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A DISTRIBUTIONAL ANALYSIS OF DISPLACEMENT COSTS IN AN ECONOMIC DEPRESSION AND RECOVERY

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Abstract: We study the earnings losses of Finnish private sector workers who lost their jobs at two very different points in the business cycle. The first group was displaced in 1992 (depression period) and the second one in 1997 (recovery period). The focal point of the analysis is the quantile displacement effect, the change in the earnings distribution due to involuntary job separation. We use mass layoffs and plant closures to identify groups of workers who were displaced from exogenous causes. The effect of displacement is strongest at the lower end of the earnings distribution, and small or negligible at the upper end. Women and those displaced during the depression period are subject to larger earnings losses.

Key words: Displacement, earnings losses, unemployment, quantile regression.

Tiivistelmä: Tarkastelemme palkkatulojen menetyksiä yksityisen sektorin työntekijöiden keskuudessa, jotka menettivät työpaikkansa kahdessa hyvin erilaisessa suhdannetilanteessa. Ensimmäinen ryhmä menetti työnsä 1992 (lamaperiodi) ja toinen 1997 (kasvujakso). Tutkimme työpaikan menetyksen vaikutusta palkkatulojen jakauman sijaintiin ja muotoon. Hyödynnämme massairtisanomisia ja toimipaikkojen sulkemisia löytääksemme työntekijät, jotka menettivät työn eksogeenisista tekijöistä johtuen. Tulosten mukaan työpaikan menetys vaikuttaa voimakkaimmin palkkatulojen jakauman alapäässä. Jakauman yläpäässä vaikutus on varsin pieni tai sitä ei ole ollenkaan. Naiset kärsivät miehiä suurempia palkkatulojen menetyksiä. Työpaikan menetys lama-aikana johtaa selvästi suurempiin palkkatulojen menetyksiin.

Asiasanat: Irtisanominen, palkkatulot, työttömyys, kvanttiiliregressio.

Contents

1 Introduction

In all labor markets a large number of workers lose their jobs every year. Some job losers are re-employed quickly without significant earnings losses. Others remain unemployed for long periods, have to accept large cuts in the wage rate, or may be pushed out of the labor market. Job displacement can lead to substantial individual costs in terms of foregone earnings and employment. These costs have been the focus of a number of recent studies. Evidence from the US studies suggests that the average earnings losses of displaced workers are large and persistent, being around 10-25 percent even several years after the job loss (see Ruhm, 1991, Jacobson *et al.*, 1993, and Stevens, 1997). However, the reduction in employment following displacement has been found to be relatively short-lived in the US labor market. Some studies, including Couch (2001), Burda and Mertens (2001), and Bender *et al.* (2002) for Germany, Huttunen *et al.* (2006) for Norway, and Hijzen *et al.* (2006) for the UK, suggest that the long-term costs of job loss are small or non-existent in the European labor markers. On the other hand, studies by Borland *et al.* (2002) for the UK, Margolis (1999) for France, Carneiro and Portugal (2006) for Portugal, Eliason and Storrie (2006) for Sweden, and Appelqvist (2007) for Finland find the long-term losses to be much larger and more concordant with the earlier studies for the US. Although the results from these studies are not directly comparable due to the different time periods analyzed and large dissimilarities in the underlying data and research design, there seem to be significant differences in the displacement cost between countries.

With the exception of Carneiro and Portugal (2006) and Eliason and Storrie (2006),[1] the existing analysis of earnings losses associated with displacement has employed classical least-squares regression methods. Although the resulting effect on the conditional mean of earnings is of considerable interest, the distributional aspects of earnings losses are equally important. Earnings dispersion provides, for example, a measure of uncertainty about future earnings. For a given mean loss a larger increase in dispersion following displacement implies a larger welfare loss for the risk-averse worker. Furthermore, the mean impact is not indicative of the size and nature of the effect of displacement in the tails of the earnings distribution, which might be of primary interest from the policy point of view. A strong negative effect in the upper tail would suggest lower chances of being re-employed in a highly paid job for the displaced worker. An equally strong effect in the lower tail is perhaps more alarming, because it would imply that many workers drop to the bottom of the earnings distribution. If such an effect still exists several years after the displacement period, it may call for directed supportive measures. When the focus of the analysis is restricted solely on the mean impacts, these pieces of information will remain missing. In general, the change in the conditional mean gives an incomplete picture of the consequences of displacement. A more complete picture can be obtained by estimating a

[1] Carneiro and Portugal (2006) apply a quantile regression model to study the distributional effects. Eliason and Storrie (2006) consider the mean effects but use matching methods to construct a comparison group for displaced workers.

family of conditional quantile functions, which is the approach we take in this study.

This study considers the effect of job displacement over the entire distribution of earnings in Finland. We use linked employer-employee panel data to construct groups of private sector employees who lost their jobs at two very different points in the business cycle. The first group was displaced in 1992 (depression period) and the second group in 1997 (recovery period). Following the standard practice, we take separations associated with mass layoffs and plant closures to be job displacements, as they are likely to be exogenous from the workers' standpoint. These groups of displaced workers and the associated comparison groups are followed over an 11-year period beginning three years before and ending seven years after the year of possible displacement. To include all the costs of job loss we also include periods with zero earnings resulting from long-term unemployment or non-participation in the analysis.

Our two follow-up periods reflect markedly different macroeconomic conditions. At the beginning of the 1990s, Finland suffered an exceptionally severe depression; GDP dropped over 10% between 1990 and 1993, causing the unemployment rate to rise from 3.2% to 16.3% (according to the Labor Force Survey).[2] Hence, experiences of workers who lost their jobs in 1992 represent an extreme case that highlights the consequences that may follow from a displacement during exceptionally difficult labor market conditions, providing a worst-case scenario for job losers. The macroeconomic climate was dramatically changed by the end of the decade. At the end of the 1990s the economy grew 3–5% per year and the unemployment rate was declining. So our second group displaced in 1997 encountered an entirely different situation. In many ways, this period is more typical and hence more in line with the research on displacement conducted elsewhere.

According to our results, the earnings losses are especially large when job loss occurs in the depression period. In that case, the entire earnings distribution still lies below the counterfactual distribution (without job loss in the reference period) seven years after the job loss. Losing a job in the recovery period also has a long-lasting effect but only at the lower end of the distribution. Women tend to suffer from larger earnings losses irrespective of the timing of job loss in the business cycle. We also find evidence of considerable heterogeneity in the displacement effect. The effect of job loss is concentrated in the lower end of the distribution, being relatively moderate (the depression period) or even negligible (the recovery period) at the upper end. Finally, we show that job loss not only causes a decline in the expected earnings, but also raises uncertainty about the future earnings level due to a substantial increase in the earnings dispersion.

The rest of the paper proceeds as follows. In the next section we discuss the evaluation issues and define the quantile displacement effect. Section 3 describes our data and the selection of different worker groups for the regression analysis. Descriptive evidence is presented in section 4, which is followed by the regression results in section 5. The final section concludes.

[2] For a discussion of the Finnish depression, see e.g. Honkapohja and Koskela (1999).

2 Evaluation issues

We are interested in the effect of job displacement in a past period on the current earnings. Since the involuntary job loss can be viewed as a "treatment", we can discuss the evaluation issues along the lines of the extensive literature on program evaluation. Let Y_1 be the earnings in the current period if the worker was displaced in a reference period, and let Y_0 be the earnings in the counterfactual situation without displacement in the reference period. If we could observe workers in both states, the displacement effect, $Y_1 - Y_0$, would be directly observed for each worker. The fundamental problem of causal inference is that both Y_1 and Y_0 are never observed for the same individual (Holland, 1986).

The observed earnings can be expressed as

$$Y = DY_1 + (1 - D)\,Y_0, \tag{1}$$

where $D = 1$ if the worker was displaced, and $D = 0$ otherwise. Displacements are certainly not randomly assigned but dismissed workers are selected in a complicated procedure that takes into account individual characteristics, some of which may not be observable for the researcher. This implies a dependence between D and (Y_1, Y_0) even after controlling for a wide array of individual characteristics. For example, a good match between a worker and a job in the reference period may imply a high value of Y_0 and a low probability of displacement. One consequence of this is that simple comparisons of outcomes between displaced and non-displaced workers do not have a causal interpretation.

A vast majority of displacement studies have exploited mass layoffs or plant/firm closures to detect workers who lost their jobs from exogenous reasons. Such workers are less likely to be laid off because of their own characteristics or performance, but as a result of a shock that hit their employer. Under this assumption one can overcome the endogeneity problem by choosing a sample where displacements result from mass layoffs or plant/firm closures. This approach is taken also in this study. Thus D is hereafter an indicator of a displacement associated with a mass layoff or a plant closure, and it is assumed to be independent of (Y_1, Y_0) given observed individual characteristics.

2.1 Expected earnings losses

Since the individual effects of displacement cannot be identified, the focus of the evaluation literature has been on estimating the average effect, $E\left(Y_1 - Y_0\right)$, or the average effect on the displaced, $E(Y_1 - Y_0\,|D = 1)$. Let us assume that $E(Y_0\,|\mathbf{X} = \mathbf{x}) = \mathbf{x}'\boldsymbol{\beta}$ and $Y_1 = Y_0 + \alpha$, where α is an individual-specific effect of displacement which is independent of D and $\mathbf{X}$. Under these assumptions we could estimate $\delta = E(\alpha) = E\left(Y_1 - Y_0\right) = E(Y_1 - Y_0\,|D = 1)$ by least squares from

$$y = \mathbf{x}'\boldsymbol{\beta} + \delta d + \varepsilon. \tag{2}$$

The estimates of displacement costs have been usually obtained from regressions similar to this stylized example.[3] There are some pitfalls worth noting. First, the data on earnings typically involve observations on zero earnings for the long-term unemployed and those who withdrew from the labor market. It follows that the outcome variable takes on the value zero with a positive probability (that is, there is a mass point at zero) but is continuously distributed over strictly positive values. A common practice in the displacement cost literature has been to restrict the analysis to a subset of observations with strictly positive earnings. This results in a selective sample of those who were able to return to work after displacement, and hence the estimated effects of displacement are potentially subject to the selection bias. This practice has been sometimes dictated by survey data that cover only the employed workers. Workers with zero earnings are typically observed in the register data but may still be excluded from the analysis due to econometric difficulties.

Angrist (2001) argues that, in the context of limited dependent variables, estimates obtained using positive outcomes only do not have a meaningful causal interpretation as treatment effects even if the data come from an ideal randomized experiment. How large the resulting selection bias is depends on the application. At least in our Finnish data, restricting the analysis to strictly positive observations would arguably lead to strongly biased results. Keeping zero observations in the analysis and applying least squares to the full sample is a problematic alternative because the conditional mean is unlikely to be linear in $\mathbf{x}$ and d. Despite the underlying non-linearity, that approach may give an acceptable approximation to the mean effect (see Angrist, 2001). Non-linear models for the conditional mean of limited dependent variables, like Tobit or sample selection models, could be used, but such models rely on strong parametric assumptions (about homoskedasticity, symmetry or functional forms) that may be difficult to justify in practice.

The displacement effect in (2) is also assumed to shift the *location* of the earnings distribution (possibly in an individual-specific way) without affecting other distributional aspects, such as dispersion, skewness or tail behavior. The ranking of workers in the earnings distribution (conditional on $\mathbf{X} = \mathbf{x}$) is, however, partly determined by past luck and success in the labor market. In the theoretical models of job search, employed workers are looking for better jobs and climb up the job ladder when a higher-paying job is found. When this time-consuming process is interrupted by involuntary job loss, the worker has to restart the job search from the bottom. Search theory suggests that the upper end of the distribution of Y_0 given $\mathbf{X} = \mathbf{x}$ is disproportionately populated by workers who have been lucky to find good jobs. In the case of job loss these workers are likely to experience larger earnings losses than their less lucky counterparts at the lower end of the distribution of Y_0 who would probably be employed in bad jobs also without a displacement. In other words, the effect of displacement increases with Y_0 given $\mathbf{X} = \mathbf{x}$. Here the displace-

[3] Displacement studies have usually exploited panel data. The estimating equations are somewhat more involved than (2) due to repeated observations on the same workers over time. With the panel data one can also allow the displacement status to be correlated with the error term by introducing fixed individual effects. But these panel data models are subject to the same pitfalls discussed in the text.

ment effect heterogeneity stems from random events that may be independent of workers' characteristics.

Alternatively, the ranking of observationally identical workers in the earnings distribution may reflect individual-specific characteristics not observed by the researcher. The upper end of the distribution of Y_0 (conditional on $\mathbf{X} = \mathbf{x}$) may be populated by high ability workers who are able to return to work quickly after a job loss at a wage rate close to their previous wage. In contrast, those at the lower end of the earnings distribution without displacement may be less able workers who would have trouble in finding work after displacement, and hence are subject to potentially large earnings losses due to long periods out of work if displaced. This kind of reasoning would suggest that the effect of displacement decreases with Y_0 given $\mathbf{X} = \mathbf{x}$.

In general, we do not have a reason to rule out heterogeneity in the displacement effect *a priori.* But this is what we would do if we estimated specifications like (2) by least squares methods. In the models for the conditional mean, it is also difficult to deal with observations with zero earnings without introducing the selection problem or imposing strong parametric assumptions. While these are technical problems, a substance matter is that the mean impact may not tell the whole story because changes in the shape of the earnings distribution are equally important. The quantile regression approach of Koenker and Batesse (1978) for distributional analysis provides a powerful alternative that overcomes these issues.

2.2 Distributional analysis

To define our displacement effect we follow the quantile treatment literature, going back to Lehmann (1974) and Doksum (1974). Let F_{Y_1} and F_{Y_0} be the cumulative distribution functions of Y_1 and Y_0, respectively. We define the *quantile displacement effect* (QDE) at the θ-th quantile as

$$\alpha_\theta = F_{Y_1}^{-1}(\theta) - F_{Y_0}^{-1}(\theta), \tag{3}$$

where $F_{Y_j}^{-1}(\theta) = \inf \left\{ y_j \left| F_{Y_j}(y_j) \geq \theta \right. \right\}$, $j = 0, 1$, for $\theta \in (0, 1)$. In other words, α_θ equals the horizontal distance between the distribution functions of potential earnings with and without displacement at given θ. A family of α_θ over θ captures heterogeneity in the displacement effect over the distribution of potential earnings. More precisely, what is captured is the difference between the two *marginal* distributions. For example, $\alpha_{0.5}$ describes the difference in the median earnings with and without displacement, not the effect of displacement on the earnings of a worker with median earnings in the absence of displacement. The difference in the marginal distributions is all we can identify from the observed data, without imposing strong additional restrictions. Nevertheless, the QDE estimates can be very informative, as they reveal whether job displacement reduces expected earnings (the distribution shifts left), increases uncertainty about the future earnings (dispersion increases), or has different effects at the lower and upper ends of the distribution.

In the absence of covariates, the natural and simple estimator of the QDE is obtained by replacing $F_{Y_1}^{-1}(\theta)$ and $F_{Y_0}^{-1}(\theta)$ with their empirical counterparts. This would require two *randomized* samples of individuals: those who were displaced in the reference period and those who were not. Then, for example, the difference in median earnings in the current period between the displaced and non-displacement groups would give an estimate of the QDE at $\theta = 0.5$. We do not believe that our sample design based on mass layoffs and plant closures is comparable to a randomized experiment, but we do assume independence of the displacement status conditional on the control variables. Since this assumption is crucial for causal interpretation of the estimated displacement effects, we shall provide (indirect) empirical evidence to support its validity in our application.

Koenker and Batesse (1978) introduced the quantile regression method for estimating conditional quantile functions. Powell (1986) developed an estimator for the conditional quantiles of limited dependent variables. We parameterize the conditional quantiles of the potential earnings as:

$$\begin{aligned} F_{Y_0}^{-1}(\theta\,|\mathbf{x}) &= \max\left\{0, \mathbf{x}'\boldsymbol{\beta}_\theta\right\}, \\ F_{Y_1}^{-1}(\theta\,|\mathbf{x}) &= \max\left\{0, \mathbf{x}'\boldsymbol{\beta}_\theta + \alpha_\theta\right\}, \end{aligned} \tag{4}$$

where the limited support of the earnings distributions is explicitly accounted for. Provided that D is independent of (Y_1, Y_0) given $\mathbf{X} = \mathbf{x}$, the conditional quantile function for observed earnings can be written as

$$F_Y^{-1}(\theta\,|\mathbf{x}, d) = \max\left\{0, \mathbf{x}'\boldsymbol{\beta}_\theta + \alpha_\theta d\right\}. \tag{5}$$

This type of models can be applied to corner solution data or to censored data with the fixed censoring point. In our application the issue is *not* data observability: the earnings are observed for all workers but they are zero for those who did not work in the period in question. That is, the outcome variable is not censored but has a mass point at zero, being continuously distributed over strictly positive values. Wooldridge (2002) calls models for such outcome variables *corner solution* models. These are statistically identical to models for censored data, but are conceptually very different, which should be kept in mind when interpreting the results. Under the corner-solution interpretation, the conditional θ-th quantile of Y is zero for $\mathbf{x}'\boldsymbol{\beta}_\theta + \alpha_\theta d \leq 0$, while it is strictly positive and linear in $\mathbf{x}$ and d for $\mathbf{x}'\boldsymbol{\beta}_\theta + \alpha_\theta d > 0$.[4] In the former case the worker is predicted to be out of work – and hence has zero earnings – with a probability no less than θ.[5]

[4] It should be stressed that we view that the limited support of the dependent variable is a technical problem. The mass point in the earnings data at zero implies that the linearity assumption of the conditional quantile function is not valid in the left tail of the distribution, whereas the consequences for higher regression quantiles are negligible. In our application, the standard quantile regression method, which ignores the limited support of the dependent variable, would lead to highly similar estimates to the corner-solution approach except for the first decile, which is the lowest quantile analyzed.

[5] In the context of censored data, $\mathbf{x}'\boldsymbol{\beta}_\theta + \alpha_\theta d < 0$ would imply that the conditional θ-th quantile of the *latent* outcome variable is negative, which is a very different interpretation of the same finding.

Since Powell's (1986) estimator does not require additional parametric assumptions, we can recover the effect of displacement over the conditional distribution in a robust way despite the limited support of the outcome variable. This is an important advantage over the models for the conditional mean. In summary, we apply the same identifying assumption – the conditional independence of D – that has been commonly assumed in linear specifications like (2), but we do *not* rule out heterogeneity in the effects of $\mathbf{X}$ and D (which would be a very strong assumption), *nor* do we exclude observations with zero earnings from our analysis (which would lead to the selection bias).[6]

3 Data and sample construction

Our data come from the Finnish Longitudinal Employer-Employee Database of Statistics Finland. This database combines information from several administrative registers for all working age persons with a permanent residence in Finland. It includes detailed information on employment and earnings history along with a number of background characteristics, like education, marital status and age. Since the data include all people, not just those who are currently employed, we can follow individuals irrespective of their labor market state, provided they have not emigrated or died. This is important because job loss may be followed by periods of unemployment and non-participation. For example, a worker who loses his or her job in a sunset industry may withdraw from the labor force temporarily in order to acquire new skills required by jobs in other industries. Thus, if all costs of job loss are to be included, we should not exclude periods out of work from our analysis. The database also includes unique identification codes for all plants (and firms) operating in Finland. This information allows us to detect all employees of a given plant at the end of any given year, as well as to identify plants that were downsizing or exiting the market in a given year.

Starting from Ruhm (1991) and Jacobson *et al.* (1993), practically all of the recent studies on the cost of job loss have employed a methodology that involves a comparison of displaced workers with a control group that did not experience displacement during a given reference period. Displacements are typically defined as permanent and involuntary separations caused by an employer-specific shock, not related to the worker's job performance. In practice, it is not possible to distinguish directly between layoffs and quits on one hand, and between employer-specific and individual-specific reasons for separations on the other. A common solution in empirical work has been to interpret separations associated with a mass layoff or a plant/firm closure to be displacements. The underlying assumption is that such separations are driven by employer-specific shocks, and hence exogenous from the worker's standpoint.

Obviously, this strategy is not completely accurate. First, displacements defined in

[6] Carneiro and Portugal (2006) also apply a quantile regression model to study earnings losses in Portugal but they exclude observations with zero earnings from their analysis.

this way may also include some voluntary quits. Second, the employer has an incentive to get rid of the least productive workers in the first place, although the seniority rules and unions may prevent the employer from choosing freely the group to be laid off. This suggests that workers who are displaced in a mass layoff are probably not a completely random group. Therefore some researchers prefer the use of plant (or firm) closures to mass layoffs. A counter-argument is that those plants that closed down are a more selective group of all plants (for example, they are much smaller on average) than downsizing plants are, suggesting that their employees may also be a rather selective group. In the absence of a superior solution, we include both groups in our analysis, but estimate distinct displacement effects for those who lost their jobs in mass layoffs and for those who were displaced due to plant closures.

Finally, the plant closure or mass layoff is likely to be expected by employees, and thereby some of them may quit earlier in anticipation of the forthcoming reduction in the workforce. If workers with better outside options are more likely to leave early, those who are displaced in the year of the mass layoff or plant closure form a selective group. On the other hand, the downsizing process can be longer than one year, so that the employer may have laid off some workers well before the period of the mass layoff or plant closure. This suggests that some of the early leavers may be low productivity employees. One could classify the early leavers as displaced workers, but then more voluntary quits and more selective dismissals would be included as well. Therefore, we instead include workers who left their jobs a year before a mass layoff or plant closure as a separate group in our analysis.

We construct two separate samples using 1992 and 1997 as base years when the event of displacement possibly took place. We focus on workers who all have initially a fairly strong labor market attachment, and thereby require that everyone included in the sample has at least three years of tenure with the same private sector employer before the base year. We also require that during these three years everyone included in the sample had exactly one employer and did not have any unemployment spells. The employers are identified using plant codes and we only include workers from plants that employ at least ten workers at the end of the year preceding the base year.[7] Furthermore, we require that all workers in the sample were 21 to 52 years old in the base year,[8] and were not self-employed at any point in the observation period.[9]

[7] We consider plants, not firms, as production units that are subject to a risk of downsizing and closure. In doing so, we avoid problems with artificial firm closures that result from changes in the firm identifiers due to mergers or dispersals and changes in ownership or industry classification. The plant codes do not suffer from the same problems, as the plant is defined as a local kind-of-activity unit in the underlying register data.

[8] By excluding individuals over 52 years of age, we rule out the possibility of early retirement via the unemployment tunnel scheme that consists of extended unemployment benefits and a particular unemployment pension scheme for the older long-term unemployed. The older unemployed entitled to this scheme are a very distinctive group, as roughly half of them have been estimated to have effectively withdrawn from job search and to be waiting passively for early retirement (Kyyrä and Ollikainen, 2008).

[9] The dependent variable in our analysis is labor income (earnings). In addition to labor income, the

For both base years, we identify a group of displaced workers (the *displacement* group) as well as a group of workers who were not displaced at that time (the *control* group). The control group includes employees who did not separate from their employer during the base year, that is, had the same plant code at the end of the base year as they had a year earlier. The displacement group consists of two subgroups: those who lost their jobs in mass layoffs and those who were displaced due to plant closures. The former subgroup includes all workers who separated during the base year from plants from which at least 50% but not all of their employees left by the end of the base year.[10] The latter subgroup consists of separating workers whose plant disappeared entirely (in terms of employment) by the end of the base year. From the displacement group we exclude workers who return to the pre-displacement plant at some later period (cf. our definition of displacement). We also include a group of workers who, during the base year, left their jobs in plants that downsized or closed down in the *next* year (the *early-leaver* group). These workers are analyzed separately and the results for this group are used as a robustness check. To summarize, we have defined the following groups for the base year t (1992 or 1997):

- **Control group:** Workers who did not change their employer during year t.
- **Displacement group:** Workers separating in year t from plants that closed down during year t (*plant-closure* subgroup) and from plants that reduced workforce at least by 50% between years $t-1$ and t but were still in operation at the end of year t (*downsizing* subgroup).
- **Early-leaver group:** Workers separating in year t from plants that closed down during year $t+1$ (*plant-closure* subgroup) and from plants that reduced their workforce by at least 50% between years t and $t+1$ but were still in operation at the end of year $t+1$ (*downsizing* subgroup).

These groups are followed over an 11-year period beginning three years before and ending seven years after the base year: from 1989 until 1999 and from 1994 until 2004, respectively. This results in two large unbalanced panel data sets. The 1992 sample has 2,471,751 observations (225,919 individuals) and the 1997 sample 2,872,552 observations (262,487 individuals).[11]

We allow separations in the control group after the base year, implying that workers in the control group may be displaced at later dates. In this respect we follow Huttunen *et al.* (2006), Hijzen *et al.* (2006), and Eliason and Storrie (2006). Some other studies require that individuals in the control group remain employed (possibly in the same firm) over the

self-employed typically have other significant sources of income, which may be hidden in the balance sheet of their firm.

[10] We discuss the robustness of our results with respect to this threshold value in section 5.3.

[11] Persons disappear from our data only if they die or move abroad. In the 1992 sample attrition is 3,608 persons from 1992 to 1999 and in the 1997 sample 3,706 persons from 1997 to 2004. There is no selection pattern according to the displacement status.

whole observation period. Of course, the members of the displacement and early-leaver groups can experience additional job losses in the later periods. Subsequent job losses can significantly increase the costs from the initial job loss (Stevens, 1997), whereas the likelihood of multiple job losses may be much higher during economic downturns (Eliason and Storrie, 2006).

4 Descriptive evidence

4.1 Background characteristics

Tables 1 and 2 present summary statistics by group status and sex. Most of the variables are measured a year before the base period, but the earnings variables and plant and firm sizes are tracked for three pre-displacement periods. The outcome variable in our analysis is annual earning, covering all salaries and wages received during a year. In section 5.3, we also discuss the results obtained using annual income as the outcome variable.

By and large, the displacement and control groups are similar in terms of age, education, and family background. With the exception of men displaced in 1997 from downsizing plants, the members of the displacement groups have slightly shorter job tenures compared to the control group. The earnings percentile in the plant describes the worker's relative position in the earnings hierarchy within the employing plant. Since the average value of this measure is rather similar for displaced and control workers, there is no evidence of selective displacements.

As expected, the relative share of displaced workers is considerably higher in the 1992 sample compared to the 1997 sample: 2.6% vs. 0.9% (4.7% vs. 1.2% if the groups of early leavers are included). The share of women seems to slightly lower among workers displaced in 1992 and conversely higher in 1997 (33% vs. 43%), which is due to an exceptionally high layoff rate in the male-dominated construction sector in 1992. In the 1997 sample a disproportionate number of workers who lost their jobs in plant closures worked in trade (including also hotels and restaurants) and in business services, which are industries characterized by a high share of small business units. Not surprisingly, the average plant and firm sizes are smallest for the plant-closure subgroup.

There are only moderate differences between the early-leaver and displacement groups. With a few exceptions, the early-leavers have more young children and have shorter job tenures on average. Differences in annual earnings are rather small, but the early leavers seem to be located at lower levels in the plant-specific earnings distributions, which may indicate that they did not quit but were dismissed. The average plant and firm sizes as well as the industry allocation are also quite different for the early leavers and displaced workers.

Table 1: Sample statistics for the 1992 sample

	Men					Women				
	Control	Displaced		Early-leavers		Control	Displaced		Early-leavers	
		d.s.	p.c.	d.s.	p.c.		d.s.	p.c.	d.s.	p.c.
Age	37.67	37.61	37.59	36.81	37.40	38.04	37.66	37.37	35.58	37.47
Yrs of education	11.36	11.08	11.03	11.71	10.88	11.06	10.87	10.88	11.13	10.76
Tenure (years)	11.30	9.06	8.72	7.27	7.97	11.25	10.79	9.78	8.30	8.74
Married (share)	0.66	0.63	0.65	0.60	0.61	0.61	0.58	0.61	0.59	0.58
Children under 7	0.40	0.37	0.42	0.42	0.42	0.23	0.27	0.29	0.48	0.30
Annual earnings:										
1989	22,814	21,106	21,551	22,213	21,025	15,811	14,869	15,062	14,423	15,197
1990	25,111	23,163	23,878	24,256	23,223	17,300	16,270	16,419	15,775	16,700
1991	26,169	23,727	24,244	24,701	23,191	18,424	16,707	16,478	15,251	16,743
Percentile:										
1989	64	62	64	59	62	45	41	45	35	43
1990	65	62	64	58	62	45	41	46	35	43
1991	65	61	63	56	60	45	38	42	30	40
Plant size:										
1989	467	145	65	294	73	364	188	49	286	64
1990	448	125	58	302	67	354	154	46	279	61
1991	436	111	41	266	50	339	140	39	271	45
Firm size:										
1989	2134	1259	645	973	751	2066	1588	381	1311	518
1990	1945	1178	523	954	527	1978	1650	324	1399	442
1991	1739	901	349	768	389	1757	1228	265	1012	334
Industry (share):										
Manufacturing	0.48	0.38	0.42	0.23	0.39	0.31	0.36	0.38	0.26	0.32
Construction	0.07	0.34	0.23	0.40	0.32	0.01	0.07	0.06	0.07	0.08
Trade	0.14	0.14	0.21	0.15	0.13	0.23	0.29	0.28	0.31	0.30
Transport	0.11	0.03	0.04	0.02	0.05	0.06	0.04	0.04	0.01	0.05
Busin. serv.	0.10	0.08	0.08	0.14	0.07	0.25	0.21	0.22	0.23	0.18
Other	0.11	0.02	0.02	0.06	0.03	0.14	0.03	0.03	0.11	0.06
N	125,267	2175	1790	639	2728	89,961	1058	908	361	1032

Notes: Unless otherwise indicated, the numbers are for 1991. d.s. = downsizing subgroup. p.c. = plant-closure subgroup. Percentile = Earnings percentile within the plant. Trade also includes hotels and restaurants. Transport also includes telecommunications.

4.2 Average months of employment

Figure 1 shows the average number of the months of work in each year for the control group and displacement groups. These groups are divided into the first quartile, inter-quartile range and fourth quartile according to their annual earnings in 1989 (the 1992 sample) or 1994 (the 1997 sample). To be included in the analysis the worker had to be employed with the same employer, without unemployment periods, at least three years before the base year, and thereby the employment levels are fixed at 12 months for the first years. This also explains a declining trend in the average employment months starting from the base period for the control group. The incidence of non-employment varies greatly with the earnings level. Displaced workers at the lower end of the pre-displacement earnings distribution are likely to be several months out of work, whereas those who were located at the upper end typically found jobs more quickly after being displaced. A similar pattern is also observed for the control groups, although the differences between the income categories of control group members are smaller than the differences within the displacement groups.

Table 2: Sample statistics for the 1997 sample

	Men					Women				
	Control	Displaced		Early-leavers		Control	Displaced		Early-leavers	
		d.s.	p.c.	d.s.	p.c.		d.s.	p.c.	d.s.	p.c.
Age	38.68	39.09	37.73	36.29	38.57	39.48	38.88	37.96	36.19	37.90
Yrs of education	11.40	11.30	11.63	11.71	11.12	11.29	11.45	11.25	11.70	11.07
Tenure (years)	12.52	12.63	8.52	8.54	12.20	12.06	11.21	9.01	9.07	10.27
Married (share)	0.61	0.60	0.57	0.54	0.57	0.57	0.56	0.52	0.55	0.55
Children under 7	0.42	0.38	0.50	0.46	0.37	0.24	0.28	0.23	0.46	0.33
Annual earnings:										
1994	25,881	26,817	25,289	24,576	24,377	19,091	18,655	17,602	18,481	17,779
1995	27,943	28,820	26,806	27,178	26,200	20,391	20,161	18,760	20,474	18,759
1996	29,240	30,295	27,497	28,931	28,041	21,403	20,774	19,714	19,738	19,770
Percentile:										
1994	61	58	67	56	54	42	40	45	33	44
1995	62	59	66	57	55	44	42	49	36	45
1996	63	60	66	60	56	45	42	49	32	45
Plant size:										
1994	370	371	43	261	134	304	346	44	435	99
1995	379	377	42	274	197	315	313	46	421	228
1996	399	533	36	276	124	329	363	40	442	134
Firm size:										
1994	1778	2370	898	797	894	1705	1541	743	1623	1041
1995	1813	2224	900	813	1194	1878	1507	761	1729	1697
1996	2160	1778	743	907	1232	1980	1351	749	1710	1704
Industry (share):										
Manufacturing	0.57	0.65	0.19	0.25	0.44	0.37	0.40	0.07	0.22	0.36
Construction	0.04	0.02	0.08	0.11	0.05	0.01	0.00	0.01	0.05	0.00
Trade	0.12	0.09	0.33	0.24	0.22	0.21	0.17	0.45	0.18	0.26
Transport	0.11	0.08	0.11	0.04	0.12	0.08	0.05	0.10	0.06	0.05
Busin. Serv.	0.09	0.09	0.19	0.29	0.17	0.19	0.16	0.30	0.45	0.30
Other	0.08	0.06	0.10	0.06	0.01	0.13	0.21	0.06	0.04	0.02
N	162,484	1075	336	112	327	96,724	820	248	108	253

Notes: Unless otherwise indicated, the numbers are for 1996. d.s. = downsizing subgroup. p.c. = plant-closure subgroup. Percentile = Earnings percentile within the plant. Trade also includes hotels and restaurants. Transport also includes telecommunications.

The graphs quite aptly demonstrate how different were the labor market conditions that the displaced workers in the two samples had to face. In the 1992 sample the average employment months of low and mid-income displaced workers dropped 5–6 months in the year following displacement and then recovered quite quickly. However, in 1999, seven years later, their average employment months were still about 2 months less than in the control group. The high-income displaced workers in 1992 fare somewhat better and over the seven years following displacement get closer to the level of the comparison group. The workers displaced during 1997 also have a distinct drop in their employment but it is nowhere close to the drastic effect in the earlier group. It also seems that the two distinct displacement groups are more homogeneous in the depression period, as their employment histories are more similar in the 1992 sample than in the later sample. In particular, the first earnings quartile of workers displaced in mass layoffs in the 1997 sample suffered from relatively moderate employment losses compared to other displaced workers in that period.

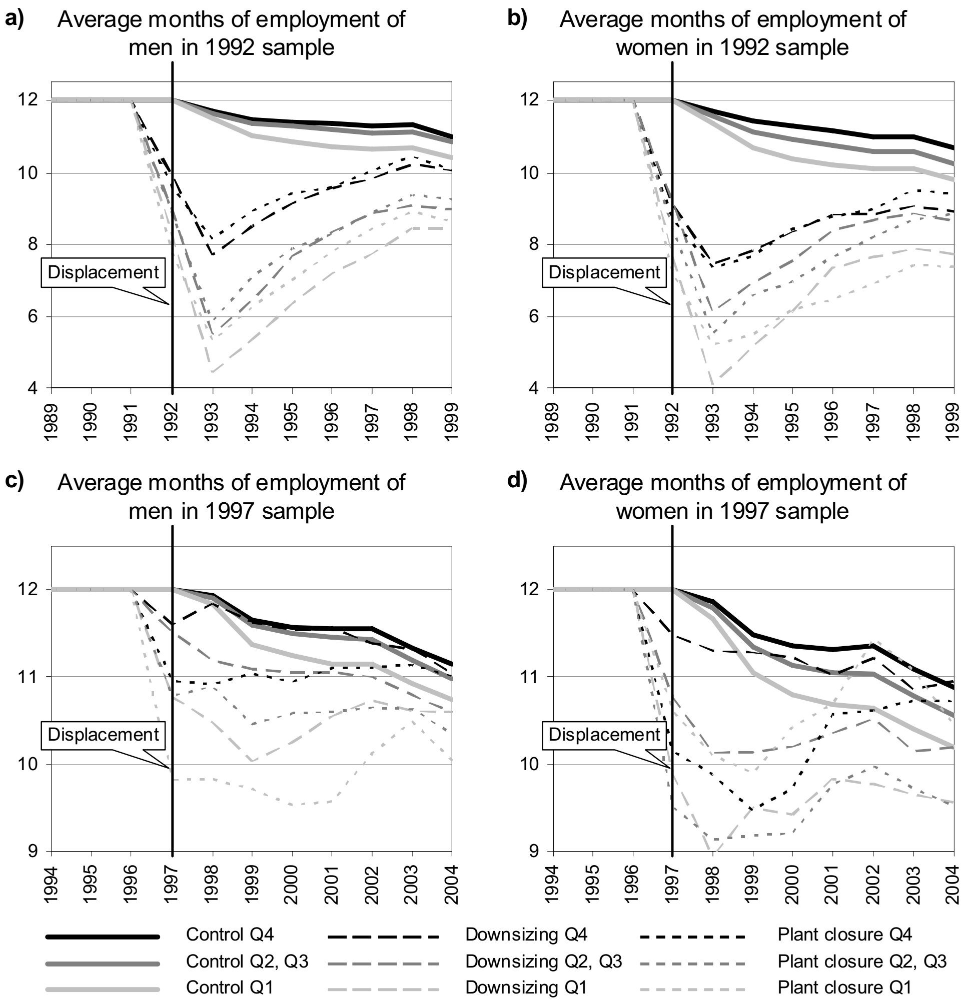

Figure 1: Average months of employment by the earnings level in 1989 (1992 sample) or 1994 (1997 sample) for control and displacement groups. *Note:* Q = 1st earnings quartile, Q2, Q3 = interquartile range, and Q4= 4th quartile.

There appears to be some gender differences as well. The employment levels of the female control groups drop generally more over time, which may be driven by a higher degree of both part-time and temporary work among women. Moreover, in the 1992 sample, women's employment history following displacement looks almost the same as for men, but women displaced in 1997 suffered from larger employment losses than displaced men.

4.3 Empirical earnings distributions

Figure 2 shows the evolution of the 1st decile, median, and 9th decile of the annual earnings for the control and displacement groups. The earnings dispersion, as measured by the ratio of the 9th to 1st decile, increases quite strongly over time in the control group in all graphs.

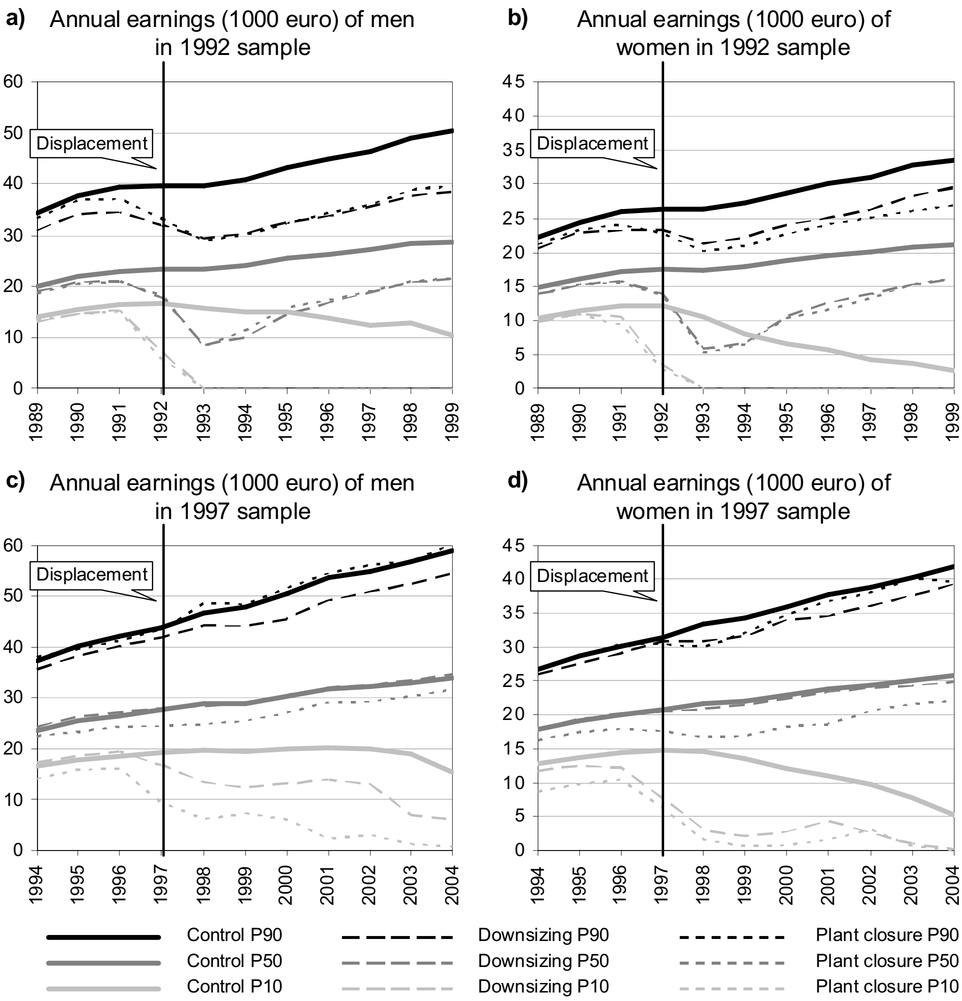

Figure 2: The 1st decile, median, and 9th decile of (nominal) annual earnings for control and displacement groups

This trend is mainly driven by increasing variation in the annual working time within the control group (see figure 1). The declining pattern of the 1st decile, in particular, is due to an increasing fraction of control workers who leave full-time employment.

In the 1992 sample, the dip in earnings after displacement is very pronounced and clear at all parts of the distribution. This is largely due to non-employment (see figure 1), and in smaller part a result of lower wages in the post-displacement jobs. Median earnings declined from 1991 to 1993 by some 60% among displaced women and men. While the 9th decile declines less, the 1st decile drops all the way to zero and remains there until 1999 for the 1992 displacement groups. Thus a large share of displaced workers were out of work in each post-displacement period. By and large, the patterns of the earnings distributions of the two distinct displacement groups in the 1992 sample are very similar. The only notable difference is the higher 9th decile of male job losers in the plant-closure group over the pre-displacement years. Also, the overall picture is very similar between women and men who were displaced in 1992.

The earnings quantiles of the displacement groups in the 1997 sample exhibit much smaller declines compared to the 1992 sample. There is basically no difference in the median earnings between those displaced in mass layoffs and the control group. Workers displaced from plants exiting in 1997 have lower median earnings in all periods, and the difference increases somewhat after the displacement. Interestingly, the lower and upper tails of the distributions of the two displacement groups evolve somewhat differently over the post-displacement period. The 9th decile of the job losers of downsizing plants drops only a little, while the 1st decile declines more clearly compared to the control group. In the plant-closure group, the 9th decile of the distribution follows very closely that of the control group, but the 1st decile of men declines sharply compared to the other displacement group. By contrast, there are no notable differences in the behavior of the lower tail of the earnings distribution between women in the two displacement groups. As a result, displacement led to the largest increase in the earnings dispersion for male job losers in the plant-closure group.

Unlike in the 1992 sample, the earnings distribution of the plant-closure group in the 1997 sample differs from those of the other groups already in the pre-displacement periods. This suggests a possibility that the plants exiting the market in the recovery period (and, hence, their employees) form a rather selective group. There may also be more early leavers in the 1997 sample because the chances of finding work in other plants were much better and because the economic environment was more predictable at the end of the 1990s. By conditioning on the control variables, we can eliminate these differences for men but not for women (see our quantile regression results below). Therefore the female plant-closure group in the 1997 sample remains a problematic group due to a potential selection problem.

5 Quantile displacement effects

The descriptive analysis suggests that displacement had a notable and long-lasting negative effect on the earnings distribution. This effect seems clearly heterogeneous, as the ratio of the earnings quantiles between the displacement and control group were found to change over time. We also recovered some differences in the background characteristics between the groups. Using the quantile regression method, we can model heterogeneity in the displacement effect while controlling for differences in the background characteristics.

Our specification for the conditional earnings quantiles differs slightly from the stylized example in section 2.2. First, we model relative effects by taking the log of the strictly positive values of annual earnings (but we do not drop zero earnings). Because of the equivariance of the quantiles to monotone transformations, this transformation of the dependent variable is completely transparent. Second, we have four groups of separating workers to be compared to the control group. These are indicated by the following dummy variables: d_{C_t} equals 1 for workers displaced in year t from plants that closed down in that year (plant-closure displacement group), d_{D_t} equals 1 for workers displaced in year t from downsizing plants (downsizing displacement group), e_{C_t} equals 1 for those who left their jobs in year t in plants that closed down in year $t+1$ (plant-closure early-leaver group), and e_{D_t} equals 1 for those who left their jobs in year t in plants that downsized in year $t+1$ (downsizing early-leaver group). The control group – those who did not change the plant during year t – serves as the reference group in the analysis. Thus, our model for the conditional θ-th quantile of the earnings in year s is

$$F_{Y_s^*}^{-1}(\theta\,|\mathbf{z}_s) = \max\left\{0, \mathbf{x}_s'\boldsymbol{\beta}_{\theta s} + \alpha_{\theta s}^C d_{C_t} + \alpha_{\theta s}^D d_{D_t} + \eta_{\theta s}^C e_{C_t} + \eta_{\theta s}^D e_{D_t}\right\}, \tag{6}$$

where Y_s^* is log annual earnings for strictly positive earnings and zero otherwise, $t \in \{1992, 1997\}$ is the base period, $s \in \{t-3, t-2, ..., t+7\}$, $\theta \in \{.1, .2, ..., .9\}$, and $\mathbf{z}_s = (\mathbf{x}_s, d_{C_t}, d_{D_t}, e_{C_t}, e_{D_t})$. The vector of control variables $\mathbf{x}_s$ includes age, age squared, pre-displacement tenure, education level (5 levels), place of residence (5 regions), marital status, indicator of children under the schooling age, the log annual earnings in year $t-4$, and the size category (4 classes) and industry (6 main industries) of the firm in year $t-1$. The past earnings are included to control for the effect of unobserved characteristics.

By taking the exponent of the right-hand side of (6), provided it is not zero, we obtain the conditional θ-th quantile of the annual earnings in year s. Coefficients of the group dummies capture proportional differences compared to the non-displacement case. For example, provided that $\mathbf{x}_s'\boldsymbol{\beta}_{\theta s} > 0$ and $\mathbf{x}_s'\boldsymbol{\beta}_{\theta s} + \alpha_{\theta s}^C > 0$, $\exp\left(\alpha_{\theta s}^C\right)$ gives the ratio of the θ-th quantile in year s if displaced in year t due to a plant closure to the θ-th quantile without displacement in year t. This proportional effect is independent of the values of control variables. If $\mathbf{x}_s'\boldsymbol{\beta}_{\theta s} > 0$ and $\mathbf{x}_s'\boldsymbol{\beta}_{\theta s} + \alpha_{\theta s}^C < 0$, the conditional θ-th quantile of annual earnings is zero with displacement but strictly positive without displacement, and thereby the ratio of the quantiles with and without displacement is zero, not $\exp\left(\alpha_{\theta s}^C\right)$. That is, the proportional effect interpretation does not apply to arbitrarily values of control variables. This is a relevant concern when $\exp\left(\alpha_{\theta s}^C\right)$ is close to zero, which is the case with some lowest deciles in our application below. In those cases, the θ-th quantile of annual earnings of some people is predicted to drop to zero after displacement, which should be kept in mind when interpreting our results below.[12]

Using Powell's (1986) method,[13] we have estimated the model (6) separately for women and men in the two samples. All regression parameters of each 9 deciles were allowed to vary freely across the 11 cross sections. This amounted to a total of 396 distinct quantile regressions. The point estimates of $\alpha_{\theta s}^C$ and $\alpha_{\theta s}^D$ are reported in tables 3 and 4, whereas the time patterns of $\exp\left(\alpha_{\theta s}^C\right)$ and $\exp\left(\alpha_{\theta s}^D\right)$ are shown in figures 3 and 4. Each curve in

[12] If $\mathbf{x}_s'\boldsymbol{\beta}_{\theta s} < 0$ and $\mathbf{x}_s'\boldsymbol{\beta}_{\theta s} + \alpha_{\theta s}^C < 0$, the displacement has no effect at all because the conditional θ-th quantile is zero in any case. This is not a very relevant case in our application.

[13] For details see Jolliffe *et al.* (2002).

the graph shows how the proportional displacement effect at a particular decile evolves over time. In section 5.3 we also discuss briefly the results for the early leavers; that is, the estimates of $\eta^C_{\theta s}$ and $\eta^D_{\theta s}$.

5.1 Pre-displacement effects

Under the assumption that the displacement status is exogenous, we can interpret differences in the conditional earnings distributions between the control and displacement groups as the causal effect of displacement. This conditional independence assumption also implies that, given the control variables, there should be no notable earnings differences between the groups in the periods when the displacement group was not yet affected. Although the earnings of the displacement group may have been affected some periods before the actual displacement took place, the earnings differences between the groups should disappear at some point when we go further back in time. If that does not happen, we take it as evidence against the validity of the conditional independence assumption.

As seen in table 3, the earnings distributions of workers who were displaced in 1992 due to plant downsizing or closure are very similar to that of the control group three years before the displacement period. Only one displacement dummy out of 18 for men in table 3 gets a significantly (at the 5% risk level) non-zero coefficient in 1989. Namely, the 1st decile of the earnings distribution of men losing their jobs due to plant closure in 1992 is estimated to be 0.7% lower than that of the control group. In table 3 there are five statistically significant coefficients for women in 1989. Except for the highest decile for women in the plant-closure group, all significant effects imply less than 1% difference in the deciles compared to the control group. These very small discrepancies between the female groups may result from sample noise, because only one coefficient differs significantly from zero a year later in 1990 (the 4th decile in panel C in table 3). It should be stressed that at the 1% risk level, which might be a more reasonable choice given our sample sizes, only one coefficient out of 36 differs significantly from zero in 1989 in table 3.[14] In other words, the earnings distributions of the displacement and control groups for both sexes are virtually identical four years before the base period, suggesting that the conditional independence assumption holds in the 1992 sample.

There are some statistically significant differences for men in 1990, but these are very modest in absolute value. For both women and men we find a decline in the earnings distributions of the displacement groups in 1991. At the left tail of women's distribution, the difference compared to the control group exceeds 5%, but elsewhere the differences are still only around 2%. This implies a possibility that the annual working hours of female employees with relatively low earnings have a tendency to decline prior to job loss. Many of these workers are probably part-timers whose working time and, hence, earnings vary with firm-specific business conditions. This could explain a stronger effect for women who

[14] Because of the large number of point estimates, we are expected to recover some spuriously significant effects even if there were no true effects.

Table 3: Quantile displacement effects for 1992

θ	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999
A. Men displaced men from a downsizing plant, $\alpha^D_{\theta s}$											
.1	-0.006	**-0.014**	**-0.024**	**-0.851**	**-6.815**	**-6.806**	**-6.914**	**-6.100**	**-6.039**	**-5.851**	**-6.197**
.2	-0.001	**-0.010**	**-0.025**	**-0.500**	**-5.179**	**-3.404**	**-2.524**	**-1.896**	**-1.686**	**-1.192**	**-1.153**
.3	-0.001	**-0.008**	**-0.025**	**-0.330**	**-2.260**	**-1.723**	**-1.261**	**-0.964**	**-0.660**	**-0.511**	**-0.440**
.4	0.000	-0.005	**-0.023**	**-0.215**	**-1.410**	**-1.147**	**-0.719**	**-0.506**	**-0.359**	**-0.272**	**-0.229**
.5	-0.002	**-0.008**	**-0.020**	**-0.166**	**-0.834**	**-0.694**	**-0.402**	**-0.305**	**-0.227**	**-0.180**	**-0.165**
.6	-0.004	-0.005	**-0.022**	**-0.132**	**-0.479**	**-0.377**	**-0.272**	**-0.219**	**-0.173**	**-0.141**	**-0.132**
.7	-0.003	-0.006	**-0.020**	**-0.114**	**-0.283**	**-0.252**	**-0.203**	**-0.171**	**-0.147**	**-0.120**	**-0.113**
.8	-0.003	-0.010	**-0.020**	**-0.096**	**-0.225**	**-0.195**	**-0.161**	**-0.137**	**-0.126**	**-0.102**	**-0.099**
.9	-0.008	-0.011	**-0.027**	**-0.087**	**-0.171**	**-0.160**	**-0.124**	**-0.107**	**-0.097**	**-0.087**	**-0.091**
B. Men displaced due to plant closure, $\alpha^C_{\theta s}$											
.1	**-0.007**	**-0.017**	**-0.028**	**-1.059**	**-7.622**	**-6.358**	**-5.773**	**-7.372**	**-8.671**	**-5.732**	**-5.778**
.2	-0.004	**-0.011**	**-0.015**	**-0.586**	**-7.252**	**-3.251**	**-3.093**	**-2.472**	**-1.468**	**-1.353**	**-1.154**
.3	-0.005	-0.008	**-0.015**	**-0.317**	**-2.437**	**-1.518**	**-1.278**	**-1.083**	**-0.690**	**-0.461**	**-0.460**
.4	-0.005	**-0.008**	**-0.014**	**-0.211**	**-1.528**	**-0.996**	**-0.660**	**-0.510**	**-0.349**	**-0.240**	**-0.227**
.5	-0.001	-0.002	-0.009	**-0.147**	**-0.916**	**-0.591**	**-0.349**	**-0.283**	**-0.214**	**-0.170**	**-0.173**
.6	0.000	0.004	**-0.014**	**-0.120**	**-0.422**	**-0.317**	**-0.228**	**-0.196**	**-0.155**	**-0.139**	**-0.141**
.7	0.002	0.002	**-0.014**	**-0.095**	**-0.250**	**-0.228**	**-0.177**	**-0.161**	**-0.138**	**-0.131**	**-0.133**
.8	0.003	0.008	-0.016	**-0.078**	**-0.210**	**-0.180**	**-0.150**	**-0.143**	**-0.131**	**-0.115**	**-0.126**
.9	0.011	**0.020**	0.005	**-0.077**	**-0.186**	**-0.148**	**-0.143**	**-0.124**	**-0.129**	**-0.096**	**-0.104**
C. Women displaced men from a downsizing plant, $\alpha^D_{\theta s}$											
.1	-0.002	-0.006	**-0.057**	**-1.056**	**-6.277**	**-5.641**	**-10.165**	**-5.582**	**-6.203**	**-5.253**	-2.976
.2	-0.006	-0.005	**-0.033**	**-0.651**	**-6.690**	**-4.613**	**-3.019**	**-1.989**	**-2.076**	-2.064	**-2.245**
.3	**-0.008**	-0.004	**-0.030**	**-0.428**	**-2.563**	**-1.825**	**-1.386**	**-0.969**	**-0.932**	**-0.915**	**-0.806**
.4	**-0.008**	**-0.009**	**-0.024**	**-0.245**	**-1.665**	**-1.264**	**-0.895**	**-0.647**	**-0.545**	**-0.432**	**-0.419**
.5	**-0.009**	-0.009	**-0.022**	**-0.153**	**-1.017**	**-0.882**	**-0.478**	**-0.353**	**-0.281**	**-0.248**	**-0.247**
.6	**-0.010**	-0.012	**-0.024**	**-0.106**	**-0.475**	**-0.459**	**-0.290**	**-0.228**	**-0.194**	**-0.169**	**-0.151**
.7	-0.009	-0.012	**-0.029**	**-0.090**	**-0.228**	**-0.227**	**-0.195**	**-0.167**	**-0.156**	**-0.128**	**-0.104**
.8	-0.009	-0.012	**-0.032**	**-0.068**	**-0.154**	**-0.173**	**-0.138**	**-0.133**	**-0.112**	**-0.107**	**-0.081**
.9	0.008	0.003	-0.022	**-0.046**	**-0.093**	**-0.110**	**-0.115**	**-0.085**	**-0.061**	-0.036	-0.031
D. Women displaced due to plant closure, $\alpha^C_{\theta s}$											
.1	-0.004	-0.011	**-0.084**	**-1.282**	**-7.518**	**-5.599**	**-4.873**	**-4.820**	**-8.401**	-3.239	**-4.000**
.2	0.002	-0.004	**-0.059**	**-0.837**	**-7.544**	**-3.309**	**-3.151**	**-2.826**	**-2.174**	**-1.997**	**-1.396**
.3	0.003	-0.003	**-0.041**	**-0.450**	**-2.538**	**-1.764**	**-1.459**	**-1.237**	**-0.996**	**-0.923**	**-0.789**
.4	0.002	-0.005	**-0.035**	**-0.275**	**-1.587**	**-1.242**	**-0.919**	**-0.859**	**-0.655**	**-0.391**	**-0.337**
.5	0.000	-0.007	**-0.022**	**-0.188**	**-1.118**	**-0.865**	**-0.517**	**-0.448**	**-0.327**	**-0.253**	**-0.203**
.6	-0.003	0.000	**-0.025**	**-0.137**	**-0.568**	**-0.510**	**-0.281**	**-0.236**	**-0.219**	**-0.175**	**-0.161**
.7	0.002	0.012	**-0.024**	**-0.106**	**-0.258**	**-0.233**	**-0.183**	**-0.162**	**-0.158**	**-0.127**	**-0.125**
.8	0.007	0.011	**-0.025**	**-0.106**	**-0.191**	**-0.180**	**-0.146**	**-0.141**	**-0.135**	**-0.126**	**-0.121**
.9	**0.037**	0.018	0.003	-0.043	**-0.158**	**-0.139**	**-0.137**	**-0.129**	**-0.108**	**-0.112**	**-0.117**

Notes: Significantly (95%-confidence level) non-zero coefficients in **bold**. Statistical inference based on the standard errors boostrapped using 100 replications.

are more likely to be employed on a part-time basis.

Overall, the estimates for the pre-displacement periods show very modest earnings differences between the displacement and control groups in the 1992 sample. One might expect these small pre-displacement effects to be attributable to an unexpected nature of the 1990s depression. But this explanation does not sound very convincing because the pre-displacement effects appear to be rather small, or even smaller, in the 1997 sample (see figure 4 and table 4). In the 1997 sample the earnings distributions of the displacement and control groups in 1994 are equally similar for men as they were in 1989 in the 1992 sample. In particular, none of the effects for men in 1994 differs statistically significantly from zero in table 4, which is consistent with the conditional independence assumption. Surprisingly, one year later the upper half of the earnings distribution of men to be displaced from downsizing plants lies slightly *above* that of the control group. In addition to being very small in absolute value, these differences also vanish in the next period (with the exception of the effect at the 9th decile). The displacement effect for men in the plant-closure group becomes statistically significant for the first time in 1996 at the lower end of the distribution.

For women our setting does not work quite as well. The group of women to be displaced from downsizing plants seems to earn less than the control group already in 1994 (panel C in table 4). The differences in the deciles are relatively small, being 0.9–5.9%, but statistically significant from the 3rd decile upwards and around 5% at the right tail of the distribution. This raises a doubt that our sample design fails and a selection problem remains for this particular group of women. For women in the plant-closure group we find only one statistically significant coefficient in 1994. The absolute value of this effect is relatively high, implying a difference of 5.4% in the 1st decile compared to the control group. On the other hand, the effect is associated with a rather high standard error (0.020) and it also disappears by 1996.

To summarize, with the exception of women displaced in 1997 due to plant downsizing, the earnings distributions of all other displacement groups are almost identical to that of the control group three or four years prior to the base period. Hence, our approach to detect exogenous displacements using mass layoffs and plant closures seems successful, though one female group might pose some problems. We also found relatively small pre-displacement effects one to two years before the displacement period.

5.2 Post-displacement effects

Losing a job in the middle of the depression in 1992 has a huge effect on the earnings distribution. In the year following the displacement, the median earnings of displaced men are less than half of the median of the control group (see figure 3). The decline in the median earnings is even slightly more pronounced for displaced women. Not only does the entire distribution shift down, but also its shape changes drastically. The deciles above the median drop less, whereas the lower deciles decline much more than the median. The

Table 4: Quantile displacement effects for 1997

θ	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004
A. Men displaced men from a downsizing plant, $\alpha^{D}_{\theta s}$											
.1	0.005	0.006	0.006	**-0.088**	**-0.370**	**-0.465**	**-0.444**	**-0.278**	**-0.439**	-0.768	-0.802
.2	0.006	0.006	0.006	**-0.017**	**-0.075**	**-0.082**	**-0.072**	**-0.064**	**-0.063**	**-0.074**	**-0.076**
.3	0.004	0.004	0.006	-0.005	**-0.057**	**-0.045**	**-0.028**	**-0.023**	**-0.027**	-0.021	-0.018
.4	-0.001	0.007	0.005	0.003	**-0.042**	**-0.033**	**-0.020**	-0.011	**-0.020**	-0.008	-0.004
.5	0.001	**0.008**	0.005	0.006	**-0.036**	**-0.022**	**-0.018**	-0.010	**-0.019**	-0.008	-0.012
.6	0.004	**0.010**	0.006	0.006	**-0.024**	**-0.014**	**-0.020**	-0.004	-0.004	-0.010	-0.016
.7	0.006	**0.020**	0.006	**0.019**	0.001	-0.006	-0.015	0.012	0.001	-0.006	-0.002
.8	0.006	**0.024**	0.013	**0.022**	0.020	0.006	-0.010	0.010	-0.004	-0.007	-0.012
.9	0.004	**0.024**	**0.018**	**0.031**	0.023	0.004	-0.016	0.004	-0.003	-0.003	0.003
B. Men displaced due to plant closure, $\alpha^{C}_{\theta s}$											
.1	-0.031	-0.023	**-0.057**	**-0.412**	**-1.042**	-1.138	**-1.164**	-2.005	-1.558	-1.793	-1.769
.2	-0.012	-0.010	**-0.038**	**-0.125**	**-0.285**	**-0.205**	**-0.291**	-0.215	**-0.280**	**-0.252**	**-0.288**
.3	-0.007	-0.003	**-0.026**	**-0.087**	**-0.149**	**-0.100**	**-0.137**	**-0.091**	**-0.136**	**-0.132**	**-0.137**
.4	-0.006	-0.007	-0.014	**-0.057**	**-0.095**	**-0.051**	**-0.091**	**-0.063**	**-0.078**	**-0.086**	**-0.100**
.5	-0.005	-0.002	-0.013	-0.026	**-0.052**	-0.034	-0.049	-0.013	**-0.056**	**-0.039**	-0.055
.6	0.003	0.003	-0.011	**-0.028**	-0.024	-0.016	-0.029	-0.009	-0.025	-0.032	-0.031
.7	0.004	-0.003	-0.020	-0.014	-0.020	-0.008	-0.030	-0.024	-0.019	-0.034	**-0.048**
.8	-0.010	-0.024	-0.027	-0.027	-0.030	-0.008	-0.013	-0.027	-0.011	-0.041	-0.038
.9	0.011	-0.005	-0.045	**-0.063**	-0.027	0.003	-0.013	-0.033	-0.040	-0.050	-0.058
C. Women displaced men from a downsizing plant, $\alpha^{D}_{\theta s}$											
.1	-0.010	-0.005	-0.019	**-0.524**	**-1.446**	**-1.815**	**-1.554**	**-0.901**	**-0.868**	**-2.247**	**-2.904**
.2	-0.005	-0.003	-0.003	**-0.187**	**-0.487**	**-0.318**	**-0.256**	**-0.213**	**-0.248**	**-0.365**	**-0.421**
.3	**-0.009**	-0.002	-0.005	**-0.056**	**-0.087**	**-0.075**	**-0.064**	**-0.089**	**-0.092**	**-0.138**	**-0.134**
.4	**-0.009**	-0.008	-0.009	**-0.028**	**-0.062**	**-0.057**	**-0.052**	**-0.066**	**-0.061**	**-0.078**	**-0.075**
.5	**-0.011**	**-0.014**	**-0.016**	**-0.027**	**-0.057**	**-0.052**	**-0.048**	**-0.056**	**-0.053**	**-0.066**	**-0.080**
.6	**-0.011**	**-0.020**	**-0.019**	**-0.032**	**-0.064**	**-0.061**	**-0.050**	**-0.053**	**-0.053**	**-0.057**	**-0.083**
.7	**-0.026**	**-0.034**	**-0.036**	**-0.047**	**-0.075**	**-0.078**	**-0.063**	**-0.066**	**-0.063**	**-0.061**	**-0.073**
.8	**-0.043**	**-0.047**	**-0.050**	**-0.057**	**-0.086**	**-0.091**	**-0.082**	**-0.086**	**-0.083**	**-0.085**	**-0.083**
.9	**-0.061**	**-0.062**	**-0.061**	**-0.062**	**-0.101**	**-0.104**	**-0.101**	**-0.090**	**-0.102**	**-0.084**	**-0.099**
D. Women displaced due to plant closure, $\alpha^{C}_{\theta s}$											
.1	**-0.055**	**-0.104**	-0.080	**-0.697**	**-2.141**	-3.139	-2.687	-1.142	-1.320	-1.531	**-4.320**
.2	-0.031	**-0.023**	-0.022	**-0.239**	**-0.640**	**-0.868**	**-0.626**	**-0.412**	**-0.303**	**-0.259**	**-0.312**
.3	-0.011	-0.007	-0.010	**-0.148**	**-0.353**	**-0.292**	**-0.329**	**-0.250**	**-0.226**	**-0.205**	**-0.171**
.4	0.002	-0.003	-0.015	**-0.074**	**-0.194**	**-0.189**	**-0.190**	**-0.199**	**-0.138**	**-0.123**	**-0.120**
.5	0.003	0.006	-0.002	-0.025	**-0.132**	**-0.131**	**-0.100**	**-0.137**	**-0.091**	**-0.089**	**-0.087**
.6	0.008	0.007	-0.005	-0.021	**-0.076**	**-0.101**	**-0.069**	**-0.103**	-0.048	-0.051	-0.042
.7	0.014	-0.009	-0.003	-0.007	**-0.078**	**-0.067**	**-0.068**	-0.076	-0.017	-0.004	-0.040
.8	0.017	-0.020	0.003	0.015	**-0.048**	-0.044	0.007	-0.025	-0.021	-0.005	0.014
.9	0.046	0.017	0.038	0.042	-0.015	0.026	0.058	0.077	0.050	0.059	-0.008

Notes: Significantly (95%-confidence level) non-zero coefficients in **bold**. Statistical inference based on the standard errors boostrapped using 100 replications.

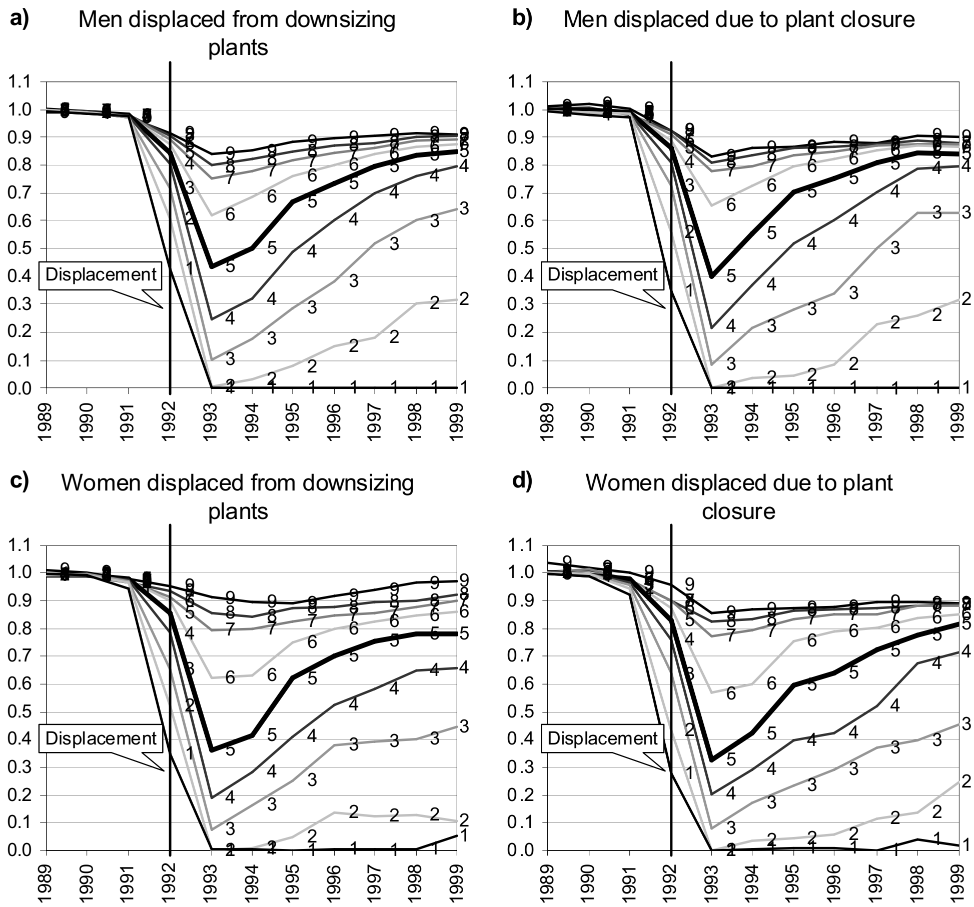

Figure 3: Proportional QDEs, $\exp(\alpha^D_{\theta s})$ and $\exp(\alpha^C_{\theta s})$, for the 1992 sample

huge proportional effect in the left tail of the distribution implies that a large fraction of displaced workers were unable to return to paid employment (see also figures 2a and 2a).

After the sharp initial drop in the first two years following the job loss, the earnings distributions of the displacement groups start to converge toward that of the control group. This recovery is rather strong between the 2nd and 6th deciles, though it slows down after a few years for women who lost their jobs in mass layoffs. This pattern can be attributed to an increasing probability of having found a suitable job after displacement.[15] It is striking, however, that the 1st decile does not show any sign of recovery, suggesting that the displaced worker has a notable risk of remaining outside paid work until the end of the follow-up period.

Displacement has a much weaker effect in the upper part of the distribution. At the 9th decile, for example, the displacement effect never exceeds 20%. Hence, a job loser can sometimes perform relatively well in the labor market compared to what would have

[15] The average number of employment months in the displacement group increased quite rapidly from 1993 to 1998.

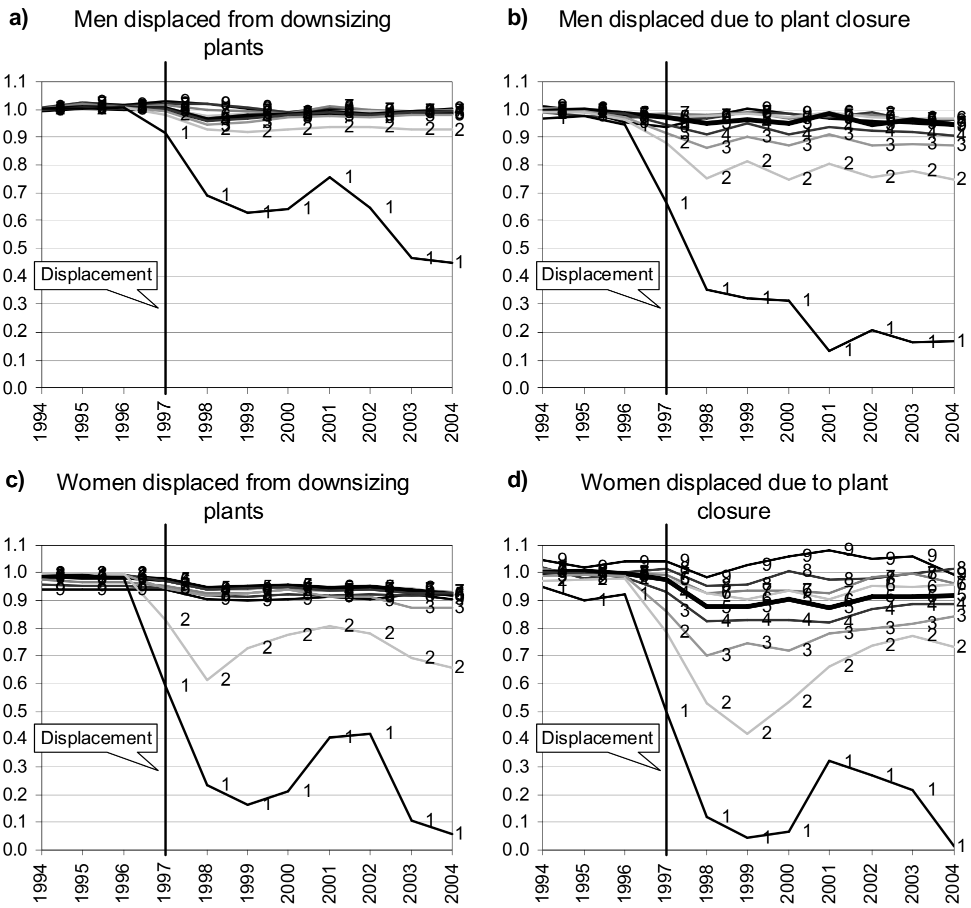

Figure 4: Proportional QDEs, $\exp(\alpha^D_{\theta s})$ and $\exp(\alpha^C_{\theta s})$, for the 1997 sample

happened without the displacement. On the other hand, the recovery of the upper deciles is rather slow and, consequently, the upper tail does not catch up with the level of the control group by the end of the observation period. Except for the effect at the 1st and 9th deciles for women displaced from downsizing plants, the displacement effect is statistically significant at all deciles in 1999 (see table 3). The two lowest deciles for the displaced worker are less than half of the counterfactual values without displacement, whereas the medians and upper deciles are 3–25% below the counterfactual values. It is remarkable that the displacement in the depression still has an effect on the entire distribution seven years after the job loss. Note also that the consequences of job loss in 1992 are rather similar for workers displaced from downsizing plants and those displaced from exiting plants.

Women are subject to larger long-term earnings losses than men. Among those displaced from downsizing plants, the displacement effect at the median is 15% and 22% in 1999 for men and women, respectively. Except for the two lowest deciles the sex difference in the displacement effects is even larger below the median, whereas it is rather small in the upper part of the distribution. A similar pattern exists for the plant-closure group, though the sex difference in the displacement effects at the median is less pronounced for that group.

When we turn our attention from the severe depression to the ensuing recovery period, the picture of earnings losses changes dramatically (see figure 4). Compared to the 1992 effect, the effect of displacement in 1997 is much smaller on average but even more concentrated in the lower end of the distribution. The displacement effect exhibits relatively little variation between the 3rd and 9th deciles (at least compared to the 1992 sample), but it is of a different order of magnitude at the two lowest deciles. In all displacement groups the lowest decile drops more than 50% compared to the control group. Despite very large absolute values, the displacement effect at the 1st decile is occasionally statistically insignificant in the last years of the observation period (see table 4), which may be due to the relatively small sample sizes of the 1997 displacement groups.

In 1997 the displacement has a statistically significant negative effect for men only at the lower end of the earnings distribution. This effect is more pronounced for those who were displaced from exiting plants. The displacement effect in the lower part of the earnings distribution gets stronger in 1998, the median being around 5% below the median of the control group. The displacement effect for men is not statistically significant above the 6th decile after 1997 except at one decile in 2004 for the plant-closure displacement group. By the end of the observation period, the displacement effect for men at the median disappears but the negative effect at the lower deciles remains statistically significant. This heterogeneity implies that a large fraction of workers experienced rather moderate earnings losses, if any, after a job displacement that took place during the period of economic growth. That is, much of the decline in mean earnings can be attributed to a relatively small group of displaced workers who suffered from notable earnings reductions due to difficulties to return to work.

Women displaced in 1997 due to plant closures suffered from larger earnings losses than male job losers did. One year after the displacement, the effect is statistically significant between the 1st and 8th deciles for the plant-closure group (see panel D in table 4). This displacement effect gets weaker over time, but remains statistically significant in the lower half of the distribution until the end of the observation period. In 2004 the effect is still some 8% at the median and even larger at the lower deciles. The effect of being displaced from an exiting plant is slightly larger in 2004 for women than for men. In the earlier periods the displacement effect is clearly stronger for women. These findings are in line with the larger losses for women in the 1992 sample. There seems to be a positive displacement effect for the female plant-closure group at the highest decile over the years 1999-2003, but this effect is not significantly different from zero in any period.

The displacement effect for women who lost their jobs in mass layoffs is significantly negative at each decile between 1997 and 2004 (panel C in table 4). That is, the entire earnings distribution of this group is estimated to lie below the distribution of the control groups for the whole post-displacement period. The effect of being displaced from a downsizing plant for women is relatively weak but very persistent, as there is little or no recovery at all over time. These estimates should be treated with caution, however, as the pre-displacement effects raised some doubts about the validity of the conditional independence assumption for this particular group.

In the 1992 sample there are no systematic differences in the displacement effects between workers who lost their jobs in mass layoffs and those who became displaced from exiting plants. By contrast, men in plants that closed down in 1997 were subject to much larger earnings reductions than those displaced from downsizing plants in that year. Our findings for women who were displaced in 1997 are inconclusive due to a potential selection problem in the downsizing subgroup. A potential explanation is that, during the exceptionally deep (and to some extent unexpected) depression, mass layoffs and plant closures were rather common, and hence perhaps less selective, events.

One of our key findings is that the effect of displacement is very heterogenous, being much larger at the lower quantiles, and this holds for women and men in both time periods. This implies a higher degree of earnings dispersion, as measured by the ratio of the upper deciles to the lower ones, for the displaced workers. In other words, job loss increases uncertainty about the future earnings level, suggesting an additional welfare loss for the risk-averse workers.

5.3 Robustness of the results

We have checked the robustness of the main results with respect to various departures from our benchmark setting. Here we describe the main findings briefly, but do not report any parameter estimates due to a huge number of them.[16] First, our analysis involves an implicit assumption that better workers did not quit and less able workers were not laid off to a large extent in the periods preceding a mass layoff or plant closure. In section 5.1 we did not find evidence of notable differences in the earnings distributions between the treatment and control groups 3–4 years before the base periods (except for women in the 1997 downsizing group). While these findings support the validity of our sample design, it is of interest to compare the earnings of the early leavers to that of the control and displacement groups. As discussed earlier, two subgroups of early leavers are included in the data: workers who separated in the base year (1992 or 1997) from plants that downsized or closed down during the *next* year. Differences in their earnings distributions compared to that of the control group are captured by the coefficients $\eta^C_{\theta s}$ and $\eta^D_{\theta s}$ in (6).

In the 1992 sample, these effects exhibit very similar patterns over time but are (almost) uniformly larger (in absolute value) than the associated displacement effects, $\alpha^C_{\theta s}$ and $\alpha^D_{\theta s}$, in the post-displacement period. That is, between 1992 and 1999 each decile of the earnings distribution of the early-leaver group typically dropped more than the corresponding decile of the associated displacement group. This result is consistent with the hypothesis that the

[16] Of course, the detailed results are available from the authors on request.

employers laid off their worst employees prior to the period of mass layoff or plant closure. On the other hand, we do not detect notable differences in the earnings distribution between the early leavers and the control groups 3–4 years before 1992,[17] implying that the early leavers are *not* a selective group compared to the control and displacement groups. A potential explanation is that the early leavers are otherwise similar but are affected by a stigma effect compared to those who separated during the period of a mass layoff or plant closure.

In the 1997 sample we find much larger earnings losses for the early-leaver group than for the displacement group. In this case there seems to be a selection problem, however. With an exception of men in downsizing plants, the earnings distribution of the early leavers was below that of the control group already in 1994. So the early leavers in 1997 seem to be a selective group of workers in terms of unobserved characteristics, and thereby their exclusion from the control and displacement groups is important for appropriate statistical inference.

Second, our threshold value for mass layoffs – a 50% reduction in employment – is essentially arbitrary. We have checked the robustness of our results with respect to this choice by lowering the threshold value to 30%. Our results remain qualitatively unchanged, but the displacement effect right after the period of job loss becomes a few percentage points stronger at the 6th and lower deciles in the 1992 group and at the 1st and 2nd decile in the 1997 group. We interpret this as an effect of including more selective dismissals in our displacement group.

Third, so far we have discussed the consequences of job losses that occurred at two specific points in time. Those years were not randomly chosen but we have conducted similar analyses for all displacements taking place in the period 1992–2001. The results from this exercise show that workers displaced in 1992 were subject to the largest earnings losses. The earnings losses exhibit a gradually decreasing trend as a function of the displacement period from 1992 to 1997. There is no notable variation in the displacement costs with respect to the timing of job loss after 1997. Thus, our results for the 1997 sample describe the costs of job loss under "normal" economic conditions, whereas the 1992 results provide an upper bound for the displacement costs that the worker can face in an exceptionally difficult economic environment.

Finally, as the dependent variable in our analysis is earnings, our estimates describe a reduction in labor income that results from shorter working time and/or lower wage rates following the displacement. When out of work, individuals are typically entitled to income transfers, like unemployment benefits, disability benefits and/or housing allowance, which can compensate for a large part of the earning losses in a welfare state like Finland. So we should expect displaced workers to experience smaller income losses than earnings losses. To address this issue we have replicated our analysis by using taxable annual income (excluding capital income) as the dependent variable in place of labor income.

[17] At the 5% risk level 3 out of 36 coefficients differ significantly from zero in 1989.

As expected, the displacement effect on annual income is much smaller than on annual earnings at the lower end of the distribution. In the 1992 sample, the displacement effect at the two lowest deciles of annual income is above 30% in 1999. The corresponding effect at the lower end of the distribution for the 1997 sample in 2004 is smaller but statistically significant, being around 10%. At the upper end of the distributions, the effects of job loss on annual income and earnings are of the same magnitude. These findings are not very surprising given that eligibility for income transfers depends on the level of labor income. But it may come as a surprise that the displacement has an equally long-lasting effect on the distribution of annual income as it has on the earnings distribution. Namely, seven years later, a job loss still has a statistically significant effect on the entire distribution of annual income for those displaced in 1992 and at the lower end of the distribution for those displaced in 1997.

6 Concluding remarks

We analyzed the costs of involuntary job loss among Finnish workers who became displaced during the period of depression or recovery. Using the quantile regression method, we estimated the effect of displacement at each decile of the earnings distribution. Our findings from both time periods suggest that 1) displaced workers suffer from substantial and persistent earnings losses, 2) women are subject to larger earnings losses, and 3) the effect of displacement is very heterogeneous, being much larger at the lower quantiles. An important implication of impact heterogeneity is that displacement does not only cause a large loss in the expected earnings, but also raises uncertainty about the level of the future earnings.

By contrasting the results of the two periods, we found much larger earnings losses for those who lost their jobs during the depression period. Men (women) who were displaced in the middle of the depression had approximately 15% (20%) lower median earnings 7 years after the job loss. As a result of exceptionally difficult labor market conditions, their earnings distribution as a whole remained below the counterfactual level until the end of the follow-up period. By contrast, job loss in the recovery period had a long-lasting effect only in the lower half of the distribution. For women displaced in 1997 the median effect seven years later was about 8%, whereas it did not differ from zero for men. These long-term losses do not vanish even when income transfers are accounted for.

As a general lesson, our analysis suggests that the mean effect alone can give a rather incomplete picture of the consequences of job loss. For example, a moderate effect of displacement on the mean or median earnings may hide a notable effect that is present only in the left tail of the distribution. When distributional effects are disregarded *a priori*, there is an obvious risk of misleading inference.

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