European University Institute

Department of Economics

Four Essays in Applied Microeconometrics

Guido Schwerdt

Thesis submitted for assessment with a view to obtaining the degree of
Doctor of Economics of the European University Institute

Florence, August, 2007

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Examining Board:

Andrea Ichino, Supervisor, University of Bologna and European University Institute
Omar Licandro, European University Institute
Barbara Petrongolo, London School of Economics
Ludger Wößmann, Ifo Institute for Economic Research, Munich

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Introduction

This thesis applies microeconometric methods to analyze various economic research questions using different data sources. It is divided in four chapters. In the following, I will shortly describe each chapter in more detail.

Chapter 1 contributes to the literature on population aging. Increasing retirement age helps solving pension problems only if employment prospects of the elderly remain intact. We use firm closure data from social security records for Austria 1978-1998 to investigate the effect of age on employment prospects. We rely on exact matching to compare workers displaced due to firm closure with similar non-displaced workers. We then use a triple-difference strategy to analyze employment and earnings of elderly relative to prime-age workers in the displacement and non-displacement groups. Results suggest that immediately after plant closure the old have lower re-employment probabilities as compared to prime-age workers but later they catch up. We use theory to interpret these results as evidence that after plant closure older workers are considered less productive by the market but are reluctant to lower their reservation wages enough to ensure the same employability of the young. With the passage of time, the shorter time horizon of the old induces them to lower their reservation wage to find a job before retiring. As a result, their employment rate catches up with the one of displaced prime-age workers.

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Chapter 2 investigates the effect of including early leavers when studying the costs of displacement. Involuntary job loss in administrative data is commonly identified by focusing on mass-layoffs or plant closures. However, such events usually do not happen without prior knowledge, which potentially leads to selection in the labor turnover of distressed firms. We find that workers separating from closing plants up to 2 quarters before closure are associated with significantly lower displacement costs and on average significantly higher pre-closure earnings levels as opposed to ultimately displaced workers. Furthermore, our results indicate that displaced workers with high pre-closure earnings experience significantly lower reductions in future employment probabilities. These findings suggest that compositional differences cause estimated displacement costs to differ between early leavers and ultimately displaced workers. Focusing exclusively on the latter group would lead to an overestimation of displacement costs.

Chapter 3 analyzes the growth in euro area labor quality. The Composition of the euro area workforce evolves over time and in response to changing labor market conditions. We construct an estimate of growth in euro area labor quality over the period 1983-2005 and show that labor quality has grown on average by 0.47% year-on-year over this time period. labor quality growth was significantly higher in the early 1990s than in the 1980s. This strong increase was driven mainly by an increase in the share of those with tertiary education and workers in prime age. Growth in labor quality moderated again towards the end of the 1990's, possibly reflecting the impact of robust employment growth resulting in the entry of workers with lower human capital. The contribution of labor quality to labor productivity has increased over time, accounting for up to one fourth of euro area labor productivity growth.

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The results point to a lower contribution of total factor productivity to euro area growth.

Chapter 4 provides empirical evidence from Germany to shed some light on the so-called ”consumption retirement puzzle”. Analyzing the consumption behavior over retirement in Germany, this chapter finds a negative correlation between income replacement and the increase in home-production-related activities. This might reconcile the observed drop in consumption at retirement with the predictions of the standard life cycle model. However, since individuals with fairly stable income over retirement also increase home production, this increase cannot be entirely attributed to a substitution effect.

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Chapter 1

Too Old to Work, Too Young to Retire?

Jointly written with Andrea Ichino, Rudolf Winter-Ebmer and Josef Zweimüller

1.1 Introduction

In most industrialized countries the labor force is aging because of both lower fertility rates and longer life expectancy. These developments lead to worries about the solvency of pay-as-you-go pension systems which in turn have induced pension reforms aimed at increasing minimum retirement age. The feasibility of keeping up the employment prospects of an increasingly older workforce is, however, questionable. From a public finance point of view, high unemployment rates of elderly workers would take away much of the gains of increases in retirement age.

Since wages and working conditions in ongoing jobs are characterized by long term implicit contracts[1] and because of regulations in terms of firing re-

[1]See for example, Lazear (1979).

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strictions and wage adjustments, the employment prospects of elderly workers are best investigated in a situation where the worker faces the job market after a displacement or plant closure. What happens if old and young workers are exogenously thrown into the labor market? The labor market position of a worker after displacement will depend on possible productivity changes due to aging (a demand effect), but also on the supply reaction of the worker in terms of search intensity and the willingness to accept wage concessions. If lower productivity of elderly workers decreases their market wage over time, their search intensity might fall, because searching for a not-so-good-anymore job is not really worthwhile. On the other hand, elderly workers might have a higher discount rate: they have fewer years in front of them to earn a labor income that would be higher than retirement income, and moreover they face a higher probability of death. This greater impatience of elderly workers makes them more willing to accept wage concessions in order to find a new job faster. Depending on the relative strength of these counteracting effects, employment rates of elderly workers after displacement could be higher or lower as compared to prime-age workers.

In the empirical part of this chapter we look at relative employment rates of elderly workers in Austria. Based on social security data for the entire Austrian workforce, we rely on exact matching to compare workers displaced due to a plant closure with a control group of non-displaced workers. The huge size of the data set at our disposal (more than 1 million records) enables us to exploit exact matching techniques in very favorable conditions that are rarely met in studies based on these methods. Within this matched

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sample, we extend the standard displacement cost specification introduced by Jacobson, LaLonde and Sullivan (1993) and use a difference-in-difference strategy to look at the employment and earnings prospects of elderly relative to prime-age workers in the displacement and non-displacement groups. Note that our estimation strategy combines the advantages of exact matching to improve the comparability of treated and control subjects, with the advantages of differencing in panel data to control for remaining confounders captured by time invariant individual, cohort and time effects. Results suggest that within ten years after displacement both prime-age and elderly workers have significantly lower employment rates as compared to the control group. More surprisingly, with respect to the benchmark represented by workers never displaced in the corresponding age cohort, elderly workers have lower employment rate than prime-age workers immediately after plant closure, but they manage to catch up over time.

We use theory to interpret these results as evidence that after plant closure older workers are considered less productive by the market but are reluctant to lower their reservation wages enough to ensure the same employability of the young. With the passage of time, the shorter time horizon of the old induce them to lower their reservation wage in order to find a job before retiring, and this explains the observed catching up.

The remainder of this chapter is organized as follows: Section 1.2 gives an overview of the related literature on productivity and wage effects over the life cycle. In Section 1.3 we propose a simple model where laid-off workers differ in the wage they can command on the market due to age-related

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productivity and in their degree of impatience, which affects the size of the wage concessions they are willing to make in order to find a job faster. Section 2.3 describes the data and the matching procedure. Section 2.5 presents some descriptive evidence, the identification strategy and the econometric estimates. Section 1.6 discusses the robustness of these results, while Section 2.6 concludes.

1.2 Productivity and Wages over the Life Cycle

How do productivity and wages develop over the life cycle? Human capital theory would predict a concave age-productivity profile due to a shorter horizon later in life, which will reduce the incentive for learning as well as some skill obsolescence as aging progresses. Empirical wage studies in the Mincer tradition have typically found such a concave pattern. While earnings are much easier measured than productivity as such, earnings are not necessarily a good proxy for productivity over the life cycle. Earnings could either be higher or lower than productivity. Models of firm-specific training imply that firms participate in paying for training in the early part of the careers, making wages higher than productivity early on. These initial investments will later be repaid by agreeing on lower wages. A testable prediction of these models is that layoff-probabilities of elderly workers are lower. An alternative hypothesis concentrates on incentive effects: incentives to work hard close to retirement age can only be kept up if wages exceed productivity. In this case, shirking workers would experience a major loss in case of dismissal and this

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prospect would keep up their effort at work (Lazear (1979)). These studies in general indicate that wages in ongoing jobs are likely not to be a good proxy of current productivity.

There are many direct studies on particular aspects of productivity mainly done by social psychologists who look at specific tasks, like cognitive abilities, finger dexterity, verbal skills, and the productivity of learning at higher age.[2] The general message is that productivity reductions at older ages are particularly strong when problem solving, learning and speed as well as physical strength are important, while older individuals maintain a relatively high productivity level in work tasks where verbal abilities and organizational skills matter more. While task-specific aging is widely documented, overall productivity in a job depends not only on being quick and adept in specific tasks, but also on experience, which can counter these aging processes by better knowledge of processes and organization.

Direct tests on productivity in jobs come from piece rate schemes and supervisors' ratings. While piece rates typically find quite substantial decreases in productivity with age, supervisors' ratings do not. Both methods are not fully satisfactory: results from piece rates are not easily generalizable because they relate to very specific jobs, mainly manual workers or simple clerical work like typing. A general disadvantage with the use of supervisors' ratings to rank individuals by age and productivity is that managers often may wish to reward older workers for their loyalty and past achievements (Skirbekk

[2]See Skirbekk (2004) for a survey. Highly specific studies look at productivity at sports activities or chess (Fair (2004)) or the productivity of researchers in science (Stephan and Levin (1988)) and economics (Oster and Hamermesh (1998)). In both cases a significant negative age-gradient has been found.

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(2004)).

In recent years the use of matched employer-employee data sets has allowed to study the impact of the workforce's age structure on productivity or sales. Most of these studies[3] find concave age-productivity profiles - in many cases these profiles are flatter than the corresponding age-earnings profiles. While matched employer-employee data offer great opportunities to combine personal with firm information, the main challenge of these approaches is to isolate the effect of workers' age on productivity from other influences, in particular the selectivity of workers' types: good workers get promoted, whereas bad workers will loose their jobs, a problem which will get more severe as the workforce ages. Moreover, more successful firms will increase their payroll, with obvious effects on the age structure of the workforce.

These age-productivity issues are relevant for the interpretation of the relationship between age and risk of job displacement. Kuhn (2002) in a collaborative study on worker displacement finds (see page 49) no direct effect of age on the risk to be displaced from a job. On the other hand, some studies show that the risk of job loss is in fact greater among older workers when the rate of technological change is highest (Bartel and Sicherman (1993), Ahituv and Zeira (2000)).

This chapter produces new evidence supporting the hypothesis that ageing worsens the employment prospects immediately after job loss when displacement occurs due to plant closure. It also highlights the existence of supply sides effects in terms of search intensity of elderly workers who, because of

[3]See for instance Hellerstein and Neumark (2004), Haltiwanger and Spletzer (1999) or Daveri and Maliranta (2005).

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their shorter time horizon, tend to look harder for job opportunities before retiring. This second type of effects has been explored less in the existing literature.

1.3 A simple model

Consider the optimization problem of a worker who has just been laid off because of a plant closure. This worker controls her reservation wage and, by accepting lower wage offers, can increase the probability of finding a new job. Let U denote the present value of the income stream that this worker faces while unemployed while E is the present value if she finds a job and becomes employed. Normalizing to zero the unemployment benefit, the usual asset value function in steady state implies that

$$rU = s(E - U) \tag{1.1}$$

where r is the subjective discount rate of the worker, and s is the flow probability that the worker receives and accepts a new wage offer becoming employed again.

We assume, that after finding a new job she does not lose it again and earns forever the instantaneous wage $w - s$, with $w \geq s$ so that the following asset value function hold as well

$$rE = w - s. \tag{1.2}$$

The assumption that the wage earned by the worker in the new job is $w -$

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s captures the idea that, to increase the probability of re-employment, the worker has to accept lower wage offers which imply lower future earnings. Since retirement income depends on the wage in the last job, the first of these two assumptions simplifies matters without loss of generality. The second assumption (that the workers receives $w - s$ forever) is less innocuous, because it rules out that the probability of losing the new job might be related to the accepted wage. However, it simplify matters without affecting the basic implications of the model.

Solving for U we obtain

$$U = \frac{s(w-s)}{r(s+r)} \tag{1.3}$$

which is the objective function that the worker maximizes choosing s. From the First Order Condition, the optimal s is:

$$s^* = \sqrt{r(r+w)} - r \tag{1.4}$$

Two propositions follow:

Proposition 1 *The derivative* $\frac{\partial s^*}{\partial r}$ *is positive which indicates that more impatient workers are willing to make wage concessions to find a new job faster.*[4]

Proposition 2 *The derivative* $\frac{\partial s^*}{\partial w}$ *is positive which indicates that workers who can command higher wages are willing to make larger wage concessions in order to find a job earlier.*

[4]To see this note that $\frac{d}{dr}\left(\sqrt{r(r+w)} - r\right) = (w - 2s^*)/(2\sqrt{(r(r+w))})$ which is positive if $w > 2s^*$. To see that this latter condition is satisfied for all $w > 0$ use $s^* = \sqrt{r(r+w)} - r$ and rewrite $w > 2s^*$.as $w + 2r > 2\sqrt{r(r+w)}$. Taking the square on both sides of this inequality establishes that $w > 0$.

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The effect of a plant closure for the young and the old

Suppose that there exist two cohorts of displaced workers after plant closure: the young and the old. Denote with a subscript y (o) variables concerning the young (the old). If older workers are considered less productive by the market after plant closure

$$w_y > w_o. \tag{1.5}$$

In this case, given Proposition 2, it would follow that immediately after being displaced the elderly are less willing to lower their reservation wage than the young, because they do not want to further reduce their future earnings beyond the low wage that they can command on the market. Formally,

$$s_y^* > s_o^*. \tag{1.6}$$

The opposite would occur if the elderly were considered more productive and were commanding higher wages. Thus, the relationship between w_y and w_o summarizes the demand components of the consequences of a plant closure for the young and for the old.

There is however a second dimension in which the young and the old differ in the event of a plant closure, and this second dimension relates to supply behavior. The old have a shorter time horizon than the young. This is true in two ways. The obvious one, is that the hazard of death is higher for the old. But perhaps more important from our viewpoint is the fact that the old have fewer years in front of them in a job which is paying considerably more than the retirement income. Because of these two reasons, the old are more

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impatient than the young, keeping everything else equal. We capture this difference assuming that

$$r_y < r_o \tag{1.7}$$

which means that because of their shorter time horizon, the old discount the future more heavily than the young and are therefore more impatient.

Given Proposition 1, this implies that, everything else equal, because of their higher discount rate the old are more willing to make wage concessions in order to find a new job faster, and therefore

$$s_y^* < s_o^*. \tag{1.8}$$

Even if the old were considered less productive by the market and commanded a lower wage after a plant closure, their impatience to find a job before retirement could induce them to make larger wage concessions, in which case their re-employment probability would be larger than the one of the young.

This very simple framework suggests that after a plant closure the re-employment probabilities and the wages (conditional on re-employment) of the young and of the old may differ in directions that depend on the interaction between demand and supply effects. Demand effects are summarized by the wage concessions that the old would have to make in order to obtain the same employment opportunities of the young. Supply effects are summarized by the differences in subjective discount rates, which are higher for the old because of their shorter time horizon.

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1.4 Data and matching strategy

We use administrative employment records from the Austrian Social Security Administration. The data set includes the universe of private sector workers in Austria covered by the social security system. All the employment records can be linked to the establishment in which the worker is employed. The data set covers the years 1978 to 1998. Daily employment and monthly earnings information is very reliable, because social security tax payments for firms as well as benefits for workers hinge on these data.[5] Monthly earnings are top-coded, which applies to approximately 10% of workers. We transformed monthly gross earnings in daily wages dividing them by effective employment duration in each month of observation.

We concentrate on workers employed in the period 1982 to 1988 – who are in the risk set for a firm breakdown in this period; this allows us to observe the workers in detail 4 years prior to bankruptcy and 10 years afterwards. We exclude firms from the construction and tourism industry, because in these sectors seasonal unemployment is very high; firms often close down out of season and reopen after several months - often with the same workforce. Moreover, we restrict ourselves to workers coming from firms observed with more than 5 employees at least once during the period 1982 and 1988 and having at least one year of tenure at their firm. To study the aging process we compare two cohorts, those of age 35 to 44 at the time of displacement – the ”young” - and those between 45 and 55 – the ”old”.

[5] See Hofer and Winter-Ebmer (2003) for a description of the data set.

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Each establishment has an employer social security number. Hence, an exit of an establishment in the data occurs when the employer identifier ceases to exist. However, some of these cases are not true firm exits, and (most of) employees continue under a new identifier. If more than 50% of the employees continue under a new employer identification number we do not consider this a failure of the establishment.[6]

Our treatment group for the matching procedure consists of 11,578 workers from firm deaths between 1982 and 1988. Our control group comprises workers from all firms not going bust between 1982 and 1988, with the same tenure, industry and age requirements as the treated; this group consists of 1,087,705 workers. Our data set is ideal for matching. We can observe all workers quarterly over the four years before plant closure and have the universe of Austrian workers available as a potential control group. Detailed past work histories, i.e. employment record and earnings, can be considered to be an almost sufficient statistic for productivity of workers (see for example Card and Sullivan (1988)).

Our matching procedure is therefore very simple: We perform exact matching between the treated and control subjects on the following criteria: sex, age, broad occupation (blue- or white-collar), location of firm (9 provinces), industry (30 industries), employment history in each of the quarters 4, 5, 6 and 7 before plant closure.[7] We do almost exact matching on continuous variables: average daily wages in the quarters 8, 9, 10 and 11 before plant closure are

[6] Workers from such firms are coded as ”ambiguous” and are neither in the treatment nor the control group.
[7] Note that we only use persons with tenure longer than one year in the current firm.

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matched by decile group[8] and firm size in the two years before plant closure is matched by quartile groups each. Thus, for each treated subject, our matching algorithm finds a control subject with identical characteristics (according to the list mentioned above) at the date of plant closure. Applying this matching procedure we are able to identify at least one control subject for 6,630 treated subjects (out of a total of 11,578 subjects in the plant closure sample).[9] In total we end up with 36,677 matched controls.

Table 1.1 gives descriptive statistics about the quality of the matching. Whereas gender, blue-collar and age are exactly matched, tenure and work experience (only available since 1972) are not matching variables in our algorithm. It is therefore interesting to see, that our matching strategy works perfectly in terms of tenure and work experience: mean differences between treated and controls are only marginal. Similarly, for average daily wages, which have been matched by deciles in the quarters 8 to 11 prior to plant closure, the differences are very small. Figure 1.1 shows that this small difference in means does not hide large individual differences between pairs: kernel density estimates for the relative distance in average wages in the quarters -8 to -11 show that both for old and young workers, most of the density is in the region between plus and minus a quarter of a percent. Considering firm size, the difference is again very small for old workers and only slightly larger for young workers.

[8]We do not want to match earnings too close to firm failure, because there might be some anticipatory wage effects of firm breakdown.

[9]We experimented also with less restrictive matching algorithms that increase the number of matches without major quantitative changes in the results.

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1.5 Results

The most general specification of our estimation problem is the following.

$$
\begin{aligned}
Y_{i,t} &= \sum_{a=36}^{55}\sum_{d=-16}^{40} \alpha_{d,a} AGE_i^a PC_i Q_{i,t}^d + \sum_{a=36}^{55}\sum_{d=-16}^{40} \beta_{d,a} AGE_i^a Q_{i,t}^d \\
&+ \sum_{d=-16}^{40} \gamma_d PC_i Q_{i,t}^d + \sum_{d=-16}^{40} \delta_d Q_{i,t}^d \\
&+ X_i \kappa + \theta_t + \epsilon_{i,t} \qquad (1.9)
\end{aligned}
$$

where: $Y_{i,t}$ is the outcome of interest(employment status or wage); i denotes workers; t is calendar time measured in quarters; AGE_i^a is a dummy taking value 1 if worker i has age a, with $a \in [35, 55]$[10]; PC_i is a dummy taking value 1 if i is displaced in a plant closure; d is the distance in quarters from potential or actual plant closure, which ranges in the data from -16 to 40 with 0 denoting the last quarter before plant closure; $Q_{i,t}^d$ is a dummy taking value 1 if i is observed in quarter t at a distance of d quarters from plant closure; X_i are pre-plant closure observable characteristics of i (including age); $\epsilon_{i,t}$ capture unobservables of i at quarter t and $\alpha_{d,a}, \beta_{d,a}, \gamma_d, \delta_d, \kappa$ and the calendar time effects θ_t are the parameters that we would like to estimate. To keep the problem manageable and results interpretable, we simplify this general specification as described below.

[10]Note that the summation running over a goes from 36 to 55 because the first age dummy is omitted to avoid collinearity.

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1.5.1 Descriptive evidence

In order to obtain a preliminary graphical image of the effect of age on the comparison between displaced and matched non-displaced subjects before and after the plant closure date, we collapse the 21 age dummies AGE_i^a into a binary dummy OLD_i defined as

$$OLD_i = \begin{cases} 1 & \text{if } a \in [45, 55], \\ 0 & \text{if } a \in [35, 44]. \end{cases} \tag{1.10}$$

In this way we concentrate our analysis on the comparison of the employment and earnings prospects of elderly relative to prime-age workers in the displacement and non-displacement groups. Thus, equation 1.9 simplifies to

$$\begin{aligned} Y_{i,t} &= \sum_{d=-16}^{40} \alpha_d OLD_i PC_i Q_{i,t}^d + \sum_{d=-16}^{40} \beta_d OLD_i Q_{i,t}^d \\ &+ \sum_{d=-16}^{40} \gamma_d PC_i Q_{i,t}^d + \sum_{d=-16}^{40} \delta_d Q_{i,t}^d + \theta_t + \epsilon_{i,t} \end{aligned} \tag{1.11}$$

where we also abstract from observables X_i.

Panel A and B of Figure 1.2 report, respectively for the young and the old, the average employment rates of the displaced and non displaced workers as a function of the distance from plant closure d, defined as follows using

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equation 1.11:

$$\begin{aligned}
E(Y_{i,t} \mid OLD_i = 0, PC_i = 0, Q^d_{i,t} = 1) &= \delta_d \\
E(Y_{i,t} \mid OLD_i = 0, PC_i = 1, Q^d_{i,t} = 1) &= \delta_d + \gamma_d \\
E(Y_{i,t} \mid OLD_i = 1, PC_i = 0, Q^d_{i,t} = 1) &= \delta_d + \beta_d \\
E(Y_{i,t} \mid OLD_i = 1, PC_i = 1, Q^d_{i,t} = 1) &= \delta_d + \beta_d + \gamma_d + \alpha_d.
\end{aligned}$$

By construction, the employment rates of both the treated and the matched control observations are equal to unity in the four quarters prior to the plant closure date. The employment rates at earlier dates show that our matching procedure works perfectly as measured by the level of the outcome variable prior to plant closure. Both for the young and for the old sample employment rates are identical in all four years before plant closure. After that date, the employment rate of non-displaced workers decreases smoothly. This reflects the dissolution of employment relationships that existed at the sampling date because workers got either unemployed or sick, retired, died, or dropped out of the labor force for other reasons.

The employment rate 40 quarters after plant closure is slightly less than 80 percent for young non-displaced workers, and somewhat more than 20 percent for old non-displaced workers. The big drop in employment rates in the latter sample is due to early retirement.[11] At the date of plant closure, workers in the old sample are between 45 and 55 years old; hence 40 quarters after this date the oldest workers in the old sample are still younger than the regular re-

[11]In Austria, the regular retirement age for male workers is 65.

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tirement age. The evolution of employment rates reflects the strong incidence of early retirement in Austria which, next to Italy, France, and Belgium, has used early retirement most heavily to cope with the employment problems of elderly workers.[12]

After plant closure, employment rates of displaced workers look quite different. Not surprisingly, in the first quarter after displacement, the employment rate decreases by almost 60 percentage points. The drop is identical for both young and old displaced workers. Employment rates increase again during the quarters but never reach a level above 80 percent (displaced young workers) and 65 percent (displaced old workers), respectively. More importantly, displaced workers never fully catch up to non-displaced workers – even ten years after the plant closure date. Among young workers, the employment rate of non-displaced workers is still about 10 percentage points larger than the employment rate of displaced workers. Among older worker the absolute percentage point difference is smaller, about 3 percentage points.

Panel C of Figure 1.2 plots the within-age-group difference between the employment rates of displaced and non-displaced workers (γ_d for the young and $\gamma_d + \alpha_d$ for the old). Panel D plots instead the the difference in difference parameters α_d. These estimates show that during the first five-year interval after plant closure the old suffer more severely than the young: the drop in employment rates of older displaced workers is significantly higher than the one of young displaced workers during the first 20 quarters.

Interestingly, the picture is turned on its head during the second five-year in-

[12] See OECD (2005).

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terval after the plant closure date. Here we observe a significantly lower drop in employment rates for the old displaced workers than for the young displaced (relative to the never displaced in the corresponding cohorts). Another way to put it is that, while for the young the employment rate decreases in an approximately parallel fashion for displaced and non-displaced workers, for the old it decreases much faster for the non-displaced.

Figure 1.3 reports analogous results for the evolution of workers' earnings, based on the same equation 1.11 in which $Y_{i,t}$ denotes the wage. Panel A and B of this figure present the evolution of earnings by displacement status, both for young and for old workers. The numbers show mean nominal daily earnings, conditional on employed workers. Obviously, changes in this measure may occur because of changes in real earnings, in inflation and in selectivity (because the set of employed workers may change). The picture is qualitatively very similar across age groups. Over time, both prime-age workers and older workers experience a strong increase in their nominal daily earnings mainly reflecting growth in real earnings and inflation. By construction, earnings of displaced and non-displaced workers are (almost) identical in quarters 11 to 8 prior to plant closure (recall that non-displaced workers were matched on the basis of earnings deciles in the third year before plant closure). Also before this time interval (quarters – 16 to -12) there are almost no differences in daily earnings between the two groups, reconfirming the robustness of our matching procedure. However, starting form quarter -8 until the date of plant closure, the difference in average daily earnings between the two groups starts to diverge slightly (becoming more than two percentage

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points in quarter 4 prior to plant closure).

In the first quarter after the plant closure date mean daily earnings of already re-employed displaced workers are significantly higher than the average daily earnings of non-displaced workers. This clearly reflects selectivity: Only 40 percent of the displaced workers were able to find a new job within the first quarter following the plant closure date. These workers are not only successful in searching for a new job, they are also the highly productive ones. In later quarters following the plant closure date employment rates start to increase again reflecting that also the less productive displaced have again found a new job. This depresses the average earnings of the re-employed displaced. From the third quarter after plant closure daily earnings of displaced workers are significantly lower than those of the non-displaced. This gap is increasing over time and reaches more than 0.5 log-points in quarters 35 to 40 after displacement (see panel C of Figure 1.3).

Interestingly, earnings losses experienced by prime-age workers are almost identical to the losses experienced by older workers. Panel D of Figure 1.3 shows that, except for quarter 40, earnings losses of older workers are not significantly different from the earnings losses of prime-age workers.

In sum, Figures 1.2 and 1.3 suggest that, while there appears to be a causal effect of age on workers' job chances after a plant closure (negative initially and positive later on), no such significant differences show up for the earnings of those who find a new job, except possibly at the end of our sample period in which the wage losses of the elderly who find a job appear slightly larger.

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1.5.2 Controlling for observed heterogeneity

Figures 1.2 and 1.3 do not control for observable differences between the groups such as broad occupation (white- / blue-collar), sex, workers' previous experience, the duration of the current job (tenure), employer size and unobservable time invariant characteristics. In order to control for these observed characteristics and to obtain summary estimates of the effects suggested by Figures 1.2 and 1.3, we modify further equation 1.11 pooling over three periods in terms of distance from plant closure. These three periods are defined by the three dummies $Q_{i,t}^{-16,0}$, $Q_{i,t}^{1,20}$, $Q_{i,t}^{21,40}$ where

$$Q_{i,t}^{l,u} = \begin{cases} Q_{i,t}^{d} & \text{if } d \in [l,u], \\ 0 & \text{otherwise.} \end{cases}$$

In words, these three dummies identify the period before plant closure, the 5 years immediately after and the following 5 years. Using these dummies we run regressions of the form

$$\begin{aligned} Y_{i,t} &= \alpha_{-16,0} OLD_i PC_i Q_{i,t}^{-16,0} + \alpha_{1,20} OLD_i PC_i Q_{i,t}^{1,20} + \alpha_{21,40} OLD_i PC_i Q_{i,t}^{21,40} \\ &+ \beta_{-16,0} OLD_i Q_{i,t}^{-16,0} + \beta_{1,20} OLD_i Q_{i,t}^{1,20} + \beta_{21,40} OLD_i Q_{i,t}^{21,40} \\ &+ \gamma_{-16,0} PC_i Q_{i,t}^{-16,0} + \gamma_{1,20} PC_i Q_{i,t}^{1,20} + \gamma_{21,40} PC_i Q_{i,t}^{21,40} \\ &+ \delta_{1,20} Q_{i,t}^{1,20} + \delta_{21,40} Q_{i,t}^{21,40} + X_i \kappa + \theta_t + \epsilon_{i,t} \end{aligned} \tag{1.12}$$

where $Y_{i,t}$ denotes the outcome variable (employment status or wage) of individual i at quarter t. The interesting coefficients to be estimated are the

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difference-in-difference parameters $\alpha_{l,u}$. These parameters estimate the interaction effects of OLD_i and PC_i within the three time intervals relative to plant closure.

Table 1.2 presents results in which the outcome variable $Y_{i,t}$ is a dummy indicating an individual's employment status at quarter t. Column (1) reports the results from a simple OLS regression (a linear probability model) corresponding to the specification described in equation (1.12). To interpret these estimates correctly, let us proceed step by step. Suppose we want to know the employment probability of a non-displaced prime-age worker before plant closure. In that case all dummy variables are equal to zero, so the constant term measures this probability. For the non-displaced prime-age workers, employment rates change over time. During the first five years after the (hypothetical) plant closure date employment rates are 4.9 points lower than before (see the row for $Q_{i,t}^{1,20}$), and during years 5 to 10 after the plant closure date, employment rates are 11.7 points lower than before the plant closure date ($Q_{i,t}^{21,40}$). This reduction in employment rates is the result of aging. On average workers are 9.5 years older during quarters 21 to 40 as compared to during quarters -16 to 0 and this increase in age is most likely the dominant force behind the reduction in employment rates.

Consider next the cohort effect at the date of plant closure on employment rates of non-displaced workers. As indicated by the coefficients $OLD * Q^{l,u}$ in Table 1.2, workers who are between age 45 and 55 at the date of plant closure (for whom $OLD * Q^{l,u}$ = 1) are only 1 percentage point less likely to be employed than workers who are between age 35 and 44 years at the date of

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plant closure (see coefficient $OLD*Q^{-16,0}$). However, this difference widens to 11 points during the five years following plant closure, and increases dramatically to 41.2 points during the quarters 21 to 40 after plant closure. This increasing gap clearly reflects a life-cycle effect as older workers start to leave the labor force and they increasingly do so as they grow older.[13]

Now compare displaced workers to non-displaced workers. The coefficients of the interaction $PC * Q^{l,u}$ show that, before the plant closure date these two groups are equally likely to be found in employment. However, after plant closure there are large and highly persistent differences in employment probabilities of these two groups. During the first five years after the date of plant closure, displaced workers have an almost 20 (!) percentage points lower employment rate than non-displaced workers. Even six to ten years after the plant closure date, the difference in employment rates between these two groups amounts to more than 10 percentage points.[14]

We are now able to discuss the question of our primary interest: Do displaced older workers face significantly worse employment prospects after a job loss than prime-age workers? The coefficients of the interaction $OLD*PC*Q^{l,u}$ give an answer to this question. Before plant closure there is no significant additional effect that goes beyond the isolated effects of displacement (PC) and age at date of plant closure (OLD) discussed above, the point estimate being even positive, but small and statistically insignificant. The interaction

[13]Notice, however, that this effect is not a pure life-cycle effect but may also reflect a calendar-time trend – when older workers take advantage of early retirement to a larger extent – which is what happened in Austria during the period under consideration

[14]Chan and Stevens (2001) find for the U.S. that employment rates of 55 years old displaced workers four years after displacement were 20 percentage points lower than the employment rate of a control group.

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effect relating to the first five years after displacement indicates significantly lower job chances of older workers after job loss. Workers aged 45-55 at the date of plant closure face an employment rate that is 2.9 percentage points lower than the one implied by the isolated effect of age at plant closure plus the isolated effect of displacement status.

However, this employment rate penalty does not persist over time. In fact, during years six to ten after plant closure the absolute difference between plant-closure and non-plant closure workers is turned on its head once we account for the interaction effect of age at plant closure and displacement status. Workers aged 45-55 at the date of plant closure now face an employment rate that is 3.4 percentage points higher than the one implied by the isolated effects of age at plant closure plus the isolated effect of displacement status.

To check the robustness of the results the other columns in Table 1.2 add additional control variables. Column 2 includes a blue collar dummy and a female dummy as additional regressors, column 3 also accounts for experience, tenure, previous wage and employer size at the plant closure date. All additionally included variables turn out highly significant with expected signs. However, the coefficients of the age, plant-closure, and quarter-indicators (and their interactions) change only slightly. In particular, the estimated effect of age on job loss due to plant closure is exactly the same as the effect estimated in column 1. As a final robustness check, we allowed for individual fixed effects in the regression (column 4 of Table 1.2).[15] Interestingly, even accounting for individual fixed effects leaves the point estimates of the

[15]In this case an individual fixed effect ν_i is added to the specification in equation (1.12), while time-invariant controls, which include all the variables related to the pre-displacement period, are omitted.

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age effects due to plant closure unchanged! Also the remaining coefficients remain very close to the simple model.

Table 1.3 presents results from an analogous difference-in-difference regression on earnings. The empirical specification is identical to the above equation but now with the individual's daily log earnings as the dependent variable. In column 1 we estimate a wage equation that includes the full set of variables.[16] It turns out that additional earnings losses following plant closure that are directly caused by age do not exist. All differences-in-differences coefficients are insignificant and negligible in size. Displaced older workers suffer from the same earnings reduction as displaced prime-age workers. However, for all age groups, wage losses are sizable: displacement due to plant closure is followed by a wage reduction of more than 5 percent in the short run and even slightly more in the longer term.[17]

The remaining coefficients in column 1 of Table 1.3 are as expected. We see that age at plant closure has a negative effect on earnings growth (as indicated by the negative coefficient of $OLD * Q^{l,u}$). Note also that nominal daily earnings grew considerably over the observation period. Female workers earn significantly less than male workers and blue collars earn less than white collars. More work experience at the date of plant closure is associated with lower earnings which may be caused by measurement error as experience is left censored in 1972 and wages are top coded or simply by the fact that older workers are already in the declining part of the experience-wage profile. It

[16]Note that only observations with positive wages are included

[17]Jacobson et al. (1993) report persistent wage losses of some 30 percent for displaced workers in the U.S., whereas Ruhm (1991) and Stevens (1997) found somewhat smaller effects. These effects were found for more-tenured workers of all age groups.

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may also be due to the fact that we include the previous wage as a regressor which may in part capture positive effect of experience. Tenure, however, has the expected positive impact on the wage. Larger firms pay better and a higher wage at the plant closure date (as a proxy for skills) is associated with higher wages at other dates.

It is also interesting to note that the earnings regression of column 1 has a very high explanatory power: Almost 80 percent of the variance in wages is explained by the variables included in this regression. Controlling for individual fixed effects changes the results only slightly (column 2 of Table 1.3). In particular, also in the fixed effects estimation, older workers do not suffer from disproportionate earnings losses after displacement. While these losses remain large and statistically significant (both in the short- and in the long-run), prime-age workers and older workers face earnings losses of very similar magnitude. The same picture remains once we run a Tobit regression (accounting for top-coding in earnings data) and when we use the LAD as a robust estimator. Also with respect to other control variables, our estimates remain highly robust and do not seem to be strongly affected by the estimation methods or the inclusion of control variables.

1.5.3 A suggested interpretation

Our results can be summarized as follows:

1. Immediately after a plant closure, the old have lower re-employment probabilities than the young.

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2. With the passage of time, the old catch up and their re-employment probability becomes larger than the one of the young.

3. Immediately after plant closure the earnings losses of the young and of the old with respect to the non-displaced are basically identical (approximately 5% in both cases).

4. With the passage of time there is some indication that the old who find a new job lose more in terms of wages with respect to the non-displaced than the young in the same condition.

In the light of the model described in Section 1.3 these four facts are compatible with the following story. Immediately after a plant closure the old are considered by the market less productive than the young and thus $w_y > w_o$. Because of Proposition 2 the old do not decrease their reservation wage enough to keep their employment probability in line with the one of the young, and therefore, $s_y > s_o$ which is Result 1.

The catching up that characterizes the old in the long run is explained by their increasing impatience induced by the approximation of retirement age and more generally by their increasingly shorter time horizon. The hypothesis here is that the discount rate increases more than proportionally with age.[18] Thus, with the passage of time after plant closure both the young and the old get older, but the discount rate (impatience) increases more for the old than for the young. This implies that $s_o - s_y$ increases as a function of the distance from plant closure, inducing, everything else equal, a relatively higher

[18] We are not aware of any direct evidence on this hypothesis, which, however, seems plausible: one day before death the discount rate is close to infinity, abstracting from bequest motives.

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employment probabilities for the old. This is Result 2.

Denoting with a superscript n the wages of the non-displaced, Result 3 says that immediately after plant closure $w_y^n - (w_y - s_y) \approx w_o^n - (w_o - s_o)$. Since[19] $w_o^n > w_y^n$ and $w_o < w_y$, which is implied by the above results, the approximate equality of the wage losses of the displaced and the non-displaced implies that $s_y > s_o$ which is again Result 1 from a different perspective.

Finally Result 4 indicates evidence that the old who find a job toward the end of the observation period lose more in terms of wages than the young with respect to the non-displaced. This means that towards the end of the period $w_y^n - (w_y - s_y) < w_o^n - (w_o - s_o)$. This is exactly what we should find given that s_o increases with the passage of time because of the greater impatience of the old.

In other words, both the employment and the wage results support the same story from different perspectives. Immediately after plant closure, the old are considered less productive, they are offered lower wages and therefore they are reluctant to make wage concessions in order to find quickly a new job. For this reasons their probability of re-employment is low in the short run after displacement. In the long run instead, their higher impatience prevails with respect to the wage effect, inducing them to accept larger wage losses in order to find a job before retirement.

[19]In our sample the average wage of the old non-displaced as opposed to the average wage of the young non-displaced is .02 log-points higher in the first 5-year period and .03 log-points higher in the second 5-year period after displacement.

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1.6 Robustness checks

Above we have seen that in general older workers do not lose more in terms of earnings if they find a job, but their employment prospects suffer more at the outset, only to catch up over time. We now study the robustness of these results.

We begin by showing that this qualitative pattern of effects prevails also when we analyse separately the four groups of workers defined by gender and by occupation (white- vs. blue-collar), although the smaller sample size within each cell reduces statistical significance. The four panels of Figure 1.4 plot the difference-in-difference parameter α_d estimated with equation 1.11 separately for each group. Within each cell we see that immediately after plant closure the employment prospects of the old displaced workers (relative to the control group of non-displaced workers) are worse than those of the young , but improve over time. This improvement is particularly pronounced for white-collar workers. Interestingly the corresponding panels of Figure 1.5 for earnings show that these are the workers who seem to be more willing to offer larger wage concessions with the passage of time after plant closure.

One may also worry about the arbitrariness of the definition of young and old. So far a worker was defined as old if her age was greater or equal than 45. In Table 1.4 we explore a finer classification of workers with respect to age. This table presents estimates of the difference-in-difference parameters $\alpha_{u,l}$ in equation 1.12, in which the dummy OLD_i has been substituted by three dummies for the age groups 40-44, 45-49 and 50-55, relative to the reference

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group of 35-39 years old. The first set of estimates, based on all workers, show that all the action comes from the oldest age group. The 50-55 years old are the only ones that really suffer in term of employment in the first 5 years after plant closure (relative to the non displaced of similar age). But there is also clear evidence that they catch up and improve relative to the younger cohorts in the following 5 years, which for them are the last ones before retirement. In terms of wages, while for this age group there are no signs of wage concessions in the first 5 years after plant closure, a negative albeit insignificant estimate of the difference-in-difference parameter is obtained for the following period, which is consistent with our suggested interpretation of the evidence.

It could be argued that the results described so far have nothing to do with our suggested interpretation, being instead driven by a composition effect. At the moment of plant closure the two samples of displaced and non-displaced workers are matched according to observables and therefore their composition is very similar. But, later on, death, disabilities and retirement decisions may change the composition of the two samples in different ways, which might explain the pattern of observed result. This possibility, however, is not supported by the evidence displayed in Table 1.5 which reports the sample averages, by age group and displacement status, of the pre-plant closure characteristics of the workers who are observed with positive wages 5 to 10 years after plant closure. In each cohort, the "left-over" displaced and non-displaced workers appear to be pretty similar on average, suggesting that attrition has not affected in different ways the composition of the two samples

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in terms of observables.

A possible objection against our empirical strategy is the potential non-random selection of the displacement sample. Our definition of displacement includes all workers who stayed with their employer until the last quarter before the firm went bankrupt. If workers anticipate the plant's shut-down, they will search for a new job. Under such circumstances, our definition of displacement produces a negative selection of workers as only the least successful workers will be included in the displaced worker sample. This may not only cause a bias in our estimate of the consequences of plant closure, but it may also affect the implications of age on workers' job prospects and post-displacement earnings. To assess the significance of these arguments, we change the definition of displacement by also including ”early leavers” in this sample. Early leavers are workers who left the plant closure firm during the last half-a-year prior to the plant closure date (between quarters -2 and -1). The implicit assumption is that the information that the firm may go bankrupt is revealed within the last half a year prior to bankruptcy. Table 1.6 presents the results of this enlarged sample.

Table 1.6 shows that including early leavers in the extended displacement sample does not change our main result: During the five years following the plant closure date, older displaced workers suffer from a reduction in the employment probability which is almost 3 percentage points larger than the reduction in the employment probability of prime-age workers. During years six to ten following the plant closure date this picture is turned on its head with a more than 3 percentage points lower reduction in employment

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probabilities for displaced older workers as compared to displaced prime-age workers. Hence, just like in the baseline model, we conclude that older workers suffer from worse employment prospects than prime-age workers but this loss fades away with the passage of time from plant closure.

Table 1.7 presents results concerning age effects on post-displacement earnings. In our extended sample that includes early leavers as displaced, older workers suffer from very similar earnings losses after displacement than prime-age workers, both over the short term and the longer term. The point-estimates of the age-specific differences are quantitatively very small and statistically insignificant. Moreover, this result turns out rather robust and holds also in the fixed effect estimation and the Tobit estimation. While the LAD estimator indicates significantly higher losses for older workers, also here the estimated differences are negligible and amount to only 0.2 percent in the short run and 0.6 percent in the long run. In sum, we conclude that anticipation of job loss by early leavers is unlikely to lead to major biases in our baseline estimates.

A further potential objection against the results in the baseline model comes from changes in unemployment insurance rules during the period under consideration. Before August 1989, an unemployed person could draw regular unemployment benefits for a maximum period of 30 weeks provided that he or she had satisfied a minimum requirement of previous insurance contributions. In August 1989 the maximum benefit duration was increased to 39 weeks for the age group 40-49 and to 52 weeks for the age group 50 and older.[20] This might lead to biases in our estimation results in the employment

[20]For a study that looks at the implications of this policy change on unemployment durations see Lalive, van Ours and

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regressions. More generous unemployment insurance rules for older workers might lead to an increase in the likelihood of being found out of employment. If a job loss destabilizes a worker's future career, displaced workers are found more often out of employment than non-displaced workers.

However, being out of employment under more generous unemployment insurance rules might amplify the consequences of job loss. As a result, lower employment probabilities of older displaced workers might, partly, be caused by more generous unemployment insurance rules rather than the job-loss as such. To account for such potential upward biases in the estimated age-specific consequences of job loss, we estimate such effects separately under the situation where older workers and prime-age workers are subject to identical unemployment insurance rules; and under the situation where these rules are more generous for older workers. If it is true that more generous unemployment insurance rules reinforce the age-effects of job loss on future employment prospects' we should see a significant negative effect for older workers that are subject to the more generous rules of the 1989 reform for older workers.

Table 1.8 presents the results. Accounting for changes in unemployment insurance rules after 1989 does not have an impact on the results. While almost exactly the same age-specific effects of job loss emerge as in the baseline model, both in the short run and in the long run, we do not see any additional effect of this reform on age-specific effects on plant closure. Hence we conclude that our basic estimates turn out quite robust. There may be several

Zweimüller (2006).

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reasons why such additional unemployment-insurance effects do not materialize. First, the plant closures we consider in our sample did occur between 1982 and 1988. This means the unemployment spells that were caused by layoffs due to plant closure were not yet subject to the new unemployment insurance rules. Any effect of the new rules could work out only through recurrent unemployment at later stages. Our estimates indicate that, for any later unemployment spells, the reform affects prime-age workers and older workers to the same extent. A second reason follows from our empirical strategy. Our sample of prime-age workers was based on the criterion that a worker had to be between 35 and 44 years old at the date of plant closure. We then follow workers for the next ten years. However, many of the prime-age workers pass the age 50 threshold (and become eligible to a longer potential duration of benefit) during the years after plant closure. This mitigates any possible bias that may result from more generous unemployment insurance rules in the first place (by making the prime-age and older workers better comparable also along the unemployment insurance dimension).

1.7 Conclusion

Older workers are in general characterized by lower employment rates than prime age workers, but it is hard to disentangle the extent to which this age effect is due to supply or demand effects. In this chapter we use data for Austria to show that, immediately after a plant closure, elderly workers have lower re-employment probabilities as compared to prime-age workers in the

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same situation. After five years, instead, the old are able to catch up and reach relatively higher employment rates. These consequences can be understood as a combination of demand effects that prevail immediately after displacement and supply effects that kick in later. More specifically, in the light of our theoretical model, these results suggests that after plant closure older workers are considered less productive by the market but are at the same time reluctant to lower their reservation wages enough to ensure the same employability of the young. With the passage of time, the shorter time horizon of the old induce them to lower their reservation wage in order to find a job before retiring, and this explains the observed catching up.

These results were obtained with an estimation strategy that combines the advantages of exact matching to improve the comparability of treated and control subjects, with the advantages of differencing in panel data to control for remaining confounders captured by time invariant individual effects, cohort effects and time effects.

We believe that these results are relevant for the debate on the opportunity of increasing the retirement age in the presence of Pay-As-You-Go pension systems with an aging population. Increasing the retirement age may produce a fraction of individuals who are "too old to work but too young to retire".

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1.A Tables

Table 1.1: Descriptive Statistics by displacement status and cohort

	"$Young$"		"Old"	
	displ	non-displ	displ	non-displ
Female	.49	.49	.48	.48
Blue collar	.33	.33	.42	.42
Age (years)	40	40	49	49
	(2.8)	(2.8)	(3.2)	(3.2)
Tenure (days)	2797	2794	3330	3328
	(1558)	(1550)	(1651)	(1618)
Experience (days)	4205	4192	4502	4485
	(1100)	(1130)	(1021)	(1053)
Average daily wage (euros)	29.91	30.09	30.57	30.74
	(13.79)	(13.57)	(13.91)	(13.99)
Firm size	85	67	97	100
	(263)	(198)	(256)	(256)

Note: Sample averages with standard deviations in parentheses. All variables, except wage and firm size, are measured at the quarter immediately before (potential or actual) plant closure. The average daily wage is in nominal terms and measured 2 years before plant closure. Firm size is measured 3 quarters before plant closure.

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Table 1.2: Estimation results for employment

	OLS	OLS	OLS	FE
	(1)	(2)	(3)	(4)
OLD*PC*$Q^{-16,0}$	.002	.002	.002	
	(.002)	(.002)	(.002)	
OLD*PC*$Q^{1,20}$	-.029	-.029	-.03	-.031
	(.01)**	(.01)**	(.01)**	(.011)**
OLD*PC*$Q^{21,40}$	.034	.034	.034	.032
	(.012)**	(.012)**	(.012)**	(.012)**
PC*$Q^{-16,0}$	-.001	-.001	-.001	
	(.002)	(.002)	(.002)	
PC*$Q^{1,20}$	-.199	-.199	-.199	-.198
	(.006)**	(.006)**	(.006)**	(.006)**
PC*$Q^{21,40}$	-.11	-.11	-.11	-.109
	(.007)**	(.007)**	(.007)**	(.007)**
OLD*$Q^{-16,0}$	.01	.01	.002	
	(.001)**	(.002)**	(.002)	
OLD*$Q^{1,20}$	-.11	-.11	-.118	-.122
	(.004)**	(.004)**	(.005)**	(.005)**
OLD*$Q^{21,40}$	-.412	-.411	-.419	-.425
	(.007)**	(.007)**	(.007)**	(.007)**
$Q^{1,20}$	-.049	-.052	-.031	-.046
	(.003)**	(.003)**	(.003)**	(.002)**
$Q^{21,40}$	-.117	-.122	-.079	-.14
	(.007)**	(.007)**	(.007)**	(.004)**
Blue collar		-.013	.0002	
		(.005)**	(.005)	
Female		-.059	-.034	
		(.004)**	(.005)**	
Experience (at t=0 in years)			.006	
			(.0008)**	
Tenure (at t=0 in years)			.002	
			(.0005)**	
Firm size (in logs.)			.008	
			(.001)**	
Avg. daily wage (in logs.)			.031	
			(.005)**	
Const.	.96	.997	.689	.98
	(.029)**	(.028)**	(.042)**	(.002)**
Obs.	2465250	2465250	2465250	2465250
R^2	.251	.255	.26	.529
F statistic	227.24	228.512	215.131	2586.184

Note: Estimates based on equation 1.12 controlling for industry and location of firm (except for the specification with fixed effects which absorb all time invariant observables and unobservables). The dependent variable is a dummy for the employment status of the worker. Standard errors in parentheses.

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Table 1.3: Estimation results for earnings

	OLS	FE	TOBIT	LAD
	(1)	(2)	(3)	(4)
OLD*PC*$Q^{-16,0}$	.004		.006	.0008
	(.003)		(.001)**	(.001)
OLD*PC*$Q^{1,20}$	-.001	.004	-.001	.004
	(.009)	(.009)	(.001)	(.001)**
OLD*PC*$Q^{21,40}$	-.002	.003	-.01	.0001
	(.013)	(.013)	(.002)**	(.001)
PC*$Q^{-16,0}$	-.006		-.022	-.003
	(.002)**		(.0008)**	(.0006)**
PC*$Q^{1,20}$	-.051	-.052	-.068	-.027
	(.006)**	(.006)**	(.0008)**	(.0006)**
PC*$Q^{21,40}$	-.056	-.053	-.08	-.035
	(.007)**	(.008)**	(.0009)**	(.0007)**
OLD*$Q^{-16,0}$	.005		.005	.004
	(.002)*		(.0009)**	(.0006)**
OLD*$Q^{1,20}$	-.014	-.024	-.014	-.018
	(.003)**	(.003)**	(.0009)**	(.0006)**
OLD*$Q^{21,40}$	-.043	-.066	-.035	-.041
	(.007)**	(.006)**	(.001)**	(.0007)**
$Q^{1,20}$	.138	.247	.138	.228
	(.003)**	(.002)**	(.0009)**	(.0005)**
$Q^{21,40}$	.269	.497	.274	.474
	(.007)**	(.004)**	(.001)**	(.0005)**
Female	-.069		-.081	-.01
	(.004)**		(.0005)**	(.0004)**
Blue collar	-.09		-.107	-.04
	(.004)**		(.0005)**	(.0004)**
Experience (at t=0 in years)	-.007		-.008	0
	(.0007)**		(.00009)**	(.00006)*
Tenure (at t=0 in years)	.001		.001	-.0005
	(.0003)**		(.00005)**	(.00004)**
Firm size (in logs.)	.009		.013	.002
	(.001)**		(.0002)**	(.0001)**
Avg. daily wage (in logs.)	.753		.784	.902
	(.006)**		(.0005)**	(.0004)**
Const.	1.735	5.959	1.356	.625
	(.038)**	(.001)**	(.009)**	(.003)**
Obs.	1983948	1983948	1983948	1983948
R^2	.785	.87		
F statistic	1386.125	4678.09		

Note: Estimates based on equation 1.12 controlling for industry and location of firm (except for the specification with fixed effects which absorb all time invariant observables and unobservables). The dependent variable is the log-wage of the worker. Standard errors in parentheses.

Table 1.4: Estimation results for three age groups relative to the 35-39 age group

	Employment		Wages	
	$Q^{1,20}$	$Q^{21,40}$	$Q^{1,20}$	$Q^{21,40}$
40-44	.003	.005	-.009	-.026
	(.012)	(.015)	(.012)	(.015)
45-49	-.006	.01	-.002	-.011
	(.013)	(.017)	(.012)	(.016)
50-55	-.058	.064	0	-.016
	(.016)**	(.014)**	(.013)	(.025)

Note: Estimates of the difference-in-difference parameters $\alpha_{l,u}$ based on the fixed effects specification of equation 1.12 for three age groups relative to the 35-39 age group. The dependent variable is a dummy for the employment status of the worker. Standard errors in parentheses. All the other coefficients of each regression are omitted to save space.

Table 1.5: Weighted averages by age group and displacement status for the "left-over" workers

	Young		Old	
	Displ.	Non-Displ.	Displ.	Non-Displ.
Female	.48	.48	.39	.42
Blue collar	.35	.33	.41	.37
Age (years)	40	40	48	48
	(2.83)	(2.84)	(2.56)	(2.62)
Tenure (days)	2825	2807	3276	3315
	(1560)	(1551)	(1674)	(1607)
Experience (days)	4239	4207	4511	4475
	(1088)	(1121)	(1049)	(1099)
Average daily wage (euros)	30.08	30.24	32.19	32.40
	(13.66)	(13.55)	(14.23)	(14.52)
Firm size	87	69	119	85
	(62.98)	(201.95)	(306.74)	(228.83)

Note: Sample averages of the pre-plant closure characteristics, by age group and displacement status, for the workers who are observed with positive wages 5 to 10 years after plant closure. All variables, except wage and firm size, are measured at the quarter immediately before (potential or actual) plant closure. The wage is in nominal terms and measured 2 years before plant closure, firmsize is measured 3 quarters before closure. Standard deviations in parentheses.

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Table 1.6: Estimation results for employment including the "early leavers" in the sample

	OLS	OLS	OLS	FE
	(1)	(2)	(3)	(4)
OLD*PC*$Q^{-16,0}$	.0008	.0008	.001	
	(.002)	(.002)	(.002)	
OLD*PC*$Q^{1,20}$	-.027	-.027	-.027	-.028
	(.008)**	(.008)**	(.008)**	(.008)**
OLD*PC*$Q^{21,40}$	.032	.032	.032	.031
	(.009)**	(.009)**	(.009)**	(.009)**
Obs.	4051446	4051446	4051446	4051446
R^2	.266	.27	.275	.532
F statistic	413.163	418.215	392.225	5093.412

Note: Estimates of the difference-in-difference parameters $\alpha_{u,l}$ based on equation 1.12 for the "early leavers" sample. Specifications as in the corresponding columns of Table 1.2. The dependent variable is a dummy for the employment status of the worker. Standard errors in parentheses. All the other coefficients of each regression are omitted to save space.

Table 1.7: Estimation results for earnings including the "early leavers" in the sample

	OLS	FE	TOBIT	LAD
	(1)	(2)	(3)	(4)
OLD*PC*$Q^{-16,0}$	.003		.002	.0005
	(.002)		(.0009)	(.0006)
OLD*PC*$Q^{1,20}$	-.005	-.001	-.008	-.002
	(.006)	(.006)	(.0009)**	(.0007)**
OLD*PC*$Q^{21,40}$	-.009	-.004	-.021	-.006
	(.009)	(.009)	(.001)**	(.0008)**
Obs.	3232619	3232619	3232619	3232619
R^2	.789	.87		
F statistic	2119.144	8721.834		

Note: Estimates of the difference-in-difference parameters $\alpha_{l,u}$ based on equation 1.12 for the "early leavers" sample. Specifications as in the corresponding columns of Table 1.3. The dependent variable is the log wage of the worker. Standard errors in parentheses. All the other coefficients of each regression are omitted to save space.

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Table 1.8: Estimation results for employment with the “reform dummy”

	OLS	OLS	OLS	FE
	(1)	(2)	(3)	(4)
OLD*PC*$Q^{-16,0}$	.002	.002	.002	
	(.002)	(.002)	(.002)	
OLD*PC*$Q^{1,20}$	-.039	-.039	-.039	-.045
	(.011)**	(.011)**	(.011)**	(.011)**
OLD*PC*$Q^{21,40}$	.04	.04	.038	.007
	(.022)	(.021)	(.021)	(.017)
OLD*PC*$Q^{1,20}$*reform	-.005	-.005	-.004	-.007
	(.02)	(.02)	(.02)	(.014)
OLD*PC*$Q^{21,40}$*reform	-.008	-.008	-.007	.022
	(.023)	(.023)	(.023)	(.016)
Obs.	2465250	2465250	2465250	2465250
R^2	.252	.256	.261	.531
F statistic	210.088	207.102	193.86	1446.486

Note: Estimates of the difference-in-difference parameters $\alpha_{u,l}$ based on equation 1.12 for the“early leavers” sample. Specifications as in the corresponding columns of Table 1.2 including the additional interaction with the “reform dummy”. The dependent variable is a dummy for the employment status of the worker. Standard errors in parentheses. All the other coefficients of each regression are omitted to save space.

1.B Figures

Figure 1.1: Relative Difference in average pre-displacement wages between treated and matched controls

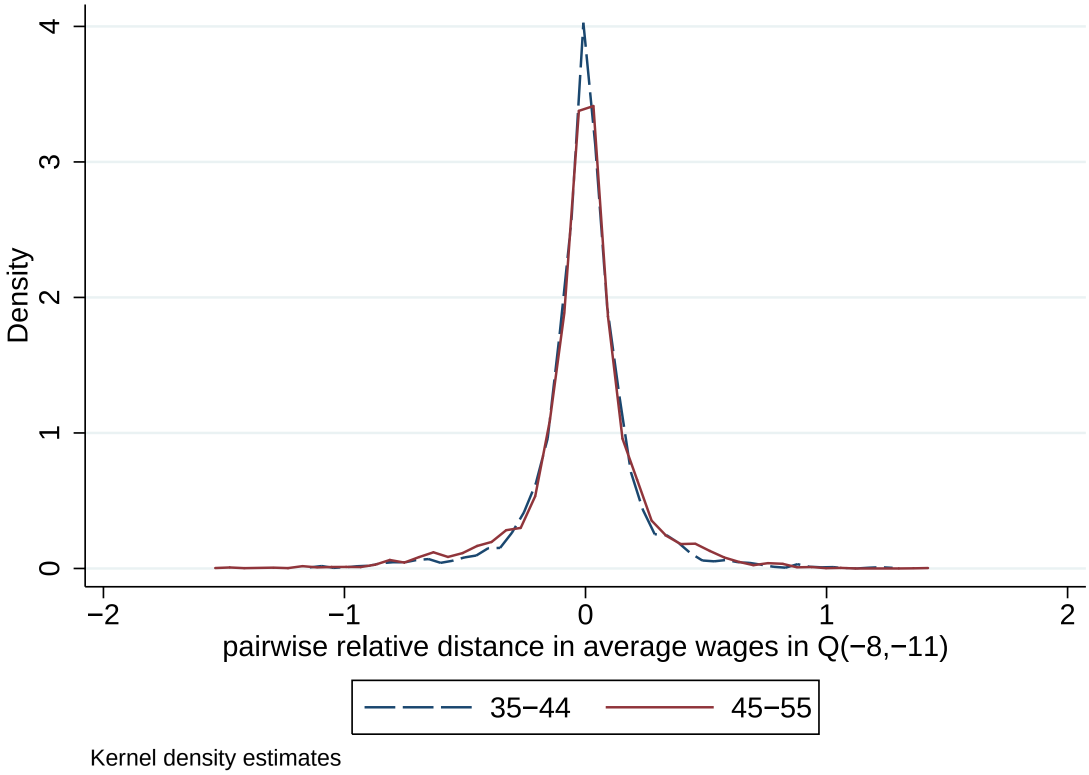

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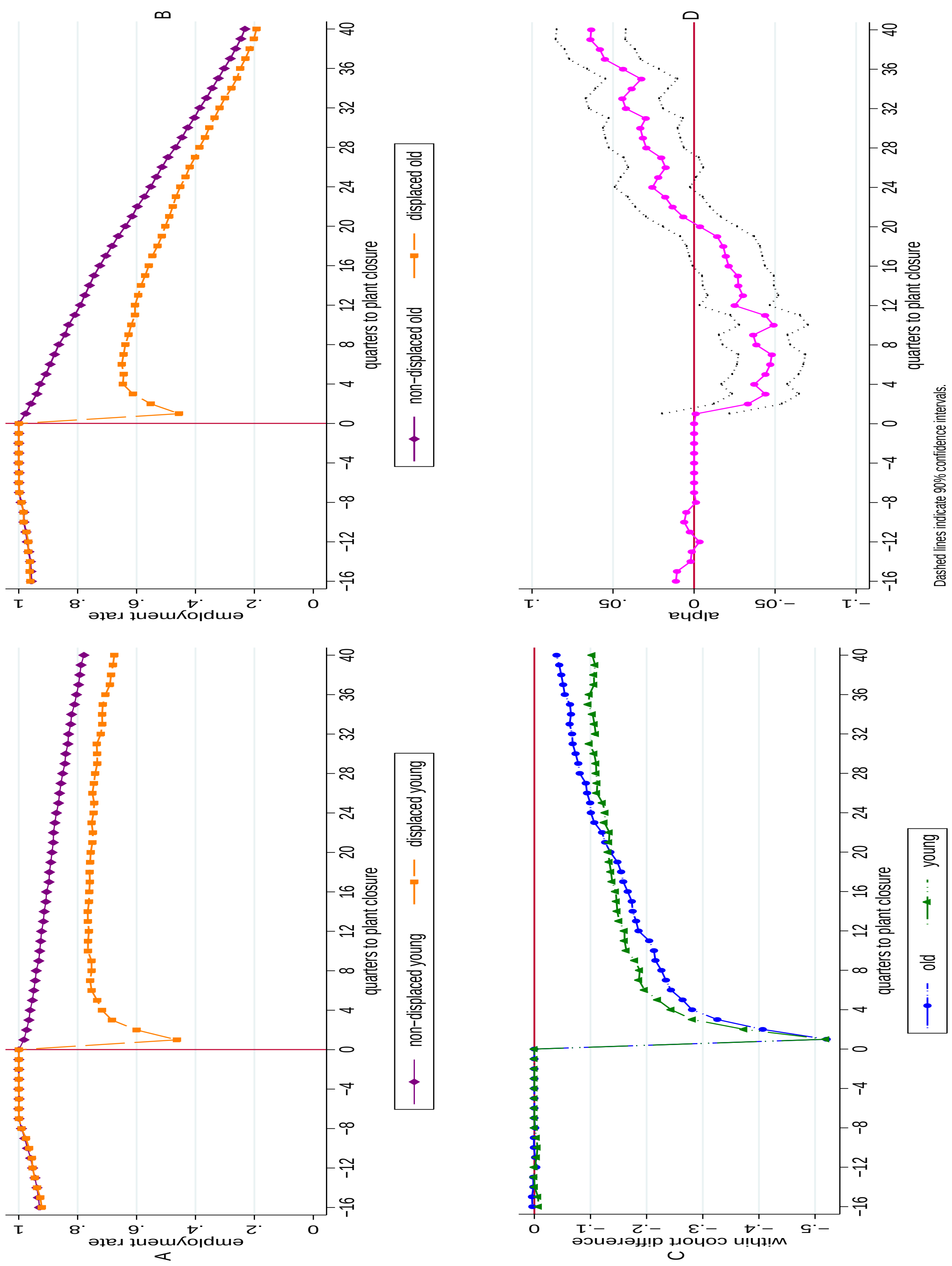
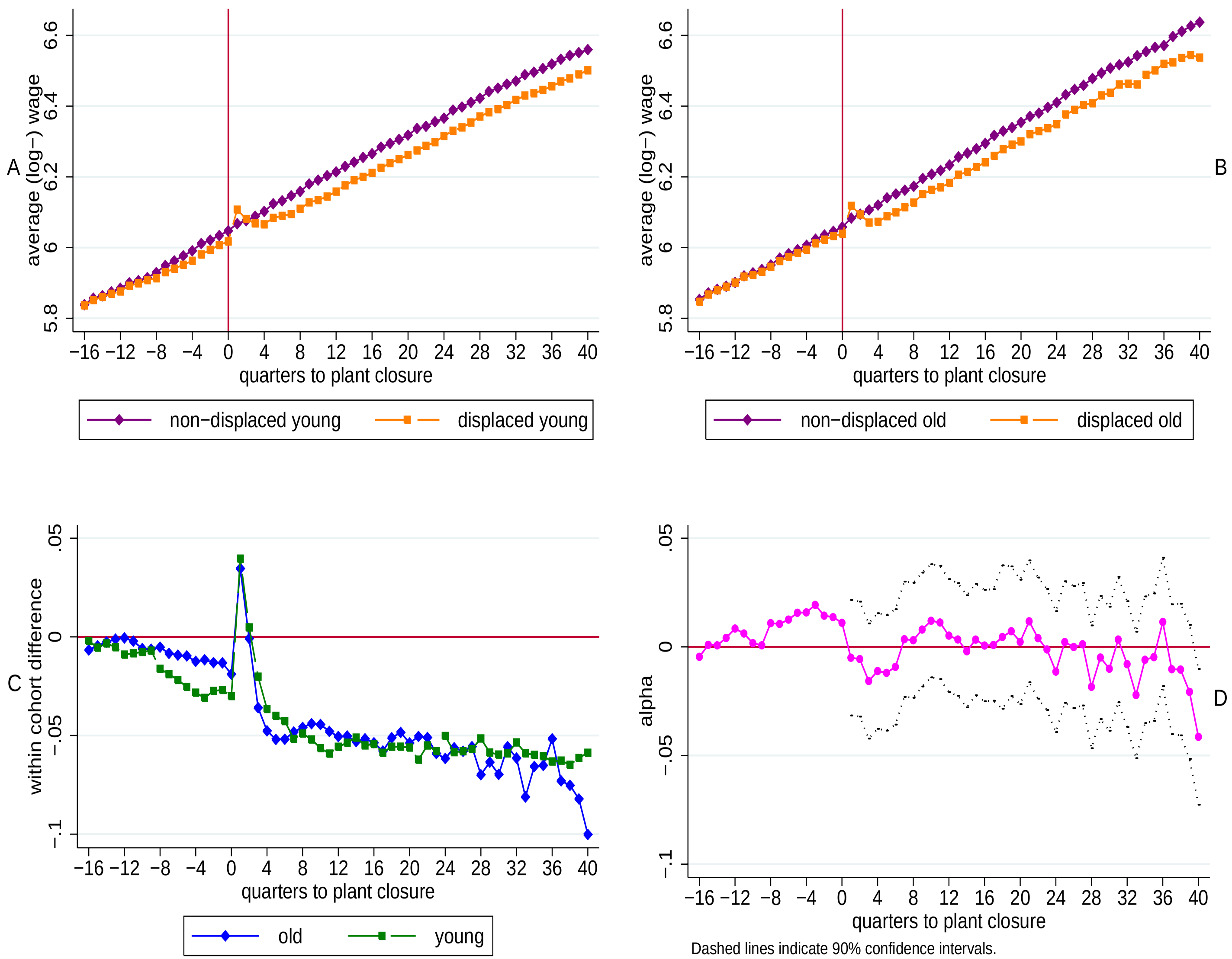
Figure 1.2: Descriptive Statistics on Employment



DOI: 10.2870/11131

Figure 1.3: Descriptive Statistics on Wages



Dashed lines indicate 90% confidence intervals.

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Figure 1.4: Main Effects on Employment by Gender and Occupation

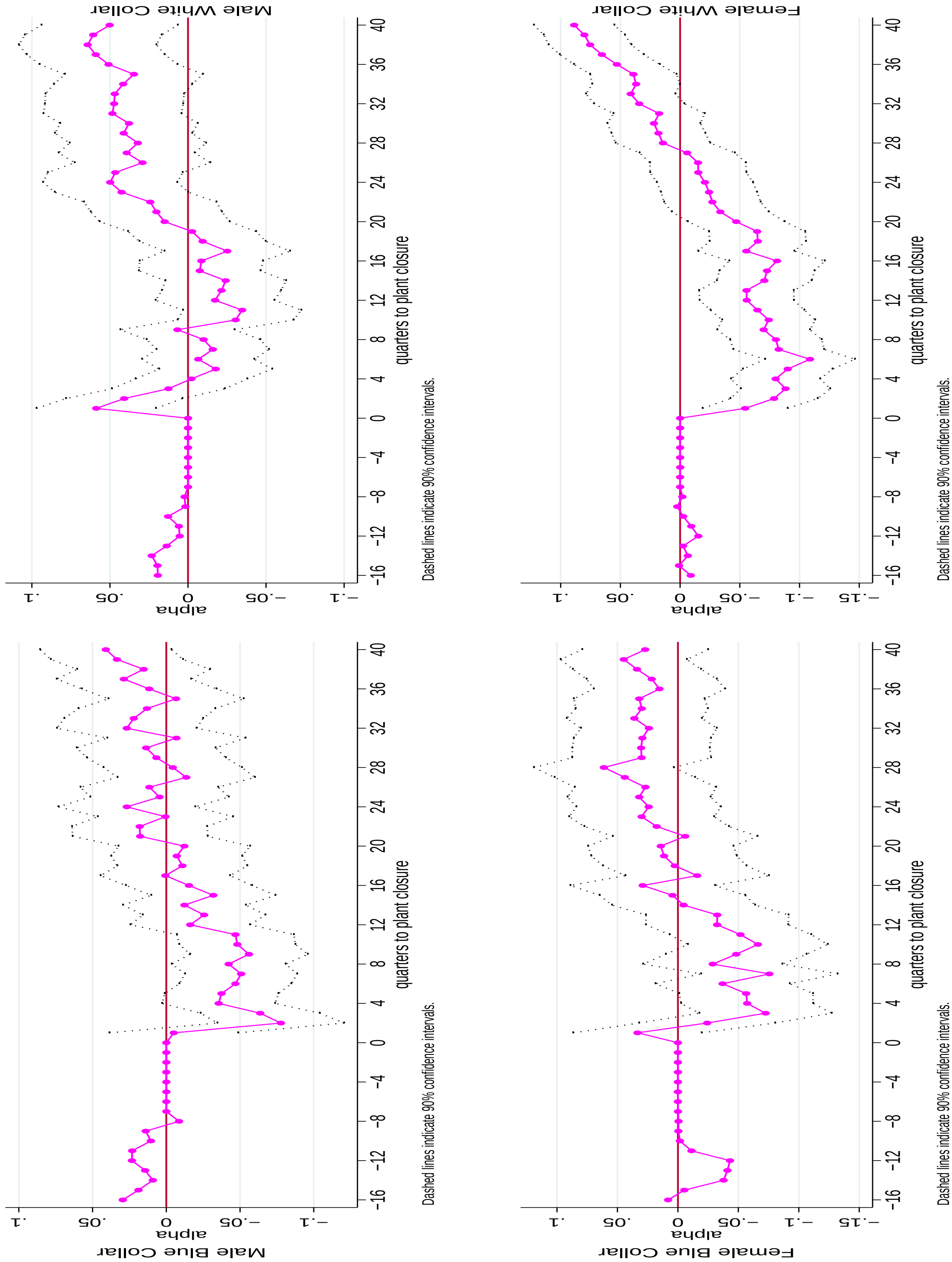

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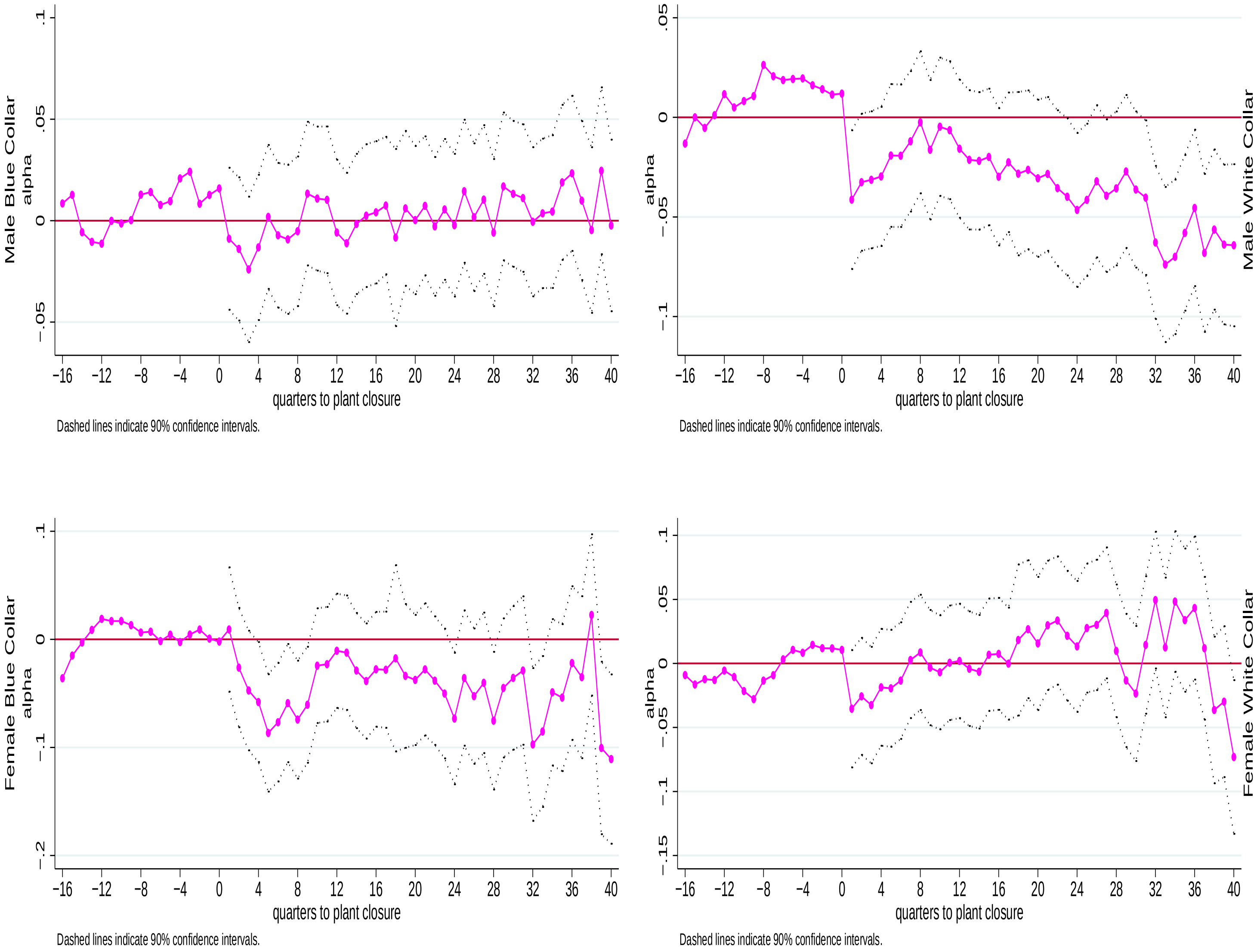
Figure 1.5: Main Effects on Earnings by Gender and Occupation



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Chapter 2

Labor Turnover before Plant Closure: 'Leaving the sinking ship' vs. 'Captain throwing ballast overboard'

2.1 Introduction

Displaced workers have been the subject of an extensive literature.[1] Introduced in the seminal paper by Jacobson et al. (1993), the standard specification to measure the effect of displacement is borrowed from the evaluation literature: the labor market performance of displaced workers (*treatment* group) is compared to the one of non-displaced workers (*control* group). Typically, this type of analysis requires administrative data, as long-term earnings and employment information must be available for displaced as well as non-displaced workers. A well-known challenge in these studies is the identification of involuntary job separations. The most popular strategy to overcome this difficulty is to focus on displacement-generating events such

[1] See Kletzer (1998), Fallick (1996) and Farber (1999) for reviews of the literature on displaced workers.

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as mass-layoffs or plant closures (the limit case of a mass-layoff). Separations observed at the moment of such events are assumed to be the result of an exogenous shock and, therefore, independent of a worker's quality. Thus displaced workers should be a random sample of the workforce.

However, as plant closures typically do not happen as a complete surprise to either management or workers[2], it seems realistic to assume that the ultimate shutdown of an establishment is preceded by a period in which both workers and management have time to react strategically. Knowledge of future distress will influence both firm's firing- as well as workers' quitting-decisions. The firm might choose to retain its most productive workers[3], while workers with relatively better labor market opportunities might choose to avoid displacement and quit before closure. As a consequence of this selection process the average cost of separation might also vary relative to the closure of a plant. However, as presumably both mechanisms, "workers leaving the sinking ship" and "the management throwing ballast overboard", are at work simultaneously[4], post-separation outcomes of early leavers might on average be better, equal or worse compared to post-separation outcomes of ultimately displaced workers.

In this study we investigate the labor turnover process in closing plants as well as differences in post-separation outcomes based on matched employer-employee data for the universe of Austrian workers. In particular, we test

[2]Advance notice legislation is the most obvious reason why information on impending lay-offs becomes available beforehand. See Addison and Portugal (1987), Jones and Kuhn (1995) and Ruhm (1994) for studies that investigate the effects of advance notice regulations.

[3]Several studies such as Farber and Gibbons (1996), Felli and Harris (1996) and Altonji and Pierret (2001) show that learning about workers' abilities occurs and that it influences the firm's employment decisions.

[4]For previous evidence on this, see Pfann and Hamermesh (2001) and Lengermann and Vilhuber (2002).

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empirically three key propositions linked to the selection hypothesis in the labor turnover process before plant closure. Firstly, we investigate whether post-separation outcomes differ significantly between early leavers and ultimately displaced workers. Secondly, if the selection hypothesis is correct, the group of early leavers might be associated with a different level of average productivity compared to ultimately displaced workers. We test this proposition by means of estimating pre-closure earnings regressions. Finally, we investigate the relationship between pre-closure earnings and the effect of displacement in order to understand whether differences between early leavers and ultimately displaced workers in terms of pre-closure earnings can explain differences in post-separation outcomes.

Although the literature on displaced workers is vast, few studies have empirically examined the labor turnover process in dying establishments. One recent paper analyzing changes in the composition of worker flows prior to displacement is that of Pfann and Hamermesh (2001). This study tests a model of two-sided learning using personnel data from Fokker Aircraft that cover the paths of layoffs and voluntary quitting through its bankruptcy. The basic idea of the model is, that parties to an employment relationship may learn about each other's intentions about ending the relationship by forming expectations based on the other party's prior behavior that ended similar relationships. Empirically Pfann and Hamermesh (2001) find that learning does occur. In particular, they find that workers with a lower firing probability during the closure process have longer job tenure, are males, have higher educational attainment, have technical/vocational schooling, are married, have taken more internal and external training courses and have a higher job evaluation. On the other hand, workers with lower quit propensities are between 35 and 50 years old, have longer tenure and are less well educated. In another paper, Lengermann and Vilhuber (2002) extend the signalling-model of Gibbons and Katz (1991) by introducing the idea that better workers may seek to avoid being viewed as being of average quality by leaving the firm prior to displacement, while those of lesser quality have an incentive to wait until displacement occurs. Using unemployment insurance records for the state of Maryland and proxying for worker quality by employing a measure derived from individual fixed effects stemming from wage regressions,[5] they find evidence for high-skilled workers leaving as well as firms laying off low skilled workers in periods before displacement. Bowlus and Vilhuber (2002), in another study, test the implications of a partial equilibrium search model with notice on impending displacement. Using data from US universal wage records, they find evidence that workers leaving a distressed firm before a mass-layoff have higher re-employment wages as opposed to ultimately displaced workers.

These findings foster the concern that focusing on the ultimately displaced workers might lead to biased estimates of the effect of displacement. A concern that, however, has been long recognized by the displacement literature. The standard approach to overcome this potential problem is to include all separations happening within a certain time window before the displacement generating event.[6] A strategy that faces the trade-off between neglecting early

[5]Following the technique pioneered by Abowd, Kramarz and Margolis (1999).

[6]Jacobson et al. (1993) focused on separators whose firms' employment in the year following their departure was more

leavers and including a considerable amount of normal workforce turnover. While the choice of a time window has been quite ad-hoc in previous studies, we go beyond the existing literature by providing a rationale for the choice of a particular window. The key assumption that guides us in this exercise is that post-separation outcomes should be indistinguishable between separations from closing plants and separations from non-closing plants if observed separations in closing plants are completely unrelated to the upcoming shutdown of the establishment. Applying this selection criterion reveals that only separations up to 2 quarters before closure should unequivocally be regarded as early leavers.

Moreover, we exploit the size of our available data set to increase the comparability between displaced and non-displaced workers by employing an exact-matching selection algorithm for adequate control subjects. We then extend the standard specification of Jacobson et al. (1993) by allowing for heterogeneous displacement effects between early leavers and ultimately displaced workers. Our findings show that early leavers have significantly better post-separation labor market prospects, both in terms of employment probability as well as earnings, as opposed to ultimately displaced workers. Moreover, pre-closure earnings regressions reveal that early leavers are associated with significantly higher pre-closure earnings even conditional on several individual and plant characteristics. Ultimately, we show that displaced workers belonging to the upper part of the pre-closure earnings distribution are as-

than 30% below their max in the 1970's. Bender, Dustmann, Margolis and Meghir (2002) choose a rigid time window of two years before plant closure. Eliason and Storrie (2004) introduce a flexible time window, that varies with plantsize, of up to three years.

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sociated with significantly higher post-separation employment probabilities. Taken together, the two latter findings provide an explanation for the better post-separation labor market performance of early leavers.

The chapter proceeds as follows. Section 2.2 discusses the common definition and practice of measurement of displacement effects and formulates the key propositions tested in this chapter. In Section 2.3 we describe the data and the sample selection. Section 2.4 provides descriptive evidence on pre-closure characteristics and post-separation outcome variables of separators from closing establishments. Estimation methods to test the main hypotheses and results are presented in Section 2.5. Section 2.6 concludes.

2.2 Definition and Measurement of Displacement Effects

The formal definition of displacement costs proposed in Jacobson et al. (1993) is given by

$$E(y_{it}|D_{i,s} = 1, I_{i,s-p}) - E(y_{it}|D_{i,v} = 0 \; \forall \; v, I_{i,s-p}), \tag{2.1}$$

where y_{it} denotes the earnings of worker i at date t and $D_{i,s} = 1$ if worker i was displaced at date s (and $D_{i,s} = 0$ otherwise). The information available at date $s - p$ is given by $I_{i,s-p}$ and p is sufficiently large that the events that eventually lead to displacement would not have begun by date $s - p$.

The most straightforward specification of a statistical model to estimate earnings losses corresponding to the definition in equation (2.1), that is presented in Jacobson et al. (1993), reads as follows

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$$y_{it} = \alpha_i + \gamma_t + \sum_{k \geq -m} D_{it}^k \delta_k + \epsilon_{it}. \tag{2.2}$$

This model represents workers' earnings histories (y_{it}) and identifies displacement costs with a subset of the model's parameters (δ_k). The specification allows the pooling of information for workers displaced at different periods, by introducing a set of dummy variables for the number of quarters before and after worker's separation, D_{it}^k, where $D_{it}^k = 1$ if, in period t, worker i had been displaced k quarters earlier (or, if k is negative, worker i was displaced $-k$ quarters later). Moreover, worker's earnings depend on some controls for calendar time effects (γ_t) and individual fixed effects (α_i).

Taking this model to the data involves several difficulties. First, it typically requires administrative data in order to obtain information on long-term labor market outcomes of displaced as well as non-displaced workers. The use of administrative data, however, normally implies the shortcoming of having no information about the cause of an observed separation. The most popular strategy to overcome this problem is to focus on separations occurring at the moment of displacement-generating events such as mass-layoffs or plant closures, which can be identified in matched employer-employee data by reductions in plant-/firm-level employment. To cope with the possibility of displacements happening prior to the identified displacement generating event, it is common practice to include all separations observed within a certain time window before the actual event. Note, therefore that this standard specification encompasses two types of displaced workers: "early leavers" (those

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who separated before the displacement generating event) and ”ultimately displaced workers” (those who remained employed until the bitter end). Hence, the set of dummy variables identifying displaced workers in equation (2.2) could be decomposed into $D_{it}^k = UD_{it}^k + EL_{it}^k$, where UD_{it}^k and EL_{it}^k have an identical interpretation as D_{it}^k with the additional distinction that UD_{it}^k identifies ultimately displaced workers, while EL_{it}^k identifies early leavers.

Incorporating this definition in equation (2.2) results in the following expression

$$y_{it} = \alpha_i + \gamma_t + \sum_{k \geq -m} (UD_{it}^k + EL_{it}^k)\delta_k + \epsilon_{it}. \tag{2.3}$$

This chapter now proposes the simple idea that displacement effects ($\delta's$) vary according to the timing of separation relative to the closure of a plant. In particular, displacement effects are different for early leavers and ultimately displaced workers. Making this distinction is motivated by economic theory. Given advance knowledge about the upcoming event, a search model of the labor market implies that such knowledge lowers the value of a given employment relationship as the probability of ending in unemployment increases.[7] This, in turn, lowers the worker's reservation wage and increases a worker's search intensity. If workers are heterogeneous with respect to their outside opportunities, then workers with better labor market prospects might engage more intensively in on-the-job search, receive more job offers and consequently have higher quit rates.[8] On the other hand, a negative de-

[7] The search framework is typically used in studies examining the effect of advance notice of job-displacement. See Ruhm (1994), Friesen (1997) and Bowlus and Vilhuber (2002).

[8] This study takes the point of view that any separation -whether a layoff or a quit- should be included in the treatment group if the separation is related to the upcoming closure. A distressed firm might, for instance, enforce wage cuts. A worker, who would have normally remained in the firm, might therefore quit.

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mand shock for the firm's product results in reductions in the demand for labor. If firms have discretion on whom to lay off and private knowledge about workers' "true" productivity, the firm chooses to lay off less productive workers first, who are presumably associated with relatively bad labor market prospects. In sum, both mechanisms suggest that a selection on individual characteristics exists in the labor turnover of distressed firms. Empirical evidence presented in Pfann and Hamermesh (2001) and Lengermann and Vilhuber (2002) supports this selection hypothesis. Consequently, displacement effects of early leavers and ultimately displaced workers might vary due to this selection process based on workers' characteristics.

We formulate this potential implication of selection in the turnover process of closing plants as a testable proposition:

Proposition 1 *Displacement effects vary according to the timing of separation relative to the closure of a plant. In particular, workers separating early in the closure process face different displacement costs as opposed to ultimately displaced workers:*

$$\delta^{EL} \neq \delta^{UD},$$

where, omitting any subscript indicating the distance to separation, δ^{EL} *and* δ^{UD} *refer to the effect of displacement for early leavers and ultimately displaced workers, respectively.*

Proposition 1 states the first key hypothesis this study aims to test. However, even if observed displacement effects differ between early leavers and ultimately displaced workers, these differences could be due to reasons other than selection based on workers' characteristics. While previous studies have investigated differences between early leavers along various dimensions, we limit our focus to differences in pre-closure earnings. Acknowledging the limited capability of earnings to proxy for workers' productivity, we nevertheless expect earnings to be positively correlated with individual productivity.[9] Hence, if selection based on productivity-related worker characteristics exists, we might see differences between early leavers and ultimately displaced workers in terms of pre-closure earnings. However, as presumably both mechanisms, firms' laying off less productive workers and workers with better labor market prospects quitting, are at work simultaneously, the two channels might offset each other in such a way that on average no differences in pre-closure earnings exist.

Thus, we expect to see differences in average pre-closure earnings only if one selection mechanism "dominates" the other. To test for this form of dominance, we formulate the following testable proposition:

Proposition 2 *Average pre-closure earnings of displaced workers vary according to the timing of separation relative to the closure of a plant. In particular, workers separating early in the closure process are associated with different levels of average pre-closure earnings compared to ultimately displaced workers.*

Ultimately, differences in average pre-earnings levels between early leavers and ultimately displaced workers can serve only as a potential explanation for

[9]The standard assumption that labor earns its marginal product might be violated for several reasons such as implicit incentive contracts or union bargaining.

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differences in post-separation outcomes between these groups, if pre-closure earnings levels are related to the effect of displacement. Consequently, it remains to test whether displacement effects are correlated with pre-closure earnings levels:

Proposition 3 *Pre-closure earnings are correlated with the effect of displacement. In particular, workers belonging to the upper part of the pre-closure earnings distribution are associated with different costs of displacement as opposed to workers' positioned at the lower end of the distribution.*

Testing the validity of the latter two propositions could shed some light on the link between selection in the labor turnover process before plant closure and differences in displacement effects relative to plant closure.

2.3 Data Description

The data stems from the *Austrian social security database* (ASSD). The data set includes the universe of private sector workers in Austria covered by the social security system. All employment records can be linked to the establishment in which the worker is employed. It contains detailed information on individuals' employment and earnings histories as well as certain individual characteristics. Daily employment and monthly earnings information is extremely reliable, because social security tax payments for firms as well as benefits for workers hinge on these data.[10] Monthly earnings are top-coded, which applies to approximately 10% of workers. We transformed monthly

[10] See Hofer and Winter-Ebmer (2003) for a description of the data set.

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gross earnings in daily wages dividing them by effective employment duration in each month of observation. Furthermore, the data includes information on employers such as geographical location, industry and size of the establishment.

The data set covers the period from 1978 until 1993 at a quarterly frequency, where the 10th of February, May, August and November serve as reference dates for the data collection. This setup implies that an individual is recorded as employed in a given quarter only if she is employed at the corresponding reference date. We concentrate on workers employed in the period 1982 to 1988 - who are in the risk set for a plant closure in this period; this allows us to observe the workers in detail 5 years prior to bankruptcy and 5 years afterwards.

The ASSD contains no direct information on plant closures. Following best practice in the displacement literature we identify a plant closure by the disappearance of a plant identifier. Each establishment has an employer social security number. Hence, a shutdown of an establishment in the data occurs when the employer identifier ceases to exist. As the unit of analysis is a plant as opposed to a firm, the possibility remains that a disappearance of an establishment identifier reflects re-organization or takeovers. To avoid including these “false plant deaths” we impose the following restriction: A plant is coded as a closing plant at the reference date t if two conditions are satisfied: (i) The plant identifier disappears during the three months following the reference date t (not observed anymore at $t + 1$) and does not re-emerge during

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the following year[11]. (ii) Less than 50% of the employees of an establishment find a new employment relationship under the same, new establishment identifier.[12] The latter condition minimizes the inclusion of "false plant deaths", but might eliminate also some true plant closure, where large groups of workers move "together" from one dying firm into the same new firm.[13]

The sample selection follows closely the one applied in Ichino, Schwerdt, Winter-Ebmer and Zweimueller (2006). The sample contains workers who fulfill the following conditions, at least at one of the quarter reference dates from 1982 to 1988: (i) Workers from plants not belonging to the construction and tourism industry. (ii) Workers from plants that once had at least 5 workers between 1978 and 1988. (iii) Blue and white collar workers with at least one year of tenure. (iv) Workers between 35 and 55 years of age.

The first two criteria are meant to exclude seasonal employment and establishments without basically any dependent employees. The latter two criteria should ensure that all workers present similar legal requirements for layoff. Low tenure workers and older workers might be easier to layoff due to probation periods or early retirement regulations.

The setup described above allows us to identify 4,703 closing plants between 1983 and 1988. Table 2.1 shows the incidence of plant closure by quarter and year. It reveals a clear seasonal pattern of plant closures. Almost one third of all closures occur in the last quarter of a year. The number of closures per

[11]This condition is set to one year, because the plant identifiers are assigned anew after two years.

[12]Workers from such establishments are coded as "ambiguous" and are neither in the treatment nor the control group.

[13]This might be especially relevant in the European context, because of legal requirements before mass-layoffs such as "social plans". In this case the displacing firm might have gone through extraordinary efforts to secure re-employment of its workers at other firms.

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year increases slightly during the 1980s. The distribution of plant closures over the nine federal states of Austria is displayed in Table 2.2. Almost one third of all closures happen in Vienna, the biggest and economically most powerful province of Austria.

The upper panel of figure 2.1 plots the total number of employees in all plants closing between 1985 and 1988 against quarters relative to closure.[14] While total employment decreases over all three years before closure, it becomes apparent that the number of separations increases sharply in the last year before closure. In fact the number of employees more than halves from 28296 one year before closure to 12126 workers just before the closure. This drastic decline suggests that some of these separations are related to the upcoming closure of the establishments.

The bottom panel shows two examples of employment trends at the plant level before closure. Broadly speaking we observe two types of closing establishments in the data. Type A, represented by the lower left figure, shows no or a slow decline in total employment before closure. Type B (lower right figure) is characterized by sharp stepwise downsizing in the quarters just before closure. Especially the latter type gives reason to believe that displacement (or closure-related separations) happens even several quarters before the ultimate closure.

Finally, it is worth noting another point at this stage. The figures on total employment in figure 2.1 are based on a generated variable that counts all employees in the social security records associated with the respective plant

[14] Note that total employment in figure 2.1 refers only to a subset of the 4,703 closing plants. Namely to all plants closing between 1985 and 1988 for which information on plantsize is available for all 12 quarter before closure.

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identifier. However, not all employees fulfill the selection criteria outlined above. Moreover, as the final analysis is conducted based on an exact matching procedure (see section 2.5), some workers, although fulfilling the above criteria, could not be matched to a control and, therefore, are not included in the empirical analysis. The dotted lines in the lower panel of figure 2.1 indicate the number of workers included in the empirical analysis. Notably, the number of workers included in the empirical analysis shows a more stable pattern before closure than total employment does. This reflects that a significant number of separations before closure include low tenure workers or workers not in the age group between 35 and 55.

2.4 Descriptive Statistics on Separations before Plant Closure

It is common practice in the displacement literature to include also separations happening within a certain time-window prior to the displacement-generating event. While this reduces the possibility of neglecting early leavers, it increase at the same time the chances of including a considerable amount of normal workforce turnover. Thus, we first analyze separations happening before plant closure to detect potential patterns that might distinguish plant-closure-related separations from normal turnover. In the following, we therefore present various descriptive statistics for different separators groups distinguished by the timing of the separation relative to the closure of the plant.

Figure 2.2 shows changes in average workforce characteristics in all closing

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plants before closure. All variables are held constant at their level three years before closure. Any variation, therefore, stems from changes in the composition of the workforce.[15] The top left panel reveals that the share of female workers remains relatively stable at around 49% during quarters 12 to 4 before closure, but increases during the last year before closure by 6 percentage points. This indicates that early leavers are mainly men. Furthermore, early leavers are also mainly blue collar workers, which can be seen from the top right panel. The share of white collar workers in dying establishments jumps up by 12 percentage points in the last year of existence. Before this period, the share of white collars is steadily declining.

The higher share of blue-collar workers might be explained by institutional factors. In particular, the legislation on advance notice varies for blue and white collar workers in Austria. Depending on age and tenure, blue collar workers receive an advance notice of displacement up to two weeks before dismissal. White collar workers, on the other hand, receive such a notice between 1.5 and 5 months before dismissal.[16] Hence, if economic difficulties make downsizing necessary, it is less difficult to layoff blue-collar workers.

The middle panels show average experience and job tenure in days. Average experience rises up to the fourth quarter before closure by 30 days reflecting the fact that these very early separators have below average experience levels. During the last year of the plant's existence, more experienced workers tend to leave the plant, so that average experience again declines by 20 days. Average tenure, on the other hand, increase over the entire three year period before

[15]New hires are not included. Hence, compositional changes are solely induced by separations.

[16]See OECD (2005) for an overview of employment protection legislation in several OECD countries.

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closure. However, while average tenure grows by around 110 days from quarter 12 to quarter 4 before closure, the increase in tenure almost vanishes to only 6 days during the last year before closure. Recall that tenure refers to the level three years before closure and that newly hired worked as well as workers with less than one year of tenure are not included in this average tenure measure. Hence, the initial increase in average tenure is not surprising as a correlation between the probability of leaving the firm and the tenure level is economically intuitive. Models including firm-specific human capital, heterogeneous job-matches or wage-seniority would imply such a correlation.[17] This makes it the more interesting to see that workers leaving shortly before plant closure are not characterized by below average tenure levels.

Average age is plotted in the lower left panel. It decreases slightly over the entire pre-closure period. No different pattern is apparent during the last year before closure. Hence the observed decrease in the average work experience and the flattening of the increase in tenure during this period is not a mere by-product of an age-effect in the sense that older workers are leaving in increasing numbers shortly before closure.

Descriptive statistics on daily earnings can be seen in the bottom right panel. Average daily earnings in euros at their level 3 years before closure are plotted against time relative to closure. Initially average earnings increase slightly by 30 cents from quarter -12 to quarter -4. Thereafter, up until closure, earnings drop by 80 cents, which roughly corresponds to a 2.5 per cent earnings drop. This indicates that early leavers are associated with higher average earnings

[17] See Becker (2000), Jovanovic (1979) and Lazear (1979) for examples of such models.

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compared to ultimately displaced workers.

To analyze the short-run effect of early separation we focus on the labor market status of separators in the first quarter after leaving the closing plant. As earnings data is available, we are able to evaluate a new employment relationship based on the associated daily wage. That is, we classify the new job relative to the previous job. In particular, we categorize the employment status in the first post closure quarter according to three different states: (i) not employed, (ii) employed with a lower wage, (iii) employed with a higher or equal wage.

One advantage of looking at the directions of separations is that it provides some evidence on the cause of separation, namely on whether the employment relationship ends because of a layoff or quit. Typically it is impossible to distinguish between these two causes in non-survey data. However, when observing an individual employed in a higher wage job immediately after separation it seems likely that this individual quit their previous job. On the other hand, observing an individual accepting a lower wage or not being employed might indicate a layoff.

Figure 2.3 displays the percentage of workers ending up in either of the three states in the first quarter after separation by separation groups. First, notice that the distribution over the 3 outcomes varies quite a bit in the quarters -12 to -6 before closure with the results for quarter -10 being an outlier. However, as these separations occur at least one-and-a-half years before the closure of the plant, it is unlikely that a huge fraction of them is related to the closure event.

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Starting from quarter -6 until quarter -1 a downward trend in the percentage of separators not employed immediately after separation becomes apparent. While 66 % of all separators leaving at quarter -6 end up not being employed in the next quarter, only 44 % of the separators leaving the distressed establishment in quarter -1 share the same fate. However, among those who stayed until the end 59% end up in non-employment in the first quarter after plant closure.

Analogously, the fraction of separators immediately accepting a lower paid job increases until quarter -1 (up to 35 %) and then drops back for ultimately displaced workers (18 %). Interestingly, no such pattern exists for workers finding a higher paid job immediately.

This already provides some first evidence that in the short run early leavers perform better compared to ultimately displaced workers. To investigate this aspect further, we conduct a survival analysis. Figure 2.4 plots the Kaplan-Meier estimates for survival in non-employment after separation by quarter of separation relative to plant closure. However, in light of the earlier descriptive results we focus in the following on separations happening in the last year before closure. The graph reveals that while there appears to be no significant difference in terms of search time between ultimately displaced workers and early leavers leaving the closing plant in quarter -4 and -3, separators in quarters -2 and -1 find new employment more quickly. 75 % of early leavers leaving at -1 manage to find a new job within 2 quarters after separation and only 10 % of this group remain non-employed within the first 4 years after separation. In contrast, among the ultimately displaced workers around 30 %

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remain non-employed during the first 2 quarters and still roughly 15 % during the first 4 quarters after after closure.

To understand how overall employment probabilities change by quarter of separation relative to plant closure, figure 2.5 shows average employment by separator groups in the 16 quarters before and 20 quarters after separation. While prior to separation no significant differences exist, the employment probabilities of late early leavers (d=-1 and d=-2) dominate the respective probabilities of the other three groups in the first 20 quarters after separation.

Finally figure 2.6 provides unconditional evidence on the evolution of nominal log daily earnings, conditional on being employed, by separation groups. Obviously, changes in this measure may occur because of changes in real earnings, in inflation and because the set of employed workers may change. The evolution of earnings is qualitatively very similar for all separation groups. Over time, nominal daily earnings increase strongly, mainly reflecting growth in real earnings and inflation. Three aspects are particularly worth mentioning: firstly, at all quarters the level of earnings is the lowest for the group of ultimately displaced workers. However, the difference with any other group is always quite small, never exceeding more than .1 log points. Secondly, all groups have a spike in the evolution of wages directly after separation. This clearly reflects selectivity as the workers who are able to find a new job immediately are probably also the more productive ones. Thirdly, no higher earnings loss due to separation is obvious for ultimately displaced workers as opposed to early leavers conditional on being employed.

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2.5 Estimation and Results

Borrowed from the evaluation literature, the seminal study of Jacobson et al. (1993) introduced the idea of studying the effects of displacement in a difference-in-difference setup. This way the effects of an involuntary job-loss are not identified by a simple pre/post comparison, but by the difference in differences when compared to pre/post outcomes of an adequate control group. The post outcome of the control group should conceptually serve as an estimate for the counterfactual outcome that would have occurred in the absence of displacement. To account for any remaining heterogeneity in the composition of the displaced and the non-displaced and to isolate the pure effect of displacement, individual fixed effects are included in the analysis to capture any time-invariant differences.

We go beyond this approach by employing an exact matching algorithm to further increase the comparability of treated and control subjects. Selection of a control group based on exact matching is feasible in this study given the enormous size of our data set. One advantage of exact matching is the creation of a common support for the treatment and control group. That is, we extract from the administrative records only those controls for a given treated, who have identical (or almost identical) characteristics. The characteristics with which we perform exact matching are gender, age, broad occupational status, industry and region of the employer. Moreover, we conduct almost exact matching based on quartile groups on continuous variables such as firm size and average daily earnings one year prior to displacement. Figure 2.7

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visualizes how the matching algorithm works. Note two further points: (i) Besides being not employed in a closing plant, a valid control has to fulfill also the sample selection criteria described in section 2.3. (ii) The matching is performed at the last quarter the treated was observed being employed in the closing firm.

Before turning to the estimation of displacement effects, we exploit this setup by comparing post-separation outcomes of early separators from closing plants with those of separators from surviving plants. This provides a test for the validity of including early leavers in the displacement group. The rationale behind this exercise is that if observed separations prior to the closure of a plant were due to "normal" labor turnover, which is not related to the upcoming plant closure, then post-separation outcomes should be indistinguishable from post-separation outcomes of separations happening in non-closure plants.

Equation 2.4 presents an empirical model to measure differences in post-separation outcomes between separators from closure and non-closure plants:

$$Y_{it} = K_{it}^{1,20} \tilde{D}_i^d \delta^d + K_{it}^{1,20} \kappa + \alpha_i + \theta_t + \epsilon_{it}. \tag{2.4}$$

Y_{it} represents the outcome variable of interest, α_i is an individual-specific fixed effect, θ_t captures the effect of calendar time and ϵ_{it} is an error term uncorrelated with all variables appearing on the right side of the equation. $K_{it}^{1,20}$ indicates the period relative to separation. For simplicity we don't estimate a single parameter for each quarter k relative to closure, but rather restrict

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our attention to the average effect over the first 5 years after separation. The dummy variable $K_{it}^{1,20}$ takes the value one if the separation happened up to 20 quarters before ($0 < k \leq 20$) and zero otherwise.

Separators from closing plants are identified by a dummy variable $\tilde{D}_i^d$. The dummy $\tilde{D}_i^d$ takes the value one if individual i separated from a closing plant. The superscript d indicates the quarter of separation relative to the closure of the plant. We estimate equation (2.4) separately for all separations happening up to 4 quarters before the plant is last observed in the data. That is, separately for d= -1, -2, -3 and -4.

The control groups are selected based on the matching algorithm presented in figure 2.7. For each separator from a closing plant only separators from non-closing plants with almost identical characteristics are selected as controls. The quality of this matching procedure is shown in table 2.3. For all 4 pairs of treatment and control groups mean differences in observed characteristics are relatively small. Only tenure and plantsize show somewhat larger differences. However, a difference in average tenure of up to 200 days is relatively small compared to overall average values of around 2500 days. While tenure is not a matching variable, treated and control have been matched based on quartiles of the plantsize distribution. Nevertheless, for all pairs the average plantsize is considerably higher as for separators from non-closing plants. However, as plantsize is measured at the quarter immediately before separation, the plantsize of closing plants is most likely already affected by the circumstances leading to the shut-down of the plant.[18]

[18]Matching on plantsize might, therefore, appear questionable. However, the main results remain robust when no match-

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Table 2.4 presents the results from estimating equation (2.4). The upper panel of table 2.4 shows estimation results with an employment dummy as dependent variable, while the lower panel shows estimation results with log daily earnings conditional on employment as the outcome variable. Controlling for individual fixed effects and calendar time effects, row 2 of table 2.4 reveals negative separation effects in terms of employment probabilities for all four groups. Estimated separation effects range from -.37 to -.42 indicating a common loss in terms of employment probabilities in the first 5 years after separation of around 40 percentage points.

The estimated interaction effect $K^{1,20}\tilde{D}^d$ can be seen in row 1 of table 2.4. The results reveal a significant effect of separating from a closure plant that goes beyond the isolated effect of separation for early leavers separating in d equal to -1 or -2. While separators leaving closing plants 3 and 4 quarters before closure are indistinguishable from normal separations, the estimated coefficients indicate a reduced loss in terms of employment probabilities for early leavers separating 1 or 2 quarters before closure of 9.4 and 7.2 percentage points, respectively. In terms of daily earnings no significant differences between separators form closing and non-closing plants can be found.

The results of this exercise provide evidence that at least a high fraction of all separations happening during the closure process of a plant are directly related to the upcoming closure and, therefore, should be included in the treatment group in the analysis of displacement effects. Given the results presented above, we feel confident in including at least all separations happening

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ing on plantsize or matching on plantsize one year before closure is performed.

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up to two quarters before closure into the displacement group.

We can now define more specifically a dummy variable identifying early leavers. Let EL_i take the value one if individual i is observed working in a closing plant in the two last quarters before the plant closes ($-2 \leq d < 0$, but who is not employed at the plant at the very last quarter ($d = 0$) the plant is observed in the data and takes the value zero otherwise. Analogously we (re-)define the dummy variable D_i to identify all workers separating due to a plant closure. This includes the above defined group of early leavers as well as ultimately displaced workers.

With this notation in mind we are now able to test Proposition 1. Equation (2.5) defines a model to measure the effects of displacement that allows for heterogeneous displacement effects:

$$Y_{it} = K_{it}^{1,20}\kappa + K_{it}^{1,20}D_i\delta + K_{it}^{1,20}D_iEL_i\gamma + K_{it}^{1,20}EL_i\xi + \alpha_i + \theta_t + \epsilon_{it}. \quad (2.5)$$

We again measure these effects separately for employment probabilities and earnings. Y_{it} denotes the outcome variable of interest. As before, α_i is an individual-specific fixed effect, θ_t captures the effect of calendar time, $K_{it}^{1,20}$ identifies the 5 years time period after separation and ϵ_{it} is an error term uncorrelated with all right-hand-side variables.

Equation (2.5) extends the model defined in equation (2.4) by the two interaction effects $K_{it}^{1,20}D_iEL_i$ and $K_{it}^{1,20}EL_i$. The latter effect is supposed to capture any systematic difference between early leavers and their matched

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controls that goes beyond the isolated effect of $K_{it}^{1,20}$. The coefficient γ, that is associated with the interaction effect $K_{it}^{1,20} D_i EL_i$, is our key parameter interest. It measures the additional effect of being an early leaver that goes beyond the common effect of displacement δ.

Note, there's another important difference in the estimation of equation (2.5) in comparison to equation (2.4). The control group consists now of any matched controls, who are employed at a non-closure plant at the last quarter the corresponding treated was last observed working for the closing plant. This does not restrict future employment patterns of the control group in any way. Neither does it restrict the control to separate within the next quarter as well (as it did in the comparison with normal turnover presented above), nor does it restrict a control to a continuously employed worker as is the case in many displacement studies.[19] In this study we take the point of view that an adequate control should not be restricted in any way to proxy for the counterfactual outcome in the absence of displacement. A control should be distinguishable from a treated only insofar that the control does not suffer from displacement due to a plant closure. A control might, however, lose the job due to other reasons.

As before, the selection of adequate controls is based on the exact matching algorithm presented in figure 2.7. Table 2.5 presents evidence on the quality of the matching. Again, mean differences between displaced and non-displaced workers are small, with somehow larger differences in tenure and plantsize. While basically no difference in average tenure exists for ultimately

[19] See for example Jacobson et al. (1993).

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displaced workers, the displaced early leavers are associated with on average 228 days of tenure less than their matched controls. This difference is, however, only about 0.13 standard deviations. Moreover, if tenure is associated with more stable employment and higher earnings, the worse matching for early leavers in terms of tenure should (if at all) downward-bias the effect of displacement for early leavers as opposed to ultimately displaced workers.

Moreover, both ultimately displaced workers and early leavers are on average employed in smaller plants. As discussed before, the smaller plantsize for displaced workers is most likely a by-product of the economic ill-being of closing plants. As already seen in the descriptive statistics, the group of early leavers consist more of men and blue collar workers compared to the group of ultimately displaced workers.

Table 2.6 presents the results of estimating equation (2.5). Column 1 shows estimated coefficients from a regression with an employment dummy as dependent variable. The estimate for δ, which can be seen in row 4, reveals that the overall effect of displacement in terms of employment probability is estimated to be -0.23, implying a reduction in post-displacement employment probability of 23 percentage points in the first 5 years after displacement. This effect goes beyond the pure time effect $K_{1,20}$ of -.07, which represents the dissolution of employment relationships present even in the absence of displacement. While no systematic differences can be found between early leavers and their matched controls, the additional effect of leaving early is estimated to be highly significant at around 0.07. This implies that early leavers face a 7 percentage points higher employment probability as opposed to ulti-

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mately displaced workers.

Column 2 presents analogous difference-in-difference estimation results with log daily earnings (conditional on being employed) as dependent variable. Focusing on the key parameters of interest in column 2, we find a common earnings loss due to displacement of 6 percent, but a significant 1.2 percentage points lower loss for early leavers. Column 3 shows earnings results with a less restrictive sample selection. Similar to the selection criteria applied in other studies, we assign zero earnings for individuals not employed in a given quarter, but include only observations with positive earnings within a calendar year. Hence, this "unconditional" earnings measure captures also earnings losses through short-term non-employment, which increases the estimated common loss of displacement to 61 percent. The loss for early leavers is now 14 percentage points lower.

In sum, table 2.2 reveals that the cost of displacement is significantly lower for early leavers compared to ultimately displaced workers. The difference in displacement effects might be explained by compositional differences between these two groups. Section 2.4 already provided descriptive evidence on differences in average workers characteristics, which fosters the conjecture that a selection process has set in during the closure procedure. Moreover, previous studies have also found evidence for the presence of selection in the labor turnover process before plant closure.[20]

However, workers and management have a competing agenda. Highly productive workers might leave a distressed plant to avoid ultimate displacement,

[20] See Lengermann and Vilhuber (2002) and Pfann and Hamermesh (2001).

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whereas low productivity workers might be the first to be laid-off when a negative demand shock makes downsizing necessary. Hence, it remains an empirical challenge to answer how average productivity varies between early leavers and ultimately displaced workers.

To test proposition 2 we estimate a model of pre-separation daily earnings. However, note two important caveats of interpreting observed earnings differentials as differences in productivity: First, the use of earnings as a measure of worker productivity is based on the underlying assumption that wages are equal to the marginal products of labor. Various characteristics of actual labor markets, such as discrimination, union bargaining, signalling and mismatch, may result in violations of this assumption. Secondly, our measure of daily earnings does not reflect differences in labor input in terms of hours worked. Nevertheless, earnings remain the best available proxy for a worker's productivity given the data in hand.

Equation (2.6) presents a model of pre-separation earnings,

$$ln(w_{it}) = EL_i\lambda + X'_{it}\beta + \theta_t + \epsilon_{it}, \tag{2.6}$$

where the dependent variable $ln(w_{it})$ represents log daily earnings, EL_i takes the value one if individual i is an early leaver and takes the value zero if individual i is an ultimately displaced worker, X_{it} a set of control variables, θ_t captures the effect of calendar time and ϵ_{it} is an error term uncorrelated with all right-hand-side variables.

Table 2.7 presents the results of estimating equation (2.6). All regressions

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control for calendar time effects as well as for relative distance to the closure of the plant. The latter variable is an important control as earnings might be contaminated due to the economic ill-being of the employer. Column 1 represents the results from regressing the early leaver dummy on log daily earnings. The estimated coefficient λ is negative at -0.05 and highly significant. This unconditional evidence suggests that early leavers are associated with 5% lower daily earnings in the 17 quarters before displacement. Including personal characteristics such as age, gender, broad occupation and tenure pushes up the estimate to 0.72.[21]

Including plant characteristics such as plantsize, industry and location of the plant drives down the estimated coefficient for early leavers again, as can be seen in column 3. However, the estimate remains significant and positive at 0.41. Finally, estimating a Tobit specification accounting for top-coding in the earnings data does not change the results significantly.

In sum, all specifications reveal significantly higher pre-closure earnings levels for early leavers. We take this as evidence that proposition 2 is correct.

To understand how higher average pre-closure earnings affects displacement effects, we estimate a displacement effect model allowing for heterogeneous displacement effects along the pre-separation earnings distribution. For simplicity we focus on quartile groups. That is, we allow for different displacement effects for each quartile. The model is specified as follows:

[21]The estimated earnings differential between men and women of -0.56 in column 2 most likely reflects the typical higher share of part-time work among women.

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$$Y_{it} = K_{it}^{1,20}\kappa + K_{it}^{1,20}D_i\delta + \sum_{q=1}^{3} K_{it}^{1,20}D_iQrt(q)_i\phi^q + \alpha_i + \theta_t + \epsilon_{it}. \quad (2.7)$$

Equation (2.7) extends the model defined in equation (2.5) by the term $\sum_{q=1}^{3} K_{it}^{1,20}D_iQrt(q)_i\phi^q$, where $Qrt(q)_i$ is a dummy variable taking the value one if individual i belongs to the q^{th} quartile of the pre-separation earnings distribution and the parameter ϕ^q measures the additional displacement effect for individuals belonging to the q^{th} quartile relative to the baseline effect of the omitted category represented by the 4^{th} quartile of the pre-separation earnings distribution.

Before turning to the estimation of equation (2.7), we have a closer look at the pre-separation earnings distribution. Figure 2.8 shows kernel density estimates of the distribution of pre-separation earnings by displacement groups. Both distributions are skewed to the right. However, the distribution for early leavers is slightly more skewed compared to that of ultimately displaced workers. Moreover, the distribution for early leavers has clearly a higher mass on high earnings. On the other hand, the density is also higher at the lower end of the earnings distribution. These results are in line with previous findings on early leavers. Lengermann and Vilhuber (2002), for instance, find evidence for high-skilled workers leaving as well as firms laying off low skilled workers in periods before displacement.

To understand how these distributional differences affect the estimation results of equation (2.7), we estimate equation (2.7) separately for early leavers,

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ultimately displaced workers and the two groups jointly. Table 2.8 presents the estimation results separately for employment (column 1-3) and earnings conditional on being employed (columns 4-6).

In terms of employment probabilities, significant losses exist for the baseline category of workers belonging to the highest quartile of the pre-separation earnings distribution. This can be seen in row 1. The estimates range from -.11 for the early leavers sample up to -.19 for the ultimately displaced workers sample. Based on the combined sample, high earnings workers are estimated to face a reduction of 16 percentage points in their post-separation employment probabilities. The interaction terms reveal that displacement costs are significantly higher in terms of future employment for low earnings workers. The workers belonging to the lowest quartile of the pre-separation earnings distribution suffer the most. They face an additional reduction in employment probabilities of 11 percentage points. Workers in the second quartile also endure an additional loss of 5 percentage points compared to high earnings workers. Workers in the third quartile suffer no significant additional loss.

Note that the pattern of displacement effects between quartiles is very similar when estimating equation 2.7 based on the early leavers and the ultimately displaced workers sub-samples separately. While these results clearly suggest that above median earnings workers suffer significantly less in terms of future employment, the results on earnings in column 4 to 6 show a reversed pattern. Here, it appears that high earnings workers lose the most as can be seen from the positive and significant coefficients in column 4. This pattern is

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also confirmed in the sub-sample regressions shown in column 3 and 6. However, while these results seem striking, they have to be interpreted carefully. Recall that the estimations on log daily earnings only include observations with positive earnings. Hence, only those separators that successfully found new employment after their separation are included. This, however, leads to compositional differences within groups. In particular, these results seem to suggest that those workers who are successful in finding a new job are also the more productive workers. As low pre-separation-earnings workers are associated with significantly lower employment probabilities as opposed to high pre-separation-earnings workers, the results are not clear-cut as differences in post-separation earnings might be entirely driven by selection within these groups.

Results in column 7 to 9 report earnings losses on an "unconditional" earnings measure. As this measure captures earnings losses caused by short-term non-employment, most interaction effects with quartile groups become smaller and insignificant.[22] This reveals that results on earnings losses caused by a job loss strongly depend on the underlying earnings measure.

Regarding proposition 3, we therefore conclude, that - while no conclusive evidence based on earnings exists - displacement costs in terms of employment probabilities vary clearly with the level of pre-closure earnings. In particular, the findings suggest that workers with above median pre-closure earnings are associated with significantly lower losses in terms of future employment probabilities as opposed to below median workers.

[22]With the exception of the interaction effects of the second quartile in the early leavers sample and the third quartile in the joint sample.

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2.6 Conclusion

In this chapter our first task was to analyze job separations happening before plant closure. We find that separators leaving a dying establishment up to two quarters before closure are predominately men and blue collar workers. Moreover, early leavers separating up to two quarters before plant closure, are associated with significantly better post-separation labor market outcomes as opposed to separators from non-closing plants. Earlier separations from closing plants are, however, indistinguishable from normal turnover. This finding is particularly important for the economic literature that utilizes plant closures to identify involuntary and exogenous job losses in administrative data. As plant closures usually do not happen without prior notice, management and workers adjust their expectations about the value of a given employment relationship in response to the arrival of such information. Hence a negative shock that ultimately leads to closure might cause separations from dying plants even before the ultimate shutdown. While the empirical literature has acknowledged this by focusing on all separations within a certain time window prior to plant closure, the choice of that window often appears to be quite arbitrary. Facing the tradeoff between neglecting early leavers and including a significant amount of normal workforce turnover, the comparison with separators from surviving plants in terms of post-separation outcomes provides a good guideline for choosing a particular time window. Our results suggest that at least all separations up to 2 quarters before closure should be included in the treatment group of displaced workers.

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Given this identification of early leavers, we tested three propositions related to the selection hypothesis in the labor turnover process before plant closure. A clear picture emerged: early leavers suffer significantly less from separating from a closing plant compared to ultimately displaced workers. They suffer less especially in terms of future employment probabilities. Moreover, early leavers are associated with significantly higher pre-closure earnings levels conditional on several personal and firm characteristics. Finally, displacement costs (in terms of future employment probabilities) are significantly lower for workers with higher pre-closure earnings.

These findings are in line with the hypothesis that prior knowledge about the upcoming plant closure induces both management and workers to react in terms of their firing and quitting decisions. As a consequence, selection based on workers' characteristics occurs: firms laying off less productive workers, while workers with better outside options quit. However, on average early leavers appear to be more productive as suggested by higher average pre-closure earnings. As displacement costs in terms of future employment are lower for high-earnings workers, the observed difference in displacement effects between early leavers and ultimately displaced workers could be explained by compositional differences between these groups that result from a selection in the turnover process before plant closure.

A key implication of these findings is that any study on worker displacement utilizing plant closures as a quasi-experiment is well advised to include also separations occurring before the ultimate shutdown. According to our results, focusing solely on ultimately displaced workers would lead to serious over-

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estimation of the cost of displacement.

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2.A Tables

Table 2.1: Plant closures per quarter between 1983 and 1988

Year of plant closure	Quarter of plant closure 1	2	3	4	Total
1983	167	184	151	243	745
1984	174	188	145	224	731
1985	151	184	182	246	763
1986	199	185	178	251	813
1987	176	197	176	294	843
1988	175	182	166	285	808
Total	1,042	1,120	998	1,543	4,703

Table 2.2: Plant closures by federal state and year

Region	Year of plant closure						
	1983	1984	1985	1986	1987	1988	Total
Wien	250	236	253	258	276	255	1,528
Niederoestereich	144	125	146	119	155	152	841
Burgenland	21	14	14	17	19	19	104
Oberoesterreich	98	92	83	117	102	108	600
Steiermark	60	79	81	81	76	70	447
Kaernten	41	39	32	53	48	38	251
Salzburg	43	53	55	52	62	55	320
Tirol	56	54	65	71	55	57	358
Vorarlberg	29	23	26	37	30	35	180
Total	742	715	755	805	823	789	4,629

Note: For 74 establishments no information on the location is available.

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Table 2.3: Matching quality 1: Weighted averages for separators from closing and non-closing plants by distance to closure

	$d=-1$		$d=-2$		$d=-3$		$d=-4$	
Separation from	PC	NPC	PC	NPC	PC	NPC	PC	NPC
Female	.39	.39	.41	.41	.42	.42	.37	.37
Blue Collar	.57	.57	.7	.7	.41	.41	.43	.43
Age (years)	43	43	44	44	44	44	43	43
Tenure (days)	2780	2459	2755	2465	2354	2530	2654	2452
Experience (days)	4532	4337	4371	4230	4233	4202	4168	4124
Daily Earnings (euro)	37	36	33	33	38	37	39	38
Plantsize	64	143	110	160	56	94	103	150

Note: Sample averages of pre-separation characteristics for separations from closing (PC) and non-closing (NPC) plants and by distance to closure (d) in quarters. All variables, except earnings and plantsize, are measured at the quarter immediately before separation. Earnings are are in nominal terms.

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Table 2.4: Comparison with ”normal” Turnover

	Employment			
	$d=-1$	$d=-2$	$d=-3$	$d=-4$
$K^{1,20}\tilde{D}^d$	.094 (.017)**	.072 (.022)**	.048 (.031)	.014 (.03)
$K^{1,20}$	-.38 (.013)**	-.403 (.017)**	-.418 (.024)**	-.374 (.023)**
Time dummies	yes	yes	yes	yes
Fixed effects	yes	yes	yes	yes
Const.	.953 (.025)**	.897 (.029)**	.928 (.14)**	1.009 (.064)**
Obs.	75237	48763	24924	25296
R^2	.481	.498	.511	.515
F statistic	103.694	78.143	36.133	39.15
	Daily Earnings			
	$d=-1$	$d=-2$	$d=-3$	$d=-4$
$K^{1,20}\tilde{D}^d$	-.001 (.012)	.017 (.017)	.008 (.024)	-.014 (.022)
$K^{1,20}$	-.031 (.01)**	-.067 (.013)**	-.051 (.02)**	-.056 (.018)**
Time dummies	yes	yes	yes	yes
Fixed effects	yes	yes	yes	yes
Const.	5.863 (.015)**	5.827 (.016)**	5.73 (.065)**	5.922 (.04)**
Obs.	55377	34117	18153	18347
R^2	.896	.896	.889	.901
F statistic	126.435	80.265	40.429	56.463

Note: Dependent variable is employment in the top panel and log daily earnings bottom panel. Regressions are run separately for different groups of separations distinguished by the relative distance to plant closure (d). All regressions control for individual fixed effects and for calendar time effects. Standard errors in parentheses.

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Table 2.5: Matching quality 2: Weighted averages by displacement status and distance to closure

	ultimately displaced		*early leavers*	
	displ	non-displ	displ	non-displ
Female	.53	.53	.4	.4
Blue Collar	.4	.4	.68	.68
Age (years)	44	44	44	44
	(5.7)	(5.7)	(5.8)	(5.8)
Tenure (days)	2916	2900	2876	3104
	(1755)	(1701)	(1726)	(1671)
Experience (days)	4420	4402	4423	4430
	(1178)	(1180)	(1065)	(1070)
Daily Earnings (euro)	34	34	35	35
	(16)	(16)	(14)	(14)
Plantsize	21	37	116	200
	(42)	(98)	(148)	(260)

Note: Sample averages of pre-separation characteristics, by displacement status and by distance to closure. All variables, except earnings and plantsize, are measured at the quarter immediately before separation. Earnings are in nominal terms. Standard deviations in parentheses.

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Table 2.6: Displacement Effects

	Employment	**Daily Earnings**	
		conditional on employment	unconditional
	(1)	(2)	(3)
$K^{1,20}*D*EL$	.071	.012	.139
	(.006)**	(.005)*	(.019)**
$K^{1,20}*D$	-.228	-.06	-.614
	(.004)**	(.004)**	(.013)**
$K^{1,20}*EL$	-.001	-.015	-.00007
	(.002)	(.001)**	(.006)
$K^{1,20}$	-.068	.012	.792
	(.002)**	(.001)**	(.008)**
Time dummies	yes	yes	yes
Fixed effects	yes	yes	yes
Const.	.984	5.73	6.302
	(.022)**	(.013)**	(.052)**
Obs.	6540163	5740536	6124850
R^2	.459	.914	.177
F statistic	1107.077	4678.701	2535.624

Note: Dependent variable is an employment dummy in column 1 and log daily earnings in columns 2 and 3. Results in column 2 are based on observations with positive earnings within a quarter, while in column 3 all observations with positive earnings within a calendar year are included. All regressions control for calendar time and individual fixed effects. Standard errors in parentheses.

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Table 2.7: Pre Closure Earnings

	OLS	OLS	OLS	Tobit
	(1)	(2)	(3)	(4)
EL	.049	.072	.041	.042
	(.007)**	(.005)**	(.005)**	(.001)**
Age		-.0002	-.0008	-.0007
		(.0005)	(.0004)*	(.0001)**
Female dummy		-.561	-.5	-.517
		(.006)**	(.006)**	(.002)**
Tenure		.00004	.00004	.00004
		(1.62e-06)**	(1.56e-06)**	(4.48e-07)**
White collar		.326	.348	.373
		(.006)**	(.006)**	(.002)**
Plant size			.00004	.00004
			(1.00e-05)**	(1.92e-06)**
Industry dummies	no	no	yes	yes
Location dummies	no	no	yes	yes
Distance to closure	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes
Const.	5.488	5.589	5.579	5.603
	(.051)**	(.042)**	(.056)**	(.042)**
Obs.	264881	263436	263436	263436
R^2	.033	.482	.533	
F statistic	98.685	418.997	312.409	

Note: Dependent variable is always log daily earnings. All regressions control for calendar time effects as well as for the relative distance to the ultimate closure of the plant. Standard errors in parentheses.

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Table 2.8: Displacement effects by quartile of pre-closure earnings distribution

	Employment			Daily Earnings					
				conditional on employment			unconditional		
	ALL	PC	EL	ALL	PC	EL	ALL	PC	EL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$K^{1,20}*D$	-.158	-.189	-.111	-.155	-.176	-.126	-.553	-.633	-.437
	(.006)**	(.008)**	(.009)**	(.004)**	(.006)**	(.006)**	(.018)**	(.024)**	(.026)**
$QRT(1)*K^{1,20}*D$	-.113	-.105	-.119	.273	.308	.217	-.013	.024	-.056
	(.009)**	(.011)**	(.014)**	(.008)**	(.01)**	(.011)**	(.027)	(.035)	(.041)
$QRT(2)*K^{1,20}*D$	-.05	-.044	-.071	.103	.115	.085	-.048	-.013	-.119
	(.008)**	(.012)**	(.012)**	(.006)**	(.009)**	(.008)**	(.025)	(.035)	(.036)**
$QRT(3)*K^{1,20}*D$	.009	.006	-.0003	.055	.055	.049	.058	.063	.02
	(.008)	(.011)	(.012)	(.006)**	(.008)**	(.007)**	(.024)*	(.034)	(.034)
$K^{1,20}$	-.07	-.081	-.055	.007	.008	.004	.791	.738	.863
	(.002)**	(.002)**	(.003)**	(.001)**	(.002)**	(.001)*	(.007)**	(.01)**	(.011)**
Time dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes
Fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Const.	.982	.963	1	5.739	5.716	6.506	6.313	6.244	6.195
	(.022)**	(.022)**	(.007)**	(.013)**	(.013)**	(.004)**	(.052)**	(.052)**	(.02)**
Obs.	6540163	3543734	2996429	5740536	3107894	2632642	6124850	3316486	2808364
R^2	.46	.473	.44	.918	.915	.923	.177	.184	.168
F statistic	1050.498	636.304	460.768	4160.534	2060.109	2604.618	2380.725	1235.344	1270.239

Note: Dependent variable is an employment dummy in columns 1 to 3 and log daily earnings in columns 4 to 6. Regressions are based either on the full sample (ALL), on a sub-sample including ultimately displaced workers and matched controls (UD) or on a sub-sample including early leavers and matched controls (EL). All regressions control for individual fixed effects and calendar time effects. Standard errors in parentheses.

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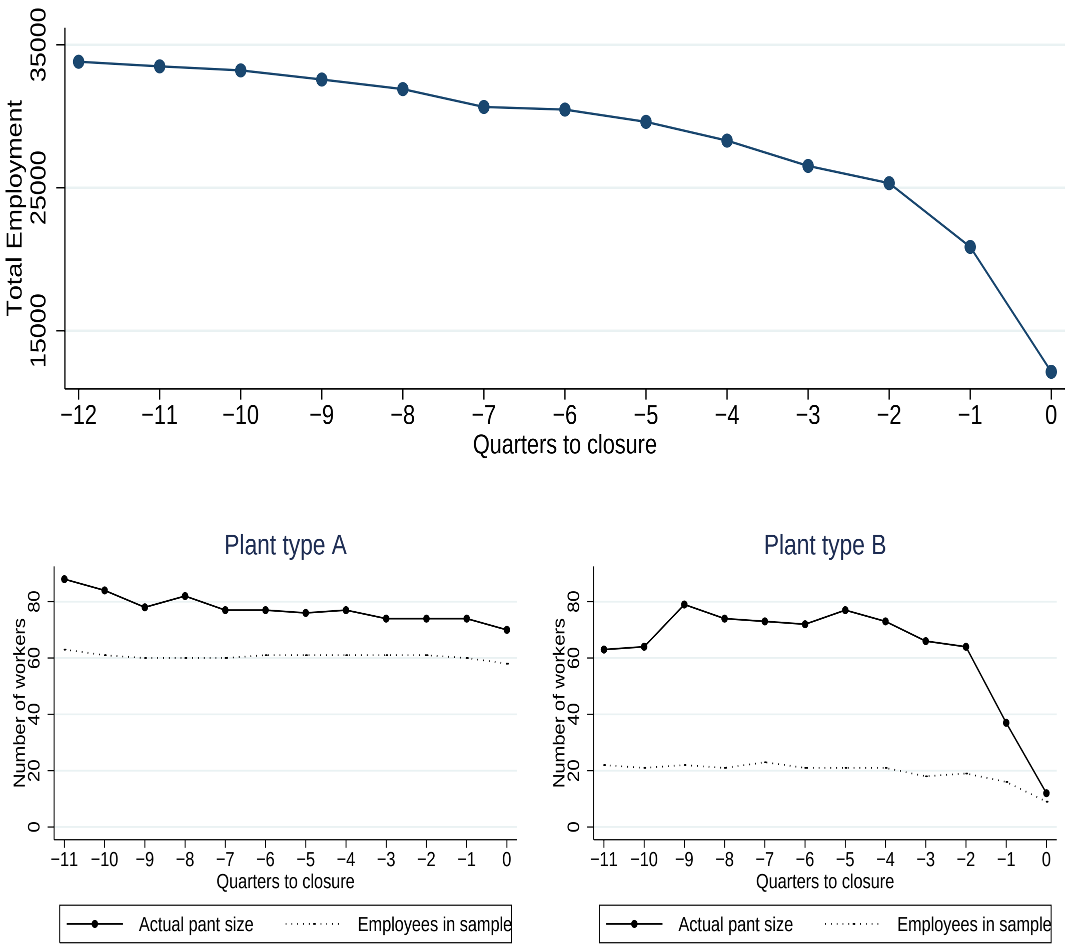
2.B Figures

Figure 2.1: Employment changes in closing plants



Note: The upper panel shows total employment in all plants closing between 1985 and 1988 relative to closure. The lower panel shows employment and the number of employees fulfilling the selection criteria before closure in two representative plants.

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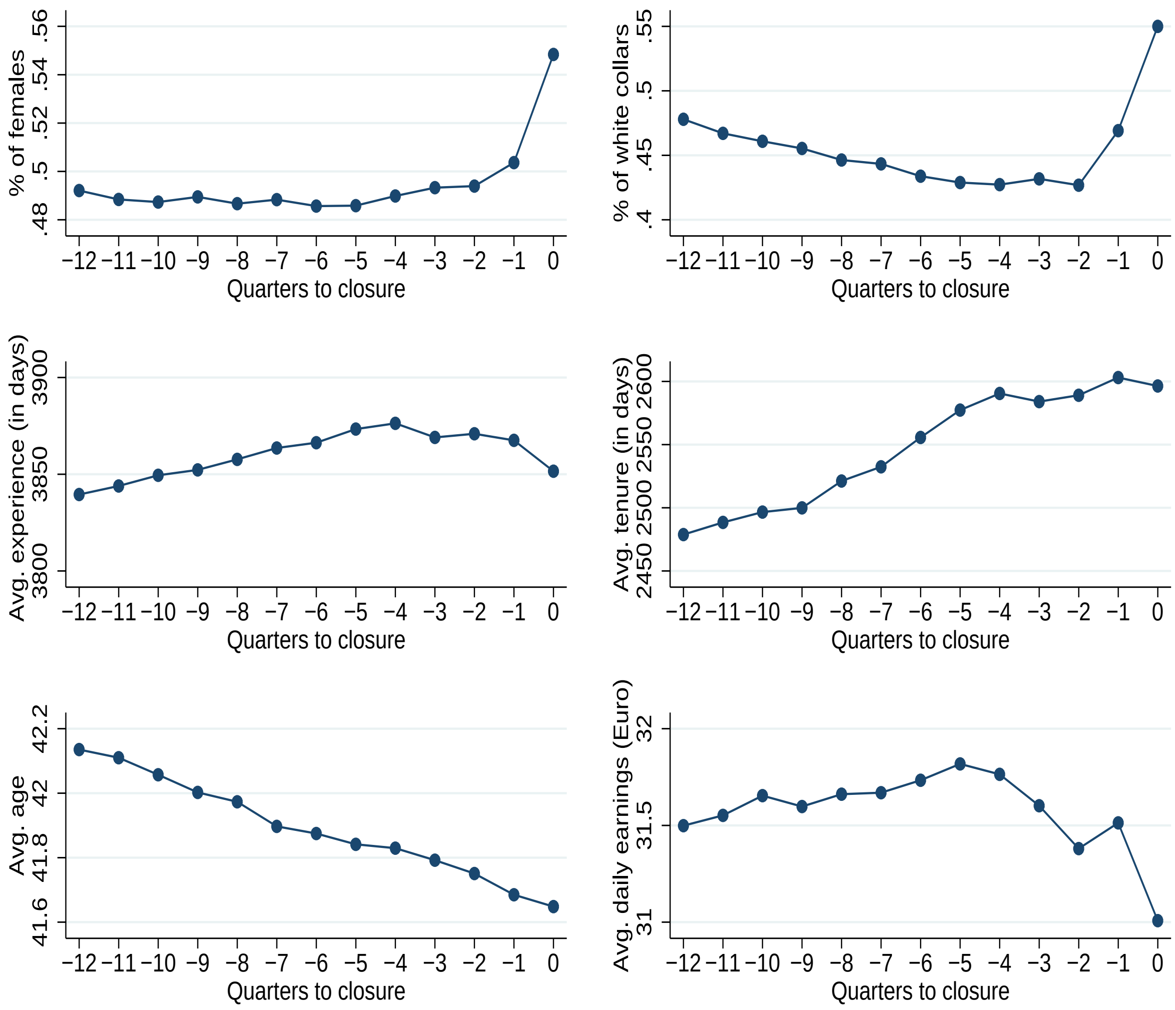
Figure 2.2: Evolution of Average Workforce Characteristics in Closing Plants before Closure



Note: All variables refer to their respective level 12 quarters before closure.

Figure 2.3: Employment Status in the 1st quarter after separation by quarter relative to plant closure

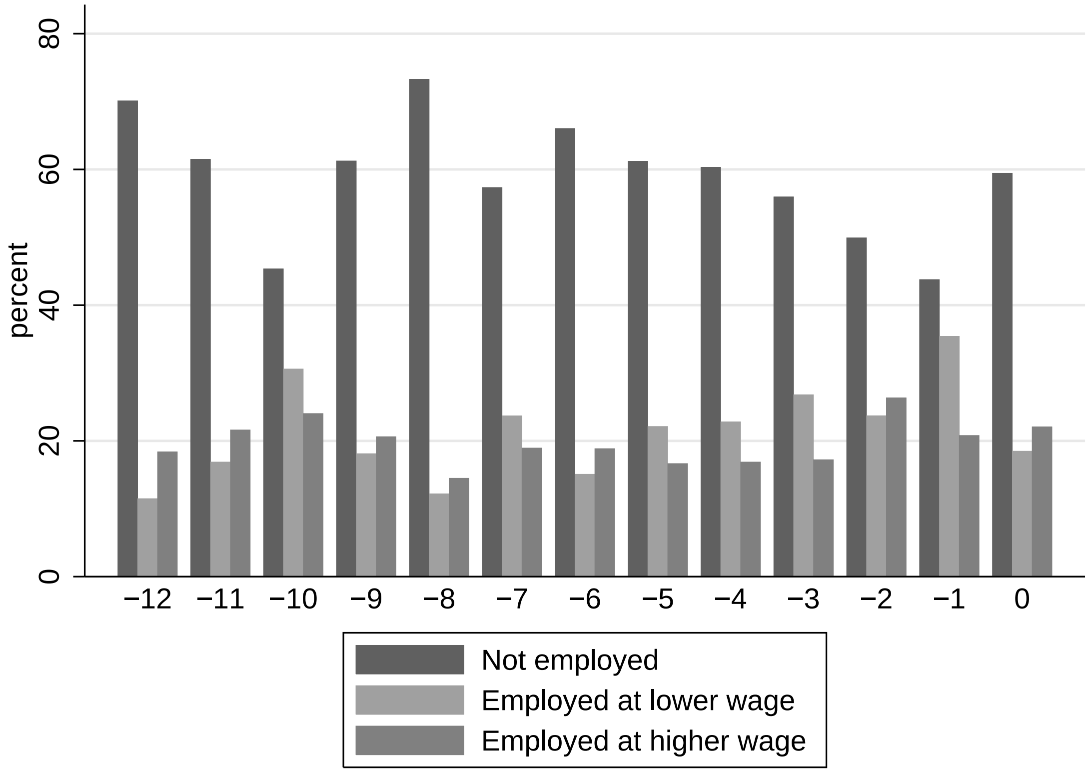


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Figure 2.4: Kaplan-Meier estimates for survival in non-employment after separation by separation-quarter relative to plant closure.

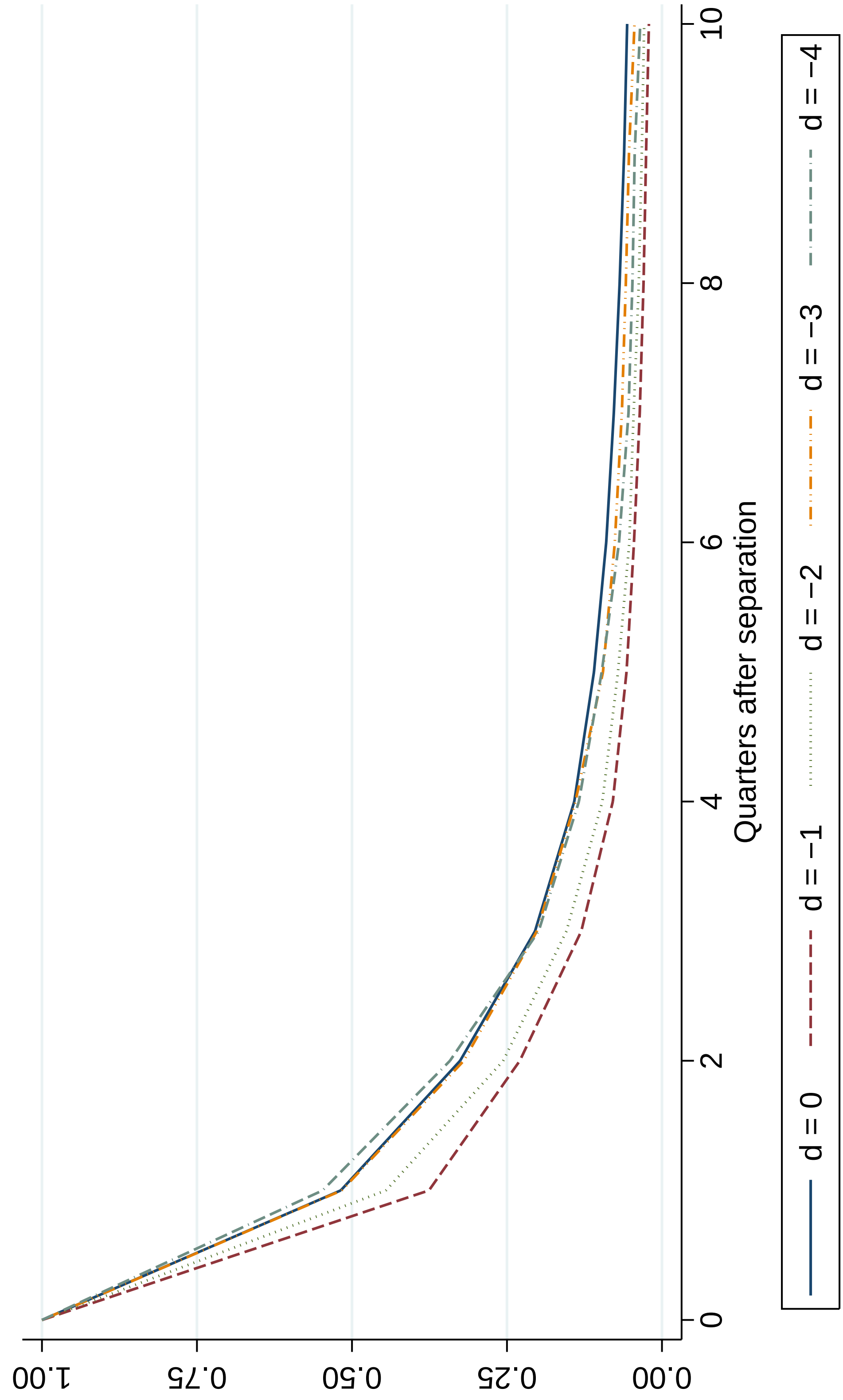

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Figure 2.5: Average employment by separator group

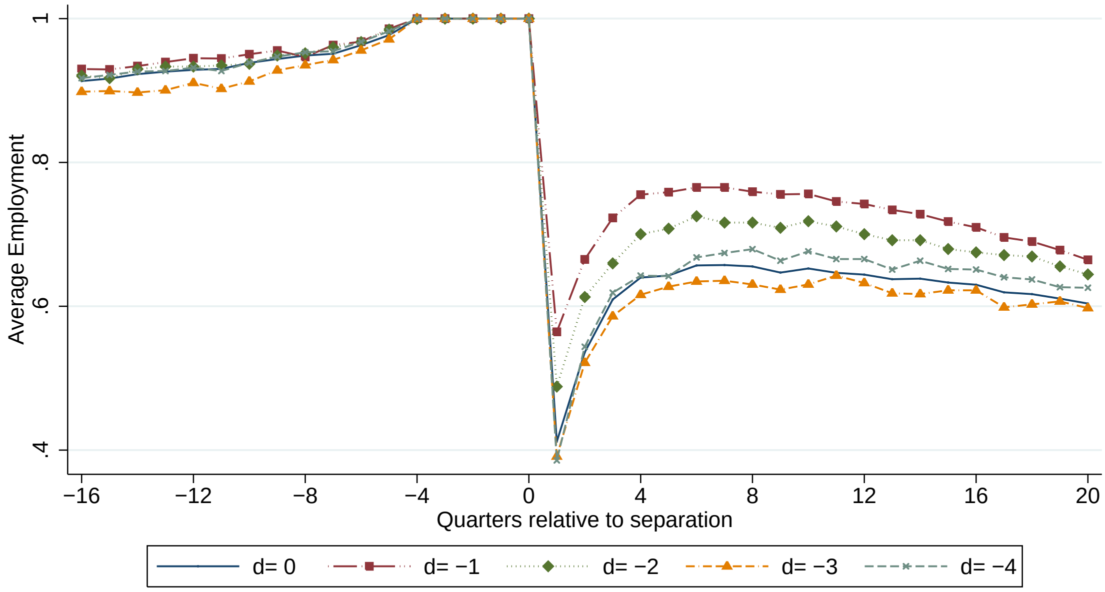

Figure 2.6: Average log daily earnings by separator group conditional on employment

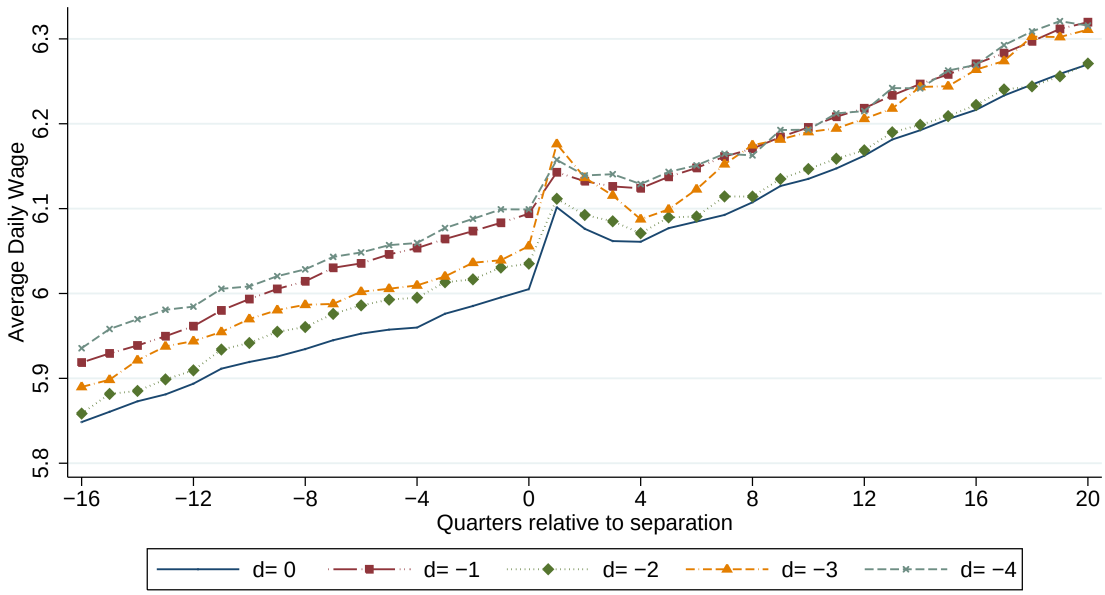

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Figure 2.7: The Matching Algorithm

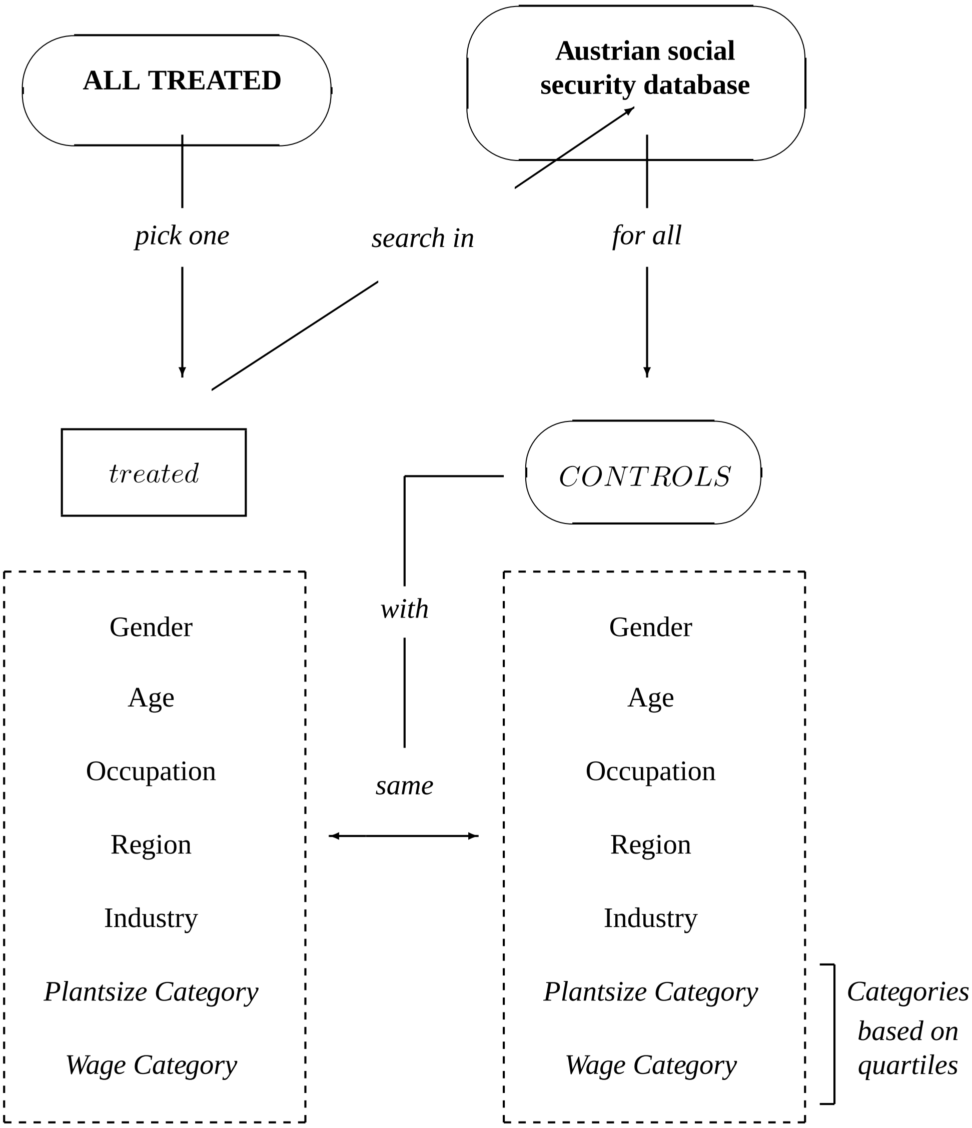

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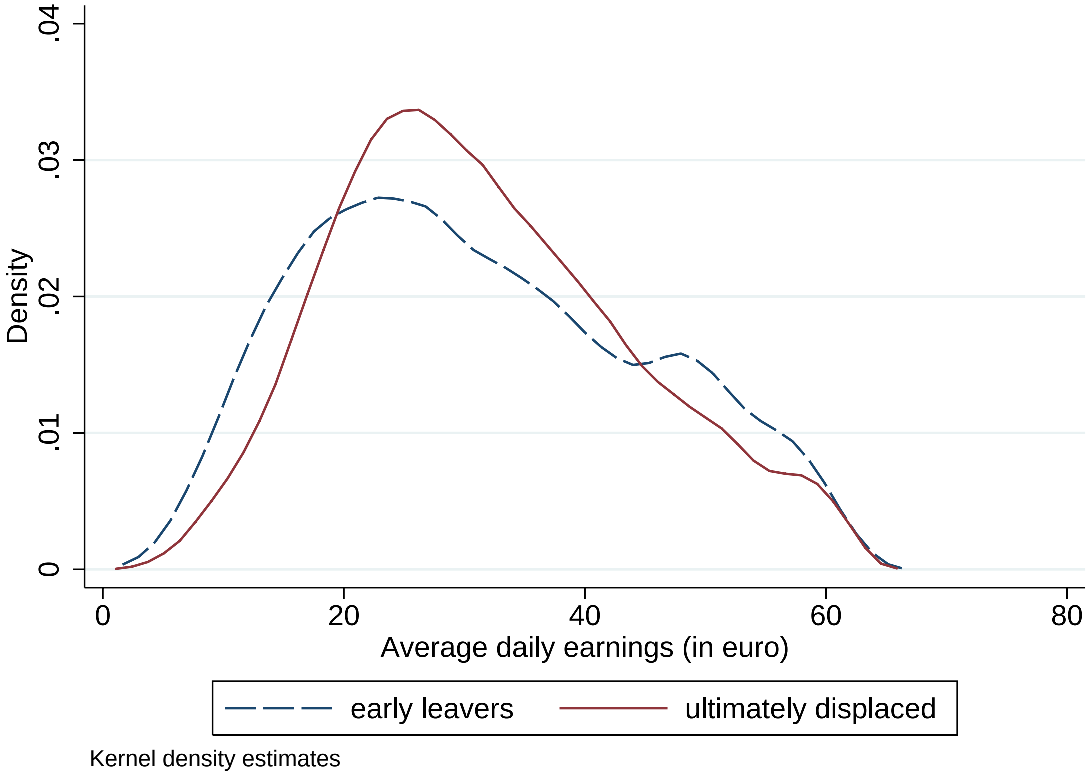
Figure 2.8: Pre-Separation Earnings Distribution by Displacement Group



Note: Average daily earnings are measured over the last 4 years before separation.

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Chapter 3

Growth in Euro Area Labor Quality

Jointly written with Jarrko Turunen

3.1 Introduction

The composition of the euro area workforce in terms of the personal characteristics of persons employed, such as average educational attainment and labor market experience, evolves over time and in response to changing labor market conditions. As a result, the euro area stock of human capital and the associated returns to human capital also change over time, thus contributing to changes in aggregate labor productivity. However, standard unadjusted measures of labor input ignore changes in human capital - changes in average labor quality - leading to an underestimation of the contribution of the labor input to economic growth. Best practise in the area of productivity measurement suggests that changes in labor quality should be taken into account by using a quality-adjusted number of hours actually worked as a measure of labor input OECD (2001).

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A sustained decline in euro area labor productivity growth since the1980s highlights the need for understanding how euro area labor quality growth has evolved. Existing analysis of the causes of the decline in labor productivity growth suggest that lower productivity growth is due to both a decline in capital deepening and lower total factor productivity (TFP) growth over this time period (see Gomez-Salvador, Musso, Stocker and Turunen (2006)). At the same time, robust euro area employment growth in the late 1990's together with economic policies aimed at encouraging employment of lower skilled workers in many euro area countries may have resulted in a shift in the composition of the workforce towards workers with lower human capital. However, we are not aware of attempts to quantify the growth in euro area labor quality and its contribution to the decline in labor productivity growth. Estimates for some euro area countries suggest that excluding changes in labor quality indeed result in a significant underestimation of the contribution of labor input to productivity growth Jorgenson (2005). In the meantime, the central role of human capital in contributing to productivity growth has been acknowledged in key European economic policy recommendations. In particular, further improving knowledge and innovation is one of the key areas identified in the mid-term review of the Lisbon agenda.[1]

Human capital is also given a prominent role in modern growth theory. Endogenous growth models suggest that human capital may generate economic growth in the long term (see Barro and i Martin (2004)). These theories interpret capital broadly to include human capital and incorporate mechanisms

[1]See $europa.eu.int/growthandjobs/pdf/COM2005\_024\_en.pdf$ and ECB (2005).

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such as innovation and learning-by-doing that can generate non-diminishing returns to capital and thus a positive contribution to long-term growth. Nevertheless, empirical evidence from aggregate data on the role of human capital in explaining growth is somewhat mixed. For example, Bils and Klenow (2000) argue that schooling may have only a limited impact on growth. In contrast, a large body of evidence using microdata has shown that investment in education does result in increased individual earnings, suggesting that the social return to schooling is also positive (Krueger and Lindahl (2001)).

In this paper we present first evidence of changes in labor quality in the euro area and evaluate the significance of changes in human capital for recent developments in productivity growth. We construct a quality-adjusted index of labor input in the euro area covering the period 1983-2005 using a methodology that is similar to that used by the US Bureau of Labor Statistics (*Labor Composition and U.S. Productivity Growth, 1948-90* (1993)). Measuring labor quality for the euro area requires combining information from different sources, such as microdata from the European Community Household Panel (ECHP) and the European labor Force Survey (LFS). In addition to our benchmark calculation, we explore the robustness of our results by constructing alternative indices based on other possible methods, as well as taking advantage of available microdata to estimate the contribution of changes in labor quality over the late 1990's using a direct regression based approach suggested in Aaronson and Sullivan (2001). We also construct partial labor quality indices to show what changes in the composition of the euro area workforce have driven changes in overall labor quality. Finally, we use the series to illustrate

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the impact of changes in labor quality on labor productivity growth using a standard growth accounting framework.

The results suggest that euro area labor quality has increased continuously since the early 1980s, growing on average by 0.47% year-on-year. As a result, improvements in human capital have on average accounted for up to one fourth of euro area labor productivity growth. As regards changes over time, labor quality growth was significantly higher in the early 1990s than in the 1980s. The strong increase in the early 1990's appears to have been driven by an increase in the share of those with tertiary education and in the share of workers in prime age during this time period. Growth in labor quality moderated again towards the end of the 1990's, possibly reflecting the impact of robust employment growth resulting in the entry of marginal workers with lower human capital. Accounting for positive labor quality growth lowers existing estimates of total factor productivity growth.

The rest of this paper is organized as follows. In section 2 we survey the existing literature on calculating measures of labor quality and the methodological issues involved. In section 3 we describe the data sources and methodology that we use to construct a quality-adjusted index of labor input in the euro area. In section 4, we show our main results and analyze their robustness. We also describe changes in returns to human capital characteristics and the composition of the euro area labor force that steer changes in labor quality growth. In section 5 we use the newly-constructed index to estimate the contribution of changes in labor quality to the labor productivity growth over this time period. Finally, we conclude in section 6 with a summary and conclusions for

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economic policies.

3.2 Related literature

Jorgenson, Gollop and Fraumeni (1987) and Ho and Jorgenson (1999) contain a description of the standard methods used to account for labor quality and include benchmark estimates of labor quality for the US. Ho and Jorgenson construct a quality-adjusted measure of labor input based on a cross-classification of hours worked into a number of cells by observed worker characteristics (sex, age, education and self-employment status). They then compute changes in the aggregate labor input as a weighted average of the change in hours worked for each cell and time period, where the weights are given by the average share of compensation attributable to each cell in two adjacent years. Finally, Ho and Jorgenson calculate growth in labor quality as the difference between growth in this aggregate labor input and growth in a raw measure of hours worked. Using this approach they find that in 1948-1995 labor quality grew on average by 0.6% per year in the US. Ho and Jorgenson also find that the rise in average level of educational attainment is the main driver of the increase in quality, whereas changes in the age structure of the work force, such as the entry of a large inexperienced cohort (the ”baby boomers”) into the labor force, also explains changes in labor quality growth over time in labor quality growth.

Alternative estimates for the US using different methodologies are provided by the *Labor Composition and U.S. Productivity Growth, 1948-90* (1993)

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and Aaronson and Sullivan (2001). The BLS method differs from Ho and Jorgenson mainly in the estimation of the weights. In particular, instead of calculating simple averages of compensation for each cell, the BLS uses a regression approach to estimate cell means. This involves using microdata to estimate earnings equations with a number of individual characteristics, including education and work experience, as explanatory variables, and using the predicted wages obtained from these regressions for each worker group as the weights to calculate aggregate labor input. Compared to Ho and Jorgenson (1999), the BLS approach allows for estimating the weights using a larger number of observations, thus improving the robustness of the results.[2]

Aaronson and Sullivan (2001) extend the regression approach taken by the BLS further to calculate the labor quality measure using microdata of individuals only. Similar to the BLS, they obtain predicted wages for each individual using a regression approach. However, instead of using the predicted wages and hours data for each aggregate worker group, Aaronson and Sullivan combine predicted wages with actual individual data on hours worked. Estimates of labor quality growth for the US differ somewhat between these three studies. In particular, BLS (1993) finds a lower average growth rate of labor quality since the late 1940s in the US than those presented in Ho and Jorgenson (1999). However, since the 1980s the results in the three studies are similar.[3]

[2]Furthermore, the BLS uses more detailed information about actual work histories provided by matching the Current Population Survey with data from the Social Security Administration. This allows the BLS to estimate actual work experience, instead of relying on a proxy of potential work experience BLS (1993).

[3]Changes in labor quality growth also figure prominently in the recent discussion of the increase in US labor productivity growth in the late 1990's. In particular, Jorgenson (2005) find that the increase in the employment of college-educated workers contributed significantly to the increase in US productivity growth since 1995. Taking a different methodological approach Abowd, Haltiwanger, Jarmin, Lane, Lengermann, McCue, McKinney and Sandusky (2005) also derive measures

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Evidence for countries other than the US is limited, and in particular no estimate exists for the euro area as a whole. Jorgenson (2003) provides evidence of labor quality in G7 countries, including estimates for three large euro area countries, i.e. France, Germany and Italy. The results are based on the method used in Ho and Jorgenson (1999) and use a number of different data sources. His estimates for these three countries suggest that labor quality growth in the euro area has been positive between 1980-2001, ranging from approximately 0.45% annual growth in Germany to 0.86% in France (Table 12, Jorgenson (2003)). For the euro area as a whole this suggests that labor quality grew on average by approximately 0.57% per year.[4] The results also suggest that growth in labor quality was strongest in the period 1989-1995, mainly due to robust improvement in labor quality in France. Furthermore, growth in labor quality declined somewhat in all three countries in 1995-2001. While the contribution of labor quality to labor productivity growth is smaller than the contribution of the other two components of labor productivity growth, i.e. capital deepening and total factor productivity growth, it is significant. For the euro area aggregate based on France, Germany and Italy the results suggest that the contribution of labor quality growth was always positive and accounted for just below one fifth of the growth in labor productivity Jorgenson (2003).

Further evidence is available for some euro area countries. In particular, Melka and Nayman (2004) estimate labor quality growth in France, Card and

of human capital. Their methodology relies on a novel and data intensive combination of comprehensive firm level and household level data sources for the US. Their results suggest that compared to measures derived in Jorgenson (2005) average growth in human capital in all industries has been significantly higher in the late 1990's period.

[4]This rough estimate is based on a weighted average of the country estimates using labor force weights.

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Freeman (2004) in Germany and Brandolini and Cipollone (2001) in Italy. O'Mahony and van Ark (2003) calculate sectoral measures of labor quality for France, the Netherlands and Germany. While the estimates in O'Mahony and van Ark (2003) are based on relatively limited data sources, they provide additional insight about sectoral diversity in labor quality growth. Their findings suggest that labor quality growth has been larger in sectors that produce information and communication technology (ICT). In addition, the slowdown in labor quality growth in 1995-2000 appears to have been most relevant in non-ICT sectors.[5]

3.3 Measuring Euro Area Labor Quality

We follow the BLS approach to estimating changes in labor quality in the euro area. Our measure of quality adjusted labor input is constructed as follows. First, using available microdata for individual workers (see below), we estimate cross-sectional wage equations separately for each country and for males and females:

$$logW_i = \alpha + \sum_{e=1}^{2} EDU_i^e \beta_e + \sum_{a=1}^{5} AGE_i^a \gamma_a + Z_i \eta + \epsilon_i \tag{3.1}$$

Where the subscript i refers to the individual. These equations are estimated using weighted OLS, using sample weights provided with the microdata.

The dependent variable W_i is the individual gross nominal hourly wage in PPP units. The use of gross nominal hourly wages is motivated by the use of

[5] **?** also construct crude measures of labor quality growth for some euro area countries.

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the labor quality estimate primarily as an input to productivity analysis within a growth accounting framework (see OECD (2001)). The PPP conversion is needed to translate nominal wages that are reported in national currencies to comparable units across countries. Following Jorgenson (2005) and Jorgenson and Nishimizu (1978), the bilateral wage PPP between country j and Germany for is used.[6]

The right hand side variables include dummy variables for two education categories EDU (with secondary education as the omitted category), five age categories AGE (with those between 34 and 45 as the omitted category) and a number of control variables Z (dummy variables for part-time employment status and for sector). Note that altogether this combination of classifications results in 36 times 12 worker-country groups.

The main source of detailed information on wages and characteristics of individual workers in euro area countries is the ECHP. The ECHP survey begins in 1994 (Austria and Finland join in 1995 and 1996, respectively) and continues until 2001. Sampling weights are available for calculating summary statistics and for performing weighted regression analysis. Wages in the ECHP are reported by survey participants as net earnings (including bonuses) in the previous month in national currency.[7] From this information gross wages are constructed using the gross/net ratio provided by the survey. We divide the monthly wage by monthly hours worked to derive a measure of nominal gross hourly wages for each individual.

[6]Jorgenson and Nishimizu (1978) argue that wage PPPs are needed to convert labor inputs into comparable units across countries. For alternative results that use PPPs based on price levels provided by the ECHP see Schwerdt and Turunen (2006).

[7]Except for France and Finland where wages are reported as gross wages.

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The education categories in the ECHP are constructed using the ISCED97 classification. They include those with lower secondary education (ISCED categories 0-2), those with upper secondary education (ISCED categories 3 and 4) and those with tertiary education (ISCED categories 5-6).[8] While more detailed education categories are available at the country level, detailed hours data from the LFS (see below) are available only for these three broad education categories. Indeed, country differences in educational systems complicate complete harmonization of the measurement of educational attainment at a more detailed level. Fosgerau, Jensen and Sorensen (2002) study the impact of extending the number of educational categories on measures of human capital in Denmark. Their results suggest that a relatively small set of educational categories (four in their case) is sufficient for measuring aggregate labor quality.

The use of education and age to proxy human capital is in line with the literature on labor quality and is informed by economic theory about the main determinants of human capital. In terms of economic theory, formal education is the main source of general human capital (as opposed to job-specific human capital), with the basic proposition that investment in education results in higher human capital and productivity (see Becker (1993)). This assumption is confirmed by an extensive literature on returns to education that documents gains to education in terms of higher individual earnings (for recent surveys see Card (1999) and Ashenfelter, Harmon and Oosterbeek (1999)). It should be noted that the level of education is a limited proxy for general human cap-

[8]A detailed description of the ISCED classification can be found in Annex 3 of OECD (2004).

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ital. For example, the level of education does not take into account the impact of possible differences in the quality of schooling or the type of education (see Barro and Lee (2001)).

In addition to formal education, workers gain human capital after finishing school through increased labor market experience and on-the-job training. However, compared to education, measuring experience is significantly more complicated and the empirical literature largely relies on incomplete proxies. A common approach to measure experience is to approximate labor market experience with age minus years spent in schooling minus the school starting age. This approach is adopted in several studies of labor quality (for example in Ho and Jorgenson (1999) and Aaronson and Sullivan (2001)). An alternative approach, taken in this study, is to use age directly as a proxy for human capital gained after school. This approach allows us to match wage information from the ECHP for age groups with the LFS information on hours worked. Furthermore, different labor market experiences for men and women result in significant differences in the accumulation of human capital and their returns between sexes. For example, it is likely that using estimated experience or age as a proxy for actual labor market experience results in different experience-earnings profiles for men and women. Finally, employment status (such as part-time employment) and sector of activity are important additional determinants of wages that may confound the estimated returns to human capital variables.

Note that the use of wages as a measure of worker productivity is based on the underlying assumption that relative wages are equal to the relative mar-

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ginal products of labor. Various characteristics of actual labor markets, such as discrimination, union bargaining, signalling and mismatch, may result in violations of this assumption (for a discussion see Ho and Jorgenson (1999)). Furthermore, some of these characteristics, such as the relative importance of union bargaining, may be more relevant in the European context than is the case in the US. However, due to lack of more direct measures, wages remain the best available proxy of worker productivity.

In a second step we construct average predicted wages $\tilde{W}_{j,t}$ for each worker country group j and year t based on the predicted wages from equation (3.1). Following BLS, average values for the control variables (part-time employment and sector) for the whole sample are used when calculating predicted wages, such that their impact is excluded from the calculation of the labor quality index (BLS (1993)). We use these predicted wages to construct weights for each worker-country group j as the average of the share of each worker group in total compensation in adjacent years:

$$\overline{s}_{j,t} = \frac{1}{2}(s_{j,t} + s_{j,t-1}) \tag{3.2}$$

Where the share $s_{j,t}$ is given by:

$$s_{j,t} = \frac{\tilde{W}_{j,t} H_{j,t}}{\sum_j \tilde{W}_{j,t} H_{j,t}} \tag{3.3}$$

Where H refers to total hours worked. As an alternative robustness check, we also construct weights using the average predicted wages $\tilde{W}_j$ (over time) to construct weights that vary over time only due to differences in the composi-

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tion of hours worked.

Using these data the change in aggregate labor input in the euro area is then calculated as:

$$ln(L_t/L_{t-1}) = \sum_j \bar{s}_{j,t} ln(H_{j,t}/H_{j,t-1}) \tag{3.4}$$

Growth in labor quality is equal to the difference between growth in aggregate labor input and growth in the raw measure of hours worked:

$$\Delta lnQ = \Delta lnL - \Delta lnH \tag{3.5}$$

We use data from the LFS as the main source to construct measures of hours worked for worker groups.[9] Eurostat collects data from national labor force surveys and provides estimates for aggregate indicators, such as hours worked cross-classified for different age-gender-education groups for each euro area country. Total hours worked have been calculated from the LFS source data using information on employment and usual weekly hours.[10] The time span of these data varies somewhat across euro area countries, but with the exception of data on educational attainment, the cross-classifications are currently available for most countries from 1983 until 2005.[11]

In addition to the principal data sources, the ECHP and the LFS, we use additional sources of information to extend the time period covered by the

[9]The LFS data used in this chapter were extracted in December 2006.

[10]Total hours usually worked were utilized for data availability reasons. Only for the post 1992 period complete information is available on usual as well as on actual hours worked. Results for this period do not differ significantly when actual hours are used instead of usual hours.

[11]LFS data for Portugal and Spain is available from 1986 onwards and for Austria and Finland from 1995 onwards.

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labor quality index. First, while we have information on hours worked cross-classified by gender and age, prior to 1992 no information is available along the educational dimension from the LFS. For example, total hours worked by 35-44 years old males are known, but information on what share of these hours can be attributed to each of the three educational categories is missing. Lack of education data in the LFS prior to 1992 requires the use of additional data sources to estimate the full cross-classification of total hours worked for the pre 1992 period. We use information from the Luxembourg Income Study (LIS) and the German Socio-Economic Panel (GSOEP) to fill this gap. LIS is a non-profit organization that collects and provides access to cross section data from household income surveys from a number of countries. The GSOEP is a large longitudinal survey of German households that is available from the early 1980s onwards. Both LIS and GSOEP provide information that is similar to the ECHP. We combine LFS hours data for the less complete age times sex cross classifications with data on hours for the complete age times sex times education cross-classifications from LIS to extrapolate education shares for a number of euro area countries. Up to three cross-section data points per country are available from the LIS. As a final step, we fill in the missing data points between LIS and LFS observations by using predicted values for the respective shares stemming from weighted regressions for each worker-country group. Time trends as well as information from the complete GSOEP series are used to construct these predicted values. Overall, the imputation of hours worked for some worker-country groups before 1992 suggests that the results from this earlier period need to be interpreted with

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some caution.

Second, we use the GSOEP to extend the time period covered by time-varying predicted wages beyond the period available in the ECHP. To do this we estimate equation (1) for Germany and construct average predicted wages for worker groups as described in equation (2) for each year in 1983-2005. We then extrapolate the average predicted wages obtained from the ECHP for each worker-country group using the predicted values from regressions for each worker-country group that include time trends as well as equivalent predicted wages from the GSOEP series. We evaluate the robustness of this extrapolation by comparing results from the main labor quality index with an index that does not rely on additional information on wages beyond the 1994-2001 period in section 4.

3.4 Results

Estimates of labor quality indicate a continuous increase in euro area labor quality in the last 20 years (see Table 1 and Figure 1). The estimated average growth rate of euro area labor quality in the 1984-2005 period is 0.47% year-on-year. The estimated growth rate for the euro area is lower that a simple aggregation of previous results for Germany, France and Italy presented in Jorgenson (2003) (averaging 0.57% in 1984-2001). This difference is likely to reflect a number of factors, including differences in underlying data, methods and country coverage. In particular, we include estimates from all euro area countries and allow changes in the composition of the euro area work-

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force across countries to influence growth in euro area labor quality. We also calculate the first order contributions of sex, age and education to euro area labor quality growth following the method described in Ho and Jorgenson (1999).[12] The results show that, as expected, education has been the main driving force of labor quality growth (see Table 1).

A comparison with existing country results, as well as exploring the sensitivity of our results to differences in data and methods used provide a useful test of robustness of the euro area labor quality estimate. The country results for the three largest euro area countries, Germany, France and Italy for the 1984-2005, period suggest that labor quality growth has been strongest in France and weakest in Germany (see Table 2). Both the overall average growth rates and the pattern of average growth rates over time are roughly consistent with results in Jorgenson (2003), with the exception of a somewhat lower estimated growth rate for Germany. However, our lower estimate for Germany is close to the estimated growth rate of 0.21% for the post 1980 period in Card and Freeman (2004). Our estimate of labor quality growth in France is also in line with the estimate by Melka and Nayman (2004) for the 1982-2001 period (0.87%). Both estimates show a significant decline in labor quality growth over time in France. Inklaar, O'Mahony and Timmer (2005) also find (for the EU4, including Germany, France, the Netherlands and the UK) that the contribution of labor quality to labor productivity growth declined in the

[12] First order indices are constructed analogously to the main index described in section 3.1. The only difference compared to the full index consists in the choice of worker-country groups, which is determined by the respective cross-classification. For example, the first order contribution of sex requires only a cross-classification along one dimension with two possible worker groups (males and females). Hence, the corresponding index for sex is calculated based on 2 times 12 worker-country groups.

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late 1990s. Overall, the comparison with existing country results supports the robustness of our estimates for the whole of the euro area.

We next explore the robustness of our results to differences in data and methods. In calculating the index shown above we have allowed for the returns to skills for different groups of workers to change over time. However, complete data on relative returns is available for the 1994-2001 period only. Before 1994 and after 2001, the relative returns are based on country specific time trends in the late 1990s period that are extrapolated based on information about changes in relative returns in Germany. We can assess the robustness of this approach in two different ways. First, we calculate an index that uses growth rates in German predicted wages only (thus excluding the impact from country specific trends in the 1990s) to extend the series backwards and forwards. Second, we construct an index of labor quality that is based on keeping the average relative returns for each country in the late 1990s period fixed throughout.

Assuming that the relative returns to individual characteristics have remained unchanged over the whole sample period may seem like a strong assumption. Empirical evidence for European countries suggests that returns to skills may indeed be more stable in the euro area than in other economic areas. For example, in their review of the literature on returns to education Ashenfelter et al. (1999) find that while studies for the US show a significant upward shift in returns to education, studies for other countries do not find such a shift. Barth and Lucifora (2006) also find that the wage premium for those with tertiary education has been ”remarkably stable” in most European countries

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since 1985. For Germany, the largest euro area country, this is confirmed by the evidence surveyed in Fitzenberger and Kohn (2006). These results suggest that relative wages (between groups of workers) may indeed be relatively rigid in European countries and necessary adjustments take place mainly in terms of labor market quantities. This is supported by empirical evidence on group-specific unemployment rates in Europe (see for example Biagi and Lucifora (2005)).

The results of the robustness tests are shown in Figure 1, together with the headline index with changing relative returns. While there are some differences in the patterns of year to year changes in the early part of the sample period, differences across methods appear small. These results suggest some caution in interpreting precise year to year movements in the early part of the sample, but overall, support the robustness of the headline estimate. The small difference between the headline estimate and the estimate that uses fixed returns suggests that changes in relative returns indeed play a small role in determining changes in labor quality. This result suggests that wage rigidities are likely to dampen relative wage adjustment in Europe.

Finally, we have explored alternative specifications of the regression equation (3.1). The results suggest that excluding the control variables (part-time and sector activity) results in a negligible increase in the estimate of labor quality growth. We have also applied a purely regression based approach proposed in Aaronson and Sullivan (2001) to estimate an alternative index for the time period covered by our microdata. The results point to somewhat stronger euro area labor quality growth on average in the 1995-2001 time period (0.64%).

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Beyond the average increase in labor quality, our estimate of labor quality shows some variation in labor quality growth over time (see Figure 1 and Table 2). In broad terms the data point to three different time periods in terms of longer-term developments in euro area labor quality. The 1980s were characterized by relatively low growth in labor quality, followed by particularly strong growth in the early 1990s. Average labor quality growth appears to have moderated again somewhat towards the end of the 1990's and during the recent slow growth period, before picking up again from 2003 onwards.

Variation over time may be associated with the business cycle or structural changes. Previous empirical evidence suggests that labor quality is likely to be counter-cyclical showing periods of ”down-skilling” in upturns and ”up-skilling” in downturns as workers with different skills move in and out of the labor force (Aaronson and Sullivan (2001) and Solon, Barsky and Parker (1994)). In particular, the share of workers with lower skills tends to increase during periods of stronger growth as firms lower their skill requirements to expand production and more low-skilled workers, faced with a higher likelihood of finding a job and possibly higher wages, are encouraged to enter the labor market.

Figure 2 shows a decomposition of the overall index and the first order indices for education and age into a trend and cyclical component using a standard Baxter-King bandpass filter. The results suggest some moderation in the trend increase in labor quality growth in the second half of the 1990s, mainly related to the contribution of age to the overall index. In addition, the cyclical components show fluctuations that are consistent with the euro area business cycle.

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Correlations of the cyclical measure of labor quality with a corresponding measure of real GDP show only a weak, lagged negative association between the two. However, recent developments, such as the significant increase in labor quality growth in the early 1990's and the subsequent decline in the course of the 1990's – a period of particularly strong employment growth – is consistent with the interpretation of countercyclical quality growth. Most recently, estimated cyclical growth in labor quality has increased significantly, suggesting that the recent slow growth period may have been characterized by some "up-skilling", in terms of contributions of both age and education. However, this period was also characterized by labor market reforms in a number of euro area countries that were specifically aimed at increasing the employment of lower skilled workers.

Focussing on factors that determine the trend increase in labor quality growth, the contribution of education to labor quality growth was particularly strong in the late 1980s and early 1990s, consistent with an increase in the share of those with tertiary education of total hours worked in the euro area during this time period. Longer term developments in educational attainment in the euro area have been characterized by a secular increase in years spent in schooling. Data on total hours worked from the LFS illustrates the significant increase in average educational attainment over the last 20 years (see Figure 3). The share of those with primary education or less has declined significantly, whereas the share of those with secondary and tertiary qualifications has increased. The recent increase in the share of the population that has tertiary (university level) qualifications has been particularly striking. Overall,

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the increase in educational attainment amounts to a significant increase in the supply of general skills in the euro area.

The contribution of age to the index of labor quality was also particular strong in the early 1990s. This coincides with an increased share of workers in prime age (aged between 35 and 54) (see Figure 4). While acting as proxy for labor market experience, the contribution of age to labor quality changes is largely driven by demographic developments. Overall trends in the euro area working age population over the last 30 years are characterized by the movement of the so-called baby boom cohort (those born in the 1950s and 1960s) through the age distribution. In particular, the shares of those in prime age, i.e. between 35-54 years of age have been steadily increasing since the early 1990's, whereas the share of younger, less experienced workers, i.e. those between 15 and 34 years of age has declined over the same time period. The increase in the share of hours worked by prime-aged workers and the decline in the share of younger workers is likely to have resulted in an increase in average labor market experience over this time period, as well as lower contemporaneous human capital investment. Compared to the changing contribution of workers below 55, the share of older workers has been relatively steady over this time period. However, the ageing of the baby-boom generation is likely to result in an increased share of total hours worked for this age group in the near future.

Finally, the first order contribution of sex to the labor quality index has been quantitatively negligible. The small negative contribution reflects the increased share of total hours worked by women (see Genre and Gomez-Salvador

(2002)).

3.5 Decomposition of productivity growth

Using the quality adjusted measure of labor input in a standard growth accounting framework provides further insight into recent developments in euro area labor productivity growth.[13] In particular, euro area labor productivity growth, measured by real GDP per hour worked, declined from an average annual growth of above 2% before the mid-1990s to just above 1% since 1996. Within a growth accounting framework, growth in labor productivity defined as real output per hour worked can be decomposed into three components: capital deepening (i.e. growth in the gross capital stock per hour worked), growth in labor quality and TFP growth.[14] Due to lack of data on labor quality for the euro area, previous exercises have estimated TFP growth as a residual item including the contribution of labor quality growth (Vijselaar and Albers (2004) and Sakellaris and Vijselaar (2005)). With positive growth in labor quality, this omission results in larger estimates of TFP growth and a possible misinterpretation of the determinants of the sustained decline in labor productivity growth.

The results of the decomposition of labor productivity, i.e. separating out the impact of labor quality growth from TFP growth point to a significant and increasing role for changes in labor quality in explaining labor productivity

[13]For a general description of the growth accounting framework, see Barro and i Martin (2004). Within the framework growth on real GDP can be decomposed into three main components: population growth, growth in labor productivity (real GDP per hour worked) and growth in labor utilization (total hours worked). labor productivity growth can be further decomposed into capital deepening, growth in labor quality and growth in TFP. For a more detailed description and an application of the growth accounting framework to euro area data see Gomez-Salvador et al. (2006).

[14]The contributions of capital deepening and labor quality are weighted by the relevant factor shares.

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growth in the past 20 years (see Figure 5). While in the early 1980's the contribution of labor quality growth accounted for only 10 percent of productivity growth, this share has increased to 26 percent in the early 2000's. However, as discussed above lower labor quality growth in the second half of the 1990s appears to have also contributed somewhat to the decline in labor productivity growth over the same time period.

Adjusting for labor quality results in significantly lower estimates of euro area TFP growth than previously estimated (see Table 3). As TFP growth is estimated as a residual, these estimates should be interpreted with some caution. For example, the current growth accounting exercise relies on capital stock estimates for the euro area that do not take into account possible changes in the quality of capital. With this caveat in mind, the results suggest that while TFP growth has been on average slower in the 1990s compared to the 1980s, a significant slowdown in TFP growth took place during the recent period of slow growth in the euro area. The slowdown in TFP growth suggests a possible decline in the contribution of technological progress to growth in the euro area.[15] Given that measured TFP growth tends to be pro-cyclical, low TFP growth during this time period is consistent with a cyclical decline in euro area real GDP growth. However, lower TFP growth may also reflect structural adjustment towards an increased use of labor inputs relative to capital in production triggered by wage moderation and labor market reforms.

[15] While TFP growth is commonly used as an indicator of developments in technological progress it is important to note that measures of TFP growth, such as the Solow residual, do not directly correspond to technological progress when the economy is characterized by frictions such as imperfect competition (for a discussion see Basu and Fernald (2002)).

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3.6 Conclusion

We have presented first evidence of changes in labor quality in the euro area by constructing a quality-adjusted index of labor input in the euro area covering the period 1983-2005. The index is constructed by combining data on wages and individual characteristics from micro data with data on hours worked for worker groups from the LFS for all euro area countries. A comparison with available country estimates and an analysis of sensitivity of the euro area index to changes in data and calculation methods suggest that the benchmark index provides a good estimate of growth in labor quality in the euro area.

The results show a continuous increase in human capital in the last 20 years. The average growth rate of euro area labor quality in 1984-2005 was 0.47% year-on-year, suggesting that up to one fourth of euro area labor productivity growth during this time period was due to improvements in human capital. A strong increase in labor quality growth in the early 1990s was driven by the stronger increase in the share of those with tertiary education, as well as an increase in the share of workers in prime age. Towards the end of the 1990s growth in labor quality moderated, possibly reflecting the impact of continued robust growth in employment and the entry of marginal workers with lower human capital. Most recently, labor quality growth increased from 2003 onwards, suggesting that the recent slow growth period may have been characterized by some cyclical ”up-skilling”. Further, we have illustrated the usefulness of the index in better understanding macroeconomic developments

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in the euro area. The results of an accounting exercise point to a significant and increasing role for changes in labor quality in explaining labor productivity growth. Accounting for positive labor quality growth lowers estimates of total factor productivity growth in the euro area.

The results show that the main drivers of changes in observed labor quality are higher education and labor market experience. While it is important to recognize that other (not measured) factors, such as quality and type of education are likely to also matter, the results suggest that economic policies designed to promote growth in euro area human capital should be geared towards an increase in educational attainment and increased on-the-job training. Needless to say, to avoid over-education, both education and training should be geared towards the needs of the job market. In this respect, changing demographics are likely to also have a strong impact on growth in labor quality in the future. While ageing of the working age population (until prime-age) generally increases average labor quality due to larger return to previous investment in human capital, it may result in lower incentives for current investment in human capital. Ageing is thus likely to result in downward pressure on the contribution of labor quality to aggregate productivity growth. At the same time, the results of the accounting exercise point to a decline in euro area total factor productivity growth. This decline argues for stronger emphasis on economic policies that promote innovation and the use of productivity enhancing technologies, as well as an increased focus on understanding the interactions between human capital and technological progress.

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3.A Tables

Table 3.1: Complete euro area results

(index: 1983=100)

	Total	First order indices			Second order indices			
		S	A	E	SA	SE	AE	SAE
1983	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00
1984	100.21	99.92	100.23	99.95	100.03	100.13	99.96	99.99
1985	100.71	99.88	100.40	100.19	100.03	100.22	99.99	100.00
1986	100.87	99.83	100.29	100.46	100.05	100.27	99.96	100.00
1987	101.12	99.80	100.33	100.73	100.05	100.24	99.97	100.01
1988	101.67	99.76	100.45	101.19	100.05	100.22	99.98	100.02
1989	102.11	99.73	100.60	101.57	100.06	100.17	99.96	100.01
1990	102.94	99.66	100.72	102.40	100.06	100.16	99.95	99.99
1991	103.72	99.47	100.97	103.14	100.03	100.14	99.96	100.00
1992	104.10	99.47	101.35	103.17	100.01	100.02	100.07	99.99
1993	104.92	99.45	101.78	103.67	99.97	99.95	100.06	100.00
1994	105.82	99.42	102.14	104.25	99.96	99.95	100.06	99.99
1995	106.39	99.41	102.39	104.64	99.93	99.92	100.06	99.99
1996	106.84	99.37	102.71	104.84	99.91	99.88	100.06	100.00
1997	107.46	99.37	102.90	105.30	99.89	99.86	100.06	100.00
1998	107.57	99.36	102.87	105.48	99.90	99.83	100.04	100.00
1999	107.86	99.31	102.84	105.86	99.90	99.82	100.04	100.00
2000	108.32	99.26	102.92	106.29	99.90	99.82	100.04	100.00
2001	108.66	99.22	103.13	106.48	99.88	99.81	100.03	100.01
2002	108.91	99.16	103.31	106.61	99.88	99.81	100.02	100.02
2003	109.51	99.12	103.60	106.95	99.87	99.81	100.02	100.02
2004	110.35	99.12	103.83	107.55	99.86	99.80	100.03	100.02
2005	110.89	99.12	104.03	107.86	99.82	99.82	100.04	100.03

Note: S refers to sex, A to age and E to education. SA is the second order contribution of sex and age.

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Table 3.2: Growth in labor quality: country estimates

(average annual growth rates)

	1984-1989	1990-1994	1995-1999	2000-2005	1984-2005	1984-2001
Euro Area	0.35	0.72	0.38	0.46	0.47	0.46
Germany	0.08	0.57	0.16	0.40	0.27	0.27
France	1.40	1.12	0.56	0.40	0.89	0.98
Italy	0.34	0.35	0.59	0.49	0.45	0.41
Jorgenson (2005):						
Germany	0.58	0.62	0.46	na.	na.	0.52
France	0.65	1.44	1.09	na.	na.	0.86
Italy	0.32	0.65	0.71	na.	na.	0.51

Source: authors' calculation and Jorgenson (2005)

Table 3.3: Growth in adjusted and unadjusted total factor productivity

(average annual growth rates)

	Unadjusted	Adjusted
1984	2.27	2.13
1985	1.40	1.08
1986	1.37	1.27
1987	1.32	1.16
1988	2.19	1.85
1989	2.20	1.94
1990	1.21	0.70
1991	-0.01	-0.48
1992	1.15	0.93
1993	-0.22	-0.71
1994	1.99	1.47
1995	1.38	1.05
1996	0.50	0.25
1997	1.60	1.25
1998	0.91	0.85
1999	1.09	0.93
2000	2.22	1.97
2001	0.40	0.22
2002	0.26	0.12
2003	-0.15	-0.47
2004	0.62	0.18
2005	0.10	-0.18

Source: authors' calculation

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3.B Figures

Figure 3.1: Growth in euro area labor quality: alternative indices

(annual growth rates)

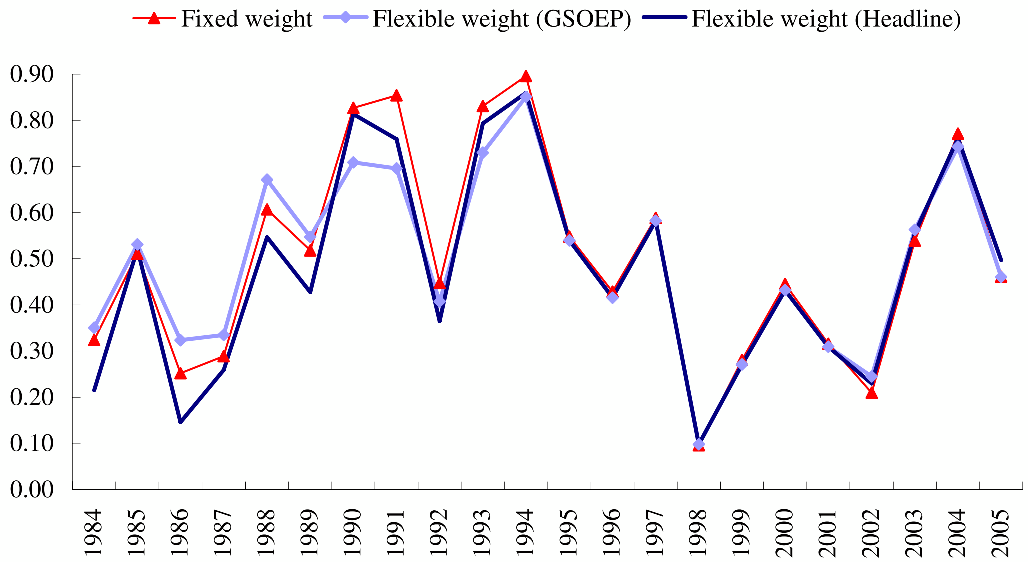

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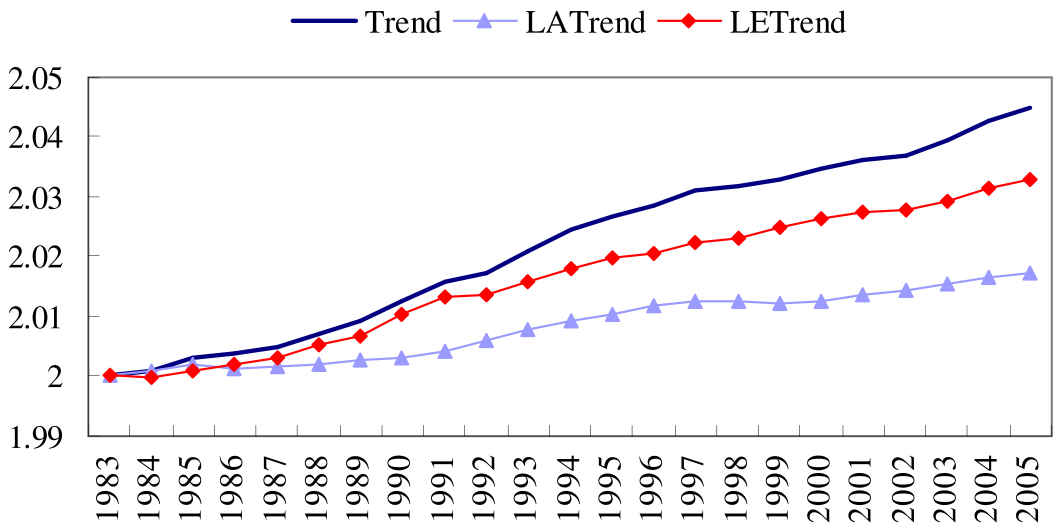
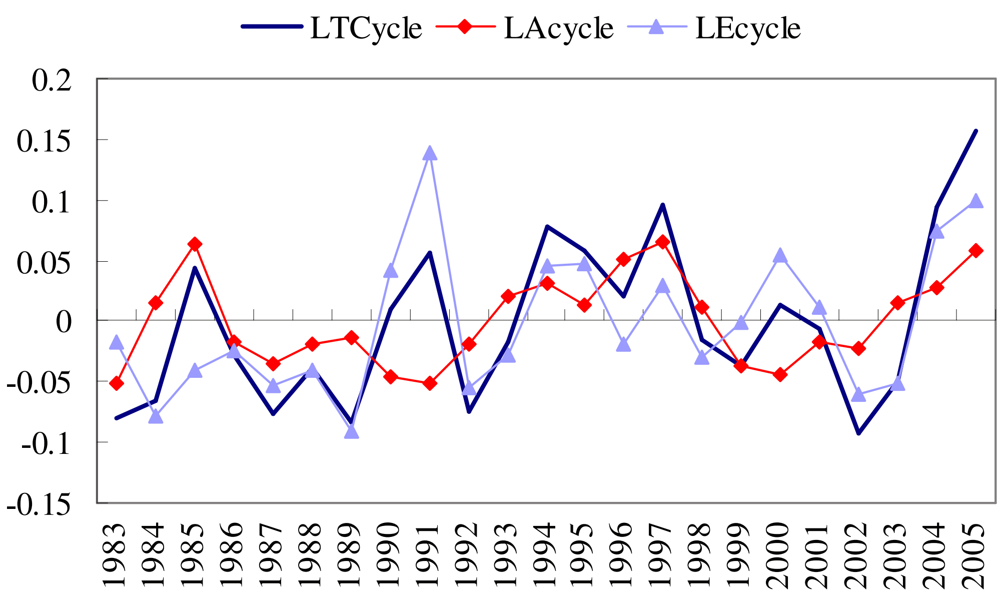
Figure 3.2: Trend/Cycle decomposition of labor quality growth

(log levels and percentage point deviations from trend)





The trend and cycle have been extracted using the Baxter-King band-pass filter (the cycle refers to the band between 2 and 8 year frequencies).

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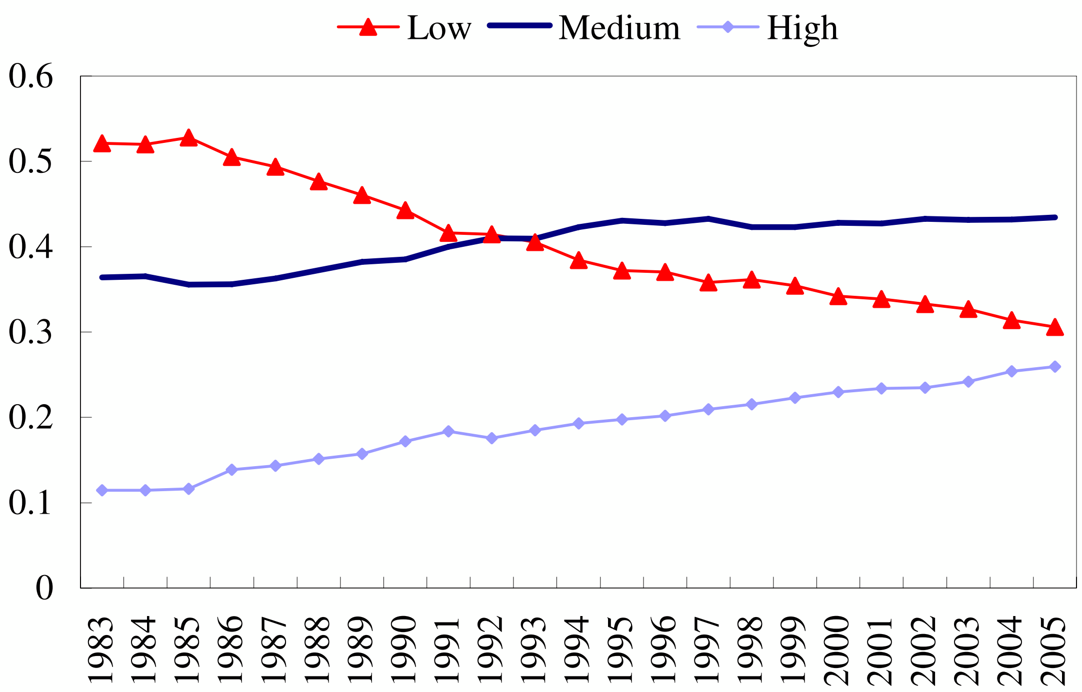
Figure 3.3: Hours worked by educational attainment

(shares)



Source: authors' calculation based on the Labor Force Survey. The shift in 1985 reflects the inclusion of Portugal and Spain for which data on hours is not available before 1985. The calculation of the labor quality index takes into account changes in the country composition.

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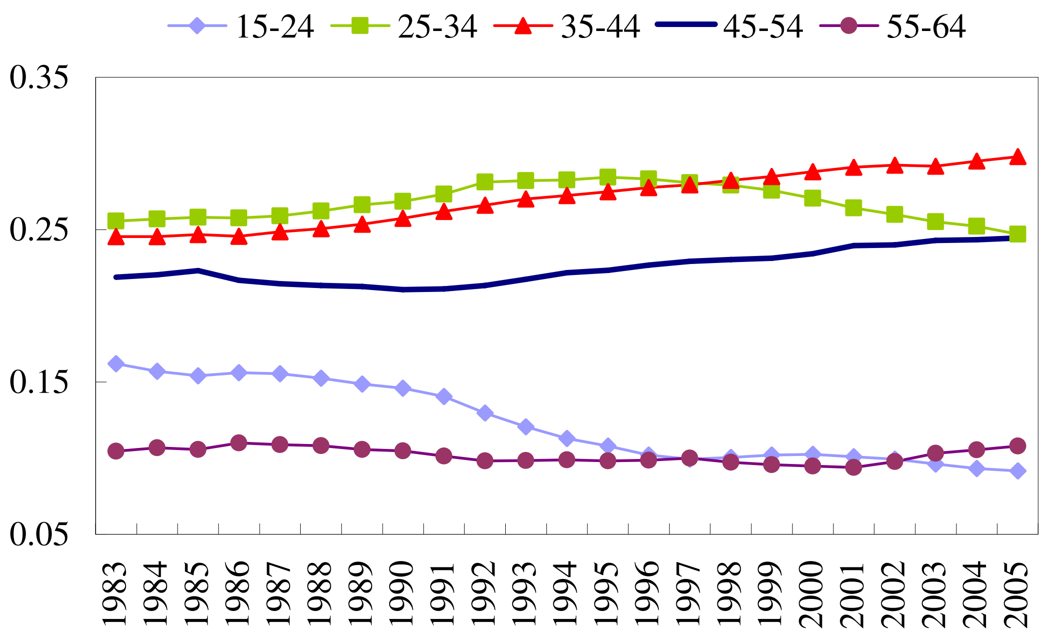
Figure 3.4: Hours worked by age groups

(shares)



Source: authors' calculation based on the Labor Force Survey.

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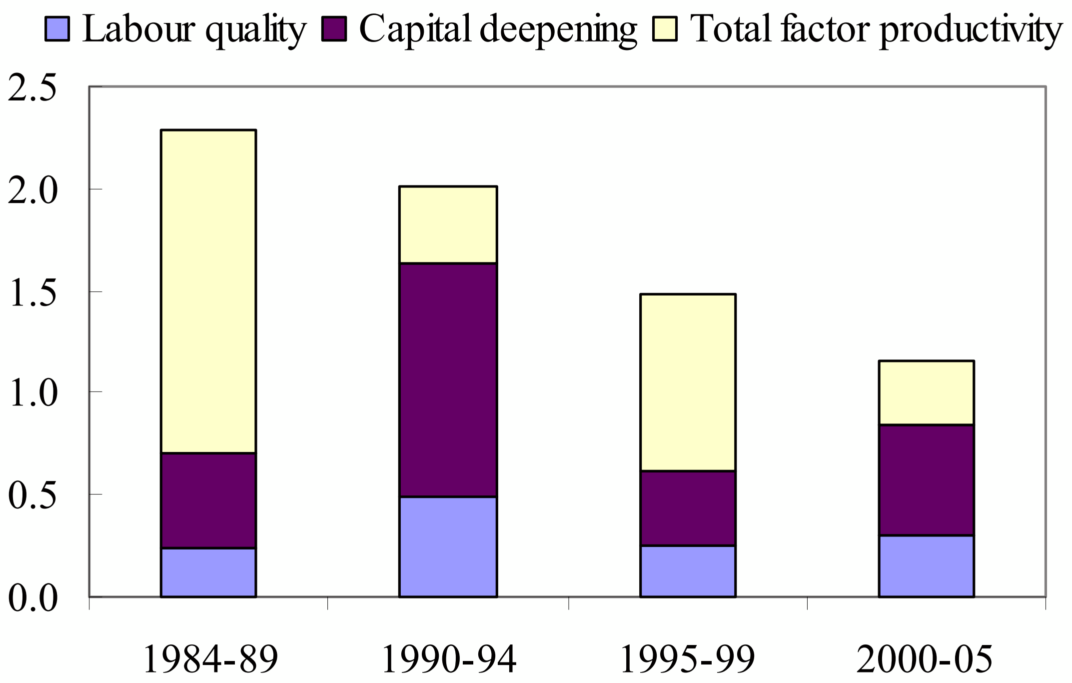
Figure 3.5: Decomposition of labor productivity growth

(contributions)



Source: authors' calculations. Data on real GDP and total hours worked are based on the Total Economy Database (September 2006) from the Groningen Growth and Development Centre. Data on (gross) capital stock are based on published ECB estimates (see ECB Monthly Bulletin, May 2006 for a description).

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Chapter 4

Why Does Consumption Fall at Retirement? Evidence from Germany[1]

4.1 Introduction

Recent studies for British and U.S. households have investigated the so-called ”Retirement-Consumption puzzle”. That is, we observe in the data a drop in consumption at retirement, while the standard life-cycle model predicts that individuals smooth (the marginal utility of) consumption.[2]

Some economists like Hurd and Rohwedder (2003) and Hurst (2003) argue that this discontinuity in consumption is consistent with permanent income models. Forward-looking rational agents adjust their consumption after retirement for reasons such as the sudden absence of work-related expenses or the substitution of home production for market-purchased goods and services.

[1]A shorter version of this chapter was published in Economic Letters. See Schwerdt (2005)

[2]See Banks, Blundell and Tanner (1998) for an early documentation of an one-time drop in consumption at retirement among British households.

Others like Bernheim, Skinner and Weinberg (2001) doubt that work-related expenses or leisure substitutes can account for the observed downfall in consumption. They find evidence for consumption tracking income over retirement, which casts some doubts on the standard life-cycle model. Their finding is much more in line with theories of heuristic rules of thumb or dynamically inconsistent decision-making.

So far empirical evidence on this matter is for the most part based on observations for U.S. and British households.[3] In this paper we follow the consumption patterns of German households with expanded data on household production. Unlike in the U.S. and Britain, the German pension system is based primarily on a mandatory public pension insurance on the pay-as-you-go basis.[4] This institutional setup leaves less responsibility to the individual and presumably less variation in the cross-section compared to a system in other countries that rely largely on private pension provisions or private savings. Therefore, unless one is willing to assume significant differences in preferences and rationality between Anglo-Saxon and German households, studying the consumption behavior of German households over retirement under this different environment should shed some light on the ”Retirement-Consumption puzzle”.

Using data from the German Socio Economic Panel (GSOEP) we find that individuals with low income replacement also discontinuously reduce their savings at retirement. However, the magnitude of the drop in savings is low

[3]One recent contribution to the literature by Miniaci, Monfardini and Weber (2003) documents the existence of a consumption drop at retirement also in Italy.

[4]Participation in the system is mandatory for all dependent employees and some groups of self-employed.

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relative to the drop in income.

Finally, we find a negative relation between the drop in consumption and changes in time devoted to activities that might be substitutes for market-purchased goods and services. However, also individuals with no drop in income increase their home production after they retire. Therefore an observed jump in home production at retirement cannot only be explained by the substitution of consumption.

4.2 Data

The GSOEP is a wide-ranging representative longitudinal study of private households in Germany that started in 1984.[5] Since 1992, it contains information about household savings.[6] Because consumption is not observed directly, We use the information on income and savings through 2002 to construct a measure for monthly consumption by deducting monthly savings from the monthly household income for each of the 11 years in the sample.

Since we are only interested in consumption close to retirement, we restrict the sample to households where the head of the household retired within the seven years between 1994 and 2000.[7] This restriction ensures that for every household information on consumption is available for at least two years before and after retirement. Furthermore, only household heads who report to be employed full-time in the year before they retire, are considered.

[5] For a detailed description see SOEPGroup (2001).

[6] The participants of the study were asked the following question:" Do you usually have an amount of money left over at the end of the month that you can save for larger purchases, emergency expenses or to acquire wealth?".

[7] Since the panel does not include direct information on the year of retirement, we identify the retirement year as follows: An individual retired in the year X if he worked more than 30 hours a week in the year before retirement, but less than 10 hours in the year after year X, is not registered as unemployed either in X or X+1 and is above 50 years of age in the year X.

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Additionally, we construct a measure for home production. The participants in the study were asked about their time use. We chose three activities that might be substitutes for market-purchased goods and services: errands (shopping, trips to government agencies, etc.), housework (washing, cooking, cleaning) and yardwork(repairs on and around the house, car repairs, work in the garden). Summing up the hours spent daily on these activities, we create a variable that is likely to proxy for home production (HP).

Altogether, 312 households fulfill the above criteria. We chose two variables by which we can split the sample into two parts: income replacement ratios (IRR) and the jump in home production over retirement (JHP). The split point is always the median value of the respective distribution. Table 4.1 in the appendix contains information about observable household characteristics belonging to either the lower or upper half of the distribution for both splitting categories.[8]

As expected, the income replacement rates are quite high. For the entire sample the average income replacement rate is 0.94 (with a standard deviation of 0.3). This is similar to the findings of Boersch-Supan, Reil-Held and Schnabel (2001).[9] The relatively high variation in replacement rates, however, might indicate measurement error in income.

[8]The income replacement ratios are computed by dividing the average monthly household income in the two years after retirement by the averaged income in the two years before retirement. The jump in home production is computed as the difference between the two respective post and pre-retirement averages.

[9]Boersch-Supan et al. (2001) report that retirement income in Germany is quite generous. It is on average 88 percent of workers' pre-retirement income. Note that their findings are based on cross-sectional comparison of worker cohorts. Given secular increases in productivity, replacement rates are likely to be considerably higher.

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4.3 Method

The sample allows to observe households at four different years relative to retirement. We pool these observations over time and estimate equations of the following kind:

$$\begin{aligned} Y_i &= X_i\delta + U_{-2i}\beta_{-2} + U_{-1i}\beta_{-1} + U_{+1i}\beta_{+1} + U_{+2i}\beta_{+2} \\ &\quad + L_{-2i}\gamma_{-2} + L_{+1i}\gamma_{+1} + L_{+2i}\gamma_{+2} + \varepsilon_i \end{aligned} \tag{4.1}$$

where Y_i is the dependent variable, X_i is a vector of household characteristics, Us and Ls are a set of interacted dummy variables indicating the time relative to retirement and whether the observation belongs to the upper or lower half of the distribution of the respective splitting variable and ε_i is a random disturbance. Note that lower-half households one year before retirement is the omitted category.[10]

Notice also that we are ultimately interested in the difference in the post/prior retirement differences between upper and lower half individuals. This difference is denoted by:

$$\Delta = \Delta U - \Delta L, \tag{4.2}$$

where ΔU is defined as $[(U_{+2i} + U_{+1i})/2] - [(U_{-2i} + U_{-1i})/2]$ and ΔL is

[10] In some regressions demeaned control variables $(X_i - \overline{X}_i)$ are used instead of the X_i's. In these cases the constant is omitted, while a dummy L_{-1} is included.

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defined analogously.

4.4 Results

Average consumption for the whole sample dropped by 8.5%, as can be seen from the first column of table 4.2, which reports the results from estimating equation (4.1) with log-consumption as the dependent variable and without splitting the sample.[11] This drop is roughly of the same magnitude as the average drop in income.

The top left panel of figure 4.1 shows how the consumption drop is distributed between income replacement groups. The figure is based on the estimated coefficients on the interaction terms from the second column of table 4.2 and is normalized for both income replacement groups to their respective log-consumption one year before retirement. As can be seen from the graph, only after retirement do the consumption paths differ. While consumption increases more than 10% for high income replacement individuals, it drops more than 30% for the low income replacement group. The difference in the consumption change after retirement between both income replacement groups is statically significant ($p < .01$).

This finding suggests that the consumption drop is negatively correlated with income replacement rates. However, by construction, consumption is the difference between income and savings, and so this result might be driven by measurement error. Since any measurement error in income translates into

[11] The controls are: sex of the head of household, his age at retirement, whether he is disabled or not, his education with respect to a high school degree, his marital status and the size of the household.

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measured consumption, a positive correlation between income replacement and consumption change is likely.

In order to check the robustness of the previous result we run the same regression with monthly savings as the dependent variable. The idea is that if we find no difference in the saving behavior between the two income replacement groups, then this would support the finding of a negative correlation between income replacement and the consumption drop. Notice, that measurement error will not systematically bias the results of this exercise in the way it did with consumption as the dependent variable, since savings and income are measured independently.

The results can be seen in the top right panel of figure 4.1. While high income replacement individuals do not significantly adjust their savings over retirement, low income replacement individuals lower their monthly savings significantly. The estimated coefficients of the regression are presented in the third column of table 4.2. Since this regression deploys demeaned controls, the estimated drop in savings for the low income replacement group, ΔL, can be interpreted as follows: An individual with average characteristics lowers his average monthly savings by 268 DM over retirement. This result is statistically significant ($p < .001$). However, average income drops by roughly 1500 DM. Hence, the reduction in savings cannot account for the drop in income, which supports the previous finding of a more pronounced consumption drop for low income replacement individuals. However, note also that measurement error in income might downward bias the estimated difference in the drop of average monthly savings. Therefore, 268 DM might

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be regarded as a lower bound for the difference in the savings drop.

One explanation given for the fall in consumption at retirement that can be tested with this data is the substitution of market consumption with home production. To do so, we split the sample according to the change in home production at retirement and redo the estimation exercises described above. The results can be seen in the bottom left panel of figure 4.1. Individuals with a high jump in home production experience a significant drop in consumption of 12 per cent. Individuals with a low increase in home production reduce consumption only by 6 per cent. The difference in the consumption drop between these two groups, however, is not significant.

To further test the substitution argument, we regress home production on a set of time dummies interacted with a dummy (high/low) for income replacement and a set of demeaned controls. The results can be seen in the bottom right panel of figure 4.1. Individuals in the low income replacement group increase their time spent on home production by 1 hour and 42 minutes a day compared to an increase of 1 hour and 12 minutes for high income replacement individuals.

This result can be interpreted in line with the assumption of forward looking rational decision makers: Individuals do not save against the anticipated drop in income, because they plan to substitute a fraction of their consumption with home production, given the sudden increase in leisure time. On the other hand, the observed correlation does not necessarily imply causation. It might as well be the case that individuals with low income replacement engage more in home production than others precisely because their drop in

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disposable income is larger.

Furthermore, time spent on home production also increases for the group of individuals with constant or even increasing income. Hence, the increased home production is not entirely due to a substitution effect. The remaining 30-minute difference in increased home production between income replacement groups, however, is fairly modest.

4.5 Conclusion

As documented before for the U.S. and Great Britain, we also find a one-time drop of roughly 8.5% in consumption at retirement using panel data for German households. By looking at independent measures of savings and income, we find that individuals with low income replacement also lower their monthly savings at retirement by nearly 270 DM, while the upper half of the income replacement distribution does not significantly adjust their savings on average. However, the magnitude of the drop in average monthly savings cannot compensate for the 1500 DM drop in average monthly income for the low income replacement group, which suggests a negative correlation between income replacement and the size of the consumption drop, as also documented in Bernheim et al. (2001).

Testing the validity of the optimal substitution argument, which can explain the consumption drop at retirement, we find a significant negative relation between income replacement groups and the increase in presumably home-production-related activities. Individuals belonging to the lower half of the

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income replacement distribution increase their home production by half an hour a day more than others. However, an increase in home production is also found when income stays the same or goes up indicating that individuals draw utility from home-production-related activities. Hence, the observed increase in home production after retirement is not entirely driven by the substitution of consumption, but also by an endowment effect that allows for an increase in home production and overall utility.

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4.A Tables

Table 4.1: Summary statistics for households by splitting categories

	IRR		JHP	
	LOW	HIGH	SMALL	BIG
IRR	0.73	1.16	0.99	0.91
JHP	1.71	1.20	-0.14	3.10
Female Head	0.18	0.17	0.19	0.16
HH Size	2.46	2.53	2.51	2.48
Disabled	0.30	0.27	0.25	0.32
Education	2.07	2.05	2.01	2.11
Retirement Age	59.77	58.31	58.17	59.93
Married	0.79	0.76	0.72	0.82
No. of HH	155	157	159	153

**The table reports the average income replacement rate, the average size of the jump in home production, the average size of the household, the fraction of disabled household heads, the average educational level of the household head , the average age of the household head at retirement, the fraction of female household heads and the number of households by splitting categories. The splitting categories are: income replacement rate and jump in home production over retirement.*

Table 4.2: Regression Results

	Dependent Variable				
	$ln(C_i)$	$ln(C_i)$	S_i	$ln(C_i)$	HP_i
	Splitting Category				
	no split	IRR	IRR^D	JHP	IRR^D
T_{-2}		-0.194	542.23	0.142	2.12
	0.006	(-4.24)	(5.86)	(3)	(10.27)
B_{-2}	(0.19)	-0.010	633.28	0.007	1.99
		(-0.22)	(6.72)	(0.15)	(9.48)
T_{-1}		-0.216	551.41	0.137	2.19
		(-4.72)	(5.95)	(2.89)	(10.61)
B_{-1}			599.21		2.07
			(6.36)		(9.85)
T_{+1}		-0.091	557.84	0.020	3.27
	-0.091	(-1.99)	(6.02)	(0.43)	(15.81)
B_{+1}	(-2.73)	-0.310	316.95	-0.067	3.79
		(-6.79)	(3.36)	(-1.44)	(18.04)
T_{+2}		-0.048	630.59	0.015	3.44
	-0.079	(-1.04)	(6.81)	(0.31	(16.65)
B_{+2}	(-2.37)	-0.330	378.8	-0.038	3.68
		(-7.22)	(4.02)	(-0.82)	(17.51)
Household Size	0.105	0.105	-33.44	0.104	-0.17
	(10.41)	(10.8)	(-1.48)	(10.39)	(-3.43)
Retirement Age	-0.007	-0.007	1.08	-0.009	0.02
	(-2.82)	(-2.72)	(0.19)	(-3.64)	(1.37)
Female Head	-0.066	-0.064	-5.37	-0.074	1.20
	(-1.79)	(-1.81)	(-0.07)	(-2.03)	(6.55)
Married	0.066	0.067	94.36	0.052	0.09
	(2.05)	(2.17)	(1.31)	(1.63)	(0.53)
Disabled	-0.063	-0.062	-227.98	-0.073	0.32
	(-2.32)	(-2.36)	(-3.73)	(-2.7)	(2.38)
Education	0.195	0.196	337.45	0.190	-0.04
	(10.12)	(10.53)	(7.8)	(9.89)	(-0.42)
constant	7.966	8.047		8.058	
	(46.76)	(47.47)		(46.76)	
ΔU		0.136	47.39	-0.122	1.2
		(4.22)	(0.63)	(-3.64)	(7.17)
ΔL		-0.315	-268.36	-0.056	1.71
		(-9.75)	(-3.57)	(-1.70)	(10.15)
$\Delta U - \Delta L$		0.451	315.76	-.066	-0.509
		(9.89)	(2.98)	(-1.40)	(-2.15)

**Sample size = 1248; 312 households. Dependent variable is either log monthly consumption, savings or hours spent daily on home production. A ^{D} indicates that demeaned controls are used in the regression. The last three rows report the estimated differences of interest. t-values are reported in parenthesis.*

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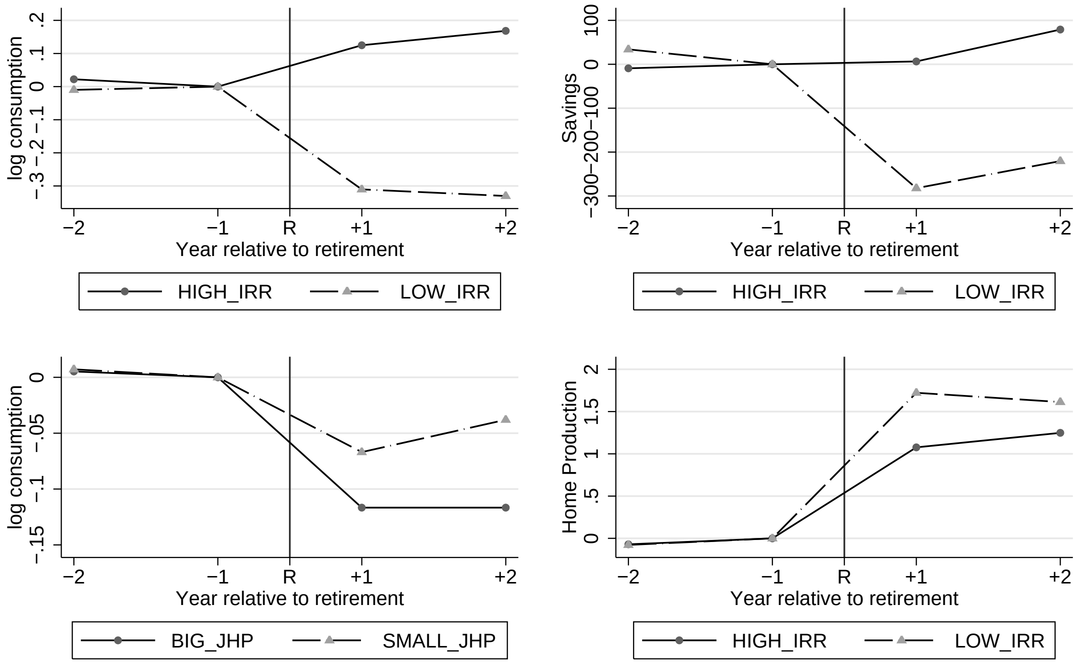
4.B Figures

Figure 4.1: Consumption, savings and home production relative to retirement



*Log−consumption, Savings and hours daily spend on home production are normalised to their level one year before retirement.

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