(0.042)	-6,3717.9**	(2,3092.487)	23,324.3	(25,752.156)
Immigrant *(d)*	-0.0213	(0.086)	-0.0111	(0.097)	-12,787.4	(54,778.146)	1,715.2	(59,784.466)
Primary education *(d)*	0.0314	(0.033)	0.0294	(0.038)	27,925.3	(20,853.038)	-14,629.9	(23,198.006)
White-collar *(d)*	-0.0394	(0.046)	-0.0403	(0.053)	-60,525.3**	(29,554.693)	11,748.8	(32,729.105)
Experience	-0.0232**	(0.008)	-0.0239**	(0.009)	-13,277.2**	(5,172.677)	-4,051.3	(5,745.672)
$Experience^2$	0.000593**	(0.000)	0.000623**	(0.000)	248.9*	(137.767)	116.8	(151.683)
Tenure	-0.00971	(0.010)	-0.00972	(0.011)	-3,191.6	(6,248.168)	2,931.3	(6,967.230)
$Tenure^2$	0.000369	(0.000)	0.000365	(0.001)	10.08	(283.327)	-208.5	(316.459)
Explanatory variables	No		Yes		No		Yes	
Year of displacement dummies	Yes		Yes		Yes		Yes	
R^2	0.1359		0.1361		0.1791		0.2391	
Sample size	2219		2219		2222		2222	

Note: See table D1. Post is one year after displacement.
Source: Statistics Denmark and own calculations.

Table D4: Regression Analysis of Treatment Effects for Non-displaced Workers in Firms Not Closing Following Owners Death

Response variable:	Employment		Annual Earnings				Hourly Wages	
	(1)		(2)		(3)		(4)	
Population:	All		All		Reemployed		Reemployed	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
Treat *(d)*	-0.00152	(0.012)	-17,416.5**	(7,878.275)	-16,850.8**	(7,468.319)	-13.95**	(4.601)
Post *(d)*	-0.0743***	(0.011)	-19,302.4**	(7,624.959)	-3,777.3	(7,331.729)	9.412**	(4.447)
Treat x Post *(d)*	-0.00726	(0.016)	722.5	(10,606.755)	2,374.4	(10,207.326)	4.113	(6.188)
Explanatory variables:								
Age	0.00590**	(0.003)	6,846.7***	(2,010.361)	6,672.5***	(1,958.416)	6.460***	(1.211)
Age^2	-0.000107**	(0.000)	-120.1***	(24.450)	-113.6***	(23.831)	-0.0826***	(0.015)
Male *(d)*	0.00688	(0.009)	6,6248.1***	(6,169.761)	6,9135.0***	(5,928.419)	35.66***	(3.638)
Immigrant *(d)*	0.0200	(0.019)	-1,942.8	(13,091.156)	7,838.1	(12,774.185)	3.609	(8.042)
Primary education *(d)*	-0.0210**	(0.008)	-26,775.1***	(5,686.919)	-27,539.0***	(5,478.460)	-20.55***	(3.324)
White-collar *(d)*	0.0158	(0.012)	58,879.6***	(7,911.847)	59,819.5***	(7,607.933)	36.24***	(4.585)
Experience	0.00416**	(0.002)	8,225.7***	(1,390.292)	7,828.2***	(1,354.800)	0.693	(0.845)
$Experience^2$	-0.0000705	(0.000)	-88.48**	(36.136)	-75.72**	(35.110)	0.0120	(0.022)
Tenure	0.00660**	(0.003)	4,886.8**	(1,715.885)	3,436.3**	(1,655.662)	-0.601	(0.993)
$Tenure^2$	-0.000282**	(0.000)	-212.2**	(77.337)	-152.4**	(74.802)	-0.00201	(0.044)
Constant	0.927***	(0.055)	89,498.6**	(38,609.615)	82,088.4**	(37,246.378)	43.27*	(22.301)
Year of displacement dummies	Yes		Yes		Yes		Yes	
R^2	0.0841		0.2481		0.2610		0.1808	
Sample size	2290		2293		2220		2150	

Note: See table D1. Post is one year after displacement.
Source: Statistics Denmark and own calculations.