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Essays in Labor Economics

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Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others. The first chapter draws on work that was carried out jointly with Bernd Fitzenberger and Karsten Kohn, which is published in the Industrial & Labor Relations Review. I was responsible for the data analysis. The writing was shared equally.

Earlier versions of the second and third chapter have been proof read for spelling and grammar by Adam Lederer.

Abstract

My thesis combines three distinct papers in labor economics. The first chapter is a collaborative work with Bernd Fitzenberger and Karsten Kohn. In this chapter we scrutinize the effects of union density and of collective bargaining coverage on the distribution of wages both in the covered and the uncovered sector. Collective bargaining in Germany takes place at either the industry or firm level. Collective bargaining coverage is much greater than union density. The share of employees covered by collective bargaining in a single firm can vary between 0% and 100%. This institutional setup suggests that researchers should explicitly distinguish union density, coverage rate at the firm level, and coverage at the individual level. Using linked employer-employee data, we estimate OLS and quantile regressions of wages on these dimensions of union influence. A higher share of employees in a firm covered by industry-wide or firm-specific contracts is associated with higher wages, but there is no clear-cut effect on wage dispersion. Yet, holding coverage at the firm level constant, individual bargaining coverage is associated with a lower wage level and less wage dispersion. A greater union density reinforces the effects of coverage, but the effect of union density is negative at all points of the wage distribution for employees who work in firms without collective bargaining coverage. Greater union density thus compresses the wage distribution while moving the distribution in firms without coverage uniformly.

The second chapter evaluates the impact of the UK Working Time Regulations 1998, which introduced mandatory paid holiday entitlement. The regulation gave (nearly) all workers the right to a minimum of 4 weeks of paid holiday per a year. With constant weekly pay this change amounts effectively to an increase in the real hourly wage of about 8.5% for someone going from 0 to 4 weeks paid holiday per year, which should lead to adjustments in employment. For employees I use complementary log-log regression to account for right-censoring of employment spells. I find no increase in the hazard to exit employment within a year after treatment. Adjustments in wages cannot explain this result as they are increasing for the treated

groups relative to the control. I also evaluate the long run trend in aggregate employment, using the predicted treatment probabilities in a difference-in-differences framework. Here I find a small and statistically significant decrease in employment. This effect is driven by a trend reversal in employment, coinciding with the treatment.

The third chapter considers how the availability of a personal computer at home changed employment for married women. I develop a theoretical model that motivates the empirical specifications. Using data from the U.S. CPS from 1984 to 2003, I find that employment is 1.5 to 7 percentage points higher for women in households with a computer. The model predicts that the increase in employment is driven by higher wages. I find having a computer at home is associated with higher wages, and employment in more computer intensive occupations, which is consistent with the model. Decomposing the changes by educational attainment shows that both women with low levels of education (high school diploma or less) and women with the highest levels of education (Master's degree or more) have high returns from home computers.

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Chapter 1

Union Density and Varieties of Coverage: The Anatomy of Union Wage Effects in Germany

1.1 Introduction

> *"Union coverage should not be considered a natural extension of union membership [...] only in half a dozen OECD countries with predominantly company bargaining do the two go closely together. By contrast, in sectoral bargaining systems employer behavior combined with administrative governance of collective contracts may be more a determinant of coverage rates than union membership."*
>
> *OECD (2004, p. 147)*

The impact of wage setting institutions on labor market performance is a recurring key issue in policy debates (OECD 1994, 2004, 2006). It is often argued that less rigid wage setting institutions allow for greater wage flexibility, which—in face of asymmetric shocks—is a necessary ingredient for a well functioning labor market. Wage bargaining institutions differ strongly across OECD countries, with pronounced cross-country differences in the share of employees covered by collective bargaining (coverage rate) and in the share of union members among employees (union density) (OECD 2004). Under firm-specific bargaining—the prevailing institutional setup in the U.S. and a small number of other OECD countries—coverage by collective bargaining and union density basically coincide in the private sector. In contrast, in many European countries, there exists centralized wage bargaining (mostly at the sectoral level) between unions and employers' associations, and the coverage rate among employees is typically higher than union density.[1]

Empirical studies typically take union membership and union recognition or collective bargaining coverage as mere alternatives to measure union effects (Lewis 1986, OECD 2004). Studies for Germany have so far been confined to using *either* collective bargaining coverage (Stephan and Gerlach 2003, 2005, Gürtzgen 2009a,b) *or* union density (Fitzenberger and Kohn 2005). We emphasize that union density and coverage (union recognition) are more than just two different indicators of collective bargaining in Germany. Union density reflects the bargaining power of unions. It is likely to have a positive effect both on wages in the covered sector and on coverage rates. Coverage rates and wages in the covered sector may also affect wage outcomes in the uncovered sector (Pencavel 1991, Chapter 6). The direction of these effects is ambiguous (Kahn 1978, Neumark and Wachter 1995). On the one hand, the *threat effect* implies that wages increase in the uncovered sector in response to higher coverage rates or to higher wages in the covered sector because uncovered firms want to discourage coverage. On the other hand, the *crowding effect* presumes that wage increases in the covered sector or increases in the coverage rates lower employment in the covered sector. For uncovered firms, this raises relative labor supply, resulting in downward pressure on wages in a competitive labor market.

Our paper scrutinizes the effects of union density *and* of collective bargaining coverage on the distribution of wages both in the covered and the uncovered sector. We argue that, on the one hand, union density is a proxy for union power and therefore influences the bargaining outcome in the covered sector. Collective bargaining coverage, on the other hand, captures the actual application of bargaining agreements. Our empirical analysis also accounts for the likely interaction between union density and coverage as well as for spillover effects of union density on the uncovered sector.

Our contribution to the literature is fourfold: First, there is only a small international literature which accounts for differential wage effects of union density

[1]OECD (2004, Chart 3.4) reports that coverage exceeds union density by more than 10 percentage points (pp) for 17 OECD countries and by more than 20 pp for 12 OECD countries.

and coverage in the covered sector, and no such study has yet been conducted for Germany. Our paper is the first to distinguish simultaneously (i) coverage by a firm-specific collective agreement (FC) versus coverage by a sector-wide collective agreement (SC), (ii) firm-level coverage measured by the share of covered employees in the firm versus coverage of individual workers, and (iii) union density. We estimate the wage effects of both coverage and union density, also accounting for the effect of union density on the uncovered sector. Second, we use a unique linked employer-employee dataset, the German Structure of Earnings Survey (GSES) 2001, which records precisely the coverage status of individual employees, but which does not include information on union membership. Third, we impute union density for homogeneously defined labor market segments based on estimated membership equations in Fitzenberger et al. (2011). As a limitation, we acknowledge that we ignore the estimation error in the imputation. Fourth, we provide both OLS and quantile regression estimates in order to estimate impacts on wage levels and on wage dispersion. As a methodological contribution, we suggest a modification of the approach taken by Angrist et al. (2006) for estimating a weighted quantile regression with clustered standard errors.

Our empirical results show a positive effect of the share of employees in a firm covered by either industry-wide or firm-specific contracts on the level of wages in the firm. At the same time, holding the share of covered employees in the firm constant, uncovered employees earn higher wages than covered employees in the same firm and this gap increases along the wage distribution. A greater union density is associated with higher wages for covered employees. Both coverage and greater union density are associated with lower wage dispersion among covered employees. The effects of union density on the wage level and on wage dispersion in the sector of uncovered firms are negative. This suggests a dominance of the crowding effect over the threat effect. But, at the same time, the threat effect may be responsible for mitigating the negative wage effect in the lower part of the wage distribution. Overall, our findings are consistent with the view that unions increase wage levels and decrease wage dispersion among covered employees, and that negative crowding effects dominate in the uncovered sector.

The remainder of this paper is organized as follows. Section 1.2 discusses the institutional setting in Germany. Followed by a brief review of the related literature on union wage effects in section 1.3. The dataset used in our analysis is described in Section 1.4, which also provides some descriptive evidence. Section 1.5 presents the econometric approach and discusses the estimation results, and Section 1.6 concludes. The Appendix provides details about the econometric approach and about the data used. The extended appendix (1.8) includes further detailed information and results.

1.2 Institutional Background

Germany is a key case where coverage exceeds union density largely because of contract recognitions by employers, which are voluntary in a legal sense. According to the German Collective Bargaining Act *(Tarifvertragsgesetz)*, collectively negotiated agreements are necessarily binding for a specific job match if the firm is a member of an employer association and the worker is a union member. However, the scope of collective agreements, in particular the scope of sectoral wage agreements, goes beyond organized parties. Not only do negotiation outcomes cover union members, but employers often recognize collective bargaining outcomes for most of their employees without ex ante legal obligation. Moreover, collectively negotiated agreements must not dictate individual wage premia for union members (compared to non-members) due to the freedom not to associate as a principle of the German constitution *(negative Koalitionsfreiheit)*.

1.2.1 Union Density

Union membership, which had merely shown some variation with the business cycle in former decades, has been declining steadily during more recent decades (Ebbinghaus (2003); Fitzenberger, Kohn, and Wang (2006)). The early 1980's mark the beginning of a pronounced trend towards deunionization: having started out at a gross union density (GUD, defined as the ratio of union members to the number of employees in the labor market) of about 40%, GUD was down to a historically low level of 27% by the year 2004. Deunionization was interrupted by a unification effect in 1990, when West German unions were very successful in recruiting members in unified Germany. However, the upsurge in aggregate GUD was not sustainable, and deunionization continued even more rapidly in the 1990's and 2000's. Some trade unions have responded to the decline in size by merging (Keller (2005)). However, unions have not been able to reverse the trend (Ebbinghaus (2003); Fichter (1997)).

Union density governs a union's threat point in the collective bargaining process and is therefore pivotal to the bargaining outcome. Fitzenberger and Kohn (2005) argue that net union density, i. e., the share of employed union members among the number of employees, is an appropriate measure for union power. The higher the number of union members paying membership fees, the higher is the union's funding. In case of industrial conflicts, higher financial power enables the union to pay strike benefits for a longer period of time. Financial power and union representation at the shop floor increase individual support for union action, the probability and the length of a strike, and therefore the expected damage inflicted upon employers. Furthermore, financially powerful unions can invest more in public relations in order to sanitize their public image. Yet, financial obligations also increase with the size of the union. Relative financial power is thus mirrored best by the share of contributors among potentially represented workers. Moreover, as union growth comes along with increased heterogeneity within the union, conflicting interests and contradictory statements increasingly undermine the union's representative role; see also Ebbinghaus (2003) and Keller (2005). Thus net union density is preferable as compared to both gross union density and the absolute number of union members.

Net union density (NUD) can not be inferred from union records and thus has to be estimated. A number of studies estimates individual determinants of union membership based on survey data.[2] Estimated membership propensities can then be used to project NUD. Aggregate NUD usually falls short of GUD by about 10 percentage points. Fitzenberger, Kohn, and Wang (2006) report that after German unification, membership in East Germany started out at a higher level than in West Germany but exhibited a stronger decline afterwards. Aggregate NUD for the years 1993 and 2003 were 38% and 19% in East Germany, and 27% and 21% in West Germany.

In our empirical analysis, we conceptualize union power by union density in homogenous labor market segments defined by region, employee characteristics, and

[2] Lorenz and Wagner (1991), Fitzenberger, Haggeney, and Ernst (1999), Schnabel and Wagner (2003, 2005, 2008), Beck and Fitzenberger (2004), Goerke and Pannenberg (2004), Fitzenberger, Kohn, and Wang (2006).

firm characteristic, and we argue that union density in a labor market segment has a positive impact on coverage. The labor market segments reflect the structure of the German wage bargaining system. The regional dimension and the sector classification account for the fact that collective negotiations take place at the industry level in different bargaining regions. The fact that collective agreements further distinguish various wage groups is captured by the two dimensions skill and age. The aggregation for the labor market segments enables us to analyze the effect of union power independent of individual membership. In the German context, it would make little sense to estimate wage effects of individual membership because collectively negotiated premia for union members are forbidden and coverage is a firm decision. This decision depends upon union power, which we proxy by NUD, in a labor market segment. Hence, even if individual membership were available in the data, we would continue to use an aggregate measure of NUD.

1.2.2 Collective Bargaining Coverage

Coverage by collective wage bargaining in Germany is a firm decision, which is voluntary in an ex ante legal sense. When recognizing sectoral collective bargaining agreements or when engaging in firm-specific bargaining, firms do respond to the demands of their employees and to the bargaining power of unions as employees' representatives. When firms decide to quit a collective bargaining agreement after it has expired, the agreement remains binding for the incumbent covered workforce for a while, and during that time, the firm can only deviate from the collective agreement for new hirings. In this context three distinct wage bargaining regimes coexist: sectoral level bargaining, firm-level bargaining, and individual level bargaining.

First, a large share of employees is covered by *sectoral* wage bargaining agreements (henceforth denoted as sectoral contracts, SC), which are based on centralized negotiations at the industry level. Employers covered by a sectoral agreement may pay higher wages than the contract wage according to the favorability principle (OECD 2004, p. 154). This results in a wage cushion or a wage drift.[3] The first regime also includes extensions of sectoral agreements by the government to an

[3] A wage cushion is also observed in other European countries (see, e. g., Cardoso and Portugal 2005).

entire industry.[4]

Second, a smaller share of employees is covered by *firm-level* agreements, which are specific to employees in one firm (henceforth denoted as firm-specific contracts, FC). Firm-specific bargaining and sectoral bargaining are *mutually exclusive* alternatives in Germany, i. e. firm-specific bargaining is not a second round of bargaining supplementing a sectoral agreement.[5] These agreements are either between a union and a firm, or a works council and a firm, where the latter is only allowed to govern wages or salaries if the firm is not subject to a collective contract or if the collective contract explicitly allows for this type of arrangement. Firm-level agreements involving a union are allowed to set wages even if a collective agreement exists, as long as the firm-level agreement is more specific than the collective agreement.

The third wage bargaining regime involves those employees who are *not covered* by collective wage bargaining at all (henceforth denoted as individual wage contracts, IC). The uncovered sector has grown in recent decades (OECD 2004, p. 154). Employees with individual contracts can work in both covered and uncovered firms. Where different groups of employees are expected to be selected out of collective coverage in covered firms. On the one hand, uncovered employees in a firm with positive coverage may be paid less than covered employees when the firm uses non-coverage as an instrument for downward wage flexibility. For example, the firm may cease to recognize a collective agreement for newly hired employees while the incumbent workforce remains covered for a while. On the other hand, uncovered employees in a firm with positive coverage may be aid more than covered employees when the firm wants to pay higher wages to highly productive employees, possibly using more flexible payment schemes.

Collective agreements constituting discriminatory wage policies with disadvantages for non-union members are forbidden by constitutional law (negative freedom of association, *negative Koalitionsfreiheit, Grundgesetz Art. 9*). As wage gains from union membership are not internalized, there exists a free-rider problem of missing individual incentives to join a union.[6] As a result, union density is considerably

[4]Contract extensions by the government have played only a negligible role until recently. Less than 1% of all employees are covered in the early 2000s due to such contract extensions (BMWA 2004).

[5]In Spain, for instance, firm-specific bargaining is a second round of bargaining (Card and de la Rica 2006).

[6]However, there are additional motives for union membership. The literature discusses se-

smaller than collective bargaining coverage. The design of the German wage-setting system thus suggests to distinguish explicitly between the effects (i) of different bargaining regimes, (ii) of bargaining coverage both at the individual and at the firm level, and (iii) of union density in a labor market segment. However, in light of the free-rider problem and of the fact that collective agreements can not discriminate between members and nonmembers, it is not meaningful to estimate a wage premium for individual membership, as done in most studies for the UK or the US (Lewis 1986, Card, Lemieux, and Riddell 2003).

While the negative freedom of association forbids collective bargaining agreements dictating individual wage premia for union members in comparison to nonmembers, wages set at the firm level and at the individual level take collective bargaining outcomes as a reference point, be it in order to reduce transactions costs, to prevent employees from joining a union (threat effect), to elicit higher effort by employees (Pencavel 1991, Chapter 6.3), or to commit to a minimum wage level in order to incentivize training effort (Dustmann and Schönberg 2009). As a result, coverage is considerably higher than union density (Schnabel 2005).

1.3 Related Literature

We first provide a short overview of the related literature on wage effects of collective bargaining and then discuss specifically the effects of union density and bargaining coverage.

Bargaining models treat the negotiation of wages in the covered sector as a rent-sharing problem, the solution to which depends upon the bargaining power of the negotiating parties (e. g., in Nash bargaining). Unions bargain for a higher wage level for the work force they represent. The monopoly union model, the right-to-manage model, or the efficient bargaining model predict a positive relationship between union power and the level of wages (Farber 1986, Oswald 1985, Pencavel 1991, Naylor 2003). Union density, i. e. the share of employed union members among all employees, is a measure of union power in Germany (Fitzenberger and Kohn 2005).[7]

lective incentives provided in addition to public goods (Olson 1965), collective-voice mechanisms (Hirschmann 1970), or the existence of social norms (Akerlof 1980, Booth 1985).

[7]The German literature refers to the share of employed union members among all employees as *net union density* in order to distinguish this measure from the ratio of all union members (employed or non-employed) to the number of employees (*gross union density*).

The higher the number of union members paying membership fees, the higher is the union's funding for union action (e. g., paying strike benefits). Financial funding and union representation at the shop floor increase individual support for union action, which raises the ability to undertake a strike and the possible damage inflicted upon employers in case of a strike.

The empirical literature finds that collective bargaining is associated with higher wage levels and lower wage dispersion (Pencavel 1991, Card 1996, Addison and Schnabel 2003). Card and de la Rica (2006) argue that firm-specific bargaining allows employees with a strong bargaining position (e. g., due to high levels of specific human capital) to seize higher rents compared to more centralized forms of collective bargaining (see Hartog et al. 2002 for a contrary view).

Some studies incorporate effects of collective bargaining on aspects of the entire wage distribution (see Card, Lemieux, and Riddell (2003) for a survey of the empirical evidence on unions and wage inequality). Agell and Lommerud (1992) and Burda (1995) argue that risk averse employees have a preference for wage compression. Unions as agents of employees may be willing to trade off a lower average wage against less wage inequality. If the earnings of employees depend upon different states of nature such as demand shocks on firms' product market, a union will bargain for a compression of the wage distribution relative to the productivity distribution (Guiso et al. 2005). A compression effect is also consistent with search and matching theories (Mortensen and Pissarides 1999). By enforcing 'equal pay for equal work', a union additionally seeks to limit favoritism and discrimination by superiors and colleagues and to encourage solidarity among the work force (Freeman 1982). These arguments predict a negative relationship between union power and wage dispersion.

German collective agreements do not constrain a firm's right to pay premia above the wage set in the collective contract (this reflects the favorability principle) such that the actual wage may exceed the contractual wage. Cardoso and Portugal (2005) refer to this gap as the *wage cushion* in order to distinguish it from the *wage drift* (the change of the gap). They find that the positive effect of union strength—as measured by the share of covered employees—on the level of contractual wages is partly offset by a smaller wage cushion. As in Card and de la Rica (2006), firms covered by (multi- or single-) firm-specific agreements pay higher wages than firms covered by industry-wide agreements.

In line with the existence of a wage cushion, the impact of collective bargaining is likely to vary along the wage distribution. If contractual wages serve as wage floors, the wage distribution is compressed from below. Büttner and Fitzenberger (2003) assume that efficiency wages are paid in the upper part of the productivity distribution, whereas contractual wages above productivity are binding for less productive employees. Then, a more powerful union will bargain for a stronger compression of the wage distribution from below.

The effects of different bargaining regimes are likely to interact with country-specific institutions and social norms (Flanagan 1999). Card and de la Rica (2006) find that firm-specific bargaining in Spain results in higher wages compared to wages set in regional and national contracts, while Hartog et al. (2002) report only minor differences between bargaining regimes in the Netherlands. Analyzing union wage effects in Italy, Dell'Aringa and Lucifora (1994) find a positive effect of recognition and a negative effect of plant-level union density.

The literature typically takes union membership and union recognition or collective bargaining coverage as mere alternatives to measure union effects (Lewis 1986, OECD 2004).[8] Differential wage effects of coverage and union density may arise from spillover effects between the covered and the uncovered sector (Kahn 1978, chapter 6 of Pencavel 1991, Neumark and Wachter 1995).[9] On the one hand, a positive relationship between wages in the covered sector (and thus coverage rate or union density) and wages in the uncovered sector arises because wage outcomes in the covered sector are taken as a reference point in the uncovered sector, possibly in order to avoid coverage (threat effect).[10] On the other hand, a negative relationship between wages in the covered sector (and thus coverage rate and union density) and wages in the uncovered sector may exist because excess labor supply

[8]Only a small literature for the U.S. and the UK focuses on the question whether coverage and membership (as different measures of union activity) have conceptually different impacts on wages (see, e. g., Hunt et al. 1987 or Koevets 2007). Because of the strong institutional differences between the U.S. and the UK on the one hand and Germany on the other hand, we do not review these studies in more detail.

[9]Pencavel (1991, Chapter 6) emphasizes the equilibrium nature of the relationship between union and nonunion wages, the two being jointly determined.

[10]In addition to the threat effect, a positive spillover effect could be at work due to rents from specific human capital and the existence of hiring and firing costs, or to efficiency wage considerations (Pencavel 1991, Chapter 6). Rosen (1969) calls spillover effects other than the threat effect "indirect" effects.

from the covered sector exerts pressure on the wage level in the uncovered sector (crowding effect). The latter effect may be reinforced by a decline of investment in the uncovered sector when union density increases (Vogel 2007). The empirical evidence for the U.S. is ambiguous. Using union density as measure of unionism, Kahn (1978) finds that the crowding effect dominates the threat effect, while Neumark and Wachter (1995) confirm this result at the industry level but not at the city level.

1.4 Data

Our empirical analysis is based on the German Structure of Earnings Survey (GSES, *Gehalts- und Lohnstrukturerhebung*) for the year 2001. We restrict our analysis to West Germany.[11]

1.4.1 German Structure of Earnings Survey 2001

The GSES is a cross-sectional linked employer-employee dataset containing about 850,000 employees in some 22,000 firms. While mainly omitting the public sector, the GSES covers the major part of industry and private services. There are several advantages of using the GSES 2001. It is one of the largest mandatory firm-level surveys available for Germany. Therefore, the data are more reliable than individual-level surveys or data collected without reporting obligation (Jacobebbinghaus 2002). The GSES not only includes workers in regular employment, but also employees in vocational training, marginal employment, or partial retirement schemes. In the GSES 2001, wages are neither truncated nor censored such that the lower and the upper parts of the wage distribution can be analyzed precisely. Most importantly, the GSES provides not only firm-level information on bargaining coverage, but also the coverage status for each individual worker.

[11]The reasons for this decision are as follows. First, the labor market in East Germany is still affected by the transition following the German unification. Second, union policy in East Germany is strongly oriented towards catching up to West German standards. Third, union action in East Germany relies on support by West German unions. Thus, it is unlikely that East German unions act independently. To give an example: When the metal workers' union went on strike in the year 2003 in East Germany to achieve the equalization of standard hours of work, the union had to rely on support from West Germany to fill their ranks (see the newspaper DIE WELT dated June 23, 2003). By the time the strike affected West German firms, this support declined quickly, and the strike was discontinued without success.

The GSES 2001 has rarely been used in the literature. Until recently, analyses with GSES data have been restricted to administrative use or to regional subsamples (Stephan and Gerlach (2003), Gerlach and Stephan (2006), Heinbach and Spindler (2007)). For extensive descriptions of the dataset see Hafner (2005), Hafner and Lenz (2008), and Statistisches Bundesamt (2000, 2004). We use the on-site-use version of the GSES available at the Research Data Center of the statistical offices of the federal states (*Länder*) in Wiesbaden. Details on our selection of data are provided in Appendix 1.7. We focus on prime-age male employees working full-time and analyze hourly wages for both blue-collar and white-collar workers. Our analysis controls for differences between the two groups of employees, and it excludes white-collar workers in upper and middle management positions who would be paid without reference to the system of collective wage setting. Definitions and summary statistics of the full set of variables used in our analysis are reported in the extended appendix (1.8).

1.4.2 Imputation of Union Density

The GSES does not provide information on union membership. We therefore extend the dataset by imputing union density as follows. In a first step, we project individual propensities for union membership for the observations in our sample based on the estimates in Fitzenberger et al. (2011), who estimate determinants of union membership by means of panel probit regressions based on data from the German Socio-Economic Panel (GSOEP), a population survey containing information on union membership status.[12]

In a second step, union density (UD) in a homogenous labor market segment is obtained by aggregating individual membership propensities in cells defined by region (7 states) × industry (30 sectors) × skill (4 groups defined by educational attainment) × age (7 five-year brackets),[13] yielding a total of 5,841 non-empty cells, denoted as labor market segments. The imputed UD is the same for all employees

[12]The projection is based on model specification (E) in the working paper Fitzenberger et al. (2006). This specification uses only explanatory variables which are available in both the GSES and the GSOEP. The variables used involve age, gender, educational attainment, employment status (white collar workers, blue collar workers, civil servants; part-timers vs. full-timers), earnings, tenure, firm size, industry, and a set of time dummies. The empirical model is estimated separately for West Germany and East Germany. The imputations for the purpose of this paper are based on estimates for West Germany.

[13]The highest age bracket contains only 55-year-old employees.

in a labor market segment, and we do not distinguish UD by type of bargaining because within a labor market segment it would be the same union which engages both in sectoral and firm-specific bargaining.

The above definition of labor market segments reflects the structure of the German wage bargaining system. The regional dimension and the sector classification account for the fact that collective negotiations take place at the industry level in different bargaining regions. The fact that collective agreements further distinguish various wage groups is captured by the two dimensions skill and age. The aggregation for the labor market segments enables us to analyze the effect of union power independent of individual membership. In the German context, it would make little sense to estimate wage effects of individual membership because collectively negotiated premia for union members are forbidden and coverage is a firm decision. This decision depends upon union power, which we proxy by UD, in a labor market segment. Hence, even if individual membership were available in the data, we would continue to use an aggregate measure of UD.

1.4.3 Descriptive Evidence

We focus on male employees (both blue- and white-collar) in West Germany (excluding Berlin) and distinguish the following three regimes of bargaining coverage:

(SC) sectoral collective contract negotiated between an employers' association and a union,

(FC) firm-specific contract negotiated between a single firm and representatives of its employees (typically a union), and

(IC) individual contract negotiated between employee and employer.

The first column of Table 1.1 displays the size of the respective regimes in our GSES sample. The numbers are broadly in line with the literature cited above, accounting for differences in the datasets used. 57% of West German employees are paid according to sectoral collective contracts, henceforth called sectoral contracts. With another 8% covered by firm-specific contracts, this leaves about a third with individual contracts.

– Table 1.1 about here –

The share of covered employees within firms follows a bimodal distribution (Figure 1.1). About 40% of firms in the sample do not apply any sectoral or firm-specific contracts at all. In another 7% of firms, all employees are covered. This leaves more than one half of all firms with partial coverage. This fact enables us to distinguish coverage effects at the individual level and at the firm level. Typically, the majority of employees is covered in firms with a positive share of covered employees.

– Figure 1.1 about here –

Columns two and three of Table 1.1 concern log hourly wages by wage-setting regimes. On average, employees with individual contracts earn the lowest wages (2.783). Wages paid according to a sectoral collective agreement are markedly higher (2.818), and the highest wages are paid by firms subject to a firm-specific contract (2.852). Wage dispersion as measured by the standard deviation of log hourly wages is lowest among employees under sectoral coverage (0.284) and only slightly higher in case of firm-specific contracts (0.315). Employees with individually negotiated wages face a considerably higher standard deviation (0.412).[14]

Across the 5,841 labor market segments (cells) in our data, the average imputed union density UD is 18.5%. Weighted by cell employment, the weighted average of UD is 22.4%. Note that UD is not identical for all employees in a firm. UD is markedly lower than collective bargaining coverage. Again, the numbers are in accordance with the literature.

1.5 Econometric Investigation

The literature and the discussion in the previous sections suggest that both coverage by collective bargaining and a greater union density are associated with lower wage dispersion. Coverage is also associated with higher wage levels while the link between union density and the wage level is ambiguous. Sectoral collective contracts may be associated with lower wage inequality than firm-specific contracts.

Observed differences in wage levels and in wage dispersion are not necessarily

[14] More detailed evidence on the wage distributions of different groups of employees (men, women working full-time, and women working part-time, separate for blue-collar and white-collar workers in East and West German firms) is provided by Kohn and Lembcke (2007).

caused by the prevalence of different bargaining regimes. First, they may conceal differences in union power between different labor market segments. Second, they may result from underlying heterogeneity in employee or firm characteristics. We investigate both of these issues by means of OLS and quantile wage regressions. Our analysis distinguishes: (i) coverage by a sectoral collective contract versus coverage by a firm-specific collective contract, (ii) extent of coverage at the firm level versus coverage of individual workers, and (iii) union density. Moreover, we carefully analyze interaction effects between the different measures. As a caveat, our analysis does not take account of the estimation error in union density.[15]

1.5.1 OLS Wage Regressions

We analyze the different channels of union impact on the wage level by means of wage regressions with individual and firm controls. Log hourly wages ($log(w)$) are regressed on the set of covariates $X \equiv [Z, F, V]$, including individual worker characteristics Z, firm characteristics F, and a vector of union variables V. The estimates are based on a sample of individuals $i = 1, ..., N$ in firms $c = 1, ..., C$. Weights are used to account for different sampling probabilities. Moreover, since our data are sampled at the firm level and X contains information from different levels of aggregation, the estimated covariance of the estimator $\hat{\beta}$ takes account of clustering at the firm level (Froot 1989, Moulton 1990, Williams 2000).

The set of union variables V contains individual dummy variables for (a) coverage by sectoral contracts (SC) or firm-specific contracts (FC), leaving individual contracts as the base category, (b) the share of employees in each firm covered by a sectoral contract (SHARE_SC) or a firm-specific contract (SHARE_FC),[16] and (c) union density (UD). Allowing for interaction effects between the variables from different levels, a benchmark specification can be written as

$$\begin{aligned} log(w_{ic}) \; &= \; \beta_0 + Z_{ic}\beta_Z + F_c\beta_F + SC_{ic}\beta_{V1} + FC_{ic}\beta_{V2} + SHARE\_SC_c\beta_{V3} \\ &\quad + SHARE\_FC_c\beta_{V4} + UD_{ic}\beta_{V5} + SC_{ic} \cdot SHARE\_SC_c\beta_{V6} \end{aligned} \tag{1.1}$$

[15] Note that union density is calculated as an average in a labor market segment, which is likely to reduce somewhat the imputation error.

[16] SHARE_SC is the average of the dummy variable SC within the firm and SHARE_FC is the average of the dummy variable FC within the firm. The large number of firms with SHARE_SC or SHARE_FC lying between 0% and 100% (see Figure 1.1) identify the coefficients.

$$+FC_{ic} \cdot SHARE\_FC_c\beta_{V7} + SC_{ic} \cdot UD_{ic}\beta_{V8} + FC_{ic} \cdot UD_{ic}\beta_{V9}$$
$$+SHARE\_SC_c \cdot UD_{ic}\beta_{V10} + SHARE\_FC_c \cdot UD_{ic}\beta_{V11} + u_{ic},$$

where β_{V1} to β_{V5} measure base effects, β_{V6} and β_{V7} capture the different nature of individual coverage in high-coverage firms as compared to low-coverage firms, and β_{V8} and β_{V9} allow for the possibility that UD (power) effects differ by coverage. The specification allows us to test whether UD only affects covered employees or both covered and uncovered employees. Positive values of β_{V10} and β_{V11} indicate that strong unions achieve their wage objective most successfully in high-coverage firms.

Table 1.2 displays results for different sets of wage bargaining indicators, using our preferred set of all other individual and firm covariates. Specification (i), including only dummy variables for individual coverage, yields significant but rather small effects, with different signs for sectoral and firm-level contracts. While employees covered by a sectoral contract earn 0.9% less than uncovered employees, employees covered by a firm-specific contract earn 1.9% more. Results for the shares of covered employees in specification (ii) are different, though. Both sectoral and firm-level contracts show a positive and significant effect, which is in line with the literature. An increase in the share of employees in a firm covered by a sectoral (firm-specific) contract by 10 percentage points (pp) is associated with a 0.34% (0.67%) increase in wages. Individual coverage and firm-level coverage shares are both included in specification (iii). While the share variables show a sizable positive effect, individual coverage by firm-specific or sectoral contract shows negative effects, holding firm coverage, i.e. the share of covered employees, constant. It therefore proves important to distinguish the effects of individual coverage from the effects of firm-level shares of covered employees. In a firm with full coverage, the combined effect of individual coverage and firm coverage is estimated to be positive ($-.107 + .148 = 0.041$ for a sectoral contract and $-.095 + .169 = 0.074$ for a firm-specific contract), i.e. wages in a firm with full coverage are higher on average than wages in uncovered firms.

– Table 1.2 about here –

Specification (iv) additionally allows for interaction effects, which turn out negative. Thus, the effect of individual coverage is particularly negative in firms with a high share of workers covered by a sectoral contract. However, this result does not hold for firm-specific contracts as the effect of FC×SHARE_FC is small and insignificant. Average partial effects for individual coverage remain negative. For example, the marginal effect for individual coverage by a sectoral contract, evaluated at the average coverage rate of 0.565, is –10.1%.[17] In the following, unless stated otherwise, marginal effects are evaluated as average partial effects (APEs) at the respective average coverage shares. An employee in a firm with an average rate of collective coverage earns about 10% less than an uncovered employee in the same firm. In turn, the marginal effect of an increase in the share of covered employees differs between covered and uncovered employees. While both effects are positive, the effect for covered employees is reduced by the interaction term. In combination, the marginal effect for covered employees is a 0.8% wage increase for a 10 pp increase in the share of employees covered by a sectoral contract, while the wage increase is 1.8% for uncovered employees.

Specifications (v) to (viii) introduce UD into the regressions. The base effect of UD has a negative sign and is significant at the 1% level in all specifications.[18] Moreover, the inclusion of UD basically does not alter the effects of coverage. Merely the coefficients of SC and FC become slightly more pronounced. Again, we generally find a positive effect of collective coverage at the firm level, but negative effects of individual coverage. In specification (v), the UD effect picks up the effects of the omitted coverage variables. When coverage effects are included in specification (vi), a 10 pp increase in UD is associated with a decline in wages of about 1%. Therefore, unions either put only a small weight on the wage-level objective, or they are not very effective in using their power to increase wages. Specification (vii), which additionally allows for interaction effects between UD and individual coverage, shows a corresponding decline of about 3% for employees with individual contracts. The positive interaction effects then imply a reduction of only 1% for employees covered by a sectoral contract, and even a small wage-increasing effect of UD in case of firm-specific contracts. This means that stronger unions achieve higher wages for covered employees only in the case of firm-specific bargaining.

[17] $\beta_{V1} + \beta_{V6}\overline{SHARESC} = -0.048 - 0.094 \cdot 0.565 = -0.101$, where $\overline{SHARESC}$ denotes the average share of employees covered by sectoral contracts.

[18] Note that we ignore the estimation error in UD, see Section 3.2 and Footnote 15. Strictly speaking, we cannot be sure that UD is significant because it is likely that standard errors are biased downward.

The inclusion of interaction terms between UD and the coverage shares in specification (viii) does not show any significant coefficients. Therefore, we resort to specification (vii) as our preferred specification for further analysis. Column (iii) of Table 1.3 reproduces the results of the preferred specification. The lower panel of this table reports the implied estimates of the APEs.[19] The numbers corroborate the above findings.

The APEs of individual coverage are negative, while the firm-level shares of covered employees show positive APEs. This finding is in line with a risk premium paid to individuals not covered by a collective contract, as contract wages provide wages floors for covered individuals. Uncovered individuals may be rewarded for higher flexibility, or performance pay may be more important to them.[20] Unfortunately, our analysis does not allow us to distinguish between these different explanations for the negative effect of individual coverage.

The APE of UD is also negative. This result suggests that the crowding effect from the covered sector to the uncovered sector dominates the threat effect. Alternatively, it would also be consistent with the argument that firms in segments with strong unions invest less in capital (Vogel 2007). One may be concerned that the negative effect of UD may reflect a reverse causality such that employees expecting low wages may join a union to win additional protection. We cannot rule out such an effect. However, it is unlikely that this effect dominates because it could not explain the positive interaction effect between UD and coverage (Table 1.2, specification (vii)), as discussed above) implying that stronger unions are associated with a higher wage gap between covered and uncovered employees. This interaction effect is inconsistent with UD merely reflecting low wage prospects. Note that UD varies only by labor market segments, i.e. a possible selection effect would not operate via individual wage prospects, and UD is the same among covered and uncovered employees in a labor market segment. As we discuss below (Table 1.3, specification (iv)), the aforementioned interaction effects are very similar when UD is replaced by fixed effects for each labor market segment.

[19]The APE of, say, SC is calculated as $\widehat{APE} = \hat{\beta}_{SC} + \hat{\beta}_{SC \times SHARESC} \cdot \overline{SHARESC} + \hat{\beta}_{UD \times SC} \cdot \overline{UD}$. Since we distinguish differential impacts of, e. g., UD for covered and uncovered employees, we cannot restrict our attention to APEs only.

[20]Note that our estimates control for a large set of individual and firm characteristics, including firm-size and professional status (see the extended appendix 1.8). Of course, we cannot fully exclude the possibility of selection effects based on unobserved differences.

In order to test the sensitivity of our preferred specification with respect to the set of included covariates, Table 1.3 uses our preferred set of wage bargaining variables and displays the results of specifications including

(i) no covariates,
(ii) only worker characteristics such as human capital variables (educational attainment, age, tenure) and workplace-related characteristics (region, indicators for shift-work or work on Sundays, etc.),
(iii) worker (see above) and firm characteristics such as size and industry of the firm or average characteristics of the firm's workforce,[21] and
(iv) worker and firm characteristics as in (iii), but covariate UD replaced by 5,841 dummy variables for the labor market segments. The specification still includes the interaction effects involving UD.
(v) worker and firm characteristics as in (iii), but the analysis restricted to larger labor market segments (cells with more than 18 employees in our sample).

By changing the set of conditioning variables, we can assess the effect of selection into different wage bargaining regimes and into different levels of UD with regard to differences in observable characteristics (Card and de la Rica 2006). Selection effects involve both types of firms and types of employees. Although our analysis cannot use a convincing research design allowing for the estimation of causal effects, one may suspect that selection on unobservables works in a similar way as the selection on observables.[22]

– Table 1.3 about here –

Controlling for both individual and firm characteristics notably reduces the effects of both coverage and UD. This way, we control in a cross-sectional wage regression for some of the likely endogeneity in UD and coverage. For example, the APE of SC is –9% in specification (iii), while it would be –20% in specification (i). For covered employees, the partial effect of SHARE_SC even changes sign. While a higher share of covered employees is associated with a higher wage in specification

[21]This specification is the same as specification (vii) in Table 1.2. Note that estimating the model with firm-fixed effects is not feasible because the coverage share does not vary within a firm.

[22]Note that specification (iii) in Table 1.3 shows an R^2 of 71%, i. e., the observable variables in our linked employer-employee data explain a fairly high share of the variation in wages. Thus, unobservables are less important than it is typically the case in wage regressions.

(iii) the effect is negative in specification (i). The effect of UD also changes sign between specifications (ii) and (iii). The latter result suggests that the direction of selection into UD differs regarding individual and firm characteristics. On the one hand, controlling for individual characteristics increases the estimated UD coefficients. UD is negatively correlated with individual characteristics that tend to be associated with higher wages, i.e. unions tend to represent workers with lower wages. On the other hand, controlling for firm characteristics reduces the estimated UD coefficients. UD is positively correlated with firm characteristics that tend to be associated with higher wages. The motivation to unionize is larger when there are rents that can be extracted from successful firms, which tend to pay higher wages.

The findings of the sensitivity analysis highlight the importance of controlling for individual as well as for firm characteristics to account for the selection effects based on observables. Therefore, we only report results of specifications controlling for both individual and firm characteristics for the quantile regressions in Section 1.5.2 below.

Specifications (iv) and (v) serve as additional robustness checks regarding the fact that UD is an estimated quantity.[23] Specification (iv) replaces UD by dummy variables for the 5,841 labor market segments with variation in UD. Otherwise, the specification is the same as in (iii). Again using specification (iii), (v) restricts the analysis to cells with more than 18 employees in our sample, omitting the smallest 50% of all cells. This check is reasonable because UD may involve a higher estimation error for smaller cells. Specifications (iv) and (v) show very similar coefficient estimates (and APEs) compared to specification (iii), and none of the substantive results change. We thus conclude that our estimates are unlikely to be affected substantially by the fact that UD is an estimated covariate.

1.5.2 Quantile Regression Results

Least squares regressions focus on explaining conditional average wages (the wage level) only. Still, collective bargaining is likely to affect the entire conditional distribution of wages as well, e. g., because unions have egalitarian objectives and union action is targeted specifically towards low-wage earners. Uncovered employ-

[23]We are grateful to a referee who suggested these specifications as robustness checks. Further robustness checks similar to specification (v) can be found in the extended appendix 1.8.

ees working in covered firms may receive particularly low wages when firms use this instrument to hire cheap labor (e. g. after leaving an employers' association) but all incumbent workers stay covered by persisting collective bargaining contracts. Alternatively, uncovered employees working in firms also employing covered employees may receive higher wages when firms use this instrument to pay some highly productive employees particularly high wages without having to follow a collective contract (e. g. because these employees take management responsibilities in their firms). Possibly, such employees earn a risk premium. In contrast to workers paid a wage cushion, for whom the contract wage provides an effective minimum wage, such high-wage workers cannot rely on contract wages as a fall back position. These arguments suggest that wage bargaining and UD also affect wage dispersion, implying different partial effects at different quantiles of the conditional wage distribution.

For these reasons, we estimate quantile regressions. Analogous to the OLS regressions discussed in the preceding section, we employ sampling weights, and the inference has to account for clustering. We suggest a modification of the approach taken by Angrist et al. (2006) for estimating clustered standard errors in weighted quantile regression. The asymptotic variance $VAR(\hat{\beta}(\tau))$ generally depends upon the observation-specific density of the dependent variable at the conditional quantile. Following Koenker (1994), we estimate the density based on the fitted values of conditional quantile regression estimates, and we choose the bandwidth based on Hall and Sheather's (1988) rule. Appendix 1.7 gives the details on how to estimate $VAR(\hat{\beta}(\tau))$ while accounting for weights and cluster effects.[24] To investigate whether accounting for cluster effects makes a difference, we re-estimate standard errors for the median regression ignoring clustering at the firm level.[25] These non-clustered but heteroscedasticity-consistent standard errors tend to be much smaller (see Table 1.11 in the extended appendix 1.8).

Table 1.4 reports quantile regression results for our preferred specification. Again, the upper panel reports regression coefficients and the lower panel the corresponding APEs. In general, effects at the median are close to those obtained from least

[24]So far, the approach is not standard in econometric software packages such as STATA, the software employed in this paper. Bootstrapping as an alternative way to estimate $VAR(\hat{\beta}(\tau))$ is not feasible due to computational constraints at the Research Data Center.

[25]We are grateful to a referee who suggested this check.

squares estimation, and the estimated coefficients are statistically significant.[26]

– Table 1.4 about here –

The effects of both coverage shares at the firm level (SHARE_SC and SHARE_FC) do not change much across the distribution. Therefore, a firm's decision to apply a sectoral or a firm-specific contract increases wages across the entire distribution in a similar way, compared to a situation of no coverage by collective bargaining. The negative impact of individual coverage is stronger at the top of the conditional distribution. While the APE of a sectoral contract is –6% at the 10th percentile, it increases up to –12% at the 90th percentile. Therefore, coverage by a sectoral contract reduces wage inequality at the individual level. The adoption of a sectoral contract is in fact a means to reduce 'unjustified' pay gaps among employees with the same observable characteristics. This may in turn encourage solidarity among the workforce. Since the coverage effect is also negative at the bottom of the wage distribution, there is no evidence for a negative selection out of collective coverage in this part of the distribution. The effect of firm-specific contracts on individual wage dispersion is also negative, but not as pronounced as that of sectoral contracts. Incidently, comparing wages under firm-specific wage contracts with those in uncovered firms (shares of covered employees are zero) shows that this gap is largest around the median.

Table 1.5 illustrates the effects of individual coverage. We display differences in predicted log wages between a covered and an uncovered employee in the same firm, evaluated at mean covariates and for different combinations of UD and shares of covered employees within the firm.[27] At each of the displayed quantiles, we compare employees in a labor market segment with high union density (UD=37%) versus low union density (UD=10%), and firms with high coverage rate (SHARE=95%) versus low coverage rate (SHARE=50%). For both sectoral contracts (SC, panel A) and firm-specific contracts (FC, panel B), the effects are always negative. The gap is lower when union density is higher. The wage gap is also lower in firms with a higher share of covered employees. Compared to the effects of sectoral coverage, the

[26] Only the interaction FC×SHARE_FC is insignificant at all quantiles, as in the OLS regression.

[27] We thank a referee for the suggestion to summarize wage differences for some representative scenarios.

effect of being covered in a firm with a firm-specific contract is more pronounced when the share of covered employees is high. In all cases, the gap increases along the conditional wage distribution.

– Table 1.5 about here –

The impact of UD also changes strongly across the wage distribution. The negative base effect is strongest at the upper end of the distribution. Yet, the positive interaction effects of UD and the coverage regimes SC and FC also increase throughout the distribution. Consequently, the differences in the impact of union power on covered and uncovered employees are strongest at the top of the distribution. The APE of UD indicates that, on average, a 10 pp increase in union density shows no significant effect at the 10th percentile, but a significant reduction of 2.4% at the 90th percentile. Thus, a higher UD reduces wage inequality, which is in line with the preference of unions for less wage inequality. However, our finding cannot be solely attributed to unions' egalitarian policy because this would also imply positive wage effects in the lower part of the wage distribution, even if the unions were willing to accept a lower mean wage. Instead, the uniformly negative effect at all quantiles suggests that the crowding effect dominates the threat effect. At the same time, the threat effect may be responsible for mitigating the negative wage effect in the lower part of the wage distribution. The finding is also consistent with the presumption that firms in labor market segments with strong unions invest less in capital. This strategy would reduce labor productivity and wages across the entire distribution as a response to an increase in UD. Finally, the fact that the effect of UD is insignificant at the bottom and significantly negative higher up in the wage distribution provides further evidence against the concern of reverse causality in the sense that employees expecting low wages would join a union. If this were the case, we would expect a significantly negative effect of UD at the bottom of the wage distribution.

What do our results reveal concerning the differences between sectoral and firm-specific contracts? Wages are higher on average under firm-specific than under sectoral wage contracts, and this also holds along the wage distribution. While the wage difference between the two contract regimes is quite low at the bottom of the wage distribution, it grows along the wage distribution and is highest at the top (at the 90th percentile the wage gap between an employee covered by a firm-specific contract and an employee covered by a sectoral contract is about 5.5 log points in a firm with 100% coverage, see Table 1.4). As recognition of a sectoral wage contract is basically a firm decision, it is surprising at first glance that firms would not stick to the cheaper sectoral wage contracts. However, firms may choose firm-specific agreements to respond to the demands of their employees (especially those at the median or the top of the wage distribution). These employees may have the bargaining power to extract higher rents from the firms than possible under sectoral wage contracts, similar to the discussion in Card and de la Rica (2006). Furthermore, Gürtzgen (2009b) finds that wages paid according to firm-specific contracts have a higher responsiveness to firm profitability than industry-wide contracts. Firm-level contracts are associated with higher wage inequality among covered workers. This could be associated with, e. g., higher functional flexibility in exchange for higher wages, something that is sufficiently easy to monitor at the firm level but more difficult to monitor at the sectoral level. Our data do not allow us to test this hypothesis, though.

1.6 Conclusions

The design of wage-setting regimes in a large number of OECD countries suggests that researchers should explicitly distinguish between union density and coverage by different forms of collective bargaining. Our analysis distinguishes individual coverage, coverage at the firm level, and union density in homogenous labor markets. Using data from the German Structure of Earnings Survey (GSES) 2001, a large-scale linked employer-employee dataset, we analyze simultaneously how wages are associated with coverage and with union density.

Unfortunately, we cannot explicitly take account of the apparent endogeneity of union density and collective coverage, and therefore, strictly speaking, our results should not be interpreted as causal effects. However, we control for different sets of covariates in our regression to assess the effects of selection based on observables. The endogeneity problem is reduced by controlling for both individual and firm characteristics. Our results highlight the importance of using rich linked employer-employee data.

Estimating OLS and quantile regressions of wages, we find that the firm-level share of employees subject to sectoral or firm-specific collective bargaining contracts has a positive impact on the wage level, i. e., firms which adopt a collective contract pay higher wages. This effect is found along the entire wage distribution. Yet, individual bargaining coverage in a covered firm shows a negative impact both on the wage level and on wage dispersion. The negative wage effect of individual coverage is stronger at higher quantiles of the conditional wage distribution. Collective bargaining coverage thus reduces wage inequality. Our findings are in line with the hypothesis that firms apply collective contracts to implement a transparent wage policy. However, a positive premium is paid to workers in a covered firm who are not paid according to a collective contract, i. e., to those who are not covered individually. These workers tend to be higher paid than workers in the same firm with otherwise similar characteristics. Wages of these workers are particularly high in high-wage firms for which the coverage effect is also high, possibly because collective contracts can extract higher rents from these firms. Similar to Card and de la Rica (2006), this effect is even stronger under firm-specific bargaining than under sectoral bargaining.

Having controlled for different coverage effects, we also find significant effects of union density on the wage level and on wage dispersion. A greater union density is associated with lower wages, and the effect is strongest among uncovered individuals and at the top of the wage distribution. At the same time, a greater union density reinforces the positive wage effects of coverage at the firm level. A greater union density also reduces wage dispersion. This finding is in accordance with an egalitarian wage policy of unions. However, wages in the uncovered sector decline uniformly across the entire wage distribution in response to an increase in union density. This result cannot be rationalized with unions' preference for less wage inequality alone. Instead, it suggests a dominance of crowding effects from the covered sector to the uncovered sector, which are due to an increase in labor supply in the uncovered sector caused by higher wages—and thus lower employment—in the covered sector. The effect may also be due to a decline of firms' capital investment when union density increases. Our analysis does not allow us to distinguish these transmission channels, which should be explored in future research.

In light of the decline of union membership and coverage over time, future research should also analyze the relationship between the changes of union density and coverage on the one hand, and changes in the wage distribution on the other hand. In addition to the data for 2001 used in this paper, such research could be based on GSES data collected for the years 2006 and 2010. However, the data for 2010 are not yet available.

1.7 Appendix

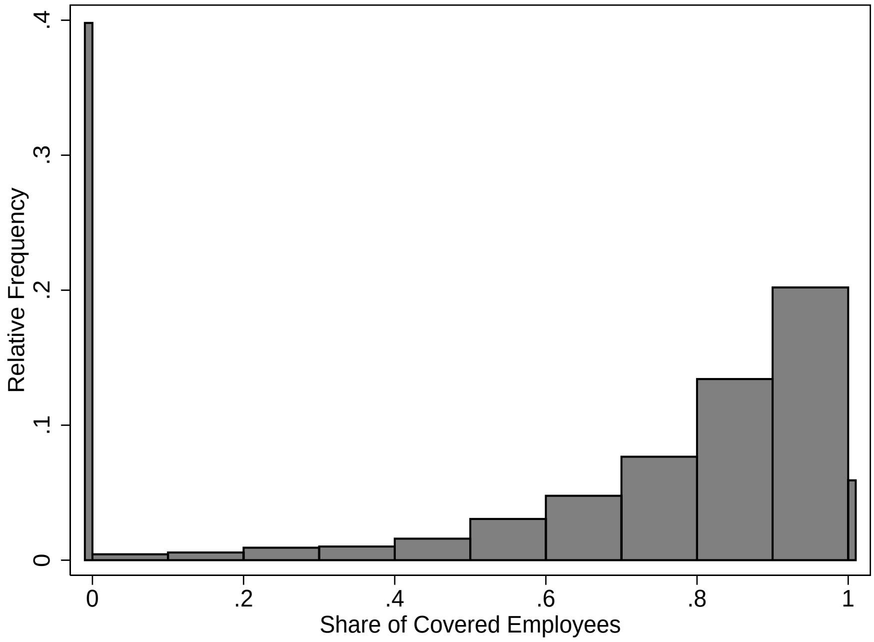
Tables and Figures

Table 1.1: Wage Setting Regimes and Wages

regime	share of employees	log hourly wages mean	log hourly wages std. dev.
sectoral collective contracts (SC)	0.565	2.818	0.284
firm-specific collective contracts (FC)	0.075	2.852	0.315
individual contracts (IC)	0.360	2.783	0.412
total	1.000	2.808	0.338
N	316,805		

Shares of employees in different wage setting regimes. Log hourly wages (in Euros). Descriptives calculated using sampling probabilities. Data source: Extended GSES 2001.

Figure 1.1: Bargaining Coverage Within Firms



Histogram: Distribution of firms with different shares of employees (as a fraction of a firm's total employment) covered by a sectoral or a firm-specific collective contract. Data source: Extended GSES 2001.

Table 1.2: Wage Regressions I: Different Measures of the Wage Setting System

	(i)		(ii)		(iii)		(iv)	
variable	coef.	std. err.	coef.	std. err.	coef.	std. err.	coef.	std. err.
SECTOR CONTRACT	-0.009*	(0.004)			-0.107**	(0.006)	-0.048**	(0.011)
FIRM CONTRACT	0.019*	(0.010)			-0.095**	(0.013)	-0.081**	(0.031)
SHARE EMPL W/ SC			0.034**	(0.005)	0.148**	(0.008)	0.176**	(0.009)
SHARE EMPL W/ FC			0.067**	(0.011)	0.169**	(0.017)	0.165**	(0.018)
SHARE_SC × SC							-0.094**	(0.015)
SHARE_FC × FC							-0.012	(0.044)
R^2	0.703		0.705		0.709		0.709	
N	316,805		316,805		316,805		316,805	

	(v)		(vi)		(vii)		(viii)	
variable	coef.	std. err.	coef.	std. err.	coef.	std. err.	coef.	std. err.
SECTOR CONTRACT			-0.107**	(0.006)	-0.082**	(0.012)	-0.077**	(0.014)
FIRM CONTRACT			-0.094**	(0.013)	-0.149**	(0.033)	-0.172**	(0.033)
SHARE EMPL W/ SC			0.147**	(0.008)	0.171**	(0.008)	0.166**	(0.016)
SHARE EMPL W/ FC			0.168**	(0.016)	0.165**	(0.018)	0.185**	(0.030)
SHARE_SC × SC					-0.096**	(0.015)	-0.097**	(0.015)
SHARE_FC × FC					-0.031	(0.043)	-0.025	(0.041)
UNION DENSITY	-0.139**	(0.051)	-0.111*	(0.048)	-0.296**	(0.055)	-0.300**	(0.060)
UD × SC					0.203**	(0.032)	0.180**	(0.044)
UD × FC					0.367**	(0.068)	0.461**	(0.097)
UD × SHARE_SC							0.031	(0.061)
UD × SHARE_FC							-0.103	(0.117)
R^2	0.703		0.709		0.710		0.710	
N	316,805		316,805		316,805		316,805	

Regressions include a full set of worker and firm characteristics; see the extended appendix 1.8 for full regression results. Estimation by OLS, observations weighted by inverse sampling probabilities. Clustered standard errors in parentheses. */ **: significance at the 5% / 1% level.
Data source: Extended GSES 2001.

Table 1.3: Wage Regressions II: Different Sets of Covariates

	(i)		(ii)		(iii)		(iv)		(v)	
	coef.	std. err.	coef.	std. err.	coef.	std. err.	coef.	std. err.	coef.	std. err.
SECTOR CONTRACT	0.030	(0.033)	-0.057**	(0.018)	-0.082**	(0.012)	-0.061**	(0.013)	-0.077**	(0.012)
FIRM CONTRACT	-0.169*	(0.074)	-0.121**	(0.045)	-0.149**	(0.033)	-0.129**	(0.032)	-0.146**	(0.035)
SHARE EMPL W/ SC	0.708**	(0.015)	0.227**	(0.009)	0.171**	(0.008)	0.173**	(0.008)	0.170**	(0.008)
SHARE EMPL W/ FC	0.644**	(0.033)	0.185**	(0.020)	0.165**	(0.018)	0.166**	(0.017)	0.161**	(0.018)
SHARE_SC × SC	-0.781**	(0.043)	-0.160**	(0.018)	-0.096**	(0.015)	-0.110**	(0.016)	-0.096**	(0.015)
SHARE_FC × FC	-0.454**	(0.073)	-0.075	(0.047)	-0.031	(0.043)	-0.044	(0.041)	-0.032	(0.044)
UNION DENSITY	-0.783**	(0.057)	0.213**	(0.040)	-0.296**	(0.055)			-0.292**	(0.057)
UD × SC	0.812**	(0.071)	0.296**	(0.059)	0.203**	(0.032)	0.156**	(0.030)	0.189**	(0.032)
UD × FC	0.783**	(0.171)	0.453**	(0.118)	0.367**	(0.068)	0.320**	(0.065)	0.367**	(0.070)
Controls										
individual[a]	no		yes		yes		yes		yes	
firm-level[a]	no		no		yes		yes		yes	
fixed-effects[b]	no		no		no		yes		no	
R^2	0.143		0.660		0.710		0.731		0.709	
N	316,805		316,805		316,805		316,805		297,428	
Average Partial Effects (APEs)										
SECTOR CONTRACT	-0.200**	(0.009)	-0.075**	(0.006)	-0.087**	(0.005)	-0.084**	(0.005)	-0.085**	(0.005)
FIRM CONTRACT	-0.025	(0.052)	-0.025	(0.030)	-0.068*	(0.028)	-0.061*	(0.027)	-0.065*	(0.029)
SHARE EMPL W/ SC	0.266**	(0.018)	0.137**	(0.009)	0.117**	(0.009)	0.111**	(0.011)	0.115**	(0.011)
SHARE EMPL W/ FC	0.610**	(0.033)	0.179**	(0.019)	0.162**	(0.016)	0.162**	(0.039)	0.159**	(0.042)
UNION DENSITY	-0.265**	(0.038)	0.414**	(0.045)	-0.154**	(0.048)			-0.157**	(0.057)

Regressions by OLS, observations weighted by inverse sampling probabilities. Upper panel: regression coefficients. Lower panel: corresponding average partial effects. Clustered standard errors in parentheses. */ **: significance at the 5% / 1% level. [a] See the extended appendix 1.8 for definitions of all covariates and full regression results. [b] Fixed effects for aggregate labor market segments as defined in Section 1.4.2. Data source: Extended GSES 2001.

Table 1.4: Wage Regressions III: Quantile Regressions

Percentile	(10)		(25)		(50)		(75)		(90)	
	coef.	std. err.	coef.	std. err.	coef.	std. err.	coef.	std. err.	coef.	std. err.
SECTOR CONTRACT	-0.035*	(0.016)	-0.049**	(0.011)	-0.071**	(0.012)	-0.105**	(0.013)	-0.124**	(0.018)
FIRM CONTRACT	-0.104**	(0.033)	-0.135**	(0.029)	-0.157**	(0.031)	-0.169**	(0.034)	-0.183**	(0.047)
SHARE EMPL W/ SC	0.168**	(0.009)	0.184**	(0.009)	0.188**	(0.008)	0.176**	(0.009)	0.158**	(0.013)
SHARE EMPL W/ FC	0.129**	(0.027)	0.170**	(0.018)	0.173**	(0.015)	0.159**	(0.014)	0.131**	(0.035)
SHARE_SC × SC	-0.104**	(0.019)	-0.117**	(0.014)	-0.109**	(0.014)	-0.094**	(0.016)	-0.074**	(0.022)
SHARE_FC × FC	0.002	(0.050)	-0.035	(0.036)	-0.030	(0.036)	-0.025	(0.037)	-0.010	(0.065)
UNION DENSITY	-0.173**	(0.057)	-0.223**	(0.039)	-0.256**	(0.045)	-0.315**	(0.056)	-0.391**	(0.086)
UD × SC	0.149**	(0.040)	0.151**	(0.028)	0.155**	(0.029)	0.210**	(0.035)	0.209**	(0.051)
UD × FC	0.199**	(0.061)	0.278**	(0.065)	0.349**	(0.075)	0.417**	(0.084)	0.489**	(0.074)
N	316,805		316,805		316,805		316,805		316,805	
					Average Partial Effects (APEs)					
SECTOR CONTRACT	-0.056**	(0.007)	-0.077**	(0.006)	-0.094**	(0.006)	-0.107**	(0.006)	-0.116**	(0.008)
FIRM CONTRACT	-0.059*	(0.029)	-0.076**	(0.023)	-0.081**	(0.024)	-0.078**	(0.024)	-0.074	(0.042)
SHARE EMPL W/ SC	0.109**	(0.011)	0.117**	(0.008)	0.126**	(0.008)	0.123**	(0.009)	0.117**	(0.012)
SHARE EMPL W/ FC	0.129**	(0.025)	0.168**	(0.016)	0.171**	(0.014)	0.157**	(0.013)	0.131**	(0.033)
UNION DENSITY	-0.074	(0.044)	-0.117**	(0.032)	-0.142**	(0.040)	-0.165**	(0.049)	-0.237**	(0.074)

Regressions include a full set of worker and firm characteristics; see the extended appendix 1.8 for full regression results. Quantile regression, observations weighted by inverse sampling probabilities. Upper panel: regression coefficients. Lower panel: corresponding average partial effects. Clustered standard errors in parentheses; see Appendix 1.7 for details of the implementation. */ **: significance at the 5% / 1% level. Data source: Extended GSES 2001.

Table 1.5: Quantile-Specific Wage Effects of Individual Coverage: Scenarios by Union Density and Bargaining Coverage within Firms[a]

Percentile	(10)		(50)		(90)	
SHARE OF EMPL COVERED \ UNION DENSITY	0.10	0.37	0.10	0.37	0.10	0.37
	(A) Effects of Individual Coverage by Sectoral Contract					
0.50	-0.119	-0.079	-0.159	-0.117	-0.173	-0.117
0.95	-0.072	-0.032	-0.110	-0.068	-0.140	-0.083
	(B) Effects of Individual Coverage by Firm-Specific Contract					
0.50	-0.087	-0.046	-0.170	-0.128	-0.171	-0.115
0.95	-0.088	-0.047	-0.156	-0.114	-0.167	-0.111

a: Scenarios show differences in predicted log wages based on quantile regression estimates in Table 1.4.

Panel (A): firm covered by a sectoral contract (SC)

$$Q_{ln(w)}(\tau|X=\bar{x}, UD=ud, SC=1, SHARE\_SC=coverage, FC=0, SHARE\_FC=0)$$
$$-Q_{ln(w)}(\tau|X=\bar{x}, UD=ud, SC=0, SHARE\_SC=coverage, FC=0, SHARE\_FC=0)$$

Panel (B): firm covered by a firm-specific contract (FC)

$$Q_{ln(w)}(\tau|X=\bar{x}, UD=ud, SC=0, SHARE\_SC=0, FC=1, SHARE\_FC=coverage)$$
$$-Q_{ln(w)}(\tau|X=\bar{x}, UD=ud, SC=0, SHARE\_SC=0, FC=0, SHARE\_FC=coverage)$$

Standard Errors for Quantile Regression with Sampling Weights and Clustering

Clustering allows for dependence of observations within clusters (see Froot 1989, Moulton 1990, or Williams 2000 for OLS). The asymptotic distribution of $\hat{\beta}(\tau)$ for a given quantile τ in such a setting is

$$\sqrt{N}(\hat{\beta}(\tau) - \beta(\tau)) \sim N(0, J(\tau)^{-1}\Sigma(\tau)J(\tau)^{-1}) \tag{1.2}$$

with

$$\Sigma(\tau) \equiv \frac{1}{N}\sum_{c=1}^{C} E[(\sum_{i\in c} X_{ic}(\tau - \mathbb{1}\{Y_{ic} < X'_{ic}\beta(\tau)\}))(\sum_{j\in c} X'_{jc}(\tau - \mathbb{1}\{Y_{jc} < X'_{jc}\beta(\tau)\}))] \tag{1.3}$$

and

$$J(\tau) \equiv \frac{1}{N}\sum_{c=1}^{C} E[\sum_{i\in c} f_y(X'_{ic}\beta(\tau)|X_{ic})X_{ic}X'_{ic}] = E[\sum_{i\in c} f_u(0|X_{ic})X_{ic}X'_{ic}], \tag{1.4}$$

for observation i in cluster c. N is the total number of observations, and C is the total number of clusters. These expressions, for the case without weights, assume that the model is correctly specified (Angrist et al. 2006) and that correlation of the error term is restricted to pairs of observations within the same cluster. f_u denotes the density of the error term (Hendricks and Koenker 1992, Koenker 2005, Melly 2006).

In contrast to Angrist et al. (2006), we follow Koenker (1994) for the estimation of the observation-specific density. We use the "Hendricks-Koenker sandwich"

$$\hat{f}_i = 2h_N / \left(X'_i(\hat{\beta}(\tau + h_N) - \hat{\beta}(\tau - h_N))\right) \tag{1.5}$$

and employ Hall and Sheather's (1988) rule for the bandwidth h_N:

$$h_N = \frac{1}{N^{1/3}} z_{\alpha}^{2/3} [1.5 s(\tau)/s''(\tau)]^{1/3}, \tag{1.6}$$

where z_α satisfies $\Phi(z_\alpha) = 1 - \alpha/2$ for the construction of $1 - \alpha$ confidence intervals and $s(\tau)$ denotes the sparsity function.[28] As in Koenker (1994), we use the normal distribution to estimate

$$s(\tau)/s''(\tau) = \frac{f^2}{2(f'/f)^2 + [(f'/f)^2 - f''/f]} = \frac{\phi(\Phi^{-1}(\tau))^2}{2(\Phi^{-1}(\tau))^2 + 1}. \tag{1.7}$$

[28] The sandwich formula is extensively described in Koenker (2005, pp. 79–80). Koenker also mentions the "Powell sandwich", which is employed by, e. g., Angrist et al. (2006).

In analogy to Angrist et al. (2006), we take account of sampling weights and clustering at the firm level. We acknowledge that the sampling weights in the GSES are equal for all individuals $i = 1, ..., N_c$ within a cluster c. With sampling weights w_c normalized to sum up to one, $\sum_{c=1}^{C} w_c = 1$, $\Sigma(\tau)$ and $J(\tau)$ can be estimated consistently by

$$\hat{\Sigma}(\tau) = \frac{1}{N} \sum_{c=1}^{C} w_c^2 \sum_{i=1}^{N_c} \sum_{j=1}^{N_c} X_{ic}(\tau - \mathbb{1}\{Y_{ic} < X_{ic}'\hat{\beta}(\tau)\})(\tau - \mathbb{1}\{Y_{jc} < X_{jc}\hat{\beta}(\tau)\})X_{jc}' \quad (1.8)$$

and

$$\hat{J}(\tau) = \frac{1}{N} \sum_{c=1}^{C} w_c \sum_{i=1}^{N_c} \hat{f}_{ic} X_{ic} X_{ic}' \,. \quad (1.9)$$

Sample Description

The German Structure of Earnings Survey (GSES, *Gehalts- und Lohnstrukturerhebung*) 2001 is a linked employer-employee dataset administered by the German Statistical Office in accordance with European and German law (European Council Regulation (EC) No. 530/1999, amended by EC 1916/2000; German Law on Wage Statistics, *LohnStatG*).[29] It is a sample of all firms in manufacturing and private service sectors with at least ten employees. Sampling takes place at the firm or establishment level. At a first stage, firms are randomly drawn from every federal state, where the sampling probability varies between 5.3% for the largest state (North Rhine-Westphalia) and 19.4% for the smallest (Bremen). At a second stage, employees are randomly chosen from the firms sampled at the first stage. The share of employees sampled depends upon firm size and ranges between 6.25% for the largest firms and 100% for firms with less than 20 employees. The dataset provides sampling weights.

Since 2005, the GSES 2001 is available for on-site use at the Research Data Centers of the statistical offices of the federal states (FDZ). This on-site version was made anonymous in some respects. It includes all firms and employees from the original data except for one firm in Berlin (the only firm in Berlin falling into NACE section C). Regional information is condensed to 12 “states”, i. e., some of the smaller German States were aggregated, and some industries have been aggregated

[29] See Hafner (2005), Hafner and Lenz (2008), and Statistisches Bundesamt (2000, 2004) for detailed descriptions of the GSES data.

at the two-digit level. In total, the on-site dataset consists of 22,040 establishments with 846,156 employees.

We focus on prime-age (25–55-year-old) male full-time employees in West Germany (without Berlin), including both blue and white-collar workers. Employees in vocational training, interns, and employees subject to partial retirement schemes are dropped, as compensation for these groups does not follow the regular compensation schedule, but special regulations or even special collective bargaining contracts apply. We also exclude white-collar workers in the highest professional status category (category 1), who can reasonably be expected to pursue objectives of upper and middle management and whose wages are hardly in the focus of collective wage setting. Individuals who worked less than 90% of their contractual working hours in October 2001 and individuals paid subject to a collective contract with a missing contract identification number are dropped.

Part-time and full-time employees are distinguished based on employers' assessments recorded in the GSES. For blue-collar workers, actual working time and not contractual working time is relevant for monthly payments. We exclude individuals with an actual working time of more than 390 hours in October 2001.

We analyze gross hourly wages including premia. This measure is more appropriate than wages without premia if premia are paid on a regular basis. We impose a lower bound of one Euro for hourly wages.

The GSES 2001 is extended by imputed union density as explained in Section 1.4.2. Table 1.6 summarizes the definitions of the collective bargaining and union variables used in this paper, as well as the definitions and summary statistics for all variables used in the empirical analysis.

1.8 Extended Appendix

Table 1.6: Definition of Variables and Descriptive Statistics (Complete List)

Variable	Acronym	Description	mean	std. dev.
Collective Bargaining and Union Variables				
SECTOR CONTRACT	SC	Sectoral collective contract negotiated between an employers' association and a union applies to individual job match.	0.565	0.496
FIRM CONTRACT	FC	Firm-specific contract negotiated between a single firm and representatives of its employees (typically a union or a works council) applies to individual job match (dummy variable).	0.075	0.263
INDIV CONTRACT	IC	Individual contract negotiated between employer and employee (dummy variable).	0.360	0.480
SHARE EMPL W/ SC	SHARE_SC	Share of employees in the firm covered by a sectoral collective contract (variation at firm level).	0.529	0.421
SHARE EMPL W/ FC	SHARE_FC	Share of employees in the firm covered by a firm-specific collective contract (variation at firm level).	0.071	0.239
UNION DENSITY	UD	(Net) Union density: Share of employed union members among all employees, defined for 5,841 aggregate labor market segments spanned by the dimensions region (7 states) × industry (30 sectors) × educational attainment (4 groups) × age (7 classes).	0.224	0.104
LN_WAGE		Natural logarithm of the hourly wage, calculated as total wage paid divided by total hours worked	2.808	0.338
Individual-Level Covariates				
AGE		Age in years/100.	0.395	0.080
AGE SQUARED		AGE squared.	0.163	0.064
TENURE		Tenure in years/10.	0.937	0.930
TENURE SQUARED		TENURE squared.	1.742	2.761
LOW EDUC		Low level of education: no training beyond a school degree (or no school degree at all).	0.146	0.353
MED EDUC		Intermediate level of education: vocational training.	0.688	0.463
HIGH EDUC		High level of education: university or technical college degree.	0.100	0.300
EDUC N.A.		Missing information on the level of education.	0.066	0.249
BC SPEC SKILL		Blue-collar worker, professional status category 1: vocationally trained or comparably experienced worker with special skills and highly involved tasks.	0.118	0.323
BC VOC TRAIN		Blue-collar worker, professional status category 2: vocationally trained or comparably experienced worker.	0.229	0.420

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... table 1.6 continued

Variable	Description	mean	std. dev.
BC ON THE JOB	Blue-collar worker, professional status category 3: worker trained on-the-job.	0.155	0.362
BC LABORER	Blue-collar worker, professional status category 4: laborer.	0.086	0.281
WC LIM EXEC	White-collar worker, professional status category 2: executive employee with limited procuration.	0.162	0.369
WC SPEC SKILL	White-collar worker, professional status category 3: employee with special skills or experience who works on his own responsibility on highly involved or complex tasks.	0.102	0.302
WC VOC TRAIN INVOLV	White-collar worker, professional status category 4: vocationally trained or comparably experienced employee who works autonomously on involved tasks.	0.099	0.298
WC VOC TRAIN AUTON	White-collar worker, professional status category 5: vocationally trained or comparably experienced employee working autonomously.	0.041	0.199
WC SIMPLE TASK	White-collar worker, professional status category 6: employee working on simple tasks.	0.007	0.084
NIGHT	Individual worked night shifts.	0.208	0.406
SUNDAY	Individual worked on Sundays or on holidays.	0.128	0.334
SHIFT	Individual worked shift.	0.146	0.353
OVERTIME	Individual worked overtime.	0.269	0.444
Firm-Level Covariates			
FIRM SHARE FEMALE	Share of female employees.	0.231	0.183
FIRM SHARE AGE1	Share of employees of age 19 or younger.	0.034	0.052
FIRM SHARE AGE2	Share of employees of age 20–24.	0.070	0.060
FIRM SHARE AGE3	Share of employees of age 25–29.	0.096	0.063
FIRM SHARE AGE4	Share of employees of age 30–34.	0.150	0.072
FIRM SHARE AGE5	Share of employees of age 35–39.	0.174	0.069
FIRM SHARE AGE6	Share of employees of age 40–44.	0.151	0.066
FIRM SHARE AGE7	Share of employees of age 45–49.	0.124	0.064
FIRM SHARE AGE8	Share of employees of age 50–54.	0.104	0.062
FIRM SHARE AGE9	Share of employees of age 55–59.	0.066	0.052
FIRM SHARE AGE10	Share of employees of age 60 or older.	0.032	0.042
FIRM SHARE TENURE1	Share of employees with less than 1 year of tenure.	0.135	0.141
FIRM SHARE TENURE2	Share of employees with 1–2 years of tenure.	0.178	0.140
FIRM SHARE TENURE3	Share of employees with 3–5 years of tenure.	0.142	0.117
FIRM SHARE TENURE4	Share of employees with 6–10 years of tenure.	0.164	0.116
FIRM SHARE TENURE5	Share of employees with 11–15 years of tenure.	0.136	0.094
FIRM SHARE TENURE6	Share of employees with 16–20 years of tenure.	0.079	0.071
FIRM SHARE TENURE7	Share of employees with 21–25 years of tenure.	0.071	0.070
FIRM SHARE TENURE8	Share of employees with 26–30 years of tenure.	0.048	0.056
FIRM SHARE TENURE9	Share of employees with 31 or more years of tenure.	0.048	0.061
FIRM SHARE LOW EDUC	Share of employees with LOW EDUC.	0.198	0.174
FIRM SHARE MED EDUC	Share of employees with MED EDUC.	0.642	0.222
FIRM SHARE HIGH EDUC	Share of employees with HIGH EDUC.	0.086	0.137
FIRM SHARE EDUC N.A.	Share of employees with EDUC N.A.	0.073	0.181

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... table 1.6 continued

Variable	Description	mean	std. dev.
HOURS WORKED	Average hours worked in the firm/100.	0.162	0.369
FIRM SHARE IRREG	Share of employees for whom any of NIGHT, SUNDAY, or SHIFT applies.	0.245	0.276
FIRM SHARE OVERTIME	Share of employees working overtime.	0.223	0.242
FIRM SHARE BC	Share of blue collar workers.	0.452	0.301
FIRM SHARE NOT FTIME	Share of employees who do not work full-time.	0.149	0.145
FIRMSIZE1	Firm has between 10 and 49 employees.	0.238	0.426
FIRMSIZE2	Firm has between 50 and 249 employees.	0.320	0.466
FIRMSIZE3	Firm has between 250 and 499 employees.	0.123	0.328
FIRMSIZE4	Firm has between 500 and 999 employees.	0.110	0.312
FIRMSIZE5	Firm has between 1000 and 1999 employees.	0.069	0.254
FIRMSIZE6	Firm has 2000 or more employees.	0.140	0.347
OWN PRIVATE	Firm is privately owned.	0.934	0.249
OWN PUBLIC1	Firm is partly public-owned (<50%).	0.027	0.162
OWN PUBLIC2	Firm is mainly public-owned (>50%).	0.039	0.194
SECTOR1	Mining and quarrying (NACE: 10–14)	0.011	0.105
SECTOR2	Manufacture of food products, beverages and tobacco (NACE: 15–16)	0.034	0.181
SECTOR3	Manufacture of textiles and textile products; leather and leather products (NACE: 17–19)	0.019	0.138
SECTOR4	Manufacture of wood and wood products; pulp, paper and paper products (NACE: 20–21)	0.033	0.179
SECTOR5	Publishing, printing and reproduction of recorded media (NACE: 22)	0.036	0.186
SECTOR6	Manufacture of coke, refined petroleum products and nuclear fuel; chemicals and chemical products (NACE: 23–24)	0.027	0.162
SECTOR7	Manufacture of rubber and plastic products (NACE: 25)	0.033	0.178
SECTOR8	Manufacture of other non-metallic mineral products (NACE: 26)	0.028	0.164
SECTOR9	Manufacture of basic metals; fabricated metal products, except from machinery and equipment (NACE: 27–28)	0.055	0.229
SECTOR10	Manufacture of machinery and equipment n.e.c. (NACE: 29)	0.045	0.207
SECTOR11	Manufacture of electrical machinery and apparatus n.e.c. (NACE: 31)	0.025	0.157
SECTOR12	Manufacture of electrical and optical equipment; radio, television, and communication equipment and apparatus (NACE: 30 + 32)	0.021	0.144
SECTOR13	Manufacture of medical, precision and optical instruments, watches and clocks (NACE: 33)	0.023	0.149
SECTOR14	Manufacture of transport equipment (NACE: 34–35)	0.032	0.176
SECTOR15	Manufacture n.e.c. (NACE: 36–37)	0.024	0.154
SECTOR16	Electricity, gas and water supply (NACE: 40–41)	0.025	0.155
SECTOR17	Construction (NACE: 45)	0.082	0.274
SECTOR18	Sale, maintenance and repair of motor vehicles and motorcycles; retail sale of automotive fuel (NACE: 50)	0.031	0.173

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... table 1.6 continued

Variable	Description	mean	std. dev.
SECTOR19	Wholesale trade and commission trade except of motor vehicles and motorcycles (NACE: 51)	0.056	0.231
SECTOR20	Retail trade, except from motor vehicles and motorcycles; repair of personal and household goods (NACE: 52)	0.050	0.219
SECTOR21	Hotels and restaurants (NACE: 55)	0.027	0.161
SECTOR22	Land transport; transport via pipelines; air transport (NACE: 60 + 62)	0.028	0.165
SECTOR23	Water transport (NACE: 61)	0.008	0.088
SECTOR24	Supporting and auxiliary transport activities; activities of travel agencies (NACE: 63)	0.044	0.204
SECTOR25	Post and telecommunications (NACE: 64)	0.023	0.150
SECTOR26	Financial intermediation, except from insurance and pension funding; activities auxiliary to financial intermediation, except from insurance and pension funding (NACE: 65 + 67.1)	0.022	0.148
SECTOR27	Insurance and pension funding, except compulsory social security; activities auxiliary to insurance and pension funding (NACE: 66 + 67.2)	0.016	0.126
SECTOR28	Real estate activities; renting of machinery and equipment without operator and of personal and household goods (NACE: 70–71)	0.015	0.123
SECTOR29	Computer and related activities (NACE: 72)	0.022	0.146
SECTOR30	Research and development; other business activities (NACE: 73–74)	0.075	0.264
REGION1	Firm is located in Schleswig-Holstein or Hamburg.	0.106	0.308
REGION2	Firm is located in Lower Saxony or Bremen.	0.158	0.365
REGION3	Firm is located in North Rhine-Westphalia.	0.203	0.402
REGION4	Firm is located in Hesse.	0.105	0.306
REGION5	Firm is located in Rhineland-Palatinate or Saarland.	0.104	0.305
REGION6	Firm is located in Baden-Württemberg.	0.158	0.365
REGION7	Firm is located in Bavaria.	0.166	0.372

Descriptives calculated using sampling probabilities. Data source: Extended GSES 2001.

Table 1.7: OLS Wage Regressions Ia: Different Measures of the Wage Setting System (all Covariates)

	(i)		(ii)		(iii)		(iv)	
variable	coef.	std. err.	coef.	std. err.	coef.	std. err.	coef.	std. err.
SECTOR CONTRACT	-0.009	(0.004)			-0.107	(0.006)	-0.048	(0.011)
FIRM CONTRACT	0.019	(0.010)			-0.095	(0.013)	-0.081	(0.031)
SHARE EMPL W/ SC			0.034	(0.005)	0.148	(0.008)	0.176	(0.009)
SHARE EMPL W/ FC			0.067	(0.011)	0.169	(0.017)	0.165	(0.018)
SHARE_SC × SC							-0.094	(0.015)
SHARE_FC × FC							-0.012	(0.044)
AGE	0.172	(0.006)	0.173	(0.006)	0.170	(0.006)	0.170	(0.006)
AGE SQUARED	-0.018	(0.001)	-0.018	(0.001)	-0.018	(0.001)	-0.018	(0.001)

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... table 1.7 continued

	(i)		(ii)		(iii)		(iv)	
variable	coef.	std. err.	coef.	std. err.	coef.	std. err.	coef.	std. err.
TENURE	0.053	(0.002)	0.053	(0.002)	0.054	(0.002)	0.054	(0.002)
TENURE SQUARED	-0.011	(0.001)	-0.011	(0.001)	-0.011	(0.001)	-0.011	(0.001)
LOW EDUC	-0.027	(0.004)	-0.029	(0.004)	-0.030	(0.004)	-0.029	(0.004)
MED EDUC	-0.036	(0.002)	-0.036	(0.002)	-0.036	(0.002)	-0.036	(0.002)
HIGH EDUC	0.073	(0.003)	0.073	(0.003)	0.063	(0.003)	0.062	(0.003)
BC SPEC SKILL	0.081	(0.004)	0.076	(0.004)	0.078	(0.004)	0.078	(0.004)
BC ON THE JOB	-0.102	(0.003)	-0.101	(0.003)	-0.101	(0.003)	-0.101	(0.003)
BC LABORER	-0.210	(0.005)	-0.209	(0.005)	-0.209	(0.005)	-0.209	(0.005)
WC LIM EXEC	0.497	(0.004)	0.496	(0.004)	0.481	(0.004)	0.478	(0.004)
WC SPEC SKILL	0.266	(0.005)	0.268	(0.004)	0.261	(0.004)	0.260	(0.004)
WC VOC TRAIN INVOLV	0.179	(0.004)	0.177	(0.004)	0.177	(0.004)	0.177	(0.004)
WC VOC TRAIN AUTON	-0.033	(0.006)	-0.035	(0.006)	-0.036	(0.006)	-0.037	(0.006)
WC SIMPLE TASK	-0.195	(0.015)	-0.194	(0.015)	-0.199	(0.015)	-0.199	(0.015)
NIGHT	0.025	(0.003)	0.025	(0.003)	0.026	(0.003)	0.027	(0.003)
SUNDAY	0.052	(0.004)	0.052	(0.004)	0.054	(0.004)	0.053	(0.004)
SHIFT	0.048	(0.005)	0.048	(0.005)	0.049	(0.005)	0.050	(0.005)
OVERTIME	-0.010	(0.002)	-0.010	(0.002)	-0.007	(0.002)	-0.007	(0.002)
FIRM SHARE FEMALE	-0.079	(0.011)	-0.078	(0.011)	-0.086	(0.011)	-0.084	(0.011)
FIRM SHARE AGE1	0.030	(0.034)	0.019	(0.034)	0.078	(0.034)	0.048	(0.035)
FIRM SHARE AGE2	-0.177	(0.030)	-0.175	(0.030)	-0.160	(0.030)	-0.167	(0.030)
FIRM SHARE AGE3	-0.116	(0.028)	-0.118	(0.028)	-0.120	(0.028)	-0.118	(0.028)
FIRM SHARE AGE4	0.013	(0.027)	0.010	(0.027)	0.009	(0.027)	0.010	(0.027)
FIRM SHARE AGE6	-0.113	(0.025)	-0.116	(0.025)	-0.113	(0.025)	-0.114	(0.025)
FIRM SHARE AGE7	-0.118	(0.028)	-0.115	(0.027)	-0.109	(0.027)	-0.112	(0.027)
FIRM SHARE AGE8	0.031	(0.029)	0.021	(0.028)	0.023	(0.028)	0.018	(0.028)
FIRM SHARE AGE9	0.023	(0.033)	0.014	(0.032)	0.019	(0.032)	0.015	(0.032)
FIRM SHARE AGE10	-0.201	(0.037)	-0.183	(0.036)	-0.165	(0.037)	-0.176	(0.037)
FIRM SHARE TENURE1	-0.061	(0.024)	-0.046	(0.023)	-0.048	(0.023)	-0.048	(0.023)
FIRM SHARE TENURE2	-0.008	(0.024)	0.003	(0.024)	0.005	(0.024)	0.004	(0.024)
FIRM SHARE TENURE3	-0.002	(0.026)	0.001	(0.025)	0.000	(0.025)	-0.000	(0.025)
FIRM SHARE TENURE5	-0.012	(0.033)	-0.018	(0.033)	-0.019	(0.033)	-0.020	(0.033)
FIRM SHARE TENURE6	0.043	(0.034)	0.036	(0.033)	0.035	(0.033)	0.036	(0.033)
FIRM SHARE TENURE7	0.065	(0.041)	0.057	(0.040)	0.055	(0.040)	0.056	(0.040)
FIRM SHARE TENURE8	-0.092	(0.039)	-0.102	(0.038)	-0.106	(0.038)	-0.105	(0.038)
FIRM SHARE TENURE9	0.003	(0.037)	-0.017	(0.037)	-0.017	(0.036)	-0.020	(0.037)
FIRM SHARE MED EDUC	0.029	(0.011)	0.039	(0.011)	0.041	(0.012)	0.041	(0.012)
FIRM SHARE HIGH EDUC	-0.009	(0.010)	-0.008	(0.010)	-0.007	(0.010)	-0.007	(0.010)
FIRM SHARE EDUC N.A.	0.163	(0.016)	0.169	(0.016)	0.187	(0.016)	0.179	(0.016)
HOURS WORKED	-0.002	(0.000)	-0.002	(0.000)	-0.002	(0.000)	-0.002	(0.000)
FIRM SHARE IRREG	0.005	(0.008)	-0.005	(0.008)	-0.008	(0.008)	-0.007	(0.008)
FIRM SHARE OVERTIME	0.053	(0.010)	0.047	(0.010)	0.043	(0.010)	0.043	(0.010)
FIRM SHARE BC	0.087	(0.010)	0.087	(0.010)	0.095	(0.010)	0.093	(0.010)
FIRM SHARE NOT FTIME	-0.234	(0.021)	-0.218	(0.021)	-0.205	(0.021)	-0.211	(0.021)
FIRMSIZE1	-0.025	(0.004)	-0.019	(0.004)	-0.019	(0.004)	-0.018	(0.004)
FIRMSIZE3	0.022	(0.005)	0.019	(0.005)	0.019	(0.005)	0.018	(0.005)
FIRMSIZE4	0.033	(0.005)	0.029	(0.005)	0.029	(0.005)	0.028	(0.005)
FIRMSIZE5	0.057	(0.006)	0.053	(0.006)	0.052	(0.006)	0.051	(0.006)
FIRMSIZE6	0.073	(0.008)	0.069	(0.008)	0.069	(0.008)	0.067	(0.008)
OWN PUBLIC1	-0.015	(0.013)	-0.017	(0.013)	-0.014	(0.012)	-0.014	(0.013)
OWN PUBLIC2	-0.057	(0.008)	-0.063	(0.008)	-0.063	(0.008)	-0.061	(0.008)
SECTOR1	0.002	(0.045)	-0.005	(0.045)	-0.005	(0.044)	-0.006	(0.044)

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... table 1.7 continued

	(i)		(ii)		(iii)		(iv)	
variable	coef.	std. err.	coef.	std. err.	coef.	std. err.	coef.	std. err.
SECTOR2	0.074	(0.010)	0.070	(0.010)	0.069	(0.010)	0.066	(0.010)
SECTOR3	0.065	(0.015)	0.058	(0.015)	0.058	(0.015)	0.055	(0.015)
SECTOR4	0.110	(0.011)	0.107	(0.011)	0.107	(0.011)	0.105	(0.011)
SECTOR5	0.237	(0.011)	0.234	(0.011)	0.235	(0.011)	0.231	(0.011)
SECTOR6	0.173	(0.010)	0.168	(0.010)	0.166	(0.010)	0.163	(0.010)
SECTOR7	0.110	(0.010)	0.109	(0.010)	0.109	(0.010)	0.106	(0.010)
SECTOR8	0.097	(0.010)	0.087	(0.010)	0.087	(0.010)	0.083	(0.010)
SECTOR9	0.126	(0.011)	0.125	(0.010)	0.125	(0.010)	0.122	(0.011)
SECTOR10	0.138	(0.010)	0.135	(0.010)	0.135	(0.010)	0.132	(0.010)
SECTOR11	0.124	(0.012)	0.120	(0.011)	0.121	(0.011)	0.118	(0.012)
SECTOR12	0.103	(0.011)	0.099	(0.011)	0.099	(0.011)	0.095	(0.012)
SECTOR13	0.108	(0.012)	0.106	(0.011)	0.106	(0.011)	0.103	(0.011)
SECTOR14	0.202	(0.013)	0.198	(0.012)	0.198	(0.013)	0.197	(0.013)
SECTOR15	0.106	(0.013)	0.101	(0.013)	0.102	(0.013)	0.099	(0.013)
SECTOR16	0.169	(0.014)	0.160	(0.013)	0.159	(0.013)	0.160	(0.013)
SECTOR17	0.143	(0.010)	0.131	(0.010)	0.132	(0.010)	0.128	(0.010)
SECTOR18	0.081	(0.011)	0.074	(0.011)	0.078	(0.011)	0.072	(0.011)
SECTOR19	0.067	(0.010)	0.064	(0.010)	0.063	(0.010)	0.062	(0.010)
SECTOR20	0.031	(0.011)	0.021	(0.011)	0.020	(0.011)	0.019	(0.011)
SECTOR21	0.010	(0.013)	-0.001	(0.013)	-0.003	(0.013)	-0.005	(0.013)
SECTOR22	0.050	(0.012)	0.044	(0.012)	0.043	(0.012)	0.041	(0.012)
SECTOR23	0.128	(0.029)	0.121	(0.028)	0.123	(0.028)	0.121	(0.028)
SECTOR24	0.055	(0.010)	0.047	(0.010)	0.048	(0.010)	0.044	(0.010)
SECTOR25	0.103	(0.014)	0.086	(0.014)	0.084	(0.014)	0.084	(0.015)
SECTOR26	0.088	(0.015)	0.077	(0.015)	0.081	(0.015)	0.077	(0.015)
SECTOR27	0.074	(0.015)	0.073	(0.014)	0.068	(0.014)	0.059	(0.014)
SECTOR28	0.132	(0.013)	0.126	(0.013)	0.129	(0.013)	0.126	(0.013)
SECTOR29	0.096	(0.013)	0.096	(0.013)	0.097	(0.013)	0.100	(0.013)
REGION2	-0.043	(0.012)	-0.048	(0.012)	-0.048	(0.012)	-0.046	(0.012)
REGION3	-0.018	(0.010)	-0.021	(0.010)	-0.020	(0.010)	-0.020	(0.010)
REGION4	-0.016	(0.012)	-0.016	(0.012)	-0.016	(0.012)	-0.016	(0.012)
REGION5	-0.064	(0.011)	-0.066	(0.011)	-0.067	(0.011)	-0.065	(0.011)
REGION6	0.019	(0.011)	0.018	(0.011)	0.016	(0.011)	0.018	(0.011)
REGION7	-0.026	(0.011)	-0.029	(0.011)	-0.031	(0.011)	-0.030	(0.011)
CONSTANT	2.557	(0.043)	2.501	(0.041)	2.501	(0.041)	2.505	(0.041)

Estimation by OLS, observations weighted by inverse sampling probabilities.
Clustered standard errors in parentheses. Data source: Extended GSES 2001.

Table 1.8: OLS Wage Regressions Ib: Different Measures of the Wage Setting System (all Covariates)

	(v)		(vi)		(vii)		(viii)	
variable	coef.	std. err.	coef.	std. err.	coef.	std. err.	coef.	std. err.
SECTOR CONTRACT			-0.107	(0.006)	-0.082	(0.012)	-0.077	(0.014)
FIRM CONTRACT			-0.094	(0.013)	-0.149	(0.033)	-0.172	(0.033)
SHARE EMPL W/ SC			0.147	(0.008)	0.171	(0.008)	0.166	(0.016)

Continued on next page...

... table 1.8 continued

	(v)		(vi)		(vii)		(viii)	
variable	coef.	std. err.	coef.	std. err.	coef.	std. err.	coef.	std. err.
SHARE EMPL W/ FC			0.168	(0.016)	0.165	(0.018)	0.185	(0.030)
SHARE_SC × SC					-0.096	(0.015)	-0.097	(0.015)
SHARE_FC × FC					-0.031	(0.043)	-0.025	(0.041)
UNION DENSITY	-0.139	(0.051)	-0.111	(0.048)	-0.296	(0.055)	-0.300	(0.060)
UD × SC					0.203	(0.032)	0.180	(0.044)
UD × FC					0.367	(0.068)	0.461	(0.097)
UD × SHARE_SC							0.031	(0.061)
UD × SHARE_FC							-0.103	(0.117)
AGE	0.182	(0.007)	0.178	(0.007)	0.179	(0.007)	0.179	(0.007)
AGE SQUARED	-0.019	(0.001)	-0.018	(0.001)	-0.018	(0.001)	-0.018	(0.001)
TENURE	0.052	(0.002)	0.053	(0.002)	0.054	(0.002)	0.054	(0.002)
TENURE SQUARED	-0.011	(0.001)	-0.011	(0.001)	-0.011	(0.001)	-0.011	(0.001)
LOW EDUC	-0.032	(0.004)	-0.033	(0.004)	-0.035	(0.004)	-0.036	(0.004)
MED EDUC	-0.032	(0.002)	-0.034	(0.002)	-0.034	(0.002)	-0.034	(0.002)
HIGH EDUC	0.052	(0.008)	0.046	(0.008)	0.038	(0.008)	0.038	(0.008)
BC SPEC SKILL	0.079	(0.004)	0.078	(0.004)	0.076	(0.004)	0.076	(0.004)
BC ON THE JOB	-0.103	(0.003)	-0.101	(0.003)	-0.100	(0.003)	-0.100	(0.003)
BC LABORER	-0.212	(0.005)	-0.209	(0.005)	-0.208	(0.005)	-0.208	(0.005)
WC LIM EXEC	0.495	(0.004)	0.480	(0.004)	0.479	(0.004)	0.479	(0.004)
WC SPEC SKILL	0.265	(0.004)	0.260	(0.004)	0.260	(0.004)	0.260	(0.004)
WC VOC TRAIN INVOLV	0.178	(0.004)	0.177	(0.004)	0.177	(0.004)	0.177	(0.004)
WC VOC TRAIN AUTON	-0.034	(0.006)	-0.037	(0.006)	-0.036	(0.006)	-0.036	(0.006)
WC SIMPLE TASK	-0.196	(0.015)	-0.199	(0.015)	-0.198	(0.015)	-0.198	(0.015)
NIGHT	0.026	(0.003)	0.027	(0.003)	0.026	(0.003)	0.026	(0.003)
SUNDAY	0.052	(0.004)	0.053	(0.004)	0.052	(0.004)	0.052	(0.004)
SHIFT	0.047	(0.005)	0.049	(0.005)	0.050	(0.004)	0.050	(0.004)
OVERTIME	-0.011	(0.002)	-0.007	(0.002)	-0.006	(0.002)	-0.006	(0.002)
FIRM SHARE FEMALE	-0.078	(0.011)	-0.085	(0.011)	-0.083	(0.011)	-0.083	(0.011)
FIRM SHARE AGE1	0.031	(0.034)	0.079	(0.034)	0.042	(0.034)	0.041	(0.034)
FIRM SHARE AGE2	-0.180	(0.030)	-0.162	(0.030)	-0.166	(0.029)	-0.166	(0.029)
FIRM SHARE AGE3	-0.114	(0.028)	-0.121	(0.028)	-0.119	(0.028)	-0.119	(0.028)
FIRM SHARE AGE4	0.016	(0.027)	0.010	(0.027)	0.011	(0.027)	0.010	(0.027)
FIRM SHARE AGE6	-0.113	(0.025)	-0.113	(0.025)	-0.108	(0.024)	-0.108	(0.024)
FIRM SHARE AGE7	-0.118	(0.028)	-0.111	(0.027)	-0.104	(0.027)	-0.104	(0.027)
FIRM SHARE AGE8	0.030	(0.029)	0.020	(0.028)	0.023	(0.028)	0.023	(0.028)
FIRM SHARE AGE9	0.017	(0.033)	0.017	(0.032)	0.019	(0.032)	0.019	(0.032)
FIRM SHARE AGE10	-0.209	(0.037)	-0.167	(0.036)	-0.165	(0.036)	-0.165	(0.036)
FIRM SHARE TENURE1	-0.063	(0.025)	-0.048	(0.023)	-0.053	(0.023)	-0.053	(0.023)
FIRM SHARE TENURE2	-0.008	(0.025)	0.005	(0.023)	0.001	(0.023)	0.001	(0.023)
FIRM SHARE TENURE3	-0.003	(0.026)	0.001	(0.025)	-0.002	(0.025)	-0.002	(0.025)
FIRM SHARE TENURE5	-0.017	(0.034)	-0.018	(0.032)	-0.022	(0.032)	-0.022	(0.032)
FIRM SHARE TENURE6	0.037	(0.034)	0.035	(0.033)	0.031	(0.033)	0.031	(0.033)
FIRM SHARE TENURE7	0.065	(0.041)	0.057	(0.039)	0.051	(0.039)	0.051	(0.039)
FIRM SHARE TENURE8	-0.094	(0.039)	-0.106	(0.038)	-0.113	(0.038)	-0.113	(0.038)
FIRM SHARE TENURE9	0.007	(0.036)	-0.015	(0.036)	-0.030	(0.036)	-0.030	(0.036)
FIRM SHARE MED EDUC	0.031	(0.011)	0.042	(0.012)	0.040	(0.011)	0.040	(0.011)
FIRM SHARE HIGH EDUC	-0.010	(0.010)	-0.008	(0.010)	-0.007	(0.010)	-0.008	(0.010)
FIRM SHARE EDUC N.A.	0.165	(0.016)	0.191	(0.016)	0.179	(0.016)	0.179	(0.016)
HOURS WORKED	-0.002	(0.000)	-0.002	(0.000)	-0.002	(0.000)	-0.002	(0.000)
FIRM SHARE IRREG	0.005	(0.008)	-0.008	(0.008)	-0.010	(0.008)	-0.011	(0.008)
FIRM SHARE OVERTIME	0.052	(0.011)	0.043	(0.010)	0.041	(0.010)	0.041	(0.010)

Continued on next page...

... table 1.8 continued

	(v)		(vi)		(vii)		(viii)	
variable	coef.	std. err.	coef.	std. err.	coef.	std. err.	coef.	std. err.
FIRM SHARE BC	0.087	(0.010)	0.094	(0.010)	0.089	(0.010)	0.089	(0.010)
FIRM SHARE NOT FTIME	-0.232	(0.021)	-0.205	(0.021)	-0.204	(0.020)	-0.204	(0.021)
FIRMSIZE1	-0.024	(0.004)	-0.019	(0.004)	-0.019	(0.004)	-0.019	(0.004)
FIRMSIZE3	0.022	(0.005)	0.019	(0.005)	0.018	(0.005)	0.018	(0.005)
FIRMSIZE4	0.033	(0.005)	0.029	(0.005)	0.027	(0.005)	0.027	(0.005)
FIRMSIZE5	0.056	(0.006)	0.052	(0.006)	0.050	(0.006)	0.050	(0.006)
FIRMSIZE6	0.074	(0.008)	0.070	(0.008)	0.066	(0.008)	0.066	(0.008)
OWN PUBLIC1	-0.014	(0.013)	-0.014	(0.012)	-0.014	(0.012)	-0.014	(0.012)
OWN PUBLIC2	-0.055	(0.007)	-0.063	(0.008)	-0.062	(0.008)	-0.061	(0.008)
SECTOR1	0.025	(0.042)	0.017	(0.041)	0.021	(0.040)	0.021	(0.040)
SECTOR2	0.076	(0.011)	0.072	(0.010)	0.075	(0.011)	0.075	(0.011)
SECTOR3	0.074	(0.016)	0.068	(0.016)	0.072	(0.016)	0.072	(0.016)
SECTOR4	0.115	(0.012)	0.114	(0.011)	0.117	(0.012)	0.118	(0.012)
SECTOR5	0.236	(0.011)	0.237	(0.011)	0.238	(0.011)	0.239	(0.011)
SECTOR6	0.185	(0.012)	0.179	(0.012)	0.183	(0.012)	0.183	(0.012)
SECTOR7	0.122	(0.011)	0.120	(0.011)	0.126	(0.011)	0.127	(0.012)
SECTOR8	0.107	(0.011)	0.097	(0.011)	0.099	(0.011)	0.100	(0.011)
SECTOR9	0.141	(0.013)	0.139	(0.012)	0.146	(0.013)	0.146	(0.013)
SECTOR10	0.153	(0.012)	0.149	(0.012)	0.157	(0.012)	0.157	(0.012)
SECTOR11	0.131	(0.012)	0.129	(0.012)	0.133	(0.012)	0.133	(0.012)
SECTOR12	0.109	(0.012)	0.105	(0.012)	0.108	(0.012)	0.108	(0.012)
SECTOR13	0.112	(0.012)	0.110	(0.012)	0.113	(0.012)	0.113	(0.012)
SECTOR14	0.228	(0.017)	0.219	(0.016)	0.224	(0.016)	0.224	(0.016)
SECTOR15	0.110	(0.013)	0.107	(0.013)	0.110	(0.013)	0.111	(0.013)
SECTOR16	0.181	(0.015)	0.173	(0.015)	0.178	(0.015)	0.178	(0.015)
SECTOR17	0.133	(0.010)	0.128	(0.010)	0.124	(0.010)	0.124	(0.010)
SECTOR18	0.074	(0.011)	0.076	(0.011)	0.072	(0.011)	0.072	(0.011)
SECTOR19	0.063	(0.010)	0.061	(0.010)	0.059	(0.010)	0.059	(0.010)
SECTOR20	0.024	(0.011)	0.017	(0.011)	0.017	(0.011)	0.017	(0.011)
SECTOR21	-0.001	(0.014)	-0.010	(0.014)	-0.012	(0.014)	-0.012	(0.014)
SECTOR22	0.071	(0.014)	0.060	(0.014)	0.067	(0.014)	0.067	(0.014)
SECTOR23	0.149	(0.030)	0.135	(0.028)	0.142	(0.028)	0.143	(0.028)
SECTOR24	0.069	(0.012)	0.061	(0.011)	0.068	(0.012)	0.068	(0.012)
SECTOR25	0.142	(0.016)	0.104	(0.017)	0.099	(0.017)	0.099	(0.017)
SECTOR26	0.076	(0.015)	0.076	(0.015)	0.076	(0.014)	0.076	(0.015)
SECTOR27	0.066	(0.015)	0.063	(0.014)	0.054	(0.014)	0.054	(0.014)
SECTOR28	0.127	(0.014)	0.125	(0.013)	0.124	(0.014)	0.124	(0.014)
SECTOR29	0.094	(0.013)	0.094	(0.013)	0.094	(0.013)	0.094	(0.013)
REGION2	-0.040	(0.012)	-0.047	(0.011)	-0.046	(0.011)	-0.046	(0.011)
REGION3	-0.018	(0.010)	-0.019	(0.010)	-0.020	(0.010)	-0.020	(0.010)
REGION4	-0.015	(0.012)	-0.015	(0.011)	-0.016	(0.011)	-0.016	(0.011)
REGION5	-0.063	(0.011)	-0.065	(0.011)	-0.062	(0.011)	-0.062	(0.011)
REGION6	0.019	(0.011)	0.017	(0.011)	0.020	(0.011)	0.020	(0.011)
REGION7	-0.026	(0.011)	-0.030	(0.011)	-0.028	(0.011)	-0.028	(0.011)
CONSTANT	2.549	(0.043)	2.498	(0.041)	2.521	(0.041)	2.521	(0.041)

Estimation by OLS, observations weighted by inverse sampling probabilities.
Clustered standard errors in parentheses. Data source: Extended GSES 2001.

Table 1.9: Wage Regressions II: Different Sets of Covariates (all Covariates)

	(I)		(II)		(III)		(IV)	
	coef.	std. err.	coef.	std. err.	coef.	std. err.	coef.	std. err.
SECTOR CONTRACT	0.030	(0.033)	-0.057	(0.018)	-0.082	(0.012)	-0.061	(0.013)
FIRM CONTRACT	-0.169	(0.074)	-0.121	(0.045)	-0.149	(0.033)	-0.129	(0.032)
SHARE EMPL W/ SC	0.708	(0.015)	0.227	(0.009)	0.171	(0.008)	0.173	(0.008)
SHARE EMPL W/ FC	0.644	(0.033)	0.185	(0.020)	0.165	(0.018)	0.166	(0.017)
SHARE_SC × SC	-0.781	(0.043)	-0.160	(0.018)	-0.096	(0.015)	-0.110	(0.016)
SHARE_FC × FC	-0.454	(0.073)	-0.075	(0.047)	-0.031	(0.043)	-0.044	(0.041)
UNION DENSITY	-0.783	(0.057)	0.213	(0.040)	-0.296	(0.055)		
UD × SC	0.812	(0.071)	0.296	(0.059)	0.203	(0.032)	0.156	(0.030)
UD × FC	0.783	(0.171)	0.453	(0.118)	0.367	(0.068)	0.321	(0.065)
AGE			0.129	(0.009)	0.179	(0.007)	0.163	(0.015)
AGE SQUARED			-0.014	(0.001)	-0.018	(0.001)	-0.017	(0.002)
TENURE			0.071	(0.003)	0.054	(0.002)	0.052	(0.002)
TENURE SQUARED			-0.015	(0.001)	-0.011	(0.001)	-0.011	(0.001)
LOW EDUC			-0.017	(0.006)	-0.035	(0.004)		
MED EDUC			-0.045	(0.004)	-0.034	(0.002)		
HIGH EDUC			0.185	(0.007)	0.038	(0.008)		
BC SPEC SKILL			0.091	(0.007)	0.076	(0.004)	0.077	(0.004)
BC ON THE JOB			-0.115	(0.005)	-0.100	(0.003)	-0.099	(0.003)
BC LABORER			-0.237	(0.006)	-0.208	(0.005)	-0.207	(0.005)
WC LIM EXEC			0.519	(0.005)	0.479	(0.004)	0.480	(0.004)
WC SPEC SKILL			0.284	(0.005)	0.260	(0.004)	0.262	(0.004)
WC VOC TRAIN INVOLV			0.209	(0.005)	0.177	(0.004)	0.180	(0.004)
WC VOC TRAIN AUTON			-0.034	(0.007)	-0.036	(0.006)	-0.032	(0.006)
WC SIMPLE TASK			-0.190	(0.015)	-0.198	(0.015)	-0.188	(0.013)
NIGHT			0.024	(0.007)	0.026	(0.003)	0.028	(0.003)
SUNDAY			0.044	(0.006)	0.052	(0.004)	0.051	(0.004)
SHIFT			0.091	(0.009)	0.050	(0.004)	0.045	(0.004)
OVERTIME			-0.002	(0.003)	-0.006	(0.002)	-0.006	(0.002)
FIRM SHARE FEMALE					-0.083	(0.011)	-0.079	(0.010)
FIRM SHARE AGE1					0.042	(0.034)	0.039	(0.033)
FIRM SHARE AGE2					-0.166	(0.029)	-0.150	(0.027)
FIRM SHARE AGE3					-0.119	(0.028)	-0.119	(0.026)
FIRM SHARE AGE4					0.011	(0.027)	0.003	(0.025)
FIRM SHARE AGE6					-0.108	(0.024)	-0.104	(0.023)
FIRM SHARE AGE7					-0.104	(0.027)	-0.096	(0.025)
FIRM SHARE AGE8					0.023	(0.028)	0.013	(0.026)
FIRM SHARE AGE9					0.019	(0.032)	0.005	(0.029)
FIRM SHARE AGE10					-0.165	(0.036)	-0.173	(0.035)
FIRM SHARE TENURE1					-0.053	(0.023)	-0.038	(0.021)
FIRM SHARE TENURE2					0.001	(0.023)	0.004	(0.021)
FIRM SHARE TENURE3					-0.002	(0.025)	0.008	(0.023)
FIRM SHARE TENURE5					-0.022	(0.032)	-0.012	(0.030)
FIRM SHARE TENURE6					0.031	(0.033)	0.048	(0.028)
FIRM SHARE TENURE7					0.051	(0.039)	0.056	(0.034)
FIRM SHARE TENURE8					-0.113	(0.038)	-0.093	(0.035)
FIRM SHARE TENURE9					-0.030	(0.036)	-0.014	(0.033)
FIRM SHARE MED EDUC					0.040	(0.011)	0.025	(0.009)
FIRM SHARE HIGH EDUC					-0.007	(0.010)	-0.017	(0.009)
FIRM SHARE EDUC N.A.					0.179	(0.016)	0.161	(0.015)

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... table 1.9 continued

	(I)		(II)		(III)≡(vii)		(IV)	
	coef.	std. err.	coef.	std. err.	coef.	std. err.	coef.	std. err.
HOURS WORKED					-0.002	(0.000)	-0.002	(0.000)
FIRM SHARE IRREG					-0.010	(0.008)	-0.007	(0.007)
FIRM SHARE OVERTIME					0.041	(0.010)	0.036	(0.010)
FIRM SHARE BC					0.089	(0.010)	0.078	(0.009)
FIRM SHARE NOT FTIME					-0.204	(0.020)	-0.196	(0.019)
FIRMSIZE1					-0.019	(0.004)	-0.018	(0.003)
FIRMSIZE3					0.018	(0.005)	0.018	(0.004)
FIRMSIZE4					0.027	(0.005)	0.026	(0.005)
FIRMSIZE5					0.050	(0.006)	0.050	(0.005)
FIRMSIZE6					0.066	(0.008)	0.063	(0.007)
OWN PUBLIC1			-0.030	(0.014)	-0.014	(0.012)	-0.007	(0.011)
OWN PUBLIC2			-0.110	(0.010)	-0.062	(0.008)	-0.067	(0.008)
SECTOR1					0.021	(0.040)		
SECTOR2					0.075	(0.011)		
SECTOR3					0.072	(0.016)		
SECTOR4					0.117	(0.012)		
SECTOR5					0.238	(0.011)		
SECTOR6					0.183	(0.012)		
SECTOR7					0.126	(0.011)		
SECTOR8					0.099	(0.011)		
SECTOR9					0.146	(0.013)		
SECTOR10					0.157	(0.012)		
SECTOR11					0.133	(0.012)		
SECTOR12					0.108	(0.012)		
SECTOR13					0.113	(0.012)		
SECTOR14					0.224	(0.016)		
SECTOR15					0.110	(0.013)		
SECTOR16					0.178	(0.015)		
SECTOR17					0.124	(0.010)		
SECTOR18					0.072	(0.011)		
SECTOR19					0.059	(0.010)		
SECTOR20					0.017	(0.011)		
SECTOR21					-0.012	(0.014)		
SECTOR22					0.067	(0.014)		
SECTOR23					0.142	(0.028)		
SECTOR24					0.068	(0.012)		
SECTOR25					0.099	(0.017)		
SECTOR26					0.076	(0.014)		
SECTOR27					0.054	(0.014)		
SECTOR28					0.124	(0.014)		
SECTOR29					0.094	(0.013)		
REGION2			-0.062	(0.014)	-0.046	(0.011)		
REGION3			-0.036	(0.013)	-0.020	(0.010)		
REGION4			-0.015	(0.014)	-0.016	(0.011)		
REGION5			-0.091	(0.014)	-0.062	(0.011)		
REGION6			0.014	(0.013)	0.020	(0.011)		
REGION7			-0.038	(0.013)	-0.028	(0.011)		
CONSTANT	2.841	(0.013)	2.280	(0.022)	2.521	(0.041)	2.579	(0.044)

Estimation by OLS, observations weighted by inverse sampling probabilities.
Clustered standard errors in parentheses. Data source: Extended GSES 2001.

Table 1.10: Wage Regressions III: Quantile Regressions of Wages (all Covariates)

Percentile	(10)		(50)		(90)	
	coef.	std. err.	coef.	std. err.	coef.	std. err.
SECTOR CONTRACT	-0.035	(0.016)	-0.071	(0.012)	-0.124	(0.018)
FIRM CONTRACT	-0.104	(0.033)	-0.157	(0.031)	-0.183	(0.047)
SHARE EMPL W/ SC	0.168	(0.009)	0.188	(0.008)	0.158	(0.013)
SHARE EMPL W/ FC	0.129	(0.027)	0.173	(0.015)	0.131	(0.035)
SHARE_SC × SC	-0.104	(0.019)	-0.109	(0.014)	-0.074	(0.022)
SHARE_FC × FC	0.002	(0.050)	-0.030	(0.036)	-0.010	(0.065)
UD	-0.173	(0.057)	-0.256	(0.045)	-0.391	(0.086)
UD × SC	0.149	(0.040)	0.155	(0.029)	0.209	(0.051)
UD × FC	0.199	(0.061)	0.349	(0.075)	0.489	(0.074)
AGE	0.171	(0.009)	0.162	(0.008)	0.172	(0.012)
AGE SQUARED	-0.018	(0.001)	-0.017	(0.001)	-0.017	(0.001)
TENURE	0.066	(0.003)	0.049	(0.002)	0.045	(0.003)
TENURE SQUARED	-0.012	(0.001)	-0.009	(0.001)	-0.009	(0.001)
LOW EDUC	-0.033	(0.004)	-0.037	(0.004)	-0.040	(0.007)
MED EDUC	-0.038	(0.003)	-0.033	(0.002)	-0.029	(0.003)
HIGH EDUC	0.063	(0.007)	0.039	(0.006)	0.016	(0.012)
BC SPEC SKILL	0.054	(0.004)	0.066	(0.003)	0.101	(0.006)
BC ON THE JOB	-0.099	(0.004)	-0.092	(0.003)	-0.099	(0.005)
BC LABORER	-0.223	(0.007)	-0.190	(0.005)	-0.181	(0.006)
WC LIM EXEC	0.419	(0.004)	0.470	(0.004)	0.543	(0.007)
WC SPEC SKILL	0.226	(0.005)	0.270	(0.004)	0.293	(0.006)
WC VOC TRAIN INVOLV	0.142	(0.005)	0.189	(0.004)	0.224	(0.005)
WC VOC TRAIN AUTON	-0.046	(0.004)	-0.029	(0.005)	-0.002	(0.011)
WC SIMPLE TASK	-0.312	(0.021)	-0.201	(0.012)	-0.029	(0.061)
NIGHT	0.024	(0.004)	0.029	(0.004)	0.033	(0.005)
SUNDAY	0.038	(0.004)	0.056	(0.005)	0.061	(0.006)
SHIFT	0.044	(0.005)	0.051	(0.005)	0.047	(0.005)
OVERTIME	0.007	(0.003)	0.001	(0.002)	-0.010	(0.003)
FIRM SHARE FEMALE	-0.094	(0.013)	-0.099	(0.010)	-0.091	(0.017)
FIRM SHARE AGE1	0.043	(0.040)	0.068	(0.035)	0.050	(0.051)
FIRM SHARE AGE2	-0.102	(0.033)	-0.123	(0.029)	-0.128	(0.043)
FIRM SHARE AGE3	-0.101	(0.031)	-0.128	(0.027)	-0.122	(0.042)
FIRM SHARE AGE4	-0.007	(0.032)	0.003	(0.025)	-0.010	(0.039)
FIRM SHARE AGE6	-0.093	(0.030)	-0.086	(0.023)	-0.116	(0.034)
FIRM SHARE AGE7	-0.102	(0.032)	-0.083	(0.025)	-0.077	(0.038)
FIRM SHARE AGE8	0.019	(0.031)	0.028	(0.027)	0.017	(0.041)
FIRM SHARE AGE9	-0.015	(0.036)	0.039	(0.030)	0.096	(0.042)
FIRM SHARE AGE10	-0.198	(0.045)	-0.139	(0.035)	-0.172	(0.056)
FIRM SHARE TENURE1	-0.090	(0.017)	-0.038	(0.017)	-0.043	(0.036)
FIRM SHARE TENURE2	-0.026	(0.018)	0.010	(0.018)	0.018	(0.035)
FIRM SHARE TENURE3	0.014	(0.017)	0.009	(0.016)	-0.013	(0.038)
FIRM SHARE TENURE5	-0.002	(0.025)	0.009	(0.022)	-0.033	(0.048)
FIRM SHARE TENURE6	0.060	(0.027)	0.027	(0.026)	-0.004	(0.047)
FIRM SHARE TENURE7	0.091	(0.031)	0.067	(0.027)	0.004	(0.054)
FIRM SHARE TENURE8	-0.096	(0.036)	-0.093	(0.031)	-0.104	(0.053)
FIRM SHARE TENURE9	0.015	(0.029)	-0.025	(0.029)	-0.085	(0.053)
FIRM SHARE MED EDUC	0.003	(0.011)	0.025	(0.010)	0.063	(0.019)
FIRM SHARE HIGH EDUC	-0.002	(0.013)	-0.015	(0.009)	-0.035	(0.012)
FIRM SHARE NA EDUC	0.233	(0.018)	0.202	(0.016)	0.117	(0.025)
HOURS WORKED	-0.002	(0.000)	-0.002	(0.000)	-0.002	(0.000)
FIRM SHARE IRREG	-0.010	(0.010)	-0.006	(0.008)	-0.004	(0.010)

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... table 1.10 continued

Percentile	(10)		(50)		(90)	
	coef.	std. err.	coef.	std. err.	coef.	std. err.
FIRM SHARE OVERTIME	0.044	(0.012)	0.041	(0.008)	0.049	(0.012)
FIRM SHARE BC	0.053	(0.011)	0.068	(0.009)	0.099	(0.014)
FIRM SHARE NOT FTIME	-0.190	(0.025)	-0.199	(0.020)	-0.202	(0.029)
FIRMSIZE1	-0.014	(0.004)	-0.015	(0.003)	-0.026	(0.005)
FIRMSIZE3	0.011	(0.005)	0.020	(0.004)	0.021	(0.006)
FIRMSIZE4	0.022	(0.005)	0.030	(0.005)	0.025	(0.006)
FIRMSIZE5	0.046	(0.005)	0.050	(0.006)	0.050	(0.009)
FIRMSIZE6	0.077	(0.008)	0.065	(0.007)	0.055	(0.011)
OWN PUBLIC1	-0.023	(0.015)	-0.001	(0.011)	-0.014	(0.016)
OWN PUBLIC2	-0.043	(0.010)	-0.053	(0.007)	-0.073	(0.009)
SECTOR1	-0.023	(0.038)	0.006	(0.053)	0.026	(0.042)
SECTOR2	0.112	(0.015)	0.075	(0.012)	0.028	(0.016)
SECTOR3	0.067	(0.019)	0.060	(0.018)	0.067	(0.023)
SECTOR4	0.157	(0.013)	0.109	(0.011)	0.067	(0.017)
SECTOR5	0.245	(0.014)	0.234	(0.012)	0.240	(0.017)
SECTOR6	0.230	(0.013)	0.176	(0.012)	0.123	(0.019)
SECTOR7	0.173	(0.014)	0.118	(0.011)	0.074	(0.017)
SECTOR8	0.154	(0.013)	0.097	(0.011)	0.035	(0.017)
SECTOR9	0.173	(0.014)	0.139	(0.012)	0.111	(0.020)
SECTOR10	0.198	(0.013)	0.152	(0.011)	0.110	(0.019)
SECTOR11	0.180	(0.014)	0.126	(0.011)	0.078	(0.020)
SECTOR12	0.161	(0.013)	0.109	(0.012)	0.055	(0.017)
SECTOR13	0.156	(0.013)	0.115	(0.012)	0.081	(0.020)
SECTOR14	0.247	(0.016)	0.215	(0.014)	0.195	(0.023)
SECTOR15	0.131	(0.019)	0.110	(0.012)	0.095	(0.024)
SECTOR16	0.196	(0.018)	0.173	(0.013)	0.155	(0.020)
SECTOR17	0.180	(0.014)	0.126	(0.010)	0.056	(0.015)
SECTOR18	0.109	(0.014)	0.062	(0.012)	0.048	(0.016)
SECTOR19	0.104	(0.012)	0.058	(0.011)	0.030	(0.016)
SECTOR20	0.067	(0.013)	0.018	(0.012)	-0.013	(0.021)
SECTOR21	-0.036	(0.020)	-0.010	(0.014)	-0.016	(0.018)
SECTOR22	0.111	(0.017)	0.057	(0.014)	0.035	(0.023)
SECTOR23	0.122	(0.028)	0.140	(0.021)	0.180	(0.029)
SECTOR24	0.104	(0.015)	0.067	(0.012)	0.029	(0.018)
SECTOR25	0.123	(0.022)	0.109	(0.018)	0.052	(0.023)
SECTOR26	0.137	(0.013)	0.054	(0.011)	0.029	(0.027)
SECTOR27	0.125	(0.016)	0.052	(0.012)	-0.011	(0.024)
SECTOR28	0.169	(0.018)	0.130	(0.015)	0.077	(0.036)
SECTOR29	0.116	(0.015)	0.109	(0.013)	0.080	(0.019)
REGION2	-0.010	(0.007)	-0.022	(0.008)	-0.104	(0.032)
REGION3	0.026	(0.006)	-0.000	(0.007)	-0.084	(0.031)
REGION4	0.024	(0.007)	0.003	(0.008)	-0.087	(0.032)
REGION5	-0.013	(0.007)	-0.030	(0.008)	-0.126	(0.031)
REGION6	0.067	(0.007)	0.045	(0.008)	-0.047	(0.031)
REGION7	0.007	(0.007)	-0.011	(0.008)	-0.090	(0.032)
CONSTANT	2.315	(0.049)	2.530	(0.039)	2.854	(0.069)

Quantile regression, observations weighted by inverse sampling probabilities. Clustered standard errors in parentheses. Data source: Extended GSES 2001.

Table 1.11: Robustness Checks Regarding Small Imputation Cell Sizes and Regarding Clustering of Standard Errors for Weighted Median Wage Regressions

OLS REGRESSIONS

Variable	Benchmark Paper	Cell Size > 6	Cell Size > 18
SECTOR CONTRACT	-0.082 (0.012)	-0.081 (0.012)	-0.077 (0.012)
FIRM CONTRACT	-0.149 (0.033)	-0.147 (0.034)	-0.146 (0.035)
SHARE EMPL W/ SC	0.171 (0.008)	0.171 (0.008)	0.170 (0.008)
SHARE EMPL W/ FC	0.165 (0.018)	0.163 (0.018)	0.161 (0.018)
SHARE_SC × SC	-0.096 (0.015)	-0.096 (0.015)	-0.096 (0.015)
SHARE_FC × FC	-0.031 (0.043)	-0.032 (0.043)	-0.032 (0.044)
UNION DENSITY	-0.296 (0.055)	-0.290 (0.055)	-0.292 (0.057)
UD × SC	0.203 (0.032)	0.201 (0.032)	0.189 (0.032)
UD × FC	0.367 (0.068)	0.371 (0.068)	0.367 (0.070)

MEDIAN REGRESSIONS

Variable	Benchmark Paper	Std.err. not clustered	Cell Size > 6	Cell Size > 18
SECTOR CONTRACT	-0.071 (0.012)	-0.071 (0.005)	-0.070 (0.012)	-0.067 (0.012)
FIRM CONTRACT	-0.157 (0.031)	-0.157 (0.009)	-0.153 (0.030)	-0.154 (0.031)
SHARE EMPL W/ SC	0.188 (0.008)	0.188 (0.004)	0.188 (0.008)	0.188 (0.008)
SHARE EMPL W/ FC	0.173 (0.015)	0.173 (0.007)	0.173 (0.015)	0.173 (0.014)
SHARE_SC × SC	-0.109 (0.014)	-0.109 (0.006)	-0.110 (0.014)	-0.112 (0.014)
SHARE_FC × FC	-0.030 (0.036)	-0.030 (0.012)	-0.035 (0.034)	-0.036 (0.035)
UNION DENSITY	-0.256 (0.045)	-0.256 (0.019)	-0.248 (0.044)	-0.241 (0.044)
UD × SC	0.155 (0.029)	0.155 (0.011)	0.154 (0.029)	0.149 (0.029)
UD × FC	0.349 (0.075)	0.349 (0.017)	0.351 (0.073)	0.351 (0.075)
N	316,805	316,805	313,154	297,428

Coefficients with standard errors in parentheses. All regressions include individual and firm-level covariates.
The first column ("benchmark paper") reproduces the results reported in the paper (table 3, specification III, and table 5, column percentile (50)).
The second column uses weighted quantile regression with heteroscedascticity-consistent but non-clustered standard errors.
The third column discards observations where the NUD imputation cell size is 6 or less observations (25% of the cells). The fourth column discards cells with 18 or less observations (50% of the cells).
Note that these wage regressions are estimated based on individual level data. Therefore, the estimates reported in the paper account for different cell sizes, giving higher weights to larger cells.

Chapter 2

The Impact of Mandatory Paid Leave on Employment in the UK

2.1 Introduction

Many European countries have experimented with restricting the number of hours that individuals can work, with the aim either of improving the conditions of work for those affected or of sharing work out more equally when jobs are scarce ('work-sharing'). However, contrary to expectations of increased employment, studies of Germany (Hunt 1999) and France (Crépon and Kramarz 2002) both conclude that reductions in the standard working week reduces employment. In this paper we examine another form of restrictions on working time, the setting of minimum weeks of paid holiday entitlement. All the OECD countries with the exception of the United States, have some regulation of paid holiday entitlement.[1] In this paper we study the introduction of such a regulation in the UK where over the 1998-99 period, the law changed to give all workers (subject to some exceptions described below) the right to a minimum of 4 weeks of paid holiday a year. If there was no change in weekly pay resulting from this change (a possibility we discuss further), an employee going from 0 to 4 weeks of paid holiday is effectively going from 50 to 46 weeks of paid work (allowing for the public holidays they might have been entitled to before) an increase in the real hourly wage of about 8.5%. This is large relative to the reduction in the standard work week from 40 to 39 hours studied by Crépon and Kramarz (2002), which is an increase in the hourly wage of 2.5% and the hours reductions Hunt (1999) studies in Germany, which were a maximum of a 10% reduction.

About 9% of full-time employees in the UK Labour Force Survey (LFS) report less than three weeks entitlement to paid holidays. Another 8% are entitled to up to four weeks of leave. Since the reform was implemented at the same point in time in all of the UK we cannot use regional variation to estimate its impact. In lieu of regional variation, a natural control group are employees with paid holiday entitlement greater than the regulation threshold.

[1] See Altonji and Oldham (2003) for an U.S.-centric overview.

There are two problems with this control group. One, using the whole sample the treatment and control group are dissimilar and two, the limited panel nature of the data makes it impossible to track treated individuals for longer than one year. We address the first problem by using propensity score estimates to select the sample. This increases comparability. To address the second issue, we construct a treatment indicator that extends beyond the treatment period. We first estimate the propensity score on a pre-treatment sample. We then use the in- and out-of-sample predictions of the propensity score as treatment indicator. This allows us to extend our analysis to a longer time-horizon and to include the non-employed in our analysis.

We first evaluate the impact on employees by estimating the hazard to non-employment. Using complementary log-log regression to account for right-censoring of the data, we find no increase in the hazard to non-employment within a year after treatment. Adjustments in wages cannot explain this result as they are increasing for the treated groups relative to the control.

Since there is no effect on the employed, we evaluate the impact on total employment. Using the predicted treatment probabilities in a difference-in-differences framework we find a small statistically significant effect on employment. This effect is driven by a trend reversal in employment, coinciding with the treatment. Until the treatment occurred, employment was on a slight upward trend for the treated groups when compared to the control. After the treatment employment switched to a downward trend.

The plan of the paper is as follows. We first describe the legislation and the data we use. We then show that there was an effect of the legislation on the amount of paid holiday received, so there was a treatment. We then turn to the effects on employment which we find to be small and insignificant. It is possible that this is because there were off-setting movements in wages or other employee related expenses so we investigate this. We then evaluate the long-run impact on employment by focussing on the labor force as a whole. The final section concludes.

2.2 The Working Time Regulations 1998

In 1993 the European Council issued the Council Directive 93/104/EC, which encompasses minimum requirements on working time regulations in the member states. Implementing the directive, the British parliament passed the statutory instrument 1998 No. 1833, "The Working Time Regulations 1998" (WTR) in autumn 1998.

The European Directive demands the enactment of a national regulation either by the governing body or by a general agreement between employers' and employees' associations by November 23, 1996. Barnard (1999) lists some reasons for the delay and cites concerns of members of Parliament that the implementation was too quick and employers could not prepare for the pending changes. Despite the concerns, the WTR came into force on October 1, 1998. The regulation introduces minimum standards for adult and young (less than 18 years old) workers in three major areas: working time, rest periods and annual leave.

Weekly working hours are restricted to an average of 48 hours per week, but the regulation leaves the option for worker and employer to contractually agree on exceeding the limit. The option for an opt-out was introduced in the Council

Directive at the UK's urging and remains a disputed issue (Barnard, Deakin, and Hobbs 2003).[2] The result of this clause is that the limit on standard working hours had little effect on the usual hours worked (see table 2.1).

The WTR includes detailed regulations on daily- and weekly rest periods as well as on breaks during working time. An adult (young) worker is entitled to a rest period of 24 (48) consecutive hours per week or 48 consecutive hours in two weeks. If daily work exceeds six (four and a half) hours, the worker is supposed to have a break of 20 (30) minutes. Night work should not last—on average—more than 8 hours per night.

Most importantly for our study, the WTR introduced, starting on October 1, 1998, that every worker who has been continuously employed by the same employer for thirteen weeks gains the right to paid holidays. In 1998 and 1999 the number of weeks that a worker qualified for depended on the leave year[3] of the worker. If the leave year began on or before November 23, 1998, the worker was entitled to three weeks of paid holidays. For a leave year beginning after the 23rd, the eligibility was three weeks plus the share of week given by the time elapsed between the November 23, 1998 and 1999. And finally employees whose leave year started after the 23 November 1999 had the right to 4 weeks of paid holidays. Exempt from the eligibility to annual leave are the transport sector, workers at sea, doctors in training, and civil protection services, if their duties conflict with their rights from the WTR.

For our study the initial Working Time Regulations from 1998 are of interest. But the WTR was only one of several institutional changes that Tony Blair's "New" Labour government introduced after coming into power in 1997. Most of the impending changes were already announced in their 1997 Labour Party Manifesto and subsequently motivated in the 1998 White Paper "Fairness at Work" or they were implementations of existing EU regulations. In the next section we will give a short overview of the most important reforms (with respect to our study) at the turn of the century.

2.3 Other reforms

2.3.1 WFTC and the New Deal

With the 1998 budget, chancellor Gordon Brown introduced a reform of the family credit, the main UK in-work benefit during the 1990's.[4] Starting from October 1999 the Working Families' Tax Credit (WFTC) replaced the family credit. The reform had strong effects on labor supply incentives. Simulating the consequences of WFTC for working hours and participation in the labor market, Blundell, Duncan, McCrae, and Meghir (2000) find that for most demographic groups the incentives result in an unambiguous increase in labor supply at the extensive margin (the margin that we are concerned with in this study). The only group where employees would choose to leave work is for married couples with both partners working. The authors results

[2] In their 2004 proposal for a revision of the European Directive, the European Commission left the opt-out untouched.

[3] If not fixed by agreement, the leave year begins at the date the worker's employment began.

[4] See e.g. Blundell, Duncan, McCrae, and Meghir (2000) for a detailed discussion.

suggest a small share of employed women (or men) with working partners would choose to leave employment. The distortion is therefore negligible.

Another part of the Welfare to Work initiative of the recently elected Labour government was the extension of the New Deal program. For young unemployed the New Deal was mandatory and it involved two phases. In the first phase the unemployed received job search and application support by mentors or advisers. If this gateway program was unsuccessful the unemployed had to choose subsidized training, subsidized employment or employment in the voluntary sector, to remain eligible for unemployment benefits (Blundell, Costa Dias, Meghir, and Van Reenen 2004).[5] The impact of the New Deal on our study is indirect. Employment is subsidized and if the subsidy runs out, the employment chances might be affected detrimentally. Less than 1% of the individuals in the sample are on government schemes and do not influence our results.

2.3.2 ERA and unfair dismissal legislation

In the summer of 1999, the Employment Relations Act (ERA) came into force. The ERA intended to promote a co-operative form of trade unionism (Smith and Morton 2001) by introducing a statutory recognition procedure and strengthening the union's rights at the workplace (Oxenbridge, Brown, Deakin, and Pratten 2003). The effect on employment in our study should be negligible.

The Unfair Dismissal and Statement of Reasons for Dismissal (Variation of Qualifying Period) Order also became effective during the summer of 1999. The order reduced the required tenure for employees to sue their employer for unfair dismissal from 24 to 12 months. Marinescu (2009) finds that the reform increases the chances of remaining employed for employees with 24 to 12 months of tenure and decreases the probability of being fired in the first twelve months, which she attributes to increased recruitment efforts.

To account for the Unfair Dismissal Order we run robustness checks on a sample of employees with 2 or more years of tenure.

2.3.3 NMW

After all minimum wage regulations were abolished in 1993 (with the exception of the agricultural sector), the Labour government reintroduced a National Minimum Wage (NMW) in 1999. The NMW was based on the recommendations of the Low Pay Commission that had been enacted 2 years prior (Metcalf 1999). Starting April 1, 1999 the NMW was introduced for adult workers (here defined as 22 years and older) as a minimum hourly wage of £3.60. This wage was gradually increased to £4.50 in 2003. For young workers (age 18 to 21) the minimum was £3.00 in 1999 and £3.80 by 2003. See table 2.2 for a timeline of the changes in the minimum wage.

Stewart (2004) finds that the minimum wage did not have any detrimental employment effects. Nonetheless we find it important to account for potentially adverse effects. We do so by explicitly controlling for wages below the (next period) minimum wage.

[5] See Blundell, Costa Dias, Meghir, and Van Reenen (2004) and De Giorgi (2005) for evaluations of the New Deal.

2.3.4 Amendments to the WTR 1998

The Working Time Regulations 1998 were amended nearly annually. After only a year some minor changes were introduced in autumn 1999. The main change was that rules on keeping records for workers that agree to exclude the limit on hours worked per week were slackened. The second amendment followed in 2001. This amendment acknowledges a lawsuit filed and won by the Broadcasting, Entertainment, Cinematograph and Theatre Union at the European Court of Justice (ECJ). The ECJ ruled that the European Directive, which was implemented by the WTR, does not allow for a requirement of a minimum of 13 weeks of uninterrupted work to qualify for paid leave. Accordingly the restriction on the entitlement to paid leave was abolished by the 2001 amendment. For the 2002 amendment more exceptions that allow to treat a young worker like an adult worker were introduced, but the change in rules were only related to weekly working time and night work.

The reason for the 2003 amendment was another European Council Directive, which led to the extension of the WTR to the, so far excluded, transportation sector. The regulations for doctors in training were made explicit (instead of simply exempting them from the WTR regulations) and more detailed enforcement rules were established, including the appointment of enforcement agencies that are allowed to use inspectors in order to verify the application of the WTR.

2.4 The data

We use the UK Labour Force Survey (LFS) from 1994 to 2004.[6] The survey comprises about 145,000 individuals with 20% being replaced every quarter (i.e. without panel attrition we observe an individual at most 5 consecutive quarters). Answers are mostly given personally by the respondent but a third of our sample relies on responses by proxy.[7] Our main variable of interest, eligibility for paid holidays, is only available in the autumn quarter (Sep.-Nov.) which limits the number of observations that we can use to construct a time-series for an individual.

We select observations where the respondent is between 16 and 64 years (men) or 59 years (women) old to account for exits into retirement. We exclude the transport and fishing industry as well as the armed forces because the changes in the WTR did not apply to them. We also do not use observations from the education sector since employees working in education show very industry specific and unusual patterns in their holiday entitlement (i.e. 60 days of paid holidays). Table 2.3 lists the variables used in this study.

Interviews for the Labour Force Survey are conducted throughout the year, with data provided on a quarterly basis. The interviews refer to a "reference week" within the quarter, usually the week before the interview takes place. Since the WTR comes into place about 2/3 into the autumn wave in 1998, there is a problem with distinguishing pre- and post reform observations. It is possible to identify the reference

[6]Most of the variables we use in this study are only available in the LFS from 1994 onwards and we therefore discard earlier years.

[7]While for the most part the answers given by proxy are consistent with those given personally by the respondent, there are some patterns that seem out of place, e.g. employees gaining more than 12 months of firm tenure in a 3 month period or the date an employee started with his or her current employer changing to the current date in one wave and reverting to the prior value in the next.

week of the interview and, therefore, construct a sharp boundary for the timing of the WTR. The drawbacks of using a sharp cut-off are two-fold. First, the number of treated observations drops decidedly and, second, within quarter variation suffers from regional selection[8]. Since autumn is the only quarter containing the variable of interest (paid holiday entitlement per year), we have to rely on observations from that quarter and cannot simply use adjacent quarters for the analysis. Given the data constraints and the descriptive evidence (see section 2.5), we choose to treat the autumn 1998 wave as pre-treatment.

For the first part of our analysis we focus on the change in the hazard rate to exit employment. We construct a panel of individuals who are observed in the third quarter, who are employed[9] and observed in at least one of the following quarters. There are three states that constitute an exit from employment: unemployment, inactivity or self-/family employment, we pool these states into one and refer to it as "non-employment".

As for the eligibility to paid holidays, respondents in the LFS are asked about days of paid holiday (excluding public or bank holidays). The WTR states eligibility in terms of weeks per (leave) year and therefore depends on the number of days worked per week as well as the length of employment within a year (i.e. employees in seasonal employment are only entitled to a share of the full four weeks). Since the actual length of non-permanent contracts is only available in spring quarters in the LFS, we cannot adjust paid holidays for respondents who work only part of the year. To ensure that we measure the right amount of paid holidays we keep only employees with permanent contracts in our sample.

For a small share of employees (1.8% in 1994 to 0.7% in 2001), the information about paid holidays is not available. One third of the missing values are due to imputed values from previous quarters for the holiday variable (in case of non-response and no proxy response in a quarter), the other two thirds might be due to the "employment edit" performed by the Office for National Statistics where respondents who classify themselves as "self-employed" are reclassified as "employee" given their occupational code (holidays are not asked from respondents reporting to be self-employed). For our analysis we consider only employees with non-missing responses for the "paid holidays" variable.

The advantage of using the LFS is the large sample size, the disadvantage is that the panel is limited to 5 consecutive quarters. This means that we have a problem with identifying treated employees after the treatment took place. If compliance were perfect, there would be no more employees with less than 4 weeks of paid leave after autumn 1999. We address this issue in two ways. First, for the impact on the employed, we use the limited panel nature and cut off our sample after the reform took place. Second, for the impact on the labor force, we impute treatment probabilities.

For the first part of the analysis, the impact on the employed, we focus only on employees who report to work 5 or more days per week[10] and use the number of days

[8]By sampling design.

[9]We exclude respondents who are self-employed, unpaid family workers and workers on government schemes, as well as ILO unemployed respondents and respondents who are out of labour force.

[10]If a respondent reports to work on a 4 1/2 day week or 9-day fortnight contract, the question is not asked. Since the variable is measured in whole days, we treat these respondents as working 5 days a week. About 16% of employees report less than 5 workdays.

of paid holidays to indicate weeks of paid leave. We treat 20 days of entitlement as exactly 4 weeks of paid leave, 15–19 days as between 3 and less than 4 weeks, etc. This coding seems natural since most employees report their paid leave in multiples of 5.

The WTR was implemented at the same time across all regions in the UK. Therefore we cannot use the timing or cross-sectional variation of the reform to establish a control group. Instead we focus on employees above the WTR entitlement threshold as control. For our analysis we consider four groups. The control group are employees with more than 4 weeks of holiday entitlement, the three treated groups are employees who report no entitlement; 1–14 days (some leave but less than 3 weeks) and 15–19 days (at least 3 but less than 4 weeks) of entitlement. The former two are treated in autumn 1998, the latter in 1999.

In the second part of the paper we estimate the long run impact on the whole working age population. For this we have to construct a pseudo-panel following not individuals but groups with the same characteristics over time. The group treatment indicator is estimated using propensity score estimates from a pre-treatment sample. As we are concerned with the long run effects of the WTR, we consider all employees with a ratio of leave entitlement to usual days worked per week below 4 as treated. The estimated coefficients are then used to calculate the out-of-sample prediction for the propensity score in the treatment period and beyond. The advantage of using imputed propensity scores as treatment indicator is that we do not have to discard respondents who are not employed. We therefore keep both employees and non-employees in the sample, subject to the age and industry restrictions mentioned above.

2.5 Was there a treatment

For there to have been any plausible effect on employment or other outcomes, we must first show that there was a measurable effect on the number of days of paid holiday. Figure 2.1 describes the proportions of workers who work more than 5 days per week with different levels of paid holiday entitlement for the period 1994–2004. Depicted are the three treatment groups, no entitlement to paid holidays, 1–14 days, and 15–19 days entitlement. Omitted are the majority of employees who have 20 or more days of paid leave.

In total the initial treatment affected about 8% of the employees in the sample, with another 8% treated in 1999. In both cases the treatment seems to be delayed, the share of employees with less than three weeks of leave entitlement drops sharply from Autumn 1998 to Autumn 1999. The same drop occurs for employees with 15–19 days of paid leave from 1999 to 2000. The graph shows that the reform had an impact on the employed, but it is also evident that treatment was not comprehensive. A fraction of the employed reports paid leave entitlement below the legal minimum up until the end of the sample period.

For some employees (those that are observed employed in two Autumn quarters) we can plot the change in paid leave. Figure 2.2 depicts the average leave entitlement for all four groups considered. It is apparent that even before the WTR, paid leave entitlement increased significantly for employees who initially had no paid leave. For example, employees who had no paid leave in Autumn 1994, reported, on average, about 9 days of entitlement in 1995. But the treatment is still evident, there is a clear jump in Autumn 1998, which means that employees had more paid leave in Autumn 1999 than in previous years. The transition is smoother for the two other treated groups, but small increases in the average entitlement levels are visible. There is no change in the average for untreated employees (20 days and more), it remains constant at around 24 days of paid holidays.

While a sizable share of the workforce is affected by the WTR, two factors prevented a strong impact of the reform. The implementation seems to have been slow[11] and the the actual treatment is moderated because employees saw increases in leave entitlement even before the reform.

The first column of table 2.4 adds covariates to the graph in figure 2.1. We estimate a linear probability model with an indicator for less than 20 days of paid leave as dependent variable, i.e. we pool all three treated groups. Compared to 1994 (the base category) we find a statistically significant difference in the probability to have less than 4 weeks of leave entitlement in 1998. But the size of the effect is small with less than 1 percentage point. The effect becomes stronger in the following years with the biggest changes in 1999 and 2000.

The WTR accords employees paid leave, but whether they make use of their entitlement is up to individual choice. In figure 2.3 we plot the share of employees who report to be "on vacation" in at least one of the four interview weeks following autumn. We would expect the shares to rise for the treated groups after the WTR came into place. There is some variation across years but the share of employees on vacation seems to be fairly stable for employees with three to four and more than four weeks of paid leave. Employees with less than three weeks of paid leave exhibit an upward trend even before the WTR went into effect. This might suggest that employees did not make use of their entitlement, thereby circumventing the legislation. But the measure is too crude to draw strong conclusions.

The descriptives as well as the regressions suggest that the WTR had an effect on paid leave eligibility, at the aggregate level the share of employees in the treated groups decrease. Compliance seems to be imperfect, the shares of treated employees does not drop to zero after the reform. It also seems to be the case that treatment was delayed. While the increase in paid leave started in October 1998, the data suggests that only a very small share of employees actually received more paid leave in autumn 1998, the large changes occur over the following two years. The descriptive evidence supports the classification of observations in autumn 1998 as untreated.

2.6 The effect on employment

In this section we evaluate the effect of the legislation on flows out of employment. In our data we observe paid holiday entitlement for those in employment in autumn. We can then follow individuals through subsequent quarters and observe whether they stay in or leave employment. Potential exit states are unemployment, self-employment or inactivity. Because individuals remain in the survey for a maximum of 5 quarters and each quarter 1/5th of the sample is replaced, we observe 4/5th of

[11]Note that this is not the only possible explanation, e.g. employees might also not have been aware of their entitlement.

the sample 1 quarter into the future falling to 1/5th for a time horizon of one year.

We pool all possible exit states into one category "non-employment". Intuitively this makes sense, an employer might fire employees, but offer to hire their services if they started their own company. On the other hand, losing an already marginal job due to a real wage increase, making the wage higher than the worker's productivity might discourage the employee and lead to an exit from the labor force. More positively it might encourage the worker to increase his productivity by investing in further education.

Figure 2.4 plots the pooled exit states for all groups of employees. Each wave starts in autumn of a given year and ends in autumn of the following year. Each dot represents the non-employment share in a quarter, conditional on being employed in Autumn. For all groups the share of employees who are not employed increases over time. The increase is nearly linear for employees without paid leave and concave for all other employees. A clear ranking emerges, where employees are less likely to exit employment the more paid leave they have. Clearly low levels of paid leave entitlement are associated with less stable employment. This is in line with the correlations from the linear probability model (first column of table 2.4). Treated individuals are more likely to work in Agriculture, Mining, Construction, Hotels and Restaurants and in private households, all sectors with low wages and high turnover.

Comparing the exits across waves, we see that employees with 4 or more weeks of paid leave do not exhibit a lot of variation. The share exiting employment increases evenly each quarter and after twelve months between 3 and 4% are not employed. Employees with no paid leave follow a slightly declining trend in quarterly exits until the 1998 wave, when exit rates start to rise. Conversely employees with some paid leave (1–14 days) have fairly stable exit rates in winter and spring and exhibit a steep drop in exit rates in summer and autumn quarters starting with the 1997 wave.

One reason for the strong variability might be the small sample size, more than 80% of the employees in the LFS have 4 or more weeks of paid leave, given that we only observe 1/5th of the base sample twelve months after the grouping makes the smaller groups more prone to be affected by outliers. Another reason might be that the jobs with low levels of paid leave are also more affected by general economic conditions and therefore more volatile. Finally we would expect the WTR to have an impact. The regulation came into place in autumn 1998. Comparing the autumn 1997 wave after 12 months (at the time the regulation came into place) with the autumn 1996 wave after twelve months we see virtually no difference in the exit probabilities for all groups except employees with 1–14 days of paid leave. The share of exits from this group is actually lower than before. The following year the regulation awarded four weeks of paid leave. The increase in exits are not in the group of employees with at least 3 but less than 4 weeks of paid leave however, but rather for those with less than 3 weeks, for both the group of employees with no paid leave and the 1–14 days of paid leave group the exits are slightly higher than in previous periods. For employees with 15–19 days of paid leave we see very little change across years and during the time of treatment the share of exits seems to fall rather than increase.

To check whether the quarterly variation masks the effects of the regulation we also plot the share of employees exiting employment in any of the observed quarters in figure 2.5. Employees with three or more weeks of paid leave exhibit little change over time. In contrast employees with 1–14 days of leave have declining exit probabilities from 1994 to 1997. The trend reverses in 1998, which coincides with the WTR. For employees with no leave entitlement the unconditional exit probability increases after the introduction of the WTR.

Overall the evidence suggests that treatment did not occur instantaneously in autumn 1998 but rather as a process over the following periods. We therefore choose to treat the autumn 1998 wave as untreated and assume the treatment occurred in winter 1998. Note that we have not discussed the autumn 2000 wave overly much because while some delayed treatment seems plausible (see e.g. Crépon and Kramarz 2002) it is not clear that employees who report less than the compulsory level of leave will be treated at all or that they are in any way comparable to employees that received treatment in previous periods.

2.6.1 Exit from employment

We choose to model the exit from employment as a complementary log-log (cloglog) model. The binary nature of our dependent variable makes discrete choice methods appropriate. The complementary log-log model is preferable to the usual methods (Linear probability model, Probit or Logit) since it is the discrete time representation of a proportional hazard duration model (Prentice and Gloeckler 1978). Duration models can account for right-censored spells, which is crucial given the nature of our data. Duration models estimate the hazard of a change of state, conditional on remaining in the initial state up to the point of change (Cox 1972). The (continuous time) hazard at time t is defined as the ratio of the probability density function of exit at time t divided by the probability of survival (not exiting) up to the same point in time and denoted $\theta(t)$.[12] Define T the time of exit and $P(t \leq T) = F(T)$ the probability that exit occurred at or before T then survival up to T is given by the survival function $S(T) = 1 - F(T)$.

$$\theta(t) = \frac{f(t)}{1 - F(t)} = \frac{f(t)}{S(t)} \tag{2.1}$$

We assume that the underlying continuous time hazard satisfies the proportional hazard assumption, that is the hazard can be separated into the baseline hazard (θ_0) and a covariate dependent component where the baseline hazard proportionally scales the impact of covariates.

$$\theta(t, x) = \theta_0(t) exp(x\beta) \tag{2.2}$$

Depending on the nature (discrete or continuous) of the underlying covariate, the estimated coefficients β can be interpreted in two ways.

$$\frac{\partial \ln(\theta(t, x))}{\partial x} = \beta \tag{2.3}$$

For continuous variables, the estimated coefficients give the proportional increase in the hazard to non-employment. For discrete changes we can interpret β as the proportional change in the hazard ratio.

$$\ln\left(\frac{\theta(t, x)}{\theta(t, x - 1)}\right) = \beta \tag{2.4}$$

[12] We suppress conditioning on covariates x.

For the implementation we assume that there are only two reasons for time-varying coefficients, first we allow for a flexible baseline hazard, and second the introduction of the working time regulation. Using the complementary log-log specification this defines the discrete time hazard function (used to calculate the contribution to the likelihood function) at time $t+j$ $(h_i(t+j,x))$ as

$$\begin{aligned} h_i(t+j,x) &= 1 - exp\left(-exp\left(x\beta\right)\right) \\ x\beta &= \alpha_j + \alpha_t + \beta_X X_{it} + \beta_H H_{it} + \beta_W (WTR_{t+j} * H_{it}). \end{aligned} \quad (2.5)$$

Where X_{it} is a vector of controls, α_j captures the flexible baseline, α_t time effects, H_{it} is a vector of dummies for 3 levels of paid holidays (0 days, 1–14 days and 15–19 days of paid leave) and WTR_{t+j} is the treatment indicator; it is one for employees with less than 15 days (less than 20) of paid leave from winter 1998 (1999) onwards.

In estimating this model there is a question about what to do with the data from the period after the legislation has come into force. The data from before the legislation helps identify the variation in the hazard for different levels of paid leave. After the legislation, the problem is that the observed levels of paid holiday can no longer be used as an indication of treatment in the same way as they can before the legislation. For example, if there was full compliance and coverage, one would observe zero individuals with zero days of paid holiday after the legislation, and all those who previously had 0 weeks would now have 4 weeks, thus altering the composition of that group. The simplest approach is simply to ignore the information from after the legislation—however this throws away some useful information. The evidence from section 2.5 suggests that compliance was not immediate but that employers gradually adjusted contracts. We, therefore, follow Crépon and Kramarz (2002) in our analysis and assume that untreated employees in the year(s) after the reform became belatedly eligible for more paid holidays. While Crepon and Kramarz have information for three years for every individual, we can use at most two years in our panel. We allow employees to remain untreated until 2000 and evaluate separately estimates up to $t = 1998$, $t = 1999$ and $t = 2000$

A second concern is the disparity in prior trends, which invalidates the difference-in-differences set up. Difference-in-Differences accounts for different exit probabilities for the groups but we need to assume that in absence of the treatment the changes would have been the same. We alleviate this problem by re-estimating the model with a selected sample. The selection follows the procedure outlined by Crump, Hotz, Imbens, and Mitnik (2009). The discarded observations are the least comparable individuals, therefore the remaining individuals are more likely to have similar characteristics and trends. The selection is data driven and based on the estimated propensity score, i.e. the probability of being treated. Following Crump, Hotz, Imbens, and Mitnik (2009) all observations with fitted values less than 0.1 and more than 0.9 are excluded. We estimate the propensity score with a Probit model using the same covariates as in the complementary log-log model.

$$P\left(hols_{it} < 20\right) = \Phi\left(X_{it}\beta\right) \quad (2.6)$$

Where $hols_{it}$ are the number of days of paid leave, i.e. we consider all individuals with fewer than 20 days of paid leave as treated. And we keep all individuals where

$$\hat{P}\left(hols_{it} < 20\right) \in [0.1, 0.9]. \quad (2.7)$$

Since we should not observe treatment status after the WTR was implemented, we use only data until 1996 for the estimation to avoid the issue of non-random selection of belated treatment. We then use the out-of-sample prediction of the propensity score for the sample selection. The second column of table 2.4 shows the estimate of the propensity score.

Table 2.5 reports the complementary log-log results using data from individuals who entered the sample in or before Autumn 1998, 1999 and 2000 (first through third columns). With one exception, the estimated coefficients are positive, indicating an increase in the hazard to non-employment. But none of the estimates are statistically different from zero. After the initial treatment the change in the hazard is 0 for employees with no paid leave and negative for employees with 1–14 days of paid leave.[13] The hazard increases when we extend the sample and we find a small (but not statistically significant) increase in the hazard by 10% for employees with no paid leave and employees with 15-19 days of paid leave compared to employees with 4 or more weeks of entitlement. The baseline hazard for the the first 3 months[14] is about 0.25, meaning that one in four people with base case characteristics are expected to become non-employed within these three months. A 10% increase implies a hazard of 0.275 or about 2 in 7 people exiting employment.

Running the regression on the propensity score selected sample (table 2.6) halves the sample size in each regression, but the standard errors remain comparable.[15] The estimated coefficients are slightly larger and we no longer find a negative estimate for the autumn 1998 sample. The estimates are in the same range as before, with the strongest effect being a 13.5% increase in the exit hazard for employees with no paid leave, when we consider data until 2001. However, this effect is not significant at the 5% level. Overall it seems that the reform did not have a strong, if any, impact on employees. But note, that this is the effect in the short run, since we can follow individuals for only up to 12 months.

2.6.2 Wage and other adjustments

As discussed above, we interpret the WTR as an increase in the per unit costs of labor, which should result in reduced employment. To illustrate the point we formalize this argument in a simple theoretical model.[16] We assume that a single (aggregate) good Y is consumed and that demand for this good is inelastic. Firms produce output via a concave production function (F). Concavity of the production functions means that we assume decreasing returns to scale. In addition we assume that the function is twice differentiable and takes labor (L) as its only input. The profit maximizing firm solves the following optimization problem:

$$\begin{aligned} \max_L \Pi &= pY - wL \\ \text{with} \quad & Y = F(L) \end{aligned} \tag{2.8}$$

[13]Treatment for employees with 15–19 days only took place in Autumn 1999, therefore we do not estimate a treatment effect in the first column.

[14]Estimates are exp(-1.367), exp(-1.441) and exp(-1.465) respectively.

[15]This is to be expected. As pointed out by Crump, Hotz, Imbens, and Mitnik (2009) two opposing forces are affecting the standard errors, the lower sample size leads to higher standard errors, but discarding extreme values in the propensity score reduces the variance of the estimator.

[16]See Chapter 4 in Cahuc and Zylberberg (2004) for a detailed discussion.

The first order condition shows that firms hire workers until the marginal product is equal to the real wage per unit of labor.

$$F'(L) = \frac{w}{p} \tag{2.9}$$

Where $F'(L)$ denotes the first derivative of the production function with respect to labor. To find the impact of an increase in wages, we calculate the total derivative and solve for the change in labor demand as a response to a change in wages.

$$\frac{dL}{dw} = \frac{1}{pF''(L)} \tag{2.10}$$

Since prices are positive, the sign of the second derivative of the production function $F''(L)$ determines the direction of the impact of a wage increase on employment. Since the production function is concave, the second derivative is negative, which means that an increase in wages results in lower levels of employment.

The empirical results, at least in the short run, do not seem to confirm the theoretical prediction. A reason for this might be that the model is too simplistic. Too make the model more flexible we can consider adjustments in prices, either in the nominal wage or in the output price. If wages per unit of labor were to fall to offset the increase due to reduced annual working time there would be no change in employment. Employers can adjust wages in two ways, they can reduce nominal earnings, or they can increase the number of hours for a given level of earnings. Both will result in a change in hourly wages. Alternatively if we extend the model to allow for elastic demand, i.e. the consumers demand for the final good responds to changes in prices, we can show that the change in labor demand will be a function of the output elasticity. Depending on the size of the elasticity, the impact on labor demand will be much less than in the above benchmark model. In the following we consider both these extensions.

First we consider whether employers responded to the WTR by adjusting wages. To test this we rerun the regressions from the previous section using OLS with the natural log of the hourly wage as dependent variable. Since only survey respondents in their first and last interview (wave 1 and 5) are asked about their earnings we have to restrict the sample to those employees we observe initially in autumn and then for 5 consecutive quarters. We would expect negative coefficients if wages adjusted in response to the increase in paid leave. The results in table 2.7 indicate the opposite. Relative to wages of employees with more than four weeks of paid leave, wages for employees with less than four weeks of paid leave increase with the reform. The estimated coefficients are fairly robust across the three samples and range from about 10% for employees with no paid leave to about 6% relative wage gains for employees with more than three but less than four weeks of paid leave. All estimated coefficients are highly statistically significant. Using the matched sample (table 2.8) attenuates the coefficients, but we cannot find any negative coefficients either. Note that we do not attribute the relative wage increases to the WTR. As discussed in section 2.2, a range of reforms affected employment around the time of the reform. We try to account for them explicitly, but it seems more likely that, for example, the minimum wage legislation drives the effect seen here.

The second extension we consider is a model with elastic demand, i.e we extend the above framework by modeling demand as a function of price $Y(p)$. We assume that this function is invertible, which leads to the following revised optimization problem for the firm.

$$\max_L \Pi = p(Y)Y - wL \quad (2.11)$$
$$\text{with} \quad Y = F(L)$$

By solving the maximization problem and rearranging the first order condition we can show that the real wage is now below the marginal product.

$$F'(L) = \frac{1}{1+\eta}\frac{w}{p} \quad (2.12)$$

Where $\eta = \frac{\partial p(Y)}{\partial Y}\frac{Y}{p(Y)}$ is the elasticity of the inverse demand function with respect to output. Assuming that η is independent of Y and taking the derivative with respect to w yields the following expression:

$$\frac{\partial L}{\partial w} = \frac{1+\eta}{F'(L)^2 p'(Y) + p(Y)F''(L)} \quad (2.13)$$

The denominator is negative under standard assumptions, the numerator is positive for η between 0 and -1 and approaches 0 if (inverse) demand is very elastic, i.e. η is close to -1. To understand this result, we can consider a rearranged version of equation 2.12.

$$pF'(L) = \frac{1}{1+\eta}w \quad (2.14)$$

The left hand side is the marginal revenue for an additional unit of labor, the right hand side is the marginal cost (w) scaled by the the markup factor $\frac{1}{1+\eta}$. The smaller η the higher the markup and therefore the profit. If wages rise, the impact on labor demand is attenuated by a reduction in firms profits. While a demand elasticity close to -1 would explain our findings, it seems a rather large value given that we consider the majority of private sector industries. However, Draca, Machin, and Van Reenen (2011) present evidence that supports a profit based explanation. They evaluate the impact of the UK minimum wage on firm profits and find substantial reductions in firm profitability. Intuitively, if a job yields a certain rent, it is always preferable to reduce the rent and still make some profit, rather then to dissolve the job match and lose all rents.

There might be other mitigating factors. In the short-run employers might save on indirect costs of employment, e.g. by reducing job related training. On the one hand training employees means costs for the employer, on the other there might be an incentive to improve skills when wages increase (see Acemoglu and Pischke 1999). We consider job related training or education that employees received. Figure 2.7 depicts the share of respondents who received training in the past 3 months for any of the 4 quarters following autumn of a given year. We see that the share of employees who received job related education is very high among the employees with generous amounts of paid holidays. But even among employees with worse jobs the share is about 15%. The introduction of the WTR does not seem to change the trends in training. If anything, there was a slight upward trend for the jobs in the three categories with fewer than 20 days of paid holidays.

Another mitigating factor is that there is no statutory right for paid public or bank holidays (Bryan 2006) and the LFS question on holiday entitlement explicitly excludes public or bank holidays. From 1999 onward the survey is augmented by the question on which public or bank holidays the respondent worked. More than 70% of the employees that report no paid holidays in 1999 (and thereafter) also reported that they did not work on any public/bank holiday. Since data on public/bank holidays is only available post reform and the group of employees with "no paid holidays" in our sample should only (but does not) consist of employees with less than 3 months of tenure, this can only hint at the situation prior to the reform, but it seems that most employees were eligible for paid or unpaid leave at public and bank holidays. Which would attenuate the increase in real wage.

A final explanation might be that the effect is actually driven by the Unfair Dismissal Order. As discussed in section 2.3, employees with less than two years of tenure were less likely to lose their jobs after the dismissal legislation was changed. To check whether this is the case we exclude those employees in tables 2.9 and 2.10. The first table replicates the complementary log-log estimation for non-employment, the second the estimation for (log) hourly wages. In both tables the first three columns are for the whole sample, the last three columns select the sample based on the propensity score as described above. Again we find that the WTR did not increase the hazard to non-employment and wages increased for treated employees compared to employees who already had 4 weeks or more of paid holiday entitlement.

2.6.3 Effect on total employment

As explained in section 2.4, paid holiday entitlement below the WTR threshold should not be observed post-treatment. In addition it is impossible to know the leave entitlement for non-employees. The short-term panel nature of our data does not allow us to impute estimates for paid leave at the individual level either. We therefore estimate the propensity to be treated based on a flexible function of gender, age (quartic), highest qualification obtained, region of residence, non-uk nationality, marital status and presence of children. We estimate the treatment probability with a Probit on data from 1994–1996. We use the results to predict the propensity score for all individuals until 2004. The first stage results are reported in table 2.11.

We then use the imputed values in the second stage as treatment indicator in a difference-in-differences regression. We use OLS for the second stage and bootstrap the standard errors. For the standard errors we draw 400 samples with replacement from 120 region×education clusters and estimate both the first and the second stage on the bootstrap sample. Table 2.12 reports the second stage results, where the columns differ in the way we control for time effects. In the first and third column we use a dummy to control for time trends before and after the reform[17], in the second and fourth column we use a linear trend.

$$\begin{aligned} y_{it} &= \beta_0 + \beta_1 * \hat{p}_i + \beta_2 * d_t + \beta_3 * \hat{p}_i * d_t + e_{it} \\ y_{it} &= \beta_0 + \beta_1 * \hat{p}_i + \beta_2 * t + \beta_3 * \hat{p}_i * d_t * t + e_{it} \end{aligned}$$

With y being the employment status of individual i in quarter t, $\hat{p}$ the imputed propensity score and d a dummy that is 0 in and before autumn 1998 and 1 afterwards.

[17] We also used dummies for each year×quarter, but the results are the same as with a single post-treatment dummy.

We find a negative and statistically significant coefficient on our treatment indicator in all specifications. Employment decreases in the post treatment period by, on average, 2 percentage points when we consider treatment for all types of employees (I). Specification (II) indicates that the effect was not immediate but that it emerged gradually. The first panel of figure 2.8 corroborates this finding. Here we interact the treatment indicator with dummy variables for each post-treatment period. The declining trend in employment is evident, but also high levels of seasonal variation. Few coefficients are (individually) significantly different from zero.

So far we have considered all employees with less than four weeks of paid leave as treated. For better comparison with the results for the employed, specifications (III) and (IV) in table 2.12 and figure 2.9 replicate the exercise but consider only employees with less than four weeks of paid holidays who are also working 5 or more days per week as treated. Here we find a stronger effect of the reform, with an estimated overall decrease of 4 percentage points in employment. Again we see strong seasonal variation and a declining trend rather than an ad-hoc decrease around the time of the treatment.

Causal interpretation in difference-in-differences estimation hinges on the assumption that treatment and control would have followed the same trend without the treatment. The lower panels in figure 2.8 and 2.9 show the estimated coefficients without constraining the treatment to the post WTR period. We can see that even before the treatment took place there is seasonal and annual variation with the treatment intensity. While this casts doubt on the parallel trends assumption, it is still apparent, that there is a trend reversal coinciding with the introduction of the WTR.

Combining the results, we should, on the one hand, be cautious about the validity of the difference-in-differences strategy, but, on the other, it seems that there was a relative decline in employment post-autumn 1998 for the more treated groups. Given the wealth of reforms at the end of the 1990s (see section 2.2) and potential correlation among the treated groups (e.g. the minimum wage and the WTR are likely to affect the same individuals), it is hard to attribute these changes solely to the WTR. Nonetheless it is interesting that the change in labor market outlook coincided with the treatment.

2.7 Summary and conclusion

We set out to find the impact of a mandatory minimum entitlement to paid leave on employment. The Working Time Regulation 1998 introduced such an entitlement in the UK in Autumn 1998. We consider the impact first on employees and second on the labor force as a whole. Since the legislation has no regional variation, we rely on employees above the entitlement threshold as control group.

For employees, we use complementary log-log regression and matching to account for the structure of the data and concerns about selecting a proper control group. While the statutory right to paid leave constitutes a large change in the real wage for some employees, we cannot find a significant impact on their chance to remain employed. But due to data constraints we are limited to estimating only short-run effects.

Since treatment and control group can no longer be distinguished once the

WTR1998 is introduced, we use out-of-sample propensity score predictions as treatment indicator when estimating the impact on the labor force. Here we find a significant decline in employment. The decline is not immediate but rather incrementally increasing over time.

This would be in line with employees not being fired outright, but rather not being replaced when an employee leaves.

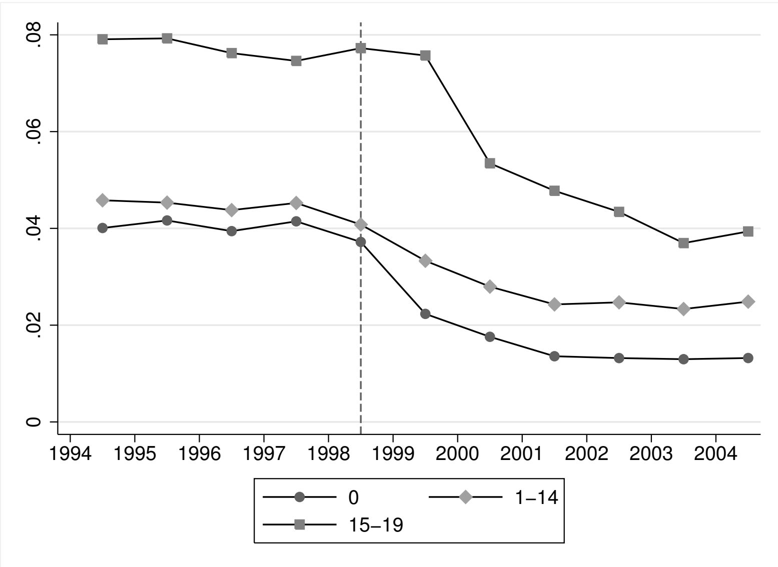
An interesting question that cannot be addressed with the available data is whether paid leave actually increases productivity. Ideal for this would be firm-level data on individual employee productivity, e.g. supermarket cashiers as used in the study by Mas and Moretti (2009), and holiday absences.

2.8 Appendix

Figures

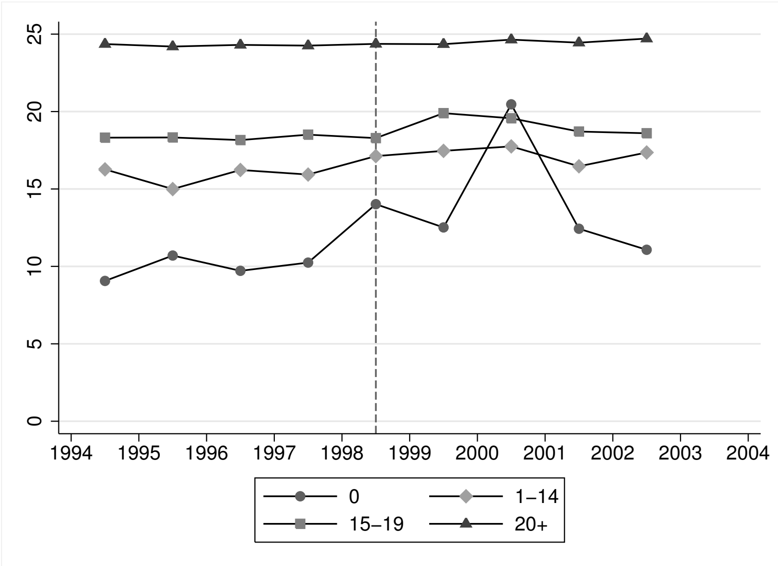
Footnote for all figures:
Source: Labour Force Survey 1994–2001, see text for data description.

Figure 2.1: Share of employees with less than 20 days of paid leave (split by group)



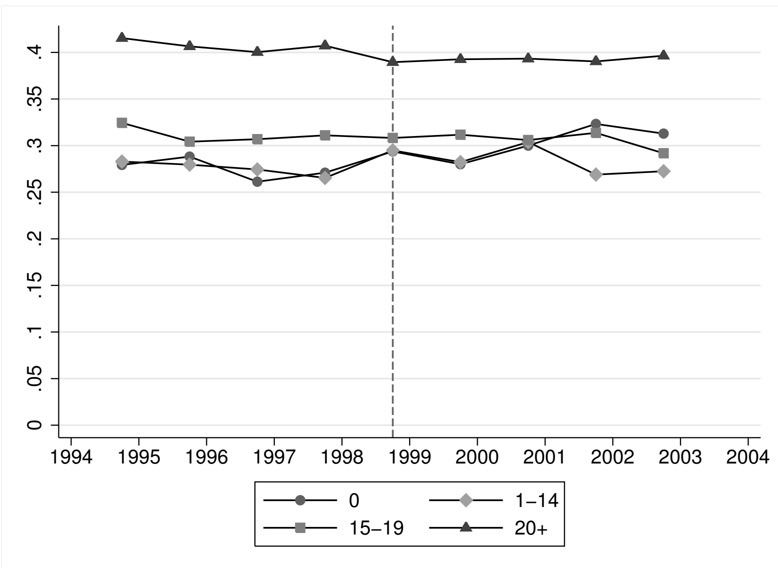
Data source: UK Labour Force Survey

Figure 2.2: Average number of days of paid leave for employees within each group 12 months later



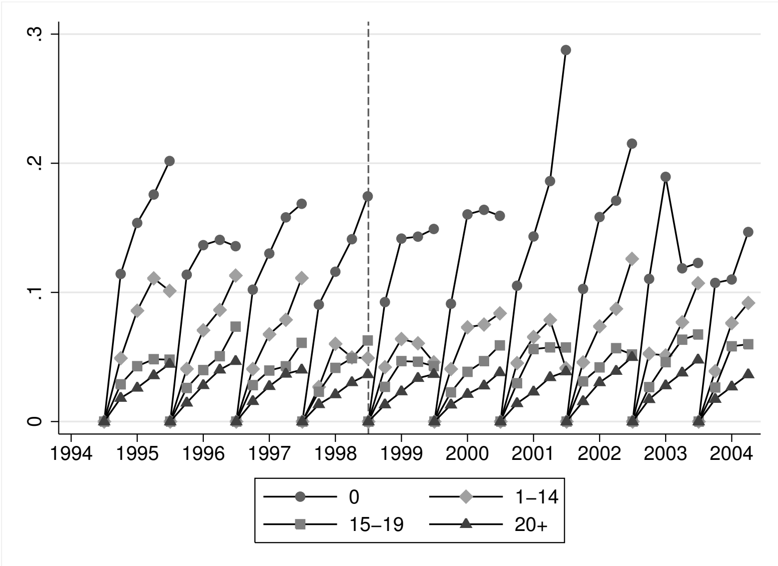
Data source: UK Labour Force Survey

Figure 2.3: Share of employees on vacation during the interview week in at least one of the following four quarters



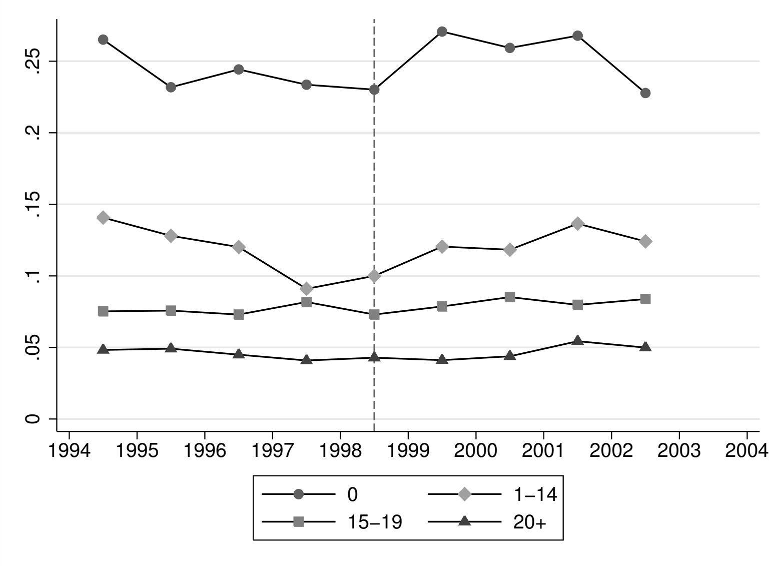
Data source: UK Labour Force Survey Observations weighted by number of quarters observed.

Figure 2.4: Share of employees exiting into non-employment (unemployed, inactive, self-employed)



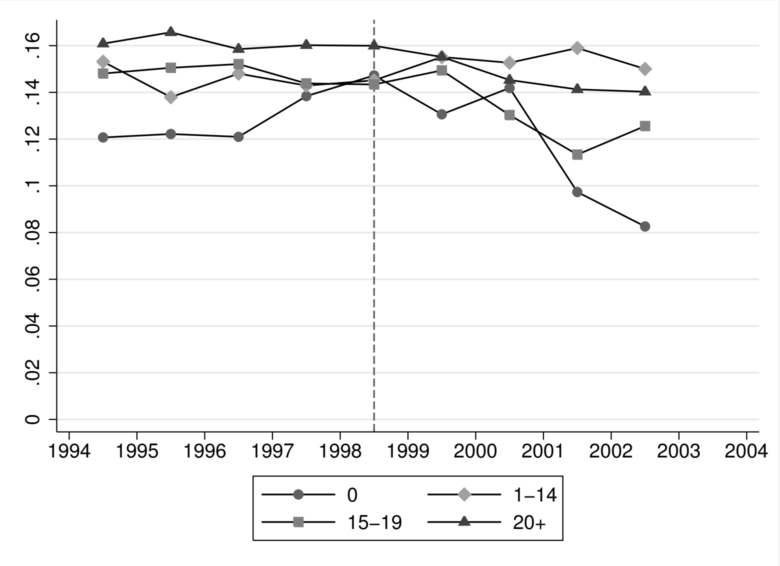
Data source: UK Labour Force Survey

Figure 2.5: Share of employees exiting into non-employment at least once in the next 4 quarters.



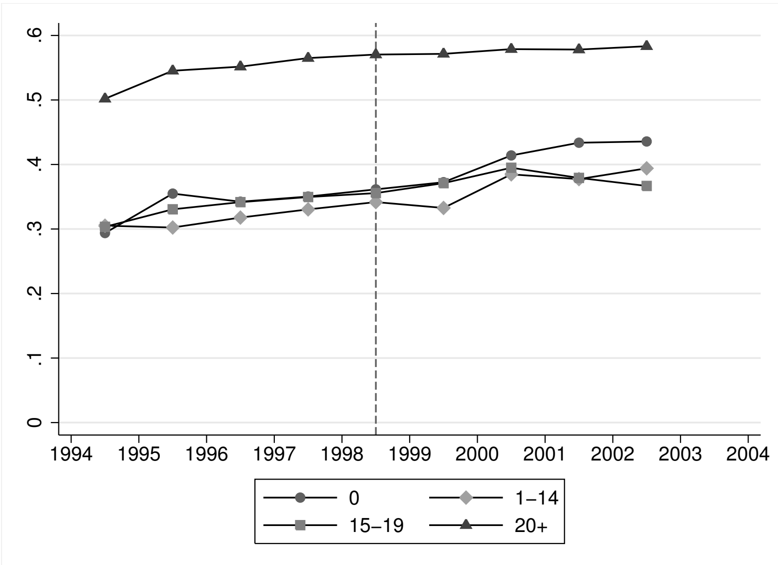
Data source: UK Labour Force Survey Observations weighted by number of quarters observed.

Figure 2.6: Share of employees that missed work due to illness in one of the interview weeks



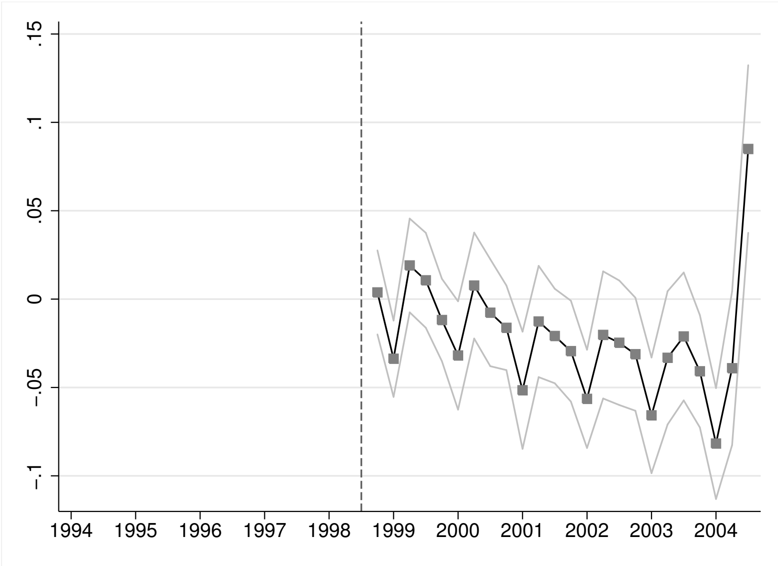
Data source: UK Labour Force Survey Observations weighted by number of quarters observed.

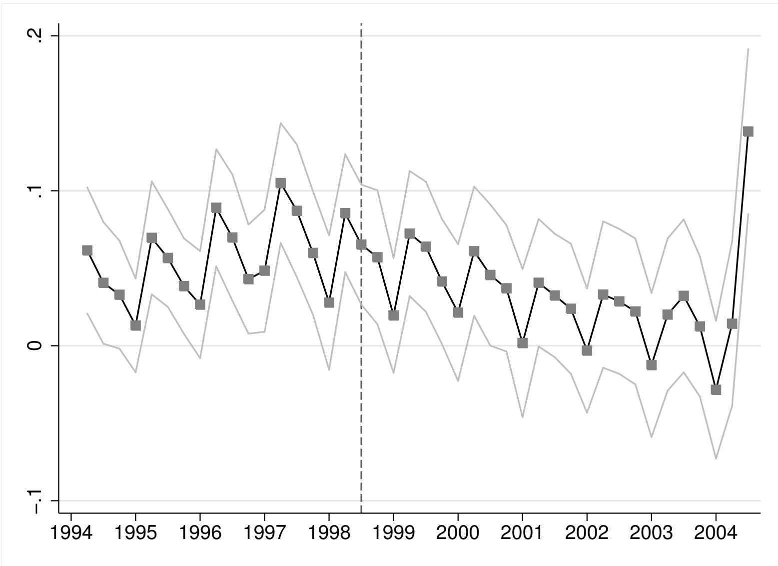
Figure 2.7: Share of employees that received training in the past 13 weeks in at least one of the next four quarters



Data source: UK Labour Force Survey Observations weighted by number of quarters observed.

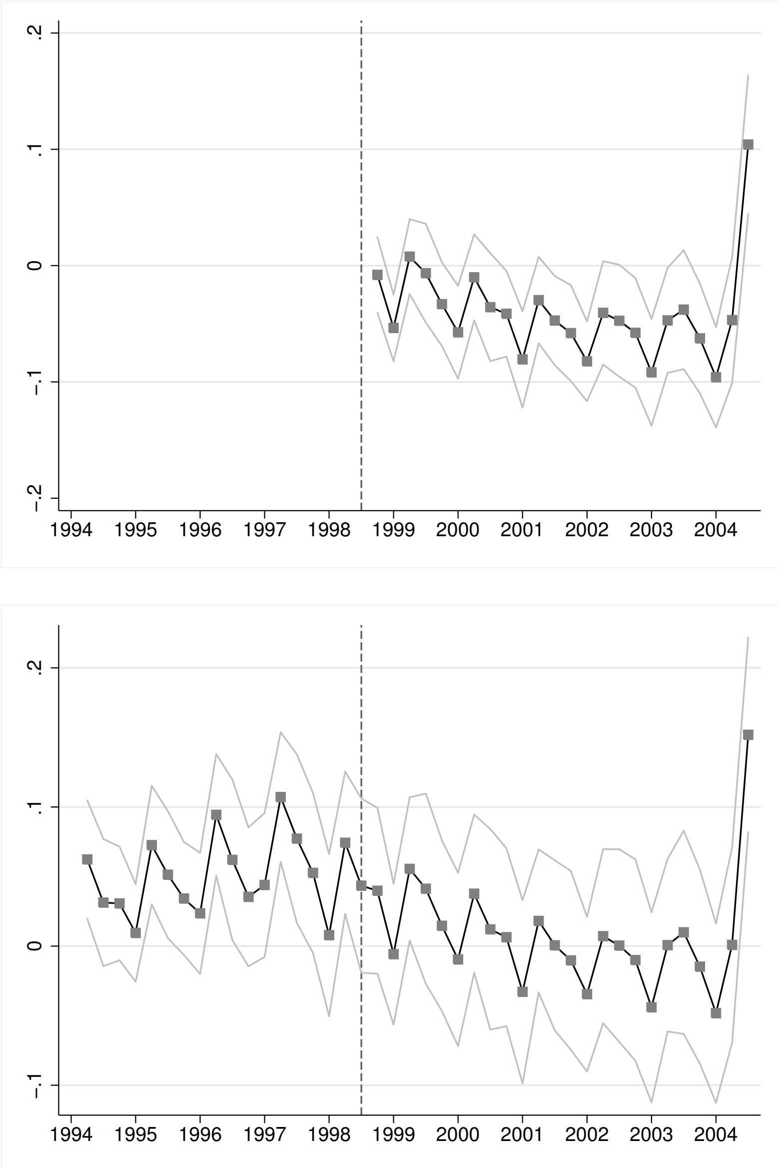
Figure 2.8: Estimated treatment effect on employment





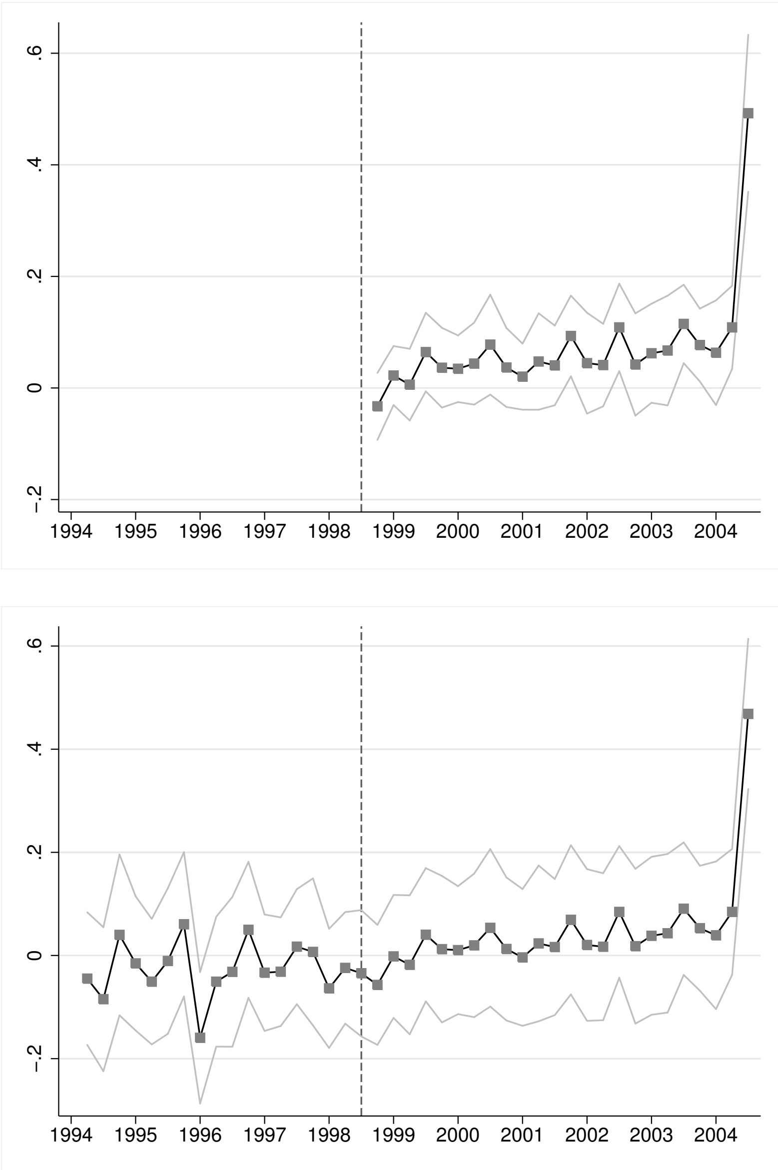
Data source: UK Labour Force Survey
Dependent variable: employed (second stage); less than 4 weeks of paid leave (first stage).
Pointwise confidence intervals based on 400 bootstrap repetitions (clustered at the region×education level).

Figure 2.9: Estimated treatment effect on full-time equivalent employment



Data source: UK Labour Force Survey
Dependent variable: employed (second stage); less than 4 weeks of paid leave and working 5 or more days per week (first stage).
Pointwise confidence intervals based on 400 bootstrap repetitions (clustered at the region×education level).

Figure 2.10: Estimated treatment effect on log hourly wages



Data source: UK Labour Force Survey
Dependent variable: log hourly wages (second stage); less than 4 weeks of paid leave (first stage).
Pointwise confidence intervals based on 400 bootstrap repetitions (clustered at the region×education level).

Tables

Table 2.1: Average usual hours worked and share of employees with more than 48 hours per week.

	All employees		5+ days employees	
Year	Avg. hours	More than 48	Avg. hours	More than 48
1994	37.531	0.155	41.027	0.182
1995	37.542	0.158	41.194	0.188
1996	37.423	0.160	41.222	0.190
1997	37.600	0.164	41.385	0.195
1998	37.575	0.161	41.334	0.190
1999	37.371	0.150	41.085	0.179
2000	37.316	0.148	41.092	0.178
2001	37.308	0.146	40.998	0.177
2002	37.134	0.141	40.881	0.170
2003	36.923	0.135	40.796	0.166
2004	36.551	0.127	40.492	0.154

Data sourse: UK Labour Force Survey

Table 2.2: National minimum wage level

Starting from	Adult workers (22 and older)	Young workers (18 to 22)
April 1999	3.60	3.00
June 2000	—	3.20
October 2000	3.70	—
October 2001	4.10	3.50
October 2002	4.20	3.60
October 2003	4.50	3.80
October 2004	4.85	4.10

Source: Low Pay Commission

Table 2.3: Descriptive statistics (mean and s.d.) for variables used in the complementary log-log regression.

Variable	1996	1997	1998	1999	2000
Exits employment w/in 12 months	0.050	0.047	0.047	0.045	0.044
	(0.217)	(0.211)	(0.211)	(0.207)	(0.206)
No paid holidays	0.039	0.041	0.037	0.022	0.018
	(0.193)	(0.199)	(0.189)	(0.147)	(0.134)
1–14 days of paid holidays	0.044	0.045	0.041	0.034	0.029
	(0.206)	(0.208)	(0.198)	(0.182)	(0.168)
15–20 days of paid holidays	0.077	0.075	0.079	0.078	0.056
	(0.267)	(0.263)	(0.270)	(0.268)	(0.231)
personal characteristics					
Number of quarters in sample	3.607	3.621	3.629	3.625	3.612
	(1.180)	(1.181)	(1.179)	(1.178)	(1.178)
Female	0.417	0.413	0.411	0.417	0.412
	(0.493)	(0.492)	(0.492)	(0.493)	(0.492)
Age	38.849	39.169	39.364	39.451	39.949
	(11.205)	(11.106)	(11.153)	(11.159)	(11.099)
Married	0.632	0.625	0.614	0.606	0.605
	(0.482)	(0.484)	(0.487)	(0.489)	(0.489)
Foreign	0.031	0.033	0.037	0.037	0.038
	(0.172)	(0.180)	(0.189)	(0.190)	(0.192)
Has kid(s)	0.410	0.407	0.401	0.403	0.409
	(0.492)	(0.491)	(0.490)	(0.490)	(0.492)
education					
1 Degree or equivalent	0.129	0.135	0.144	0.152	0.163
	(0.335)	(0.341)	(0.351)	(0.359)	(0.369)
2 Higher Education	0.096	0.096	0.102	0.103	0.102
	(0.295)	(0.295)	(0.303)	(0.303)	(0.302)
3 GCE A Level or equivalent	0.260	0.266	0.259	0.264	0.265
	(0.438)	(0.442)	(0.438)	(0.441)	(0.441)
4 GCSE A*-C or equivalent	0.224	0.225	0.222	0.226	0.223
	(0.417)	(0.418)	(0.416)	(0.418)	(0.417)
5 Other qualification	0.143	0.144	0.144	0.134	0.134
	(0.350)	(0.351)	(0.351)	(0.340)	(0.340)
6 No qualification	0.144	0.128	0.122	0.115	0.109
	(0.351)	(0.334)	(0.327)	(0.319)	(0.312)
7 No response on qualification	0.004	0.005	0.006	0.007	0.004
	(0.063)	(0.073)	(0.077)	(0.081)	(0.064)
Recognized trade apprenticeship	0.005	0.005	0.006	0.006	0.006
	(0.073)	(0.070)	(0.078)	(0.075)	(0.074)
job characteristics					
Tenure in years	8.824	8.726	8.644	8.674	8.798
	(8.414)	(8.455)	(8.436)	(8.585)	(8.726)
Below min. wage 3 months ahead	0.000	0.000	0.000	0.038	0.032
	(0.000)	(0.000)	(0.000)	(0.192)	(0.177)
Below min. wage 6 months ahead	0.000	0.000	0.037	0.038	0.032
	(0.000)	(0.000)	(0.190)	(0.192)	(0.177)
Below min. wage 9 months ahead	0.000	0.000	0.056	0.039	0.032
	(0.000)	(0.000)	(0.230)	(0.194)	(0.177)
Below min. wage 12 months ahead	0.000	0.000	0.056	0.044	0.051
	(0.000)	(0.000)	(0.230)	(0.204)	(0.221)
Works 48+ hours	1.598	1.604	1.529	1.426	1.405
	(4.879)	(4.799)	(4.589)	(4.339)	(4.416)
Full-time	0.891	0.891	0.895	0.896	0.896

Continued on next page...

... table 2.3 continued

Variable	1996	1997	1998	1999	2000
	(0.312)	(0.311)	(0.306)	(0.305)	(0.305)
Part-time	0.109	0.108	0.105	0.104	0.104
	(0.312)	(0.311)	(0.306)	(0.305)	(0.305)
Missing ft/pt	0.000	0.000	0.000	0.000	0.000
	(0.006)	(0.006)	(0.000)	(0.000)	(0.000)
Private company	0.762	0.768	0.778	0.777	0.773
	(0.426)	(0.422)	(0.416)	(0.417)	(0.419)
Managerial duties at work	0.437	0.430	0.433	0.429	0.437
	(0.496)	(0.495)	(0.496)	(0.495)	(0.496)
Ever work overtime	0.580	0.591	0.566	0.558	0.562
	(0.494)	(0.492)	(0.496)	(0.497)	(0.496)
type of work arrangement					
1 Flexible working hours	0.120	0.126	0.122	0.122	0.131
	(0.325)	(0.331)	(0.327)	(0.328)	(0.337)
2 Annualized hours contract	0.031	0.026	0.027	0.028	0.039
	(0.174)	(0.160)	(0.161)	(0.165)	(0.195)
3 Term time working	0.006	0.006	0.005	0.005	0.006
	(0.075)	(0.075)	(0.074)	(0.071)	(0.074)
4 Job sharing	0.002	0.002	0.002	0.002	0.003
	(0.049)	(0.045)	(0.047)	(0.049)	(0.052)
5 Nine day fortnight	0.005	0.004	0.004	0.004	0.005
	(0.069)	(0.066)	(0.064)	(0.063)	(0.069)
6 Four and a half day week	0.032	0.028	0.029	0.027	0.022
	(0.175)	(0.166)	(0.167)	(0.161)	(0.147)
7 Zero hours contract	0.800	0.802	0.806	0.806	0.793
	(0.400)	(0.399)	(0.396)	(0.395)	(0.405)
8 Missing work arrangement	0.004	0.006	0.005	0.005	0.001
	(0.065)	(0.077)	(0.073)	(0.073)	(0.039)
Union member	0.342	0.337	0.323	0.320	0.316
	(0.474)	(0.473)	(0.468)	(0.466)	(0.465)
5 day work week	0.830	0.828	0.839	0.837	0.846
	(0.376)	(0.378)	(0.368)	(0.369)	(0.361)
6 day work week	0.123	0.124	0.116	0.112	0.101
	(0.328)	(0.329)	(0.321)	(0.315)	(0.301)
7 day work week	0.039	0.041	0.037	0.043	0.044
	(0.194)	(0.198)	(0.188)	(0.202)	(0.204)
region					
1 Tyne	0.021	0.021	0.020	0.020	0.021
	(0.143)	(0.142)	(0.140)	(0.140)	(0.143)
2 Rest of Northern region	0.035	0.034	0.030	0.030	0.032
	(0.184)	(0.181)	(0.172)	(0.172)	(0.175)
3 South Yorkshire	0.020	0.022	0.022	0.021	0.020
	(0.141)	(0.146)	(0.146)	(0.142)	(0.141)
4 West Yorkshire	0.039	0.036	0.038	0.038	0.042
	(0.195)	(0.186)	(0.191)	(0.191)	(0.200)
5 Rest of Yorks	0.026	0.027	0.027	0.029	0.028
	(0.160)	(0.161)	(0.163)	(0.167)	(0.166)
6 East Midlands	0.070	0.071	0.071	0.069	0.071
	(0.255)	(0.257)	(0.256)	(0.253)	(0.256)
7 East Anglia	0.038	0.040	0.040	0.039	0.043
	(0.192)	(0.196)	(0.196)	(0.194)	(0.202)
8 Central London	0.044	0.044	0.047	0.047	0.044
	(0.206)	(0.205)	(0.211)	(0.211)	(0.206)
9 Inner London	0.028	0.029	0.027	0.027	0.030
	(0.166)	(0.168)	(0.163)	(0.163)	(0.169)
10 Outer London	0.049	0.051	0.051	0.054	0.046
	(0.215)	(0.219)	(0.221)	(0.226)	(0.210)

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... table 2.3 continued

Variable	1996	1997	1998	1999	2000
11 Rest of South East	0.174	0.181	0.176	0.178	0.183
	(0.379)	(0.385)	(0.381)	(0.382)	(0.386)
12 South West	0.076	0.077	0.081	0.082	0.085
	(0.266)	(0.267)	(0.272)	(0.275)	(0.279)
13 West of Midlands	0.053	0.052	0.050	0.046	0.044
	(0.224)	(0.222)	(0.219)	(0.210)	(0.205)
14 Rest of Midlands	0.043	0.046	0.046	0.045	0.045
	(0.203)	(0.208)	(0.209)	(0.207)	(0.207)
15 Greater Manchester	0.044	0.042	0.042	0.043	0.041
	(0.204)	(0.201)	(0.200)	(0.204)	(0.198)
16 Merseyside	0.019	0.020	0.018	0.018	0.018
	(0.137)	(0.139)	(0.133)	(0.132)	(0.132)
17 Rest of North West	0.045	0.044	0.040	0.040	0.037
	(0.207)	(0.205)	(0.195)	(0.196)	(0.189)
18 Wales	0.045	0.044	0.043	0.042	0.041
	(0.207)	(0.206)	(0.202)	(0.201)	(0.197)
19 Strathclyde	0.037	0.041	0.040	0.037	0.036
	(0.190)	(0.197)	(0.196)	(0.190)	(0.186)
20 Rest of Scotland	0.059	0.058	0.058	0.058	0.059
	(0.235)	(0.234)	(0.234)	(0.233)	(0.235)
21 Northern Ireland	0.032	0.020	0.032	0.034	0.035
	(0.175)	(0.141)	(0.176)	(0.182)	(0.184)
industry					
1 Agriculture, hunting and forestry	0.011	0.010	0.011	0.010	0.010
	(0.103)	(0.101)	(0.102)	(0.102)	(0.099)
2 Mining, quarrying	0.006	0.006	0.006	0.005	0.006
	(0.080)	(0.078)	(0.079)	(0.074)	(0.077)
3 Manufacturing	0.277	0.263	0.257	0.247	0.240
	(0.448)	(0.440)	(0.437)	(0.431)	(0.427)
4 Electricity, gas and water supply	0.011	0.011	0.010	0.011	0.011
	(0.104)	(0.106)	(0.102)	(0.105)	(0.104)
5 Construction	0.054	0.062	0.065	0.066	0.066
	(0.227)	(0.241)	(0.246)	(0.248)	(0.249)
6 Wholesale, retail and motor trade	0.146	0.145	0.145	0.148	0.141
	(0.354)	(0.352)	(0.352)	(0.355)	(0.348)
7 Hotels and restaurants	0.032	0.031	0.031	0.029	0.029
	(0.177)	(0.173)	(0.174)	(0.168)	(0.169)
8 Transport, storage and communications	0.049	0.054	0.054	0.056	0.058
	(0.216)	(0.225)	(0.225)	(0.229)	(0.234)
9 Financial intermediation	0.063	0.062	0.061	0.060	0.063
	(0.243)	(0.241)	(0.239)	(0.238)	(0.243)
10 Real estate and business activities	0.101	0.107	0.110	0.115	0.119
	(0.302)	(0.309)	(0.313)	(0.319)	(0.324)
11 Public adminstration and defence	0.089	0.086	0.085	0.089	0.095
	(0.284)	(0.280)	(0.279)	(0.285)	(0.293)
12 Health and social work	0.113	0.116	0.117	0.117	0.114
	(0.316)	(0.321)	(0.322)	(0.322)	(0.318)
13 Other community, social and personal	0.045	0.045	0.046	0.045	0.046
	(0.206)	(0.207)	(0.209)	(0.206)	(0.209)
14 Private hhs with empl. persons	0.002	0.002	0.002	0.002	0.001
	(0.046)	(0.044)	(0.040)	(0.043)	(0.037)
15 Industry missing	0.000	0.000	0.000	0.000	0.000
	(0.021)	(0.021)	(0.012)	(0.015)	(0.016)
occupation					
1 Managers and Senior Officials	0.181	0.182	0.185	0.183	0.192
	(0.385)	(0.386)	(0.388)	(0.387)	(0.393)
2 Professional Occupations	0.088	0.090	0.094	0.097	0.098

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... table 2.3 continued

Variable	1996	1997	1998	1999	2000
	(0.284)	(0.286)	(0.291)	(0.296)	(0.297)
3 Associate Professional and Tech, Occ.	0.119	0.119	0.117	0.120	0.125
	(0.324)	(0.323)	(0.321)	(0.325)	(0.330)
4 Administrative and Secretarial Occ.	0.172	0.168	0.167	0.169	0.163
	(0.377)	(0.374)	(0.373)	(0.375)	(0.369)
5 Skilled Trades Occ.	0.128	0.126	0.128	0.124	0.125
	(0.334)	(0.332)	(0.334)	(0.330)	(0.331)
6 Personal Service Occ.	0.039	0.041	0.041	0.045	0.042
	(0.194)	(0.199)	(0.198)	(0.207)	(0.202)
7 Sales and Customer Service Occ.	0.046	0.047	0.048	0.050	0.048
	(0.209)	(0.212)	(0.214)	(0.219)	(0.214)
8 Process, Plant and Machine Operatives	0.106	0.107	0.104	0.098	0.095
	(0.307)	(0.309)	(0.306)	(0.297)	(0.294)
9 Elementary Occ.	0.121	0.119	0.116	0.114	0.112
	(0.326)	(0.323)	(0.320)	(0.318)	(0.316)
10 Occ. missing	0.001	0.001	0.000	0.000	0.000
	(0.028)	(0.025)	(0.006)	(0.008)	(0.000)
firm size					
11–19 employees	0.085	0.087	0.080	0.083	0.082
	(0.278)	(0.282)	(0.272)	(0.276)	(0.275)
20–24 employees	0.040	0.039	0.037	0.038	0.038
	(0.196)	(0.194)	(0.189)	(0.191)	(0.190)
Less than 25 employees	0.008	0.009	0.009	0.012	0.012
	(0.087)	(0.093)	(0.092)	(0.107)	(0.110)
25–49 employees	0.114	0.114	0.114	0.114	0.112
	(0.318)	(0.317)	(0.318)	(0.318)	(0.315)
50 or more employees/don't know but over 24	0.590	0.585	0.588	0.587	0.593
	(0.492)	(0.493)	(0.492)	(0.492)	(0.491)
Missing number of employees	0.001	0.001	0.001	0.001	0.001
	(0.033)	(0.026)	(0.031)	(0.031)	(0.035)
N	29,789	28,855	29,286	27,990	26,130

Table reports the mean and standard deviation (in parenthesis) for
(first panel) exit from employment (the dependent variable)
(second panel) paid holidays (main regressors of interest)
(third panel) all other controls used in the regressions.
The numbers are for the Autumn quarter only.
All statistics were calculated using sampling weights.
Data source: UK Labour Force Survey

Table 2.4: Characteristics of treated units and propensity score

	OLS full sample	Probit 1994–1996
year = 1995	0.001 (0.003)	
year = 1996	-0.003 (0.003)	
year = 1997	-0.002 (0.004)	
year = 1998	-0.008* (0.004)	
year = 1999	-0.032** (0.004)	
year = 2000	-0.062** (0.007)	
year = 2001	-0.080** (0.009)	
year = 2002	-0.084** (0.009)	
year = 2003	-0.090** (0.010)	
year = 2004	-0.116** (0.015)	
female	-0.003 (0.004)	-0.018 (0.023)
age	-0.013** (0.001)	-0.055** (0.005)
age squared	0.000** (0.000)	0.001** (0.000)
tenure	-0.010** (0.001)	-0.070** (0.004)
tenure squared	0.000** (0.000)	0.001** (0.000)
48+ hours	0.001* (0.000)	0.005** (0.002)
full-time	0.161** (0.015)	0.624** (0.043)
private company	-0.081** (0.016)	-0.537** (0.080)
married	-0.023** (0.003)	-0.121** (0.016)
foreign	0.038** (0.004)	0.195** (0.031)
has kid(s)	0.027** (0.003)	0.115** (0.019)
manager duties	-0.010** (0.003)	-0.103** (0.019)
education = 1	-0.017** (0.005)	-0.227** (0.041)
education = 2	0.001 (0.005)	-0.062 (0.040)
education = 4	0.002 (0.003)	-0.014 (0.019)
education = 5	0.015** (0.004)	0.057* (0.024)
education = 6	0.046** (0.005)	0.206** (0.023)
education = 7	0.057** (0.013)	0.242* (0.104)

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... table 2.4 continued

	OLS full sample	Probit 1994–1996
apprenticeship	-0.011	-0.091
	(0.022)	(0.111)
ever overtime	-0.049**	-0.275**
	(0.004)	(0.021)
industry = 1	0.061**	0.257**
	(0.018)	(0.069)
industry = 2	0.084**	0.385**
	(0.012)	(0.052)
industry = 4	-0.002	-0.147**
	(0.012)	(0.049)
industry = 5	0.069**	0.251**
	(0.007)	(0.035)
industry = 6	0.016	0.113*
	(0.008)	(0.050)
industry = 7	0.075**	0.270**
	(0.023)	(0.056)
industry = 8	-0.007	0.045
	(0.012)	(0.049)
industry = 9	-0.017*	-0.178**
	(0.008)	(0.066)
industry = 10	0.036	0.255**
	(0.021)	(0.097)
industry = 11	0.051*	0.064
	(0.020)	(0.102)
industry = 12	0.032*	0.139
	(0.015)	(0.075)
industry = 13	0.053**	0.281**
	(0.013)	(0.043)
industry = 14	0.148**	0.298
	(0.026)	(0.189)
industry = 15	0.031**	-0.121**
	(0.006)	(0.027)
work arrangement = 1	0.006	-0.038
	(0.004)	(0.028)
work arrangement = 2	-0.028**	-0.171**
	(0.004)	(0.034)
work arrangement = 3	0.126**	0.420*
	(0.040)	(0.174)
work arrangement = 4	0.040	0.250
	(0.034)	(0.158)
work arrangement = 5	0.008	-0.062
	(0.012)	(0.109)
work arrangement = 6	0.007	0.083*
	(0.005)	(0.037)
work arrangement = 8	0.021*	0.181
	(0.010)	(0.122)
union member	-0.061**	-0.421**
	(0.007)	(0.025)
6 days per week	0.052**	0.241**
	(0.008)	(0.030)
7 days per week	0.076**	0.394**
	(0.010)	(0.047)
region = 1	-0.002	0.007
	(0.006)	(0.058)
region = 2	0.010	0.109**
	(0.006)	(0.041)
region = 3	0.020**	0.234**
	(0.007)	(0.044)
region = 4	0.013*	0.114**

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... table 2.4 continued

	OLS full sample	Probit 1994–1996
	(0.005)	(0.034)
region = 5	-0.009	0.029
	(0.005)	(0.041)
region = 6	0.015**	0.126**
	(0.003)	(0.030)
region = 7	-0.001	-0.002
	(0.003)	(0.035)
region = 8	-0.023**	-0.254**
	(0.005)	(0.047)
region = 9	0.014*	0.077
	(0.006)	(0.046)
region = 11	0.013**	0.083*
	(0.004)	(0.034)
region = 13	0.008**	0.057*
	(0.003)	(0.028)
region = 14	0.026**	0.151**
	(0.004)	(0.027)
region = 15	0.013**	0.052
	(0.004)	(0.038)
region = 16	0.013**	0.143**
	(0.004)	(0.039)
region = 17	0.012**	0.088*
	(0.004)	(0.043)
region = 18	0.010**	0.117**
	(0.004)	(0.033)
region = 19	0.043**	0.290**
	(0.005)	(0.032)
region = 20	0.005	0.061
	(0.005)	(0.038)
region = 21	-0.014*	-0.064
	(0.005)	(0.034)
region = 22	0.054**	0.430**
	(0.008)	(0.045)
occupation = 2	-0.001	-0.066
	(0.011)	(0.066)
occupation = 3	0.012	0.020
	(0.011)	(0.062)
occupation = 4	-0.005	0.018
	(0.009)	(0.053)
occupation = 5	0.079**	0.388**
	(0.010)	(0.056)
occupation = 6	0.085**	0.542**
	(0.017)	(0.066)
occupation = 7	0.024**	0.119
	(0.009)	(0.066)
occupation = 8	0.089**	0.449**
	(0.008)	(0.050)
occupation = 9	0.100**	0.446**
	(0.022)	(0.088)
occupation = 10	0.016*	0.042
	(0.008)	(0.046)
11–19 employees	0.018**	0.070*
	(0.006)	(0.035)
20–24 employees	0.011	0.051
	(0.006)	(0.032)
less than 25	0.062**	0.327**
	(0.011)	(0.078)
25–49 employees	-0.011	-0.042
	(0.006)	(0.034)

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... table 2.4 continued

	OLS full sample	Probit 1994–1996
missing # employees	0.071**	0.398**
	(0.018)	(0.114)
intercept	0.435**	0.205
	(0.032)	(0.111)
Observations	235,290	76,831
R^2 LL	0.158	-27,081.0

Standard errors (clustered at the industry×occupation level) in parentheses.

Dependent variable: less than 20 days of paid leave.

Data source: UK Labour Force Survey.

Occupations: 1 Managers and Senior Officials; 2 Professional Occupations; 3 Associate Professional and Technical Occupations; 4 Administrative and Secretarial Occupations; 5 Skilled Trades Occupations; 6 Personal Service Occupations; 7 Sales and Customer Service Occupations; 8 Process, Plant and Machine Operatives; 9 Elementary Occupations

Industries: 1 Agriculture, hunting and forestry; 2 Mining, quarrying; 3 Manufacturing; 4 Electricity, gas and water supply; 5 Construction; 6 Wholesale, retail and motor trade; 7 Hotels and restaurants; 8 Transport, storage and communication; 9 Financial intermediation 10 Real estate, renting and business activities; 11 Public administration and defence; 12 Health and social work; 13 Other community, social and personal; 14 Private households with employed persons; 15 Industry missing

Table 2.5: Effect of the WTR98 on the exit to non-employment (complementary log-log regression)

	Aut. 1998	Aut. 1999	Aut. 2000
treated: no paid leave	0.000	0.104	0.100
	(0.092)	(0.081)	(0.071)
treated: 1–14 days	-0.057	0.037	0.069
	(0.100)	(0.090)	(0.084)
treated: 15–19 days		0.086	0.100
		(0.093)	(0.068)
base: no paid leave	1.293**	1.289**	1.287**
	(0.054)	(0.054)	(0.052)
base: 1–14 days	0.621**	0.619**	0.622**
	(0.057)	(0.056)	(0.055)
base: 15–19 day	0.282**	0.287**	0.290**
	(0.048)	(0.049)	(0.049)
baseline +12	-0.533**	-0.512**	-0.506**
	(0.051)	(0.049)	(0.047)
baseline +9	-0.324**	-0.337**	-0.325**
	(0.041)	(0.038)	(0.036)
baseline +6	-0.128**	-0.160**	-0.150**
	(0.029)	(0.028)	(0.027)
baseline. +3	-1.465**	-1.441**	-1.367**
	(0.175)	(0.170)	(0.164)
Observations	291,428	345,439	395,700
Log-likelihood	-30,322.1	-35,522.2	-40,377.4

Standard errors (clustered at the industry×occupation level) in parentheses.
Dependent variable: exit from dependent employment.
Reported controls: 'Treated' shows the impact of the WTR98 for the different treated groups (the coefficient on the interaction between group and the introduction of the WTR98). 'Base' shows the initial difference between the treated groups and the control group and the baseline gives the estimated baseline hazard for exit to non-employment from the first to the second quarter (+3), and the change of the baseline hazard with each additional quarter in the sample (+6,+9,+12)
Additional controls: Paid leave indicators (0, 1-14, 15-19 days), gender, age, age squared, tenure, tenure squared, current wage below the minimum wage 1, 2, 3 or 4 quarters ahead, usual hours per week above 48, quarter and year dummies, part-time dummy, married, kids present, managerial status, education (7 categories), apprenticeship completed, ever works overtime, industry (2 digit), special work contracts (8 categories), union membership, days worked per week (3 categories), region (21 categories), occupation (1 digit), firm size (6 categories).
Data source: UK Labour Force Survey
Observations only from individuals that entered the LFS on or before Aut. 1998 (1st column), 1999 (2nd), 2000 (3rd).

Table 2.6: Effect of the WTR98 on the exit to non-employment (complementary log-log regression—propensity score selected sample)

	Aut. 1998	Aut. 1999	Aut. 2000
treated: no paid leave	0.029	0.134	0.135
	(0.103)	(0.086)	(0.075)
treated: 1–14 days	0.004	0.091	0.131
	(0.110)	(0.097)	(0.091)
treated: 15–19 days		0.078	0.082
		(0.094)	(0.068)
base: no paid leave	1.225**	1.218**	1.214**
	(0.059)	(0.058)	(0.057)
base: 1–14 days	0.568**	0.566**	0.569**
	(0.058)	(0.057)	(0.058)
base: 15–19 day	0.260**	0.267**	0.271**
	(0.052)	(0.054)	(0.053)
baseline +12	-0.681**	-0.672**	-0.664**
	(0.051)	(0.045)	(0.043)
baseline +9	-0.461**	-0.480**	-0.473**
	(0.045)	(0.041)	(0.038)
baseline +6	-0.197**	-0.233**	-0.229**
	(0.031)	(0.029)	(0.029)
baseline +3	-1.655**	-1.639**	-1.598**
	(0.212)	(0.201)	(0.193)
Observations	152,173	181,272	207,749
Log-likelihood	-20,867.6	-24,468.3	-27,745.0

Standard errors (clustered at the industry×occupation level) in parentheses.
Dependent variable: exit from dependent employment.
Reported controls: 'Treated' shows the impact of the WTR98 for the different treated groups (the coefficient on the interaction between group and the introduction of the WTR98). 'Base' shows the initial difference between the treated groups and the control group and the baseline gives the estimated baseline hazard for exit to non-employment from the first to the second quarter (+3), and the change of the baseline hazard with each additional quarter in the sample (+6,+9,+12)
Additional controls: Paid leave indicators (0, 1-14, 15-19 days), gender, age, age squared, tenure, tenure squared, current wage below the minimum wage 1, 2, 3 or 4 quarters ahead, usual hours per week above 48, quarter and year dummies, part-time dummy, married, kids present, managerial status, education (7 categories), apprenticeship completed, ever works overtime, industry (2 digit), special work contracts (8 categories), union membership, days worked per week (3 categories), region (21 categories), occupation (1 digit), firm size (6 categories).
Data source: UK Labour Force Survey
Observations only from individuals that entered the LFS on or before Aut. 1998 (1st column), 1999 (2nd), 2000 (3rd).

Table 2.7: Effect of the WTR98 on hourly wages (OLS regression)

	Aut. 1998	Aut. 1999	Aut. 2000
treated: no paid leave	0.062*	0.108**	0.105**
	(0.029)	(0.028)	(0.027)
treated: 1–14 days	0.104**	0.131**	0.133**
	(0.029)	(0.023)	(0.020)
treated: 15–19 days		0.064**	0.069**
		(0.016)	(0.015)
base: no paid leave	-0.112**	-0.118**	-0.122**
	(0.023)	(0.023)	(0.022)
base: 1–14 days	-0.150**	-0.155**	-0.157**
	(0.016)	(0.016)	(0.016)
base: 15–19 day	-0.122**	-0.123**	-0.125**
	(0.015)	(0.015)	(0.015)
Observations	35,951	44,012	51,984
R-squared	0.579	0.589	0.598

Standard errors (clustered at the industry×occupation level) in parentheses.
Dependent variable: log hourly wage (surveyed only in entry and exit interview).
Reported controls: 'Treated' shows the impact of the WTR98 for the different treated groups (the coefficient on the interaction between group and the introduction of the WTR98). 'Base' shows the initial difference between the treated groups and the control group.
Additional controls: Paid leave indicators (0, 1-14, 15-19 days), gender, age, age squared, tenure, tenure squared, current wage below the minimum wage 1, 2, 3 or 4 quarters ahead, usual hours per week above 48, quarter and year dummies, part-time dummy, married, kids present, managerial status, education (7 categories), apprenticeship completed, ever works overtime, industry (2 digit), special work contracts (8 categories), union membership, days worked per week (3 categories), region (21 categories), occupation (1 digit), firm size (6 categories).
Data source: UK Labour Force Survey
Observations only from individuals that entered the LFS on or before Aut. 1998 (1st column), 1999 (2nd), 2000 (3rd).

Table 2.8: Effect of the WTR98 on hourly wages (OLS regression—propensity score selected sample)

	Aut. 1998	Aut. 1999	Aut. 2000
treated: no paid leave	0.019	0.064*	0.072*
	(0.031)	(0.028)	(0.029)
treated: 1–14 days	0.067*	0.092**	0.098**
	(0.030)	(0.024)	(0.022)
treated: 15–19 days		0.054**	0.061**
		(0.015)	(0.013)
base: no paid leave	-0.092**	-0.097**	-0.103**
	(0.022)	(0.022)	(0.021)
base: 1–14 days	-0.130**	-0.133**	-0.137**
	(0.015)	(0.015)	(0.015)
base: 15–19 day	-0.111**	-0.109**	-0.110**
	(0.014)	(0.013)	(0.013)
Observations	18,389	22,546	26,737
R-squared	0.473	0.490	0.506

Standard errors (clustered at the industry×occupation level) in parentheses.
Dependent variable: log hourly wage (surveyed only in entry and exit interview).
Reported controls: 'Treated' shows the impact of the WTR98 for the different treated groups (the coefficient on the interaction between group and the introduction of the WTR98). 'Base' shows the initial difference between the treated groups and the control group.
Additional controls: Paid leave indicators (0, 1-14, 15-19 days), gender, age, age squared, tenure, tenure squared, current wage below the minimum wage 1, 2, 3 or 4 quarters ahead, usual hours per week above 48, quarter and year dummies, part-time dummy, married, kids present, managerial status, education (7 categories), apprenticeship completed, ever works overtime, industry (2 digit), special work contracts (8 categories), union membership, days worked per week (3 categories), region (21 categories), occupation (1 digit), firm size (6 categories).
Data source: UK Labour Force Survey
Observations only from individuals that entered the LFS on or before Aut. 1998 (1st column), 1999 (2nd), 2000 (3rd).

Table 2.9: Robustness check excluding employees with less than 2 years of tenure: Exit to non-employment

	without selection			with selection		
	Aut. 1998	Aut. 1999	Aut. 2000	Aut. 1998	Aut. 1999	Aut. 2000
treated: no leave	-0.148	-0.047	0.059	-0.138	-0.016	0.087
	(0.150)	(0.134)	(0.122)	(0.171)	(0.156)	(0.147)
treated: 1–14	-0.347	0.074	0.127	-0.305	0.076	0.132
	(0.195)	(0.141)	(0.126)	(0.216)	(0.156)	(0.141)
treated: 15–19		0.036	0.106		-0.073	-0.036
		(0.142)	(0.119)		(0.157)	(0.120)
base: no leave	1.430**	1.435**	1.426**	1.419**	1.427**	1.406**
	(0.074)	(0.076)	(0.072)	(0.086)	(0.089)	(0.086)
base: 1–14 days	0.564**	0.563**	0.561**	0.558**	0.560**	0.557**
	(0.071)	(0.070)	(0.067)	(0.077)	(0.076)	(0.072)
base: 15–19 day	0.179*	0.183*	0.186**	0.169*	0.180*	0.186*
	(0.072)	(0.072)	(0.071)	(0.082)	(0.082)	(0.083)
baseline +12	-0.095	-0.072	-0.082	-0.154*	-0.160*	-0.175**
	(0.064)	(0.062)	(0.061)	(0.075)	(0.066)	(0.063)
baseline +9	0.043	0.032	0.046	-0.032	-0.066	-0.066
	(0.040)	(0.034)	(0.034)	(0.052)	(0.048)	(0.046)
baseline +6	0.038	-0.001	0.012	0.003	-0.067	-0.061
	(0.038)	(0.039)	(0.035)	(0.053)	(0.050)	(0.048)
baseline +3	-1.399**	-1.302**	-1.172**	-1.644**	-1.532**	-1.406**
	(0.204)	(0.194)	(0.176)	(0.243)	(0.235)	(0.218)
Observations	236,106	278,953	319,148	102,552	121,790	139,467
Log-likelihood	-18,626.8	-21,736.8	-24,762.9	-10,158.0	-11,871.7	-13,486.2

Standard errors (clustered at the industry×occupation level) in parentheses.
Dependent variable: exit from dependent employment.
Reported controls: 'Treated' shows the impact of the WTR98 for the different treated groups (the coefficient on the interaction between group and the introduction of the WTR98). 'Base' shows the initial difference between the treated groups and the control group and the baseline gives the estimated baseline hazard for exit to non-employment from the first to the second quarter (+3), and the change of the baseline hazard with each additional quarter in the sample (+6,+9,+12)
Additional controls: Paid leave indicators (0, 1-14, 15-19 days), gender, age, age squared, tenure, tenure squared, current wage below the minimum wage 1, 2, 3 or 4 quarters ahead, usual hours per week above 48, quarter and year dummies, part-time dummy, married, kids present, managerial status, education (7 categories), apprenticeship completed, ever works overtime, industry (2 digit), special work contracts (8 categories), union membership, days worked per week (3 categories), region (21 categories), occupation (1 digit), firm size (6 categories).
Data source: UK Labour Force Survey
Observations only from individuals that entered the LFS on or before Aut. 1998 (1st/4th column), 1999 (2nd/5th), 2000 (3rd/6th).

Table 2.10: Robustness check excluding employees with less than 2 years of tenure: wage regression

	without selection			with selection		
	Aut. 1998	Aut. 1999	Aut. 2000	Aut. 1998	Aut. 1999	Aut. 2000
treated: no leave	0.125**	0.179**	0.165**	0.080	0.132**	0.127**
	(0.039)	(0.039)	(0.037)	(0.040)	(0.039)	(0.040)
treated: 1–14	0.099**	0.140**	0.147**	0.057	0.095**	0.106**
	(0.036)	(0.028)	(0.027)	(0.036)	(0.028)	(0.027)
treated: 15–19		0.069**	0.068**		0.063**	0.062**
		(0.019)	(0.019)		(0.019)	(0.018)
base: no leave	-0.134**	-0.143**	-0.146**	-0.107**	-0.115**	-0.120**
	(0.033)	(0.033)	(0.033)	(0.032)	(0.032)	(0.032)
base: 1–14 days	-0.160**	-0.166**	-0.169**	-0.134**	-0.139**	-0.143**
	(0.018)	(0.018)	(0.018)	(0.016)	(0.015)	(0.015)
base: 15–19 day	-0.124**	-0.126**	-0.128**	-0.112**	-0.110**	-0.112**
	(0.017)	(0.017)	(0.017)	(0.016)	(0.016)	(0.016)
Observations	30,273	36,983	43,452	13,764	16,831	19,804
R-squared	0.574	0.583	0.594	0.456	0.472	0.489

Standard errors (clustered at the industry×occupation level) in parentheses.
Dependent variable: log hourly wage (surveyed only in entry and exit interview).
Reported controls: 'Treated' shows the impact of the WTR98 for the different treated groups (the coefficient on the interaction between group and the introduction of the WTR98). 'Base' shows the initial difference between the treated groups and the control group.
Additional controls: Paid leave indicators (0, 1-14, 15-19 days), gender, age, age squared, tenure, tenure squared, current wage below the minimum wage 1, 2, 3 or 4 quarters ahead, usual hours per week above 48, quarter and year dummies, part-time dummy, married, kids present, managerial status, education (7 categories), apprenticeship completed, ever works overtime, industry (2 digit), special work contracts (8 categories), union membership, days worked per week (3 categories), region (21 categories), occupation (1 digit), firm size (6 categories).
Data source: UK Labour Force Survey
Observations only from individuals that entered the LFS on or before Aut. 1998 (1st/4th column), 1999 (2nd/5th), 2000 (3rd/6th).

Table 2.11: First stage propensity score estimates for long-run estimates

	(I)	(II)
age	-2.172**	-2.141**
	(0.338)	(0.504)
age squared	0.075**	0.073**
	(0.013)	(0.020)
age cubed	-0.001**	-0.001**
	(0.000)	(0.000)
age quartic	0.000**	0.000**
	(0.000)	(0.000)
2.educ×age	1.061*	1.152*
	(0.415)	(0.527)
3.educ×age	1.288**	1.370**
	(0.350)	(0.518)
4.educ×age	1.200**	1.608**
	(0.352)	(0.523)
5.educ×age	2.004**	1.914**
	(0.376)	(0.534)
6.educ×age	2.018**	1.974**
	(0.371)	(0.535)
2.educ×agesq	-0.038*	-0.042*
	(0.016)	(0.021)
3.educ×agesq	-0.046**	-0.048*
	(0.014)	(0.020)
4.educ×agesq	-0.040**	-0.056**
	(0.014)	(0.021)
5.educ×agesq	-0.072**	-0.067**
	(0.015)	(0.021)
6.educ×agesq	-0.073**	-0.070**
	(0.015)	(0.021)
2.educ×agecub	0.001*	0.001
	(0.000)	(0.000)
3.educ×agecub	0.001**	0.001*
	(0.000)	(0.000)
4.educ×agecub	0.001*	0.001*
	(0.000)	(0.000)
5.educ×agecub	0.001**	0.001**
	(0.000)	(0.000)
6.educ×agecub	0.001**	0.001**
	(0.000)	(0.000)
2.educ×agequart	-0.000*	-0.000
	(0.000)	(0.000)
3.educ×agequart	-0.000**	-0.000*
	(0.000)	(0.000)
4.educ×agequart	-0.000*	-0.000*
	(0.000)	(0.000)
5.educ×agequart	-0.000**	-0.000**
	(0.000)	(0.000)
6.educ×agequart	-0.000**	-0.000**
	(0.000)	(0.000)
female×age	-1.148**	-0.747**
	(0.101)	(0.122)
female×agesq	0.048**	0.032**
	(0.004)	(0.005)
female×agecub	-0.001**	-0.001**
	(0.000)	(0.000)
female×agequart	0.000**	0.000**
	(0.000)	(0.000)

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... table 2.11 continued

	(I)	(II)
married	-0.084**	-0.086**
	(0.013)	(0.015)
foreign	0.174**	0.255**
	(0.032)	(0.038)
has kid(s)	0.259**	0.194**
	(0.018)	(0.016)
female	9.603**	6.001**
	(0.903)	(1.044)
2.educ	-10.677**	-11.527*
	(3.785)	(4.845)
3.educ	-12.814**	-13.872**
	(3.239)	(4.763)
4.educ	-12.448**	-16.145**
	(3.262)	(4.806)
5.educ	-19.329**	-18.852**
	(3.483)	(4.906)
6.educ	-19.191**	-19.163**
	(3.404)	(4.898)
female×2.educ	-0.074	-0.030
	(0.068)	(0.081)
female×3.educ	-0.110*	-0.027
	(0.051)	(0.052)
female×4.educ	-0.089	0.014
	(0.056)	(0.053)
female×5.educ	-0.164**	-0.016
	(0.051)	(0.057)
female×6.educ	-0.051	0.091
	(0.052)	(0.054)
2.region	0.138**	0.097
	(0.036)	(0.071)
3.region	0.117**	0.146
	(0.036)	(0.076)
4.region	0.087**	0.120
	(0.033)	(0.072)
5.region	0.181**	0.103
	(0.035)	(0.070)
6.region	0.167**	0.131
	(0.035)	(0.071)
7.region	0.126**	0.068
	(0.045)	(0.076)
8.region	0.033	0.059
	(0.037)	(0.072)
9.region	-0.035	0.033
	(0.039)	(0.071)
10.region	0.058	0.003
	(0.031)	(0.069)
11.region	0.177**	0.118
	(0.035)	(0.072)
12.region	0.069	0.119
	(0.040)	(0.072)
13.region	0.096**	0.052
	(0.032)	(0.070)
14.region	0.074	0.114
	(0.044)	(0.075)
15.region	-0.017	0.020
	(0.038)	(0.070)
16.region	0.055	0.077
	(0.035)	(0.072)
17.region	0.178**	0.207**

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... table 2.11 continued

	(I)	(II)
	(0.042)	(0.069)
18.region	-0.007	0.011
	(0.038)	(0.072)
19.region	0.066	0.031
	(0.036)	(0.072)
20.region	0.259**	0.200**
	(0.053)	(0.075)
Intercept	21.663**	21.265**
	(3.145)	(4.652)
Observations	120,062	84,579
Log-likelihood	-64,420.3	-35,003.8

First stage: Generate treatment indicator as an out-of-sample prediction based on a Probit on data from 1994–1996 using age (quartic), qualification, gender, married, foreign born, kids present, region of residence, interaction of age polynomial with qualification, interaction of qualification and gender and interaction of gender and age polynomial as controls. Dependent variable is "less than 4 weeks of paid leave" in column I and "less than 4 weeks of paid leave and working 5 or more days per week" in column II.

Standard errors (clustered at the region×education level) in parentheses.

Data source: UK Labour Force Survey.

Table 2.12: Long-run impact of WTR1998 on employment

	(I)	(II)	(III)	(IV)
treatment	-0.022*	-0.003**	-0.041**	-0.005**
	(0.010)	(0.001)	(0.014)	(0.002)
group	-0.471**	-0.469**	-0.644**	-0.645**
	(0.036)	(0.037)	(0.049)	(0.050)
post	0.032**		0.033**	
	(0.002)		(0.002)	
trend		0.005**		0.005**
		(0.000)		(0.000)
constant	0.746**	0.735**	0.732**	0.721**
	(0.007)	(0.008)	(0.007)	(0.007)
Observations	3,320,112	3,320,112	3,320,112	3,320,112
R^2	0.030	0.030	0.036	0.037

First stage: Generate treatment indicator as an out-of-sample prediction based on a Probit on data from 1994–1996 using age (quartic), qualification, gender, married, foreign born, kids present, region of residence, interaction of age polynomial with qualification, interaction of qualification and gender and interaction of gender and age polynomial as controls. Dependent variable is less than 4 weeks of paid leave in columns I and II. Column III and IV dependent variable is less than 4 weeks of paid leave and working 5 or more days per week. Second stage: Dependent variable is exit from dependent employment, data from 1994–2004.
Bootstrap standard errors in parentheses. Bootstrap of the second stage resampling from clusters (500 replications, resampling from 120 clusters at the region×education level).
Data source: UK Labour Force Survey

Additional Appendix: full regression results

Table 2.13: Complementary log-log regression—Full table

	Aut. 1998	Aut. 1999	Aut. 2000
base: no paid leave	1.293**	1.289**	1.287**
	(0.054)	(0.054)	(0.052)
base: 1–14 days	0.621**	0.619**	0.622**
	(0.057)	(0.056)	(0.055)
base: 15–19 day	0.282**	0.287**	0.290**
	(0.048)	(0.049)	(0.049)
treated: no paid lve	0.000	0.104	0.100
	(0.092)	(0.081)	(0.071)
treated: 1–14 days	-0.057	0.037	0.069
	(0.100)	(0.090)	(0.084)
treated: 15–19 day		0.086	0.100
		(0.093)	(0.068)
female	0.050	0.053	0.063
	(0.042)	(0.038)	(0.040)
age	-0.095**	-0.094**	-0.098**
	(0.010)	(0.010)	(0.009)
age squared	0.001**	0.001**	0.001**
	(0.000)	(0.000)	(0.000)
tenure	-0.132**	-0.133**	-0.130**
	(0.005)	(0.005)	(0.005)
tenure squared	0.003**	0.003**	0.003**
	(0.000)	(0.000)	(0.000)
below min. wage 3		0.047	0.144
		(0.150)	(0.143)
below min. wage 6	0.046	0.049	0.049
	(0.167)	(0.162)	(0.163)
below min. wage 9	-0.115	0.370	0.025
	(0.152)	(0.456)	(0.210)
below min. wage 12		-0.532	-0.189
		(0.451)	(0.202)
48+ hours	0.009**	0.010**	0.011**
	(0.003)	(0.003)	(0.002)
baseline haz. +6	-0.128**	-0.160**	-0.150**
	(0.029)	(0.028)	(0.027)
baseline haz. +9	-0.324**	-0.337**	-0.325**
	(0.041)	(0.038)	(0.036)
baseline haz. +12	-0.533**	-0.512**	-0.506**
	(0.051)	(0.049)	(0.047)
year = 1995	-0.018	-0.021	-0.020
	(0.038)	(0.037)	(0.037)
year = 1996	-0.059	-0.063	-0.063
	(0.034)	(0.033)	(0.033)
year = 1997	-0.157**	-0.160**	-0.160**
	(0.036)	(0.036)	(0.036)
year = 1998	-0.139**	-0.164**	-0.164**
	(0.049)	(0.051)	(0.049)
year = 1999		-0.157**	-0.167**
		(0.047)	(0.046)
year = 2000			-0.142**
			(0.042)
full-time	0.085	0.116**	0.122**
	(0.045)	(0.043)	(0.042)
private company	-0.234**	-0.263**	-0.250**
	(0.069)	(0.069)	(0.060)

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... table 2.13 continued

	Aut. 1998	Aut. 1999	Aut. 2000
married	-0.056	-0.045	-0.046
	(0.036)	(0.035)	(0.032)
foreign	0.064	0.058	0.043
	(0.060)	(0.053)	(0.050)
has kid(s)	0.163**	0.153**	0.146**
	(0.033)	(0.030)	(0.028)
manager duties	-0.040	-0.037	-0.038
	(0.037)	(0.033)	(0.031)
education = 1	-0.092*	-0.060	-0.008
	(0.046)	(0.043)	(0.039)
education = 2	-0.131*	-0.129*	-0.102*
	(0.056)	(0.053)	(0.047)
education = 4	0.060	0.036	0.035
	(0.034)	(0.033)	(0.030)
education = 5	0.015	-0.000	0.008
	(0.043)	(0.042)	(0.041)
education = 6	0.116**	0.096**	0.095*
	(0.035)	(0.037)	(0.038)
education = 7	-0.369	-0.267	-0.223
	(0.200)	(0.198)	(0.167)
apprenticeship	-0.559**	-0.494**	-0.535**
	(0.185)	(0.162)	(0.172)
ever overtime	-0.335**	-0.330**	-0.344**
	(0.025)	(0.024)	(0.025)
industry = 1	0.100	0.030	-0.011
	(0.061)	(0.063)	(0.062)
industry = 2	0.020	-0.082	-0.092
	(0.168)	(0.153)	(0.154)
industry = 4	0.374*	0.368*	0.293
	(0.182)	(0.185)	(0.163)
industry = 5	0.373**	0.303**	0.270**
	(0.087)	(0.100)	(0.094)
industry = 6	-0.043	-0.097*	-0.113**
	(0.041)	(0.041)	(0.041)
industry = 7	0.065	0.009	-0.031
	(0.070)	(0.068)	(0.066)
industry = 8	-0.013	-0.036	-0.069
	(0.068)	(0.059)	(0.057)
industry = 9	0.011	-0.031	-0.023
	(0.044)	(0.045)	(0.040)
industry = 10	0.007	-0.025	-0.036
	(0.067)	(0.068)	(0.064)
industry = 11	0.062	0.025	-0.020
	(0.078)	(0.078)	(0.073)
industry = 12	-0.084	-0.104	-0.129*
	(0.072)	(0.068)	(0.064)
industry = 13	0.119	0.079	0.071
	(0.073)	(0.070)	(0.068)
industry = 14	0.152	0.068	0.060
	(0.111)	(0.104)	(0.105)
industry = 15	-0.472**	-0.622**	-0.685**
	(0.050)	(0.044)	(0.042)
work arr. = 1	-0.038	-0.069	-0.060
	(0.044)	(0.041)	(0.038)
work arr. = 2	0.008	-0.060	-0.093
	(0.078)	(0.072)	(0.071)
work arr. = 3	-0.237	-0.249*	-0.316**
	(0.129)	(0.118)	(0.108)
work arr. = 4	-0.237	-0.380*	-0.308

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... table 2.13 continued

	Aut. 1998	Aut. 1999	Aut. 2000
	(0.187)	(0.191)	(0.205)
work arr. = 5	0.312*	0.179	0.125
	(0.157)	(0.158)	(0.155)
work arr. = 6	0.108*	0.064	0.046
	(0.051)	(0.063)	(0.064)
work arr. = 8	-0.153	-0.062	-0.107
	(0.210)	(0.179)	(0.180)
union member	-0.099*	-0.096*	-0.118**
	(0.045)	(0.040)	(0.037)
6 days per week	-0.040	-0.034	-0.048
	(0.037)	(0.036)	(0.037)
7 days per week	-0.016	-0.030	-0.001
	(0.048)	(0.046)	(0.045)
region = 1	0.181*	0.140	0.157*
	(0.081)	(0.076)	(0.065)
region = 2	-0.078	-0.105	-0.059
	(0.095)	(0.102)	(0.090)
region = 3	0.105	0.080	0.053
	(0.085)	(0.072)	(0.067)
region = 4	0.035	-0.002	-0.016
	(0.073)	(0.063)	(0.056)
region = 5	-0.070	-0.096	-0.109
	(0.092)	(0.085)	(0.082)
region = 6	-0.054	-0.060	-0.051
	(0.049)	(0.046)	(0.043)
region = 7	-0.077	-0.101	-0.120
	(0.075)	(0.069)	(0.063)
region = 8	0.193**	0.165**	0.137**
	(0.059)	(0.053)	(0.051)
region = 9	0.126	0.141	0.140
	(0.089)	(0.082)	(0.075)
region = 11	0.039	0.062	0.008
	(0.061)	(0.054)	(0.048)
region = 13	-0.054	-0.043	-0.065
	(0.042)	(0.044)	(0.041)
region = 14	-0.101*	-0.098*	-0.089*
	(0.050)	(0.048)	(0.045)
region = 15	-0.113	-0.101	-0.105
	(0.071)	(0.067)	(0.065)
region = 16	0.138*	0.153**	0.119*
	(0.064)	(0.058)	(0.055)
region = 17	0.102	0.136	0.124
	(0.088)	(0.083)	(0.077)
region = 18	-0.036	-0.020	-0.006
	(0.057)	(0.053)	(0.050)
region = 19	0.024	0.018	0.013
	(0.072)	(0.069)	(0.067)
region = 20	0.011	0.028	0.023
	(0.087)	(0.079)	(0.074)
region = 21	-0.038	-0.043	-0.051
	(0.062)	(0.056)	(0.050)
region = 22	-0.086	-0.064	-0.114
	(0.081)	(0.084)	(0.064)
occ. = 2	-0.058	-0.066	-0.083
	(0.079)	(0.080)	(0.082)
occ. = 3	-0.104	-0.101	-0.076
	(0.055)	(0.060)	(0.054)
occ. = 4	-0.254**	-0.227**	-0.215**
	(0.059)	(0.058)	(0.054)

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... table 2.13 continued

	Aut. 1998	Aut. 1999	Aut. 2000
occ. = 5	-0.009	0.014	0.021
	(0.065)	(0.071)	(0.069)
occ. = 6	-0.166*	-0.172**	-0.146*
	(0.066)	(0.062)	(0.058)
occ. = 7	-0.095	-0.063	-0.054
	(0.060)	(0.058)	(0.058)
occ. = 8	-0.029	-0.032	-0.018
	(0.051)	(0.047)	(0.045)
occ. = 9	-0.049	-0.023	-0.003
	(0.068)	(0.063)	(0.060)
occ. = 10	-0.345**	-0.147**	-0.127**
	(0.046)	(0.045)	(0.041)
size 11–19	0.012	-0.015	-0.017
	(0.044)	(0.043)	(0.039)
size 20–24	-0.045	-0.037	-0.018
	(0.058)	(0.057)	(0.051)
size < 25	0.043	0.013	-0.018
	(0.106)	(0.092)	(0.084)
size 25–49	-0.042	-0.019	-0.015
	(0.041)	(0.040)	(0.037)
missing size	0.048	0.058	0.008
	(0.219)	(0.217)	(0.200)
baseline haz. +3	-1.465**	-1.441**	-1.367**
	(0.175)	(0.170)	(0.164)
Observations	291,428	345,439	395,700
Log-likelihood	-30,322.1	-35,522.2	-40,377.4

Standard errors (clustered at the industry×occupation level) in parentheses.

Dependent variable: exit from dependent employment.

Data source: UK Labour Force Survey

Observations only from individuals that entered the LFS on or before Aut. 1998 (1st column), 1999 (2nd), 2000 (3rd).

Table 2.14: Complementary log-log regression—propensity score selected sample—Full table

	Aut. 1998	Aut. 1999	Aut. 2000
base: no paid leave	1.225**	1.218**	1.214**
	(0.059)	(0.058)	(0.057)
base: 1–14 days	0.568**	0.566**	0.569**
	(0.058)	(0.057)	(0.058)
base: 15–19 day	0.260**	0.267**	0.271**
	(0.052)	(0.054)	(0.053)
treated: no paid lve	0.029	0.134	0.135
	(0.103)	(0.086)	(0.075)
treated: 1–14 days	0.004	0.091	0.131
	(0.110)	(0.097)	(0.091)
treated: 15–19 day		0.078	0.082
		(0.094)	(0.068)
female	0.057	0.053	0.075
	(0.046)	(0.041)	(0.042)
age	-0.069**	-0.068**	-0.071**
	(0.012)	(0.011)	(0.010)
age squared	0.001**	0.001**	0.001**
	(0.000)	(0.000)	(0.000)
tenure	-0.162**	-0.164**	-0.160**
	(0.006)	(0.006)	(0.007)
tenure squared	0.004**	0.004**	0.004**
	(0.000)	(0.000)	(0.000)
below min. wage 3		0.006	0.078
		(0.153)	(0.142)
below min. wage 6	0.031	0.037	0.038
	(0.173)	(0.167)	(0.167)
below min. wage 9	-0.064	0.642	0.154
	(0.165)	(0.496)	(0.206)
below min. wage 12		-0.750	-0.267
		(0.508)	(0.210)
48+ hours	0.010**	0.010**	0.011**
	(0.003)	(0.003)	(0.003)
baseline haz. +6	-0.197**	-0.233**	-0.229**
	(0.031)	(0.029)	(0.029)
baseline haz. +9	-0.461**	-0.480**	-0.473**
	(0.045)	(0.041)	(0.038)
baseline haz. +12	-0.681**	-0.672**	-0.664**
	(0.051)	(0.045)	(0.043)
year = 1995	-0.038	-0.041	-0.040
	(0.041)	(0.041)	(0.040)
year = 1996	-0.109**	-0.113**	-0.112**
	(0.038)	(0.038)	(0.038)
year = 1997	-0.160**	-0.163**	-0.162**
	(0.040)	(0.040)	(0.040)
year = 1998	-0.186**	-0.218**	-0.219**
	(0.056)	(0.056)	(0.052)
year = 1999		-0.190**	-0.201**
		(0.044)	(0.040)
year = 2000			-0.179**
			(0.051)
full-time	0.118**	0.150**	0.148**
	(0.045)	(0.043)	(0.041)
private company	-0.095	-0.117	-0.089
	(0.089)	(0.087)	(0.071)
married	-0.056	-0.048	-0.055
	(0.039)	(0.037)	(0.035)

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... table 2.14 continued

	Aut. 1998	Aut. 1999	Aut. 2000
foreign	0.008	0.004	0.002
	(0.061)	(0.055)	(0.053)
has kid(s)	0.165**	0.154**	0.156**
	(0.035)	(0.033)	(0.032)
manager duties	-0.022	-0.030	-0.039
	(0.040)	(0.033)	(0.034)
education = 1	-0.051	-0.014	0.042
	(0.071)	(0.065)	(0.058)
education = 2	-0.118	-0.102	-0.059
	(0.064)	(0.058)	(0.052)
education = 4	0.027	-0.012	0.001
	(0.039)	(0.034)	(0.032)
education = 5	0.004	-0.018	-0.007
	(0.047)	(0.045)	(0.044)
education = 6	0.140**	0.113**	0.120**
	(0.038)	(0.038)	(0.040)
education = 7	-0.447	-0.365	-0.264
	(0.239)	(0.216)	(0.167)
apprenticeship	-0.582**	-0.509**	-0.554**
	(0.194)	(0.170)	(0.182)
ever overtime	-0.303**	-0.303**	-0.314**
	(0.032)	(0.031)	(0.032)
industry = 1	0.169*	0.117	0.088
	(0.067)	(0.065)	(0.071)
industry = 2	-0.071	-0.135	-0.121
	(0.168)	(0.148)	(0.152)
industry = 4	0.111	0.148	0.161
	(0.202)	(0.262)	(0.211)
industry = 5	0.381**	0.322**	0.302**
	(0.094)	(0.102)	(0.089)
industry = 6	-0.066	-0.115*	-0.116*
	(0.052)	(0.052)	(0.050)
industry = 7	0.014	-0.025	-0.056
	(0.071)	(0.072)	(0.068)
industry = 8	-0.049	-0.045	-0.070
	(0.070)	(0.066)	(0.058)
industry = 9	-0.059	-0.107	-0.093
	(0.107)	(0.115)	(0.084)
industry = 10	-0.046	-0.070	-0.074
	(0.069)	(0.070)	(0.065)
industry = 11	-0.023	-0.046	-0.069
	(0.115)	(0.112)	(0.097)
industry = 12	-0.135	-0.155	-0.184*
	(0.087)	(0.081)	(0.076)
industry = 13	0.063	0.027	0.036
	(0.080)	(0.083)	(0.079)
industry = 14	0.198	0.112	0.105
	(0.115)	(0.111)	(0.115)
industry = 15	-0.924**	-1.023**	-1.043**
	(0.061)	(0.054)	(0.052)
work arr. = 1	-0.031	-0.073	-0.033
	(0.060)	(0.059)	(0.055)
work arr. = 2	0.022	-0.038	-0.109
	(0.100)	(0.093)	(0.091)
work arr. = 3	-0.420**	-0.403**	-0.470**
	(0.142)	(0.127)	(0.119)
work arr. = 4	-0.264	-0.404	-0.321
	(0.207)	(0.210)	(0.222)
work arr. = 5	0.381	0.261	0.250

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... table 2.14 continued

	Aut. 1998	Aut. 1999	Aut. 2000
	(0.215)	(0.206)	(0.202)
work arr. = 6	0.181**	0.136	0.106
	(0.059)	(0.070)	(0.070)
work arr. = 8	-0.147	-0.094	-0.144
	(0.219)	(0.195)	(0.197)
union member	-0.108	-0.097	-0.111*
	(0.068)	(0.056)	(0.053)
6 days per week	-0.024	-0.017	-0.031
	(0.040)	(0.039)	(0.041)
7 days per week	-0.004	-0.023	0.003
	(0.054)	(0.053)	(0.052)
region = 1	0.181	0.141	0.169*
	(0.096)	(0.095)	(0.081)
region = 2	-0.097	-0.107	-0.068
	(0.126)	(0.130)	(0.119)
region = 3	0.090	0.075	0.049
	(0.096)	(0.087)	(0.081)
region = 4	0.055	0.005	-0.019
	(0.075)	(0.067)	(0.061)
region = 5	-0.119	-0.119	-0.141
	(0.104)	(0.096)	(0.094)
region = 6	-0.046	-0.048	-0.045
	(0.054)	(0.051)	(0.051)
region = 7	-0.110	-0.121	-0.140*
	(0.074)	(0.065)	(0.060)
region = 8	0.227**	0.187*	0.168*
	(0.085)	(0.077)	(0.080)
region = 9	0.115	0.137	0.145
	(0.091)	(0.092)	(0.083)
region = 11	0.058	0.080	0.031
	(0.069)	(0.060)	(0.052)
region = 13	-0.082	-0.069	-0.094
	(0.051)	(0.055)	(0.050)
region = 14	-0.106	-0.098	-0.096
	(0.060)	(0.056)	(0.055)
region = 15	-0.088	-0.089	-0.098
	(0.082)	(0.077)	(0.076)
region = 16	0.127	0.147*	0.099
	(0.070)	(0.063)	(0.065)
region = 17	-0.038	0.010	0.011
	(0.114)	(0.104)	(0.096)
region = 18	-0.066	-0.038	-0.019
	(0.064)	(0.059)	(0.060)
region = 19	0.019	0.015	-0.003
	(0.087)	(0.086)	(0.079)
region = 20	0.017	0.053	0.029
	(0.092)	(0.080)	(0.075)
region = 21	-0.045	-0.049	-0.070
	(0.070)	(0.063)	(0.056)
region = 22	-0.031	-0.004	-0.079
	(0.083)	(0.090)	(0.070)
occ. = 2	-0.045	-0.091	-0.080
	(0.135)	(0.138)	(0.130)
occ. = 3	-0.055	-0.038	-0.024
	(0.089)	(0.096)	(0.090)
occ. = 4	-0.266**	-0.222**	-0.210**
	(0.067)	(0.067)	(0.065)
occ. = 5	0.030	0.052	0.073
	(0.077)	(0.084)	(0.079)

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... table 2.14 continued

	Aut. 1998	Aut. 1999	Aut. 2000
occ. = 6	-0.176*	-0.169*	-0.138
	(0.079)	(0.080)	(0.074)
occ. = 7	-0.104	-0.056	-0.053
	(0.072)	(0.072)	(0.074)
occ. = 8	-0.021	-0.018	0.001
	(0.067)	(0.064)	(0.060)
occ. = 9	-0.025	0.008	0.027
	(0.074)	(0.072)	(0.068)
occ. = 10	-0.292**	-0.317**	-0.284**
	(0.060)	(0.058)	(0.055)
size 11–19	-0.005	-0.033	-0.027
	(0.049)	(0.047)	(0.041)
size 20–24	-0.080	-0.058	-0.045
	(0.067)	(0.066)	(0.060)
size < 25	0.034	0.005	-0.013
	(0.102)	(0.088)	(0.081)
size 25–49	-0.083	-0.065	-0.064
	(0.049)	(0.046)	(0.044)
missing size	-0.094	-0.057	-0.107
	(0.255)	(0.241)	(0.225)
baseline haz. +3	-1.655**	-1.639**	-1.598**
	(0.212)	(0.201)	(0.193)
Observations	152,173	181,272	207,749

Standard errors (clustered at the industry×occupation level) in parentheses.

Dependent variable: exit from dependent employment.

Data source: UK Labour Force Survey

Observations only from individuals that entered the LFS on or before Aut. 1998 (1st column), 1999 (2nd), 2000 (3rd).

Table 2.15: Wage regression—Full table

	Aut. 1998	Aut. 1999	Aut. 2000
base: no paid leave	-0.112**	-0.118**	-0.122**
	(0.023)	(0.023)	(0.022)
base: 1–14 days	-0.150**	-0.155**	-0.157**
	(0.016)	(0.016)	(0.016)
base: 15–19 day	-0.122**	-0.123**	-0.125**
	(0.015)	(0.015)	(0.015)
treated: no paid lve	0.062*	0.108**	0.105**
	(0.029)	(0.028)	(0.027)
treated: 1–14 days	0.104**	0.131**	0.133**
	(0.029)	(0.023)	(0.020)
treated: 15–19 day		0.064**	0.069**
		(0.016)	(0.015)
female	-0.165**	-0.156**	-0.151**
	(0.015)	(0.014)	(0.013)
age	0.043**	0.043**	0.043**
	(0.003)	(0.003)	(0.003)
age squared	-0.000**	-0.000**	-0.000**
	(0.000)	(0.000)	(0.000)
tenure	0.008**	0.008**	0.008**
	(0.001)	(0.001)	(0.001)
tenure squared	-0.000**	-0.000**	-0.000**
	(0.000)	(0.000)	(0.000)
below min. wage 3	-0.151**	-0.167**	-0.116*
	(0.053)	(0.051)	(0.046)
below min. wage 6	0.028	0.028	0.029
	(0.059)	(0.059)	(0.059)
below min. wage 9	-0.297**	-0.288**	-0.292**
	(0.065)	(0.064)	(0.064)
below min. wage 12	-0.351**	-0.366**	-0.364**
	(0.046)	(0.021)	(0.022)
48+ hours	-0.006**	-0.006**	-0.005**
	(0.001)	(0.001)	(0.001)
base quarter +12	0.002	-0.001	-0.003
	(0.004)	(0.003)	(0.003)
year = 1995	-0.435**	-0.415*	-0.401*
	(0.163)	(0.163)	(0.164)
year = 1996	-0.395*	-0.375*	-0.359*
	(0.163)	(0.164)	(0.165)
year = 1997	-0.358*	-0.340*	-0.326
	(0.165)	(0.165)	(0.167)
year = 1998	-0.279	-0.261	-0.247
	(0.165)	(0.165)	(0.166)
year = 1999	-0.252	-0.235	-0.224
	(0.165)	(0.165)	(0.166)
year = 2000		-0.199	-0.187
		(0.165)	(0.166)
year == 2001			-0.122
			(0.168)
full-time	-0.042**	-0.041**	-0.039**
	(0.011)	(0.011)	(0.010)
ft/pt missing	-0.075**	-0.073**	-0.079**
	(0.021)	(0.021)	(0.020)
private company	0.004	-0.001	-0.003
	(0.013)	(0.013)	(0.013)
married	0.029**	0.030**	0.029**
	(0.006)	(0.005)	(0.005)
foreign	0.029	0.030	0.024

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... table 2.15 continued

	Aut. 1998	Aut. 1999	Aut. 2000
	(0.022)	(0.020)	(0.018)
has kid(s)	0.028**	0.028**	0.028**
	(0.006)	(0.006)	(0.005)
manager duties	0.108**	0.105**	0.104**
	(0.008)	(0.008)	(0.007)
education = 1	0.267**	0.265**	0.263**
	(0.020)	(0.020)	(0.020)
education = 2	0.093**	0.093**	0.089**
	(0.015)	(0.015)	(0.014)
education = 4	-0.031**	-0.033**	-0.035**
	(0.006)	(0.005)	(0.005)
education = 5	-0.094**	-0.095**	-0.097**
	(0.010)	(0.010)	(0.009)
education = 6	-0.140**	-0.136**	-0.135**
	(0.010)	(0.010)	(0.009)
education = 7	-0.052	-0.065*	-0.072**
	(0.032)	(0.029)	(0.027)
apprenticeship	-0.235**	-0.204**	-0.218**
	(0.032)	(0.028)	(0.030)
ever overtime	0.041**	0.041**	0.039**
	(0.007)	(0.007)	(0.007)
industry = 1	-0.190**	-0.180**	-0.188**
	(0.024)	(0.029)	(0.028)
industry = 2	0.144**	0.131**	0.130**
	(0.020)	(0.019)	(0.015)
industry = 4	0.094**	0.079**	0.066**
	(0.021)	(0.020)	(0.021)
industry = 5	-0.026	-0.016	-0.008
	(0.019)	(0.019)	(0.018)
industry = 6	-0.145**	-0.140**	-0.133**
	(0.019)	(0.019)	(0.017)
industry = 7	-0.210**	-0.201**	-0.197**
	(0.075)	(0.071)	(0.074)
industry = 8	-0.003	0.000	-0.001
	(0.017)	(0.014)	(0.015)
industry = 9	0.117**	0.120**	0.117**
	(0.037)	(0.035)	(0.032)
industry = 10	-0.002	0.007	0.012
	(0.023)	(0.024)	(0.023)
industry = 11	-0.014	-0.012	-0.015
	(0.021)	(0.021)	(0.020)
industry = 12	-0.076**	-0.070**	-0.068**
	(0.022)	(0.022)	(0.022)
industry = 13	-0.139**	-0.135**	-0.134**
	(0.037)	(0.039)	(0.038)
industry = 14	-0.222**	-0.190**	-0.187**
	(0.029)	(0.034)	(0.032)
industry = 15	0.098	0.107*	0.016
	(0.050)	(0.041)	(0.060)
work arr. = 1	-0.013	-0.014	-0.017
	(0.012)	(0.012)	(0.012)
work arr. = 2	0.010	0.010	0.002
	(0.016)	(0.014)	(0.014)
work arr. = 3	-0.067*	-0.060*	-0.055
	(0.032)	(0.029)	(0.030)
work arr. = 4	0.008	0.005	0.024
	(0.032)	(0.032)	(0.033)
work arr. = 5	0.009	0.015	0.016
	(0.035)	(0.034)	(0.032)

Continued on next page...

... table 2.15 continued

	Aut. 1998	Aut. 1999	Aut. 2000
work arr. = 6	-0.062**	-0.062**	-0.055**
	(0.008)	(0.008)	(0.007)
work arr. = 8	-0.008	-0.013	-0.011
	(0.034)	(0.033)	(0.032)
union member	0.029	0.026	0.028*
	(0.014)	(0.014)	(0.013)
6 days per week	-0.046**	-0.053**	-0.054**
	(0.013)	(0.013)	(0.014)
7 days per week	-0.024	-0.020	-0.019
	(0.019)	(0.018)	(0.018)
region = 1	-0.131**	-0.129**	-0.130**
	(0.016)	(0.014)	(0.014)
region = 2	-0.117**	-0.113**	-0.116**
	(0.021)	(0.019)	(0.018)
region = 3	-0.101**	-0.106**	-0.115**
	(0.017)	(0.015)	(0.015)
region = 4	-0.109**	-0.105**	-0.104**
	(0.013)	(0.012)	(0.011)
region = 5	-0.123**	-0.127**	-0.128**
	(0.017)	(0.015)	(0.014)
region = 6	-0.119**	-0.113**	-0.111**
	(0.014)	(0.014)	(0.013)
region = 7	-0.093**	-0.087**	-0.087**
	(0.013)	(0.012)	(0.011)
region = 8	0.265**	0.271**	0.268**
	(0.024)	(0.023)	(0.022)
region = 9	0.116**	0.118**	0.118**
	(0.018)	(0.016)	(0.015)
region = 11	0.064**	0.063**	0.057**
	(0.015)	(0.013)	(0.013)
region = 13	-0.107**	-0.098**	-0.101**
	(0.012)	(0.011)	(0.011)
region = 14	-0.073**	-0.073**	-0.077**
	(0.012)	(0.011)	(0.011)
region = 15	-0.110**	-0.107**	-0.106**
	(0.013)	(0.012)	(0.011)
region = 16	-0.091**	-0.090**	-0.094**
	(0.010)	(0.009)	(0.010)
region = 17	-0.119**	-0.116**	-0.113**
	(0.016)	(0.014)	(0.013)
region = 18	-0.106**	-0.102**	-0.105**
	(0.013)	(0.012)	(0.011)
region = 19	-0.124**	-0.123**	-0.127**
	(0.015)	(0.015)	(0.015)
region = 20	-0.116**	-0.115**	-0.115**
	(0.014)	(0.012)	(0.011)
region = 21	-0.111**	-0.108**	-0.111**
	(0.012)	(0.010)	(0.009)
region = 22	-0.152**	-0.151**	-0.158**
	(0.026)	(0.023)	(0.022)
occ. = 2	-0.005	-0.005	-0.008
	(0.028)	(0.028)	(0.028)
occ. = 3	-0.084**	-0.087**	-0.096**
	(0.020)	(0.020)	(0.019)
occ. = 4	-0.244**	-0.254**	-0.265**
	(0.024)	(0.023)	(0.022)
occ. = 5	-0.271**	-0.278**	-0.286**
	(0.021)	(0.021)	(0.021)
occ. = 6	-0.311**	-0.318**	-0.328**

Continued on next page...

... table 2.15 continued

	Aut. 1998	Aut. 1999	Aut. 2000
	(0.025)	(0.025)	(0.024)
occ. = 7	-0.307**	-0.304**	-0.311**
	(0.035)	(0.034)	(0.031)
occ. = 8	-0.336**	-0.339**	-0.348**
	(0.024)	(0.025)	(0.024)
occ. = 9	-0.372**	-0.376**	-0.383**
	(0.029)	(0.029)	(0.029)
occ. = 10	-0.317**	-0.319**	-0.309**
	(0.021)	(0.020)	(0.023)
size 11–19	-0.034**	-0.034**	-0.038**
	(0.008)	(0.008)	(0.008)
size 20–24	-0.023	-0.023	-0.023*
	(0.013)	(0.012)	(0.011)
size < 25	-0.062*	-0.059**	-0.056*
	(0.026)	(0.022)	(0.022)
size 25–49	-0.012	-0.013	-0.012
	(0.008)	(0.008)	(0.008)
missing size	-0.068	-0.043	-0.078
	(0.096)	(0.071)	(0.082)
intercept	1.667**	1.641**	1.639**
	(0.194)	(0.194)	(0.191)
Observations	35,951	44,012	51,984
R^2	0.579	0.589	0.598

Standard errors (clustered at the industry×occupation level) in parentheses.

Dependent variable: log hourly wage (surveyed only in entry and exit interview).

Data source: UK Labour Force Survey

Observations only from individuals that entered the LFS on or before Aut. 1998 (1st column), 1999 (2nd), 2000 (3rd).

Table 2.16: Wage regression—propensity score selected sample—Full table

	Aut. 1998	Aut. 1999	Aut. 2000
base: no paid leave	-0.092**	-0.097**	-0.103**
	(0.022)	(0.022)	(0.021)
base: 1–14 days	-0.130**	-0.133**	-0.137**
	(0.015)	(0.015)	(0.015)
base: 15–19 day	-0.111**	-0.109**	-0.110**
	(0.014)	(0.013)	(0.013)
treated: no paid lve	0.019	0.064*	0.072*
	(0.031)	(0.028)	(0.029)
treated: 1–14 days	0.067*	0.092**	0.098**
	(0.030)	(0.024)	(0.022)
treated: 15–19 day		0.054**	0.061**
		(0.015)	(0.013)
female	-0.166**	-0.153**	-0.149**
	(0.017)	(0.015)	(0.014)
age	0.046**	0.046**	0.045**
	(0.003)	(0.003)	(0.003)
age squared	-0.001**	-0.001**	-0.001**
	(0.000)	(0.000)	(0.000)
tenure	0.006**	0.006**	0.005**
	(0.001)	(0.001)	(0.001)
tenure squared	-0.000**	-0.000**	-0.000*
	(0.000)	(0.000)	(0.000)
below min. wage 3	-0.120*	-0.119*	-0.095*
	(0.050)	(0.046)	(0.041)
below min. wage 6	0.019	0.020	0.020
	(0.056)	(0.056)	(0.056)
below min. wage 9	-0.249**	-0.221**	-0.230**
	(0.048)	(0.050)	(0.050)
below min. wage 12	-0.310**	-0.344**	-0.339**
	(0.035)	(0.020)	(0.020)
48+ hours	-0.008**	-0.008**	-0.007**
	(0.001)	(0.001)	(0.001)
base quarter +12	-0.001	-0.001	-0.004
	(0.005)	(0.004)	(0.004)
year = 1995	-0.244**	-0.232**	-0.222**
	(0.035)	(0.032)	(0.031)
year = 1996	-0.213**	-0.200**	-0.188**
	(0.036)	(0.034)	(0.033)
year = 1997	-0.175**	-0.163**	-0.154**
	(0.032)	(0.031)	(0.029)
year = 1998	-0.067*	-0.054	-0.044
	(0.031)	(0.030)	(0.028)
year = 1999	-0.033	-0.025	-0.018
	(0.033)	(0.031)	(0.029)
year = 2000		0.013	0.019
		(0.031)	(0.029)
year == 2001			0.078**
			(0.028)
full-time	-0.059**	-0.058**	-0.052**
	(0.012)	(0.012)	(0.011)
private company	0.042**	0.042**	0.039**
	(0.012)	(0.013)	(0.012)
married	0.028**	0.030**	0.027**
	(0.008)	(0.007)	(0.006)
foreign	0.031	0.037	0.027
	(0.027)	(0.024)	(0.021)

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... table 2.16 continued

	Aut. 1998	Aut. 1999	Aut. 2000
has kid(s)	0.015	0.016*	0.018**
	(0.008)	(0.007)	(0.006)
manager duties	0.096**	0.091**	0.089**
	(0.009)	(0.008)	(0.007)
education = 1	0.191**	0.198**	0.192**
	(0.028)	(0.028)	(0.028)
education = 2	0.086**	0.086**	0.083**
	(0.017)	(0.016)	(0.015)
education = 4	-0.032**	-0.034**	-0.038**
	(0.007)	(0.007)	(0.006)
education = 5	-0.085**	-0.085**	-0.087**
	(0.012)	(0.012)	(0.011)
education = 6	-0.122**	-0.117**	-0.115**
	(0.010)	(0.010)	(0.009)
education = 7	-0.074	-0.076*	-0.084**
	(0.039)	(0.034)	(0.030)
apprenticeship	-0.290**	-0.261**	-0.273**
	(0.030)	(0.031)	(0.034)
ever overtime	0.038**	0.037**	0.034**
	(0.008)	(0.007)	(0.007)
industry = 1	-0.177**	-0.162**	-0.167**
	(0.022)	(0.023)	(0.025)
industry = 2	0.145**	0.130**	0.133**
	(0.026)	(0.024)	(0.018)
industry = 4	0.166**	0.099*	0.076
	(0.051)	(0.041)	(0.045)
industry = 5	0.028	0.035	0.040
	(0.024)	(0.023)	(0.022)
industry = 6	-0.123**	-0.121**	-0.117**
	(0.012)	(0.012)	(0.011)
industry = 7	-0.199**	-0.191**	-0.187**
	(0.059)	(0.057)	(0.058)
industry = 8	-0.008	-0.010	-0.011
	(0.017)	(0.014)	(0.016)
industry = 9	0.048	0.053	0.054
	(0.033)	(0.034)	(0.029)
industry = 10	0.004	0.013	0.019
	(0.031)	(0.033)	(0.032)
industry = 11	-0.082**	-0.082**	-0.099**
	(0.022)	(0.021)	(0.019)
industry = 12	-0.100**	-0.099**	-0.102**
	(0.023)	(0.022)	(0.023)
industry = 13	-0.134**	-0.131**	-0.125**
	(0.030)	(0.031)	(0.030)
industry = 14	-0.220**	-0.194**	-0.184**
	(0.029)	(0.042)	(0.038)
industry = 15	-0.200**	-0.193**	-0.239**
	(0.036)	(0.038)	(0.016)
work arr. = 1	0.019	0.025*	0.026**
	(0.012)	(0.010)	(0.009)
work arr. = 2	0.010	0.011	0.004
	(0.021)	(0.017)	(0.017)
work arr. = 3	-0.053*	-0.053**	-0.048*
	(0.023)	(0.020)	(0.020)
work arr. = 4	-0.000	-0.017	0.005
	(0.030)	(0.024)	(0.028)
work arr. = 5	-0.014	-0.062	-0.062
	(0.107)	(0.094)	(0.094)
work arr. = 6	-0.059**	-0.065**	-0.057**

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... table 2.16 continued

	Aut. 1998	Aut. 1999	Aut. 2000
	(0.008)	(0.008)	(0.007)
work arr. = 8	0.015	0.005	0.002
	(0.040)	(0.039)	(0.038)
union member	0.083**	0.080**	0.082**
	(0.014)	(0.013)	(0.012)
6 days per week	-0.031*	-0.035**	-0.034*
	(0.014)	(0.013)	(0.013)
7 days per week	0.000	0.005	0.007
	(0.018)	(0.017)	(0.016)
region = 1	-0.116**	-0.119**	-0.121**
	(0.022)	(0.020)	(0.022)
region = 2	-0.148**	-0.135**	-0.136**
	(0.027)	(0.024)	(0.022)
region = 3	-0.105**	-0.111**	-0.117**
	(0.023)	(0.019)	(0.018)
region = 4	-0.131**	-0.119**	-0.117**
	(0.016)	(0.014)	(0.013)
region = 5	-0.136**	-0.133**	-0.133**
	(0.023)	(0.021)	(0.020)
region = 6	-0.110**	-0.107**	-0.107**
	(0.016)	(0.014)	(0.013)
region = 7	-0.094**	-0.087**	-0.087**
	(0.020)	(0.017)	(0.015)
region = 8	0.253**	0.251**	0.246**
	(0.029)	(0.027)	(0.028)
region = 9	0.098**	0.099**	0.099**
	(0.027)	(0.025)	(0.023)
region = 11	0.051*	0.052**	0.044**
	(0.020)	(0.018)	(0.016)
region = 13	-0.121**	-0.111**	-0.114**
	(0.017)	(0.014)	(0.013)
region = 14	-0.082**	-0.076**	-0.077**
	(0.016)	(0.016)	(0.015)
region = 15	-0.112**	-0.111**	-0.107**
	(0.017)	(0.014)	(0.012)
region = 16	-0.095**	-0.090**	-0.093**
	(0.016)	(0.014)	(0.014)
region = 17	-0.156**	-0.149**	-0.145**
	(0.026)	(0.021)	(0.019)
region = 18	-0.101**	-0.100**	-0.102**
	(0.017)	(0.016)	(0.014)
region = 19	-0.108**	-0.111**	-0.114**
	(0.018)	(0.017)	(0.016)
region = 20	-0.132**	-0.125**	-0.124**
	(0.018)	(0.016)	(0.015)
region = 21	-0.106**	-0.106**	-0.110**
	(0.016)	(0.015)	(0.013)
region = 22	-0.168**	-0.170**	-0.174**
	(0.034)	(0.030)	(0.027)
occ. = 2	0.019	0.026	0.026
	(0.052)	(0.056)	(0.054)
occ. = 3	-0.045	-0.052	-0.056
	(0.030)	(0.033)	(0.032)
occ. = 4	-0.184**	-0.202**	-0.212**
	(0.032)	(0.033)	(0.031)
occ. = 5	-0.221**	-0.231**	-0.238**
	(0.029)	(0.030)	(0.029)
occ. = 6	-0.271**	-0.284**	-0.286**
	(0.034)	(0.035)	(0.034)

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... table 2.16 continued

	Aut. 1998	Aut. 1999	Aut. 2000
occ. = 7	-0.271**	-0.271**	-0.276**
	(0.032)	(0.033)	(0.031)
occ. = 8	-0.300**	-0.306**	-0.314**
	(0.029)	(0.031)	(0.030)
occ. = 9	-0.328**	-0.336**	-0.344**
	(0.037)	(0.038)	(0.038)
occ. = 10	-0.471**	-0.479**	-0.469**
	(0.032)	(0.033)	(0.032)
size 11–19	-0.012	-0.014	-0.016
	(0.010)	(0.008)	(0.009)
size 20–24	-0.012	-0.012	-0.011
	(0.017)	(0.016)	(0.014)
size < 25	-0.046	-0.046*	-0.039
	(0.027)	(0.023)	(0.022)
size 25–49	-0.002	-0.001	0.003
	(0.010)	(0.010)	(0.010)
missing size	-0.013	0.005	-0.045
	(0.074)	(0.052)	(0.079)
intercept	1.385**	1.369**	1.377**
	(0.072)	(0.069)	(0.060)
Observations	18,389	22,546	26,737
R^2	0.473	0.490	0.506

Standard errors (clustered at the industry×occupation level) in parentheses.

Dependent variable: log hourly wage (surveyed only in entry and exit interview).

Data source: UK Labour Force Survey

Observations only from individuals that entered the LFS on or before Aut. 1998 (1st column), 1999 (2nd), 2000 (3rd).

Table 2.17: Robustness check excluding employees with less than 2 years of tenure: exit to non-employment—Full table

	without selection			with selection		
	Aut. 1998	Aut. 1999	Aut. 2000	Aut. 1998	Aut. 1999	Aut. 2000
base: no paid leave	1.430**	1.435**	1.426**	1.419**	1.427**	1.406**
	(0.074)	(0.076)	(0.072)	(0.086)	(0.089)	(0.086)
base: 1–14 days	0.564**	0.563**	0.561**	0.558**	0.560**	0.557**
	(0.071)	(0.070)	(0.067)	(0.077)	(0.076)	(0.072)
base: 15–19 day	0.179*	0.183*	0.186**	0.169*	0.180*	0.186*
	(0.072)	(0.072)	(0.071)	(0.082)	(0.082)	(0.083)
treated: no paid lve	-0.148	-0.047	0.059	-0.138	-0.016	0.087
	(0.150)	(0.134)	(0.122)	(0.171)	(0.156)	(0.147)
treated: 1–14 days	-0.347	0.074	0.127	-0.305	0.076	0.132
	(0.195)	(0.141)	(0.126)	(0.216)	(0.156)	(0.141)
treated: 15–19 day		0.036	0.106		-0.073	-0.036
		(0.142)	(0.119)		(0.157)	(0.120)
female	0.155**	0.163**	0.168**	0.116	0.122*	0.151*
	(0.047)	(0.047)	(0.048)	(0.064)	(0.060)	(0.062)
age	-0.145**	-0.149**	-0.153**	-0.110**	-0.114**	-0.120**
	(0.011)	(0.010)	(0.010)	(0.015)	(0.014)	(0.013)
age squared	0.002**	0.002**	0.002**	0.001**	0.001**	0.002**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
tenure	-0.041**	-0.041**	-0.038**	-0.034**	-0.038**	-0.034**
	(0.005)	(0.005)	(0.005)	(0.007)	(0.007)	(0.006)
tenure squared	0.001**	0.001**	0.001**	0.001**	0.001**	0.001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
below min. wage 3		0.060	0.263		-0.061	0.108
		(0.188)	(0.180)		(0.197)	(0.180)
below min. wage 6	0.024	0.013	0.005	0.193	0.183	0.175
	(0.256)	(0.258)	(0.260)	(0.286)	(0.284)	(0.288)
below min. wage 9	0.057	0.561	0.069	-0.005	0.483	0.105
	(0.233)	(0.780)	(0.317)	(0.264)	(0.785)	(0.361)
below min. wage 12		-0.580	-0.123		-0.557	-0.208
		(0.710)	(0.227)		(0.710)	(0.261)
48+ hours	0.008*	0.008**	0.009**	0.007*	0.007*	0.009**
	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
baseline haz. +6	0.038	-0.001	0.012	0.003	-0.067	-0.061
	(0.038)	(0.039)	(0.035)	(0.053)	(0.050)	(0.048)
baseline haz. +9	0.043	0.032	0.046	-0.032	-0.066	-0.066
	(0.040)	(0.034)	(0.034)	(0.052)	(0.048)	(0.046)
baseline haz. +12	-0.095	-0.072	-0.082	-0.154*	-0.160*	-0.175**
	(0.064)	(0.062)	(0.061)	(0.075)	(0.066)	(0.063)
year = 1995	-0.034	-0.035	-0.034	-0.072	-0.072	-0.069
	(0.050)	(0.050)	(0.049)	(0.056)	(0.056)	(0.055)
year = 1996	-0.065	-0.069	-0.068	-0.156**	-0.161**	-0.158**
	(0.045)	(0.045)	(0.046)	(0.055)	(0.056)	(0.056)
year = 1997	-0.163**	-0.165**	-0.165**	-0.170**	-0.172**	-0.171**
	(0.048)	(0.048)	(0.049)	(0.054)	(0.055)	(0.054)
year = 1998	-0.148*	-0.176**	-0.184**	-0.170*	-0.215**	-0.230**
	(0.060)	(0.063)	(0.063)	(0.076)	(0.079)	(0.078)
year = 1999		-0.174*	-0.201**		-0.187*	-0.220**
		(0.073)	(0.074)		(0.087)	(0.084)
year = 2000			-0.127			-0.142
			(0.065)			(0.089)
full-time	0.051	0.076	0.102*	0.095	0.122*	0.145**
	(0.056)	(0.051)	(0.049)	(0.064)	(0.056)	(0.055)
private company	-0.323**	-0.363**	-0.358**	-0.187	-0.221	-0.231*
	(0.083)	(0.079)	(0.080)	(0.133)	(0.120)	(0.112)
married	-0.010	0.001	0.011	-0.050	-0.031	-0.018

Continued on next page...

... table 2.17 continued

	without selection			with selection		
	Aut. 1998	Aut. 1999	Aut. 2000	Aut. 1998	Aut. 1999	Aut. 2000
	(0.055)	(0.052)	(0.044)	(0.065)	(0.058)	(0.052)
foreign	0.212*	0.158*	0.163*	0.201*	0.151	0.175*
	(0.085)	(0.077)	(0.077)	(0.090)	(0.080)	(0.082)
has kid(s)	0.171**	0.154**	0.145**	0.184**	0.158**	0.167**
	(0.046)	(0.039)	(0.036)	(0.061)	(0.051)	(0.045)
manager duties	-0.004	-0.004	-0.001	-0.015	-0.010	-0.025
	(0.046)	(0.041)	(0.038)	(0.060)	(0.050)	(0.050)
education = 1	-0.121*	-0.067	-0.022	-0.247	-0.128	-0.023
	(0.053)	(0.052)	(0.047)	(0.135)	(0.104)	(0.091)
education = 2	-0.139	-0.151*	-0.120	-0.128	-0.182	-0.135
	(0.078)	(0.075)	(0.071)	(0.101)	(0.105)	(0.100)
education = 4	0.028	0.044	0.038	-0.135*	-0.110*	-0.084
	(0.049)	(0.043)	(0.042)	(0.062)	(0.051)	(0.050)
education = 5	-0.006	0.005	0.028	-0.073	-0.051	-0.024
	(0.056)	(0.050)	(0.045)	(0.070)	(0.070)	(0.064)
education = 6	0.156**	0.163**	0.158**	0.143**	0.163**	0.173**
	(0.044)	(0.043)	(0.044)	(0.054)	(0.056)	(0.054)
education = 7	-0.466*	-0.252	-0.178	-0.423	-0.286	-0.142
	(0.222)	(0.254)	(0.212)	(0.262)	(0.267)	(0.212)
apprenticeship	-0.274	-0.219	-0.277	-0.294	-0.237	-0.301
	(0.248)	(0.228)	(0.179)	(0.262)	(0.234)	(0.179)
ever overtime	-0.392**	-0.377**	-0.397**	-0.358**	-0.340**	-0.366**
	(0.038)	(0.035)	(0.036)	(0.063)	(0.055)	(0.053)
industry = 1	-0.029	-0.112	-0.174	0.022	-0.052	-0.119
	(0.108)	(0.118)	(0.138)	(0.137)	(0.151)	(0.177)
industry = 2	-0.055	-0.144	-0.160	-0.304	-0.326	-0.293
	(0.197)	(0.195)	(0.174)	(0.223)	(0.216)	(0.177)
industry = 4	0.465*	0.434*	0.369*	0.200	0.030	0.153
	(0.199)	(0.187)	(0.158)	(0.457)	(0.440)	(0.284)
industry = 5	0.378**	0.289**	0.221*	0.363**	0.286**	0.238*
	(0.099)	(0.104)	(0.102)	(0.107)	(0.109)	(0.099)
industry = 6	0.014	-0.050	-0.094**	-0.050	-0.109**	-0.130**
	(0.043)	(0.032)	(0.033)	(0.043)	(0.035)	(0.040)
industry = 7	0.091	0.049	-0.007	0.002	-0.022	-0.064
	(0.119)	(0.110)	(0.096)	(0.112)	(0.107)	(0.096)
industry = 8	0.033	-0.025	-0.046	0.029	0.005	0.021
	(0.075)	(0.069)	(0.076)	(0.078)	(0.064)	(0.048)
industry = 9	0.104	0.076	0.079	-0.055	0.022	0.032
	(0.069)	(0.060)	(0.062)	(0.217)	(0.175)	(0.175)
industry = 10	0.090	0.092	0.040	0.043	0.054	-0.004
	(0.070)	(0.067)	(0.066)	(0.075)	(0.070)	(0.066)
industry = 11	0.100	0.079	0.009	-0.088	-0.087	-0.135
	(0.096)	(0.089)	(0.090)	(0.221)	(0.206)	(0.174)
industry = 12	-0.069	-0.087	-0.131	-0.069	-0.076	-0.147
	(0.090)	(0.083)	(0.072)	(0.096)	(0.093)	(0.081)
industry = 13	0.131	0.124	0.106	0.043	0.034	0.050
	(0.082)	(0.071)	(0.059)	(0.068)	(0.064)	(0.055)
industry = 14	0.159	0.185	0.096	0.216	0.240	0.144
	(0.234)	(0.181)	(0.130)	(0.193)	(0.149)	(0.113)
industry = 15	-0.386**	-0.507**	-0.638**			
	(0.067)	(0.056)	(0.048)			
work arr. = 1	-0.064	-0.090	-0.079	-0.082	-0.117	-0.057
	(0.060)	(0.054)	(0.051)	(0.104)	(0.096)	(0.091)
work arr. = 2	-0.022	-0.110	-0.095	0.065	-0.031	-0.056
	(0.103)	(0.097)	(0.090)	(0.141)	(0.140)	(0.127)
work arr. = 3	-0.330	-0.343	-0.405*	-0.561**	-0.552**	-0.593**
	(0.205)	(0.195)	(0.177)	(0.187)	(0.172)	(0.160)
work arr. = 4	-0.453	-0.593	-0.422	-0.556	-0.702	-0.482

Continued on next page...

... table 2.17 continued

	without selection			with selection		
	Aut. 1998	Aut. 1999	Aut. 2000	Aut. 1998	Aut. 1999	Aut. 2000
	(0.370)	(0.387)	(0.331)	(0.410)	(0.426)	(0.354)
work arr. = 5	0.277	0.172	0.093	0.616	0.476	0.352
	(0.197)	(0.202)	(0.198)	(0.329)	(0.322)	(0.324)
work arr. = 6	0.004	-0.013	-0.030	0.054	0.046	0.016
	(0.074)	(0.081)	(0.083)	(0.141)	(0.143)	(0.145)
work arr. = 8	0.119	0.215	0.177	0.067	0.122	0.084
	(0.248)	(0.187)	(0.187)	(0.291)	(0.223)	(0.224)
union member	-0.057	-0.049	-0.079*	-0.007	0.003	-0.029
	(0.039)	(0.037)	(0.036)	(0.063)	(0.054)	(0.049)
6 days per week	-0.072	-0.061	-0.071	-0.076	-0.063	-0.069
	(0.054)	(0.049)	(0.051)	(0.056)	(0.054)	(0.054)
7 days per week	0.062	0.048	0.073	0.082	0.053	0.074
	(0.072)	(0.068)	(0.061)	(0.077)	(0.075)	(0.069)
region = 1	0.123	0.076	0.098	0.078	0.001	0.021
	(0.114)	(0.098)	(0.086)	(0.154)	(0.141)	(0.109)
region = 2	-0.144	-0.172	-0.124	-0.112	-0.126	-0.113
	(0.113)	(0.111)	(0.102)	(0.168)	(0.166)	(0.162)
region = 3	-0.010	-0.005	-0.042	-0.091	-0.049	-0.096
	(0.115)	(0.090)	(0.079)	(0.152)	(0.126)	(0.111)
region = 4	-0.058	-0.074	-0.086	-0.026	-0.085	-0.127
	(0.105)	(0.090)	(0.086)	(0.123)	(0.112)	(0.105)
region = 5	0.009	-0.034	-0.046	-0.007	-0.007	-0.036
	(0.101)	(0.096)	(0.093)	(0.125)	(0.115)	(0.106)
region = 6	-0.103	-0.109	-0.086	-0.085	-0.088	-0.080
	(0.071)	(0.066)	(0.069)	(0.090)	(0.088)	(0.091)
region = 7	-0.096	-0.140	-0.163	-0.148	-0.172	-0.218*
	(0.111)	(0.097)	(0.095)	(0.118)	(0.104)	(0.094)
region = 8	0.189*	0.159*	0.137	0.268	0.212	0.207
	(0.086)	(0.077)	(0.072)	(0.182)	(0.164)	(0.146)
region = 9	0.227	0.227	0.181	0.232	0.214	0.150
	(0.133)	(0.121)	(0.110)	(0.165)	(0.157)	(0.144)
region = 11	0.033	0.048	-0.030	0.064	0.072	-0.019
	(0.093)	(0.084)	(0.076)	(0.100)	(0.090)	(0.084)
region = 13	-0.003	-0.002	-0.027	-0.014	-0.012	-0.052
	(0.067)	(0.062)	(0.059)	(0.086)	(0.089)	(0.077)
region = 14	-0.119	-0.124*	-0.128*	-0.115	-0.110	-0.139*
	(0.065)	(0.063)	(0.056)	(0.076)	(0.069)	(0.063)
region = 15	-0.204*	-0.137	-0.166*	-0.204	-0.160	-0.194*
	(0.090)	(0.076)	(0.069)	(0.111)	(0.102)	(0.089)
region = 16	0.107	0.098	0.065	0.126	0.135	0.067
	(0.087)	(0.084)	(0.078)	(0.111)	(0.108)	(0.106)
region = 17	-0.033	0.004	0.005	-0.432	-0.377	-0.355
	(0.143)	(0.134)	(0.124)	(0.233)	(0.215)	(0.191)
region = 18	-0.059	-0.042	-0.078	-0.077	-0.050	-0.104
	(0.085)	(0.083)	(0.076)	(0.100)	(0.086)	(0.085)
region = 19	-0.140	-0.113	-0.102	-0.179	-0.157	-0.166
	(0.097)	(0.092)	(0.090)	(0.136)	(0.133)	(0.123)
region = 20	-0.108	-0.113	-0.093	-0.176	-0.134	-0.143
	(0.092)	(0.092)	(0.087)	(0.119)	(0.106)	(0.091)
region = 21	-0.042	-0.058	-0.076	-0.054	-0.047	-0.088
	(0.080)	(0.073)	(0.068)	(0.096)	(0.086)	(0.081)
region = 22	-0.068	-0.074	-0.110	-0.050	-0.057	-0.147
	(0.108)	(0.110)	(0.093)	(0.120)	(0.128)	(0.113)
occ. = 2	0.007	0.003	-0.018	-0.054	-0.059	-0.046
	(0.079)	(0.077)	(0.083)	(0.112)	(0.118)	(0.126)
occ. = 3	-0.076	-0.081	-0.047	-0.039	-0.051	-0.046
	(0.063)	(0.060)	(0.052)	(0.090)	(0.093)	(0.080)
occ. = 4	-0.276**	-0.278**	-0.290**	-0.296**	-0.299**	-0.339**

Continued on next page...

... table 2.17 continued

	without selection			with selection		
	Aut. 1998	Aut. 1999	Aut. 2000	Aut. 1998	Aut. 1999	Aut. 2000
	(0.078)	(0.070)	(0.064)	(0.112)	(0.099)	(0.090)
occ. = 5	-0.088	-0.044	-0.041	-0.129	-0.075	-0.052
	(0.074)	(0.072)	(0.069)	(0.100)	(0.099)	(0.095)
occ. = 6	-0.159	-0.186	-0.193*	-0.302*	-0.329**	-0.329**
	(0.124)	(0.108)	(0.098)	(0.121)	(0.118)	(0.103)
occ. = 7	-0.140	-0.122	-0.118	-0.155	-0.130	-0.164*
	(0.080)	(0.070)	(0.070)	(0.092)	(0.083)	(0.079)
occ. = 8	-0.090	-0.102	-0.085	-0.160	-0.176*	-0.161*
	(0.064)	(0.058)	(0.054)	(0.092)	(0.086)	(0.079)
occ. = 9	-0.103	-0.108	-0.100	-0.121	-0.127	-0.136
	(0.078)	(0.072)	(0.068)	(0.096)	(0.090)	(0.083)
occ. = 10	-0.481**	-0.088	-0.070	-0.282**	-0.285**	-0.278**
	(0.066)	(0.061)	(0.056)	(0.097)	(0.093)	(0.088)
size 11–19	-0.022	-0.085	-0.083	-0.051	-0.124	-0.114
	(0.070)	(0.065)	(0.059)	(0.089)	(0.082)	(0.073)
size 20–24	-0.158*	-0.155*	-0.134	-0.225*	-0.204	-0.202*
	(0.079)	(0.077)	(0.074)	(0.107)	(0.106)	(0.099)
size < 25	0.062	0.017	-0.009	0.039	-0.015	-0.028
	(0.150)	(0.133)	(0.128)	(0.164)	(0.139)	(0.126)
size 25–49	-0.059	-0.036	-0.039	-0.111	-0.100	-0.115
	(0.052)	(0.047)	(0.045)	(0.068)	(0.062)	(0.065)
missing size	0.163	0.057	-0.035	0.182	0.064	-0.034
	(0.343)	(0.328)	(0.313)	(0.399)	(0.380)	(0.368)
baseline haz. +3	-1.399**	-1.302**	-1.172**	-1.644**	-1.532**	-1.406**
	(0.204)	(0.194)	(0.176)	(0.243)	(0.235)	(0.218)
Observations	236,106	278,953	319,148	102,552	121,790	139,467

Standard errors (clustered at the industry×occupation level) in parentheses.

Dependent variable: exit from dependent employment (upper panel); log hourly wage (surveyed only in entry and exit interview, lower panel).

Data source: UK Labour Force Survey

Observations only from individuals that entered the LFS on or before Aut. 1998 (1st/4th column), 1999 (2nd/5th), 2000 (3rd/6th).

Table 2.18: Robustness check excluding employees with less than 2 years of tenure: wage regression—Full table

	without selection			with selection		
	Aut. 1998	Aut. 1999	Aut. 2000	Aut. 1998	Aut. 1999	Aut. 2000
base: no paid leave	-0.134**	-0.143**	-0.146**	-0.107**	-0.115**	-0.120**
	(0.033)	(0.033)	(0.033)	(0.032)	(0.032)	(0.032)
base: 1–14 days	-0.160**	-0.166**	-0.169**	-0.134**	-0.139**	-0.143**
	(0.018)	(0.018)	(0.018)	(0.016)	(0.015)	(0.015)
base: 15–19 day	-0.124**	-0.126**	-0.128**	-0.112**	-0.110**	-0.112**
	(0.017)	(0.017)	(0.017)	(0.016)	(0.016)	(0.016)
treated: no paid lve	0.125**	0.179**	0.165**	0.080	0.132**	0.127**
	(0.039)	(0.039)	(0.037)	(0.040)	(0.039)	(0.040)
treated: 1–14 days	0.099**	0.140**	0.147**	0.057	0.095**	0.106**
	(0.036)	(0.028)	(0.027)	(0.036)	(0.028)	(0.027)
treated: 15–19 day		0.069**	0.068**		0.063**	0.062**
		(0.019)	(0.019)		(0.019)	(0.018)
female	-0.171**	-0.161**	-0.156**	-0.179**	-0.163**	-0.158**
	(0.015)	(0.015)	(0.014)	(0.018)	(0.017)	(0.016)
age	0.038**	0.038**	0.038**	0.041**	0.040**	0.040**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
age squared	-0.000**	-0.000**	-0.000**	-0.000**	-0.000**	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
tenure	0.010**	0.009**	0.009**	0.007**	0.006**	0.005**
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
tenure squared	-0.000**	-0.000**	-0.000**	-0.000**	-0.000**	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
below min. wage 3	-0.182**	-0.195**	-0.135*	-0.162**	-0.153**	-0.120*
	(0.061)	(0.059)	(0.055)	(0.058)	(0.054)	(0.051)
below min. wage 6	0.047	0.047	0.047	0.028	0.030	0.030
	(0.076)	(0.076)	(0.076)	(0.071)	(0.070)	(0.070)
below min. wage 9	-0.340**	-0.329**	-0.334**	-0.267**	-0.230**	-0.243**
	(0.079)	(0.079)	(0.080)	(0.058)	(0.059)	(0.061)
below min. wage 12	-0.358**	-0.378**	-0.375**	-0.302**	-0.349**	-0.341**
	(0.050)	(0.026)	(0.026)	(0.038)	(0.023)	(0.022)
48+ hours	-0.006**	-0.006**	-0.005**	-0.009**	-0.008**	-0.008**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
base quarter +12	-0.002	-0.006	-0.008*	-0.005	-0.007	-0.010*
	(0.004)	(0.004)	(0.003)	(0.005)	(0.005)	(0.004)
year = 1995	-0.441**	-0.420**	-0.404*	-0.272**	-0.259**	-0.241**
	(0.157)	(0.158)	(0.159)	(0.038)	(0.036)	(0.035)
year = 1996	-0.400*	-0.379*	-0.363*	-0.242**	-0.228**	-0.209**
	(0.158)	(0.159)	(0.160)	(0.039)	(0.038)	(0.036)
year = 1997	-0.361*	-0.343*	-0.328*	-0.203**	-0.190**	-0.175**
	(0.160)	(0.161)	(0.162)	(0.036)	(0.035)	(0.033)
year = 1998	-0.284	-0.266	-0.251	-0.087*	-0.073*	-0.057
	(0.159)	(0.160)	(0.161)	(0.035)	(0.034)	(0.031)
year = 1999	-0.260	-0.243	-0.231	-0.061	-0.053	-0.040
	(0.159)	(0.160)	(0.161)	(0.035)	(0.034)	(0.031)
year = 2000		-0.204	-0.191		-0.010	0.003
		(0.160)	(0.161)		(0.036)	(0.032)
year == 2001			-0.131			0.053
			(0.164)			(0.031)
full-time	-0.039**	-0.039**	-0.038**	-0.058**	-0.060**	-0.054**
	(0.011)	(0.010)	(0.010)	(0.013)	(0.012)	(0.011)
private company	-0.001	-0.007	-0.009	0.045**	0.047**	0.040**
	(0.013)	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)
married	0.024**	0.025**	0.025**	0.024**	0.025**	0.023**
	(0.005)	(0.005)	(0.004)	(0.008)	(0.007)	(0.006)
foreign	0.027	0.032	0.026	0.036	0.042	0.031

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... table 2.18 continued

	without selection			with selection		
	Aut. 1998	Aut. 1999	Aut. 2000	Aut. 1998	Aut. 1999	Aut. 2000
	(0.027)	(0.024)	(0.021)	(0.036)	(0.032)	(0.027)
has kid(s)	0.033**	0.034**	0.033**	0.019*	0.024**	0.026**
	(0.006)	(0.005)	(0.005)	(0.008)	(0.007)	(0.007)
manager duties	0.101**	0.099**	0.099**	0.087**	0.083**	0.083**
	(0.008)	(0.008)	(0.007)	(0.010)	(0.008)	(0.008)
education = 1	0.280**	0.279**	0.279**	0.222**	0.230**	0.219**
	(0.019)	(0.020)	(0.019)	(0.026)	(0.027)	(0.024)
education = 2	0.099**	0.100**	0.099**	0.090**	0.094**	0.095**
	(0.015)	(0.015)	(0.014)	(0.020)	(0.019)	(0.017)
education = 4	-0.031**	-0.032**	-0.032**	-0.031**	-0.032**	-0.032**
	(0.006)	(0.006)	(0.006)	(0.009)	(0.008)	(0.007)
education = 5	-0.096**	-0.097**	-0.098**	-0.085**	-0.083**	-0.084**
	(0.010)	(0.009)	(0.008)	(0.011)	(0.011)	(0.009)
education = 6	-0.142**	-0.138**	-0.136**	-0.120**	-0.114**	-0.111**
	(0.011)	(0.011)	(0.010)	(0.012)	(0.011)	(0.010)
education = 7	-0.063*	-0.080**	-0.085**	-0.092*	-0.097**	-0.105**
	(0.030)	(0.029)	(0.026)	(0.036)	(0.033)	(0.027)
apprenticeship	-0.200**	-0.175**	-0.176**	-0.257**	-0.232**	-0.233**
	(0.043)	(0.042)	(0.038)	(0.047)	(0.048)	(0.043)
ever overtime	0.042**	0.043**	0.040**	0.040**	0.039**	0.035**
	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.007)
industry = 1	-0.202**	-0.194**	-0.201**	-0.191**	-0.172**	-0.178**
	(0.026)	(0.032)	(0.032)	(0.021)	(0.023)	(0.025)
industry = 2	0.140**	0.125**	0.127**	0.139**	0.122**	0.129**
	(0.019)	(0.019)	(0.015)	(0.026)	(0.027)	(0.018)
industry = 4	0.098**	0.088**	0.074**	0.181**	0.134**	0.099*
	(0.021)	(0.021)	(0.022)	(0.047)	(0.044)	(0.045)
industry = 5	-0.038*	-0.025	-0.018	0.022	0.032	0.037
	(0.019)	(0.019)	(0.017)	(0.023)	(0.022)	(0.021)
industry = 6	-0.152**	-0.148**	-0.140**	-0.130**	-0.129**	-0.123**
	(0.019)	(0.019)	(0.016)	(0.013)	(0.012)	(0.012)
industry = 7	-0.212**	-0.200**	-0.203**	-0.200**	-0.190**	-0.192**
	(0.076)	(0.075)	(0.075)	(0.057)	(0.057)	(0.057)
industry = 8	0.002	0.006	0.004	-0.000	-0.002	-0.005
	(0.016)	(0.015)	(0.015)	(0.014)	(0.012)	(0.016)
industry = 9	0.118**	0.122**	0.117**	0.048	0.052	0.046
	(0.039)	(0.036)	(0.033)	(0.047)	(0.046)	(0.041)
industry = 10	0.003	0.014	0.017	0.002	0.015	0.020
	(0.024)	(0.024)	(0.024)	(0.033)	(0.035)	(0.034)
industry = 11	-0.006	-0.002	-0.004	-0.068	-0.074*	-0.093**
	(0.022)	(0.022)	(0.021)	(0.035)	(0.033)	(0.027)
industry = 12	-0.068**	-0.061*	-0.060*	-0.103**	-0.100**	-0.103**
	(0.025)	(0.024)	(0.023)	(0.027)	(0.025)	(0.024)
industry = 13	-0.141**	-0.135**	-0.134**	-0.142**	-0.135**	-0.127**
	(0.041)	(0.042)	(0.042)	(0.035)	(0.036)	(0.034)
industry = 14	-0.161**	-0.138**	-0.145**	-0.161**	-0.152**	-0.145**
	(0.051)	(0.051)	(0.055)	(0.045)	(0.055)	(0.053)
industry = 15	0.053**	0.079**	0.025	-0.154**	-0.137**	-0.185**
	(0.017)	(0.017)	(0.015)	(0.026)	(0.024)	(0.018)
work arr. = 1	-0.010	-0.011	-0.015	0.027	0.032*	0.032*
	(0.013)	(0.013)	(0.013)	(0.017)	(0.015)	(0.013)
work arr. = 2	0.007	0.007	-0.000	-0.001	-0.001	-0.007
	(0.017)	(0.015)	(0.014)	(0.025)	(0.019)	(0.020)
work arr. = 3	-0.066	-0.063*	-0.057	-0.057*	-0.062**	-0.055**
	(0.033)	(0.029)	(0.030)	(0.023)	(0.019)	(0.019)
work arr. = 4	0.037	0.028	0.046	0.017	-0.008	0.020
	(0.035)	(0.034)	(0.035)	(0.038)	(0.027)	(0.031)
work arr. = 5	0.006	0.016	0.016	-0.032	-0.083	-0.080

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... table 2.18 continued

	without selection			with selection		
	Aut. 1998	Aut. 1999	Aut. 2000	Aut. 1998	Aut. 1999	Aut. 2000
	(0.037)	(0.034)	(0.033)	(0.141)	(0.119)	(0.114)
work arr. = 6	-0.064**	-0.062**	-0.057**	-0.064**	-0.066**	-0.061**
	(0.009)	(0.009)	(0.008)	(0.007)	(0.008)	(0.008)
work arr. = 8	-0.033	-0.037	-0.033	-0.009	-0.022	-0.022
	(0.038)	(0.037)	(0.036)	(0.045)	(0.045)	(0.044)
union member	0.023	0.020	0.023	0.084**	0.080**	0.084**
	(0.015)	(0.014)	(0.014)	(0.014)	(0.013)	(0.012)
6 days per week	-0.048**	-0.055**	-0.057**	-0.035*	-0.038**	-0.035**
	(0.013)	(0.013)	(0.014)	(0.014)	(0.013)	(0.013)
7 days per week	-0.030	-0.022	-0.021	-0.007	0.002	0.005
	(0.020)	(0.019)	(0.018)	(0.021)	(0.019)	(0.018)
region = 1	-0.124**	-0.124**	-0.126**	-0.119**	-0.123**	-0.126**
	(0.017)	(0.015)	(0.015)	(0.023)	(0.022)	(0.023)
region = 2	-0.113**	-0.111**	-0.115**	-0.158**	-0.146**	-0.150**
	(0.021)	(0.019)	(0.018)	(0.029)	(0.025)	(0.023)
region = 3	-0.087**	-0.094**	-0.103**	-0.088**	-0.098**	-0.103**
	(0.016)	(0.014)	(0.014)	(0.020)	(0.016)	(0.016)
region = 4	-0.106**	-0.104**	-0.103**	-0.144**	-0.132**	-0.126**
	(0.014)	(0.013)	(0.013)	(0.021)	(0.018)	(0.016)
region = 5	-0.120**	-0.124**	-0.126**	-0.139**	-0.135**	-0.136**
	(0.017)	(0.015)	(0.014)	(0.024)	(0.021)	(0.020)
region = 6	-0.114**	-0.109**	-0.108**	-0.110**	-0.106**	-0.108**
	(0.015)	(0.014)	(0.013)	(0.018)	(0.015)	(0.014)
region = 7	-0.096**	-0.088**	-0.090**	-0.106**	-0.095**	-0.099**
	(0.015)	(0.014)	(0.013)	(0.024)	(0.021)	(0.018)
region = 8	0.268**	0.273**	0.269**	0.242**	0.243**	0.228**
	(0.023)	(0.022)	(0.020)	(0.036)	(0.032)	(0.032)
region = 9	0.128**	0.129**	0.127**	0.102**	0.104**	0.102**
	(0.019)	(0.018)	(0.017)	(0.029)	(0.026)	(0.025)
region = 11	0.060**	0.062**	0.057**	0.042	0.047*	0.043*
	(0.016)	(0.014)	(0.013)	(0.022)	(0.019)	(0.017)
region = 13	-0.096**	-0.089**	-0.094**	-0.115**	-0.105**	-0.110**
	(0.013)	(0.012)	(0.012)	(0.020)	(0.017)	(0.015)
region = 14	-0.070**	-0.069**	-0.073**	-0.087**	-0.076**	-0.077**
	(0.013)	(0.013)	(0.012)	(0.018)	(0.018)	(0.017)
region = 15	-0.107**	-0.104**	-0.106**	-0.112**	-0.114**	-0.112**
	(0.013)	(0.013)	(0.012)	(0.017)	(0.015)	(0.013)
region = 16	-0.091**	-0.090**	-0.096**	-0.102**	-0.095**	-0.102**
	(0.011)	(0.010)	(0.010)	(0.018)	(0.017)	(0.015)
region = 17	-0.109**	-0.108**	-0.106**	-0.155**	-0.148**	-0.143**
	(0.017)	(0.016)	(0.015)	(0.031)	(0.026)	(0.022)
region = 18	-0.111**	-0.106**	-0.109**	-0.115**	-0.111**	-0.116**
	(0.015)	(0.013)	(0.012)	(0.021)	(0.018)	(0.016)
region = 19	-0.124**	-0.123**	-0.127**	-0.117**	-0.118**	-0.121**
	(0.015)	(0.015)	(0.015)	(0.020)	(0.019)	(0.018)
region = 20	-0.110**	-0.111**	-0.114**	-0.134**	-0.128**	-0.131**
	(0.015)	(0.013)	(0.012)	(0.022)	(0.020)	(0.018)
region = 21	-0.108**	-0.107**	-0.111**	-0.098**	-0.101**	-0.108**
	(0.012)	(0.011)	(0.010)	(0.017)	(0.016)	(0.015)
region = 22	-0.148**	-0.150**	-0.155**	-0.183**	-0.186**	-0.183**
	(0.026)	(0.024)	(0.023)	(0.036)	(0.031)	(0.029)
occ. = 2	-0.013	-0.014	-0.015	0.001	0.013	0.015
	(0.029)	(0.028)	(0.028)	(0.060)	(0.064)	(0.062)
occ. = 3	-0.086**	-0.088**	-0.097**	-0.042	-0.048	-0.052
	(0.020)	(0.020)	(0.019)	(0.031)	(0.033)	(0.032)
occ. = 4	-0.242**	-0.251**	-0.261**	-0.173**	-0.189**	-0.201**
	(0.024)	(0.023)	(0.021)	(0.034)	(0.035)	(0.033)
occ. = 5	-0.275**	-0.285**	-0.291**	-0.220**	-0.232**	-0.239**

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... table 2.18 continued

	without selection			with selection		
	Aut. 1998	Aut. 1999	Aut. 2000	Aut. 1998	Aut. 1999	Aut. 2000
	(0.021)	(0.021)	(0.020)	(0.029)	(0.031)	(0.030)
occ. = 6	-0.311**	-0.314**	-0.324**	-0.266**	-0.271**	-0.273**
	(0.025)	(0.025)	(0.024)	(0.035)	(0.037)	(0.035)
occ. = 7	-0.318**	-0.311**	-0.316**	-0.280**	-0.272**	-0.277**
	(0.036)	(0.035)	(0.030)	(0.032)	(0.033)	(0.031)
occ. = 8	-0.334**	-0.336**	-0.344**	-0.297**	-0.299**	-0.306**
	(0.024)	(0.024)	(0.023)	(0.030)	(0.032)	(0.031)
occ. = 9	-0.373**	-0.374**	-0.379**	-0.327**	-0.331**	-0.335**
	(0.028)	(0.028)	(0.028)	(0.037)	(0.038)	(0.037)
occ. = 10	-0.340**	-0.340**	-0.340**	-0.465**	-0.469**	-0.470**
	(0.020)	(0.021)	(0.020)	(0.032)	(0.034)	(0.034)
size 11–19	-0.031**	-0.032**	-0.036**	-0.007	-0.010	-0.011
	(0.008)	(0.008)	(0.008)	(0.011)	(0.009)	(0.009)
size 20–24	-0.022	-0.023	-0.022	-0.009	-0.009	-0.005
	(0.014)	(0.013)	(0.012)	(0.019)	(0.018)	(0.017)
size $<$ 25	-0.056	-0.055*	-0.057*	-0.036	-0.040	-0.041
	(0.029)	(0.024)	(0.024)	(0.031)	(0.026)	(0.026)
size 25–49	-0.007	-0.008	-0.009	0.006	0.007	0.009
	(0.010)	(0.009)	(0.009)	(0.012)	(0.012)	(0.011)
missing size	-0.170	-0.097	-0.167	-0.092	-0.033	-0.130
	(0.144)	(0.107)	(0.118)	(0.092)	(0.075)	(0.113)
intercept	1.764**	1.746**	1.744**	1.511**	1.505**	1.508**
	(0.195)	(0.195)	(0.192)	(0.077)	(0.075)	(0.066)
Observations	30,273	36,983	43,452	13,764	16,831	19,804
R^2	0.574	0.583	0.594	0.456	0.472	0.489

Standard errors (clustered at the industry×occupation level) in parentheses.

Dependent variable: exit from dependent employment (upper panel); log hourly wage (surveyed only in entry and exit interview, lower panel).

Data source: UK Labour Force Survey

Observations only from individuals that entered the LFS on or before Aut. 1998 (1st/4th column), 1999 (2nd/5th), 2000 (3rd/6th).

Chapter 3

Home Computers and Married Women's Labor Supply.

3.1 Introduction

Personal computers have dramatically altered the workplace. Tasks in existing occupations changed and new occupations emerged. Computers also entered our homes. The OECD (2010) stresses that access to digital infrastructure and computer literacy, along with the ability to use a computer productively, are crucial for the development of future generations. Basic computer skills are an integral part of skill training provided by temporary help agencies (Autor 2001), in some countries they are a major part of active labor market policies (e.g. for Germany, Fitzenberger and Speckesser 2007).

The impact of computerization of the workplace has been of interest in numerous studies. Krueger (1993) finds large wage premia for computer use at work, but DiNardo and Pischke (1997) raise concerns that the premia are driven by unobserved skill differences. They show wage premia for the use of pencils and other office material used by white collar workers similar to the wage premia for computer use. In a more recent study Spitz-Oener (2008) shows that, while computer use has similar wage effects as other office materials, only computer use is associated with shifts in the tasks employees perform and therefore likely to drive productivity increases. Autor, Katz, and Krueger (1998) find that skill upgrading (i.e. increased demand for highly educated workers) in industries increases (strongly) with computer utilization. Using decomposition methods on industry and occupation aggregated data, Weinberg (2000) finds that more than half of the growth in female employment from the mid-70s to the mid-90s can be attributed to increases in computer use. Several authors point out that computers by themselves do not have an impact but rather that it is the interaction of computers with skilled users (Black and Spitz-Oener 2010) and organizational procedures (Bresnahan 1999, Garicano and Heaton 2010) that result in productivity increases. Instead of focussing on workers, Malamud and Pop-Eleches (2011) consider the impact of home computer use on children's development. They use school grades and test scores from cognitive tests as measures of human capital and find that cognitive skills improve with computer use, while school grades suffer.

Having access to a computer at home opens up the opportunity to acquire and improve computer skills at low additional cost. This can be a great advantage, especially if a person is not in employment and does not receive on-the-job training. Those skills can range from the basic, e.g. touch typing and word processing, to more advanced uses, like spreadsheets, databases and programming.

In this paper I contribute to the literature by shifting the focus to adult computer skills and the impact of having the ability to acquire job relevant skills at home. In particular, I am investigating whether the availability and the use of a computer at home changes married women's labor supply. Women, especially married women, have lower participation rates and higher labor supply elasticities than men (Killingsworth and Heckman 1987). Married women have been the major force behind women's labor supply changes (Blau and Kahn 2007) and reentry into the market after a period of caring for children might be facilitated by the ability to acquire new skills at home. Women also have a comparative advantage in non-physically intensive skills, skills for which computerization of the workplace increased demand (Weinberg 2000). The focus on married women limits the external validity as labor market behavior of men and single women differs, but married women's importance in the changes in women's labor supply (Blau and Kahn 2007) justifies a separate analysis.

The paper proceeds as follows, first I present a static model of labor supply to motivate the empirical specifications and evaluate the potential sources and directions of omitted variable bias. The model shows that it is crucial to control for offer wages and non-labor income when estimating the impact of home computers on employment. The model predicts that without wage (income) controls the impact of home computers is overestimated (underestimated).

For the estimation I use data from the U.S. Current Population Survey, which includes questions on home and work computer use in several supplements starting from 1984. The data is discussed in section 3.3 followed by descriptive evidence for the change in computer use and employment over time. The descriptive results show both an increase in computer availability and employment over time.

Section 3.5 discusses the empirical strategy followed by the estimation results in the next section. Employment probabilities are higher for women who have access to a computer at home. Employment shares are about 3 percentage points higher in the late 80s and 90s, and 6 percentage points in the early 2000s. To establish that the employment effect is indeed driven by computer skills, the following two sections establish that having a computer at home is associated with higher wages, and employment in more computer intensive occupations. The final section concludes.

3.2 Theory

I consider a static labor supply model[1] to guide the empirical implementation and help to determine the source and direction of bias in the estimates. I simplify the household labor supply decision by assuming a sequential structure. The wife decides on her labor supply only after the husband's choice. In the model the wife chooses consumption C and leisure L to maximize her utility $U(.)$, subject to the budget constraint. I augment the standard model by introducing a third choice variable PC, the demand for computers.[2] I treat the demand for computers as continuous and solely as the woman's choice.[3] The computer has two functions in the model. First, it is a consumption good and owning a computer provides utility. Second, it is an investment good that increases the wage rate.

Having a computer at home offers women the opportunity to (cheaply) acquire computer skills. These skills can range from simple (touch) typing skills to complex programming or network administration skills. If these skills are valued in the market, they raise wages directly. Indirectly the wage is affected by lowering search costs. For example, writing and changing cover letters and resumes is simplified by being able to store digital copies. With the advent of internet based employment websites in the mid- to late 90s job search effort is reduced, which would increase on- and off-the-job search. While neither of those aspects of computer use has a direct impact on the wage, it increases wages indirectly via an improved job offer distribution.

I use a Cobb-Douglas specification for the women's utility function.

$$U\left(C,L,PC\right)=C^{\alpha}L^{\beta}PC^{\gamma} \tag{3.1}$$

Where, for the time being, I assume that there is no heterogeneity across women, i.e. α, β and γ are the same for everyone. The optimization problem is given by

$$\begin{aligned}\max_{C,L,PC}\quad & U\left(C,L,PC\right)=\alpha\ln\left(C\right)+\beta\ln\left(L\right)+\gamma\ln\left(PC\right) \\ \text{s.t.}\quad & C+w\left(PC,X\right)L+pPC\leq w\left(PC,X\right)T+I\end{aligned} \tag{3.2}$$

The price of the composite consumption good C is normalized to 1, the price for a computer is p, T is the total time available, $w(PC, X)$ the wage as a function of characteristics (X) and computer availability, and I is the non-labor income (in this case mainly the husband's earnings). To show the dependence of labor supply on computers I first solve the maximization problem for consumption and leisure. The resulting demand for the two goods are functions of demand for computers. I then derive demand for computers in a second step.

For an interior solution, i.e. not all time is spent on leisure, the optimal allocation of time and consumption (denoted by asterisks) equates the marginal rate of

[1]See e.g. Blundell and MaCurdy (1999)

[2]This is similar in spirit to demand models that account for labor supply, see Browning and Meghir (1991).

[3]While discrete demand might be more realistic it makes the model more unwieldy and the predictions of a discrete model do not differ.

substitution $MRS(PC, X)$ between the two goods with their price ratio.

$$\frac{U_L(C^*(PC,X),L^*(PC,X),PC)}{U_C(C^*(PC,X),L^*(PC,X),PC)} = MRS(PC,X) = \frac{\beta}{\alpha}\frac{C}{L} = w(PC,X) \quad (3.3)$$

A woman chooses to work in the market, if her wage rate exceeds her reservation wage. The reservation wage is defined as the marginal rate of substitution at the corner solution where all time is spend on leisure and only non-labor income is used for consumption.

$$\begin{aligned} T - L > 0 \quad &\Leftrightarrow \quad w\,(PC,X) \geq w^r\,(PC,X) \\ &\Leftrightarrow \quad w\,(PC,X) - \frac{\beta}{\alpha}\frac{I - pPC}{T} \geq 0 \end{aligned}$$

This can be rewritten in terms of conditional expectations using an indicator function E for employment, which equals one if a woman works in the market, i.e. $T - L > 0$ and zero otherwise. I also assume that the wage can be decomposed into three factors. A base component that only depends on characteristics $w(X)$, a second factor that picks up the impact of having a computer at home, and an error term that collects random variation. The wage needs to be concave to ensure concavity of the objective function and to avoid the corner solution of infinite computer consumption. A simple specification is

$$w(PC,X) = w(X) + \delta_1 PC - \frac{1}{2}\delta_2 PC^2 + \epsilon \quad (3.4)$$

The parameters of the wage function δ_1 and δ_2 are again homogenous, i.e. the returns for computer use are equivalent for all women. Combining and rearranging the terms yields

$$E\,(E \mid PC, I, X) = w(X) + (\delta_1 - \delta_2\frac{PC}{2} + \frac{\beta}{\alpha}\frac{p}{T})PC - \frac{\beta}{\alpha}\frac{I}{T} \quad (3.5)$$

Without a computer in the household, the labor supply decision is governed by the base wage $w(X)$, the utility parameters for leisure and consumption and the non-labor income I. With a computer, employment becomes more likely for two reasons. First, the return to for computer skills (captured by δ_1 and δ_2) increases the price of leisure (at least initially) and work becomes more attractive. Second buying a computer reduces the endowment in non-labor income by pPC, this increases the likelihood of working in the market to compensate for the loss in consumption. The combination of both effects is depicted in figure 3.1 for the case where owning a computer changes the supply decision from non-employment to providing market work (solid lines depict the decision without, dashed lines the decision with a computer). As in the basic model without demand for computers, a woman's labor supply choice is positively related to her market wage and negatively to her non-labor income. In the following, I show that wages and income are also correlated with the demand for computers. In a regression of employment on computer availability this leads to inconsistent estimates unless wage and income are included in the set of controls.

To determine what factors affect the demand for computers, I again consider the interior solution. The demand functions for leisure and consumption, derived from the first order conditions of equation (3.2), are

$$\begin{aligned} C^*(PC) &= \frac{\alpha\left(I + w\left(PC, X\right)T - pPC\right)}{(\alpha+\beta)} \\ L^*(PC) &= \frac{\beta\left(I + w\left(PC, X\right)T - pPC\right)}{w\left(PC, X\right)(\alpha+\beta)} \end{aligned}$$

Substituting these back into the woman's utility function defines the indirect utility $V(PC, X) = U\left(C^*(PC, X), L^*(PC, X), PC\right)$, which I then maximize with respect to PC.

$$\begin{aligned} \max_{PC} V(PC, X) &= \alpha \ln\left(\frac{\alpha\left(I + w\left(PC, X\right)T - pPC\right)}{(\alpha+\beta)}\right) \\ &\quad +\beta \ln\left(\frac{\beta\left(I + w\left(PC, X\right)T - pPC\right)}{w\left(PC, X\right)(\alpha+\beta)}\right) \\ &\quad +\gamma \ln(PC) \end{aligned}$$

Let $w'(PC, X)$ be the first derivative of the wage equation with respect to PC. Using this and rearranging the first order condition leads to an implicitly defined demand for computers.

$$PC^* = \frac{\gamma\left(I + w(PC^*, X)T\right)}{(\alpha+\beta+\gamma)\,p + (\alpha+\beta)\left(w'(PC^*, X)\frac{\beta(I+w(PC^*,X)T-pPC^*)}{w(PC^*,X)(\alpha+\beta)} - w'(PC^*, X)T\right)} \quad (3.6)$$

Consider first the case where computers do not impact earnings, i.e. $w'(PC, X) = 0$. In this case the share of total endowment $I+wT$ spent on computers is determined by the computer's price, p, and the relative taste for computers, $\frac{\gamma}{\alpha+\beta+\gamma}$, which is the standard Cobb-Douglas result. When wages increase with home computer demand, two opposing effects occur. First leisure becomes more expensive. This is captured by the second part of the sum, which is the demand for leisure (net of demand for computers) multiplied by the change in the price of leisure $w'(PC, X)$. The third part of the sum captures the second effect, which is the increase in the value of the time endowment. Clearly both income and wages matter for the demand for computers and are necessary controls for the estimation in section 3.5 as omitting either will result in biased estimates.

The model can help determine the direction of the bias. An increase in the base wage, $w(X)$, increases the value of the time endowment and the price of leisure, both effects will increase demand for computers. Similarly increasing non-labor income raises consumption of all goods and demand for computers increases. In combination with the correlations in equation 3.5 this means that, without controlling for income, the impact of computers on employment in a bivariate regression is underestimated and without controlling for wages the estimate is biased upward.

But even with controls for both wages and income, additional sources of bias need to be considered. A plausible concern is reverse causality, i.e. it is employment that drives the demand for computers. Though this link between employment and pc availability at home is conditional on wages and income. That means it will only be a concern if there is an effect of employment net of the increase in wages that might be associated with access to a computer at home. An example would be that employed women might have the option of working from home which would be facilitated by having a laptop or personal computer at home. It might also be the case that using a computer at work raises the utility women derive from a computer at home, i.e. they learn to use a computer at work instead of at home. This channel leads to an upward bias in the regression of employment on pc availability at home.[4]

Another potential source for bias in my estimates arises from introducing heterogeneity in either the utility function or the returns to having a computer at home. To capture heterogeneity I allow the parameters (α, β, γ, δ_1, δ_2) to vary for each woman i. Define $\pi_i = \frac{\beta_i}{\alpha_i}$ and denote mean parameter values by $\bar{\pi}$. Then I can rewrite equation 3.5 in terms of deviations from the mean.

$$\begin{aligned} E\left(E \mid PC, I, X\right) &= w(X) + (\bar{\delta}_1 - \bar{\delta}_2 \frac{PC}{2} + \bar{\pi}\frac{p}{T})PC - \bar{\pi}\frac{I}{T} \\ &\quad + E\left(\epsilon \mid PC, I, X\right) \\ &\quad + E\left((\delta_{1,i} - \bar{\delta}_1 - (\delta_{2,i} - \bar{\delta}_2)\frac{PC}{2})PC \mid PC, I, X\right) \\ &\quad - E\left((\pi_i - \bar{\pi})\frac{I - pPC}{T} \mid PC, I, X\right) \end{aligned} \tag{3.7}$$

Bias in this specification arises when deviations from parameter means are correlated with computer demand. If, for example, women with above average wage premium from computer use also enjoy using computers more in their leisure time (γ_i above average), the estimate on PC would be upward biased. On the other hand, if women who have a strong relative taste for leisure (π_i above average) also benefit more from having a computer at home, the estimate would be downward biased. If preferences vary systematically as a function on observable characteristics, I could control for these characteristics in the regression models and thereby eliminate the bias. But it is unlikely that any data set contains sufficient variables to plausibly capture taste variation.

Since controlling for both reverse causality and taste heterogeneity is not feasible I would have to either find variables that induce exogenous variation in computer demand (i.e. instrumental variables) or randomly assign computers to households to ensure unbiased estimates. Neither is feasible in this study.

[4] The direction of the bias is given by the coefficient of employment in the "reverse" regression, i.e. the regression of computer availability on employment (Stock and Watson 2007, pp. 324–325), and employment in the given examples would increase demand for a computer at home.

3.3 Data

I use data from the U.S. Current Population Survey (CPS)[5] from 1983–2005 for this study. The CPS is a monthly survey that collects data for all members of approximately 50,000 households. Once a household enters the sample it is surveyed in two waves. In both waves households are interviewed in four consecutive months with a break of eight months in between waves. In each survey the respondents answer the same set of questions on demographics and employment. Occasionally supplemental questionnaires are issued on specific topics. Questions on computer use at home and at the workplace were part of several supplements (October of 1984, 1989, 1993, 1997, December 1998, August 2000, September 2001, and October 2003). In addition to computer availability, the data contain information on number of computers in the household, age of the newest computer, frequency of computer use and what the computer is used for. From 1997 onwards the survey contains additional questions on internet use.

The data are available in ASCII format on the website of the National Bureau of Economic Research (NBER)[6] and code to import the data into Stata is available for all files from 1997 onwards. For the remaining years the data documentation is available and I adapt the available code to import the raw data. Employment information is available in all CPS samples, but information on earnings are only available in the March supplements and in the months that a household leaves the sample[7], i.e. the month of the 4th and the 8th interview. This means that for each of the eight data files, information on earnings[8] is only available for the outgoing rotation group, i.e. a fourth of the sample.

To increase the available information on earnings I add data for all working members of a household from the months that they leave the sample. For this I use the NBER's Merged Monthly Outgoing Rotation Group sample, which is readily available in Stata format. Since the CPS is an address based survey, interviewees are not necessary the same individuals across all interviews. To ensure consistency I only use matches where gender and race are the same in both interviews and the age of the person does not change by more than a reasonable margin (1–2 years). Some 5% of the observations fail this test and are discarded. I then use the earnings information that is closest to the computer supplement survey. If available, I use the earnings information of the month itself, otherwise the information from the same wave of interviews and, if those are not available, I add the data from the second wave. This means, the earnings information can come from up to 12 months prior to or after the month of interest.

To facilitate the addition of earnings across survey months the observations need to be uniquely identified. For a few observations the household identifier is not unique, these observations are discarded. For the regression analysis the data is further truncated to include only married women who live with their spouse and who are between 20 and 59 years old. Table 3.1 reports means, standard deviations and number of observations for each of the eight sample years.

[5]See the Census Bureau and Bureau of Labor Statistics' (2002) Technical Paper 63RV for details.

[6]`http://www.nber.org/data/cps_index.html`

[7]The questions were part of the outgoing surveys since 1979, before they were part of the May supplement.

[8]For hourly paid workers earnings are usually hours worked times their hourly wage, for all other workers earnings are usual weekly earnings.

3.4 Descriptive Results

Personal computers have become an integral part of everyday life, both at home and at work. Figure 3.2 shows the increase in computer use at the workplace. The solid line depicts the change for all employees, the dashed lines consider only female or male employees. In 1984 24% of all employees were already using a computer at work. The share rose quickly to 45% in the mid-90s and kept rising, albeit at a slower pace, to 55% in 2003. The numbers are very similar to those reported by Spitz-Oener (2006) for West Germany. Although computer use at work is more prevalent among female employees compared to male employees, both groups follow similar trends, with a slightly stronger increase for women. A simple explanation is that men have a comparative advantage in manually intensive tasks[9] and computer are complementary to (non-routine) cognitive task[10].

The reasoning in this paper is, that the availability of a computer at home allows women to acquire valuable skills. Figure 3.3 shows the change in the share of households with at least one computer (or laptop) at home. Few households had a computer in the mid-80s, but the share rose at an increasing rate until 2001. From the mid-90s onwards more people have access to a computer at home than at work. Households with at least one married woman are slightly more likely to own a computer. This is unsurprising as the average married household tends to be older and has higher income than the average unmarried household. There is no direct measure of skill in the data, but using educational attainment as a proxy I find that married women, with better education, are more likely to have access to a computer (figure 3.4).

How does availability of computers at home relate to employment? Figure 3.5 depicts employment for all 20–59 year old women (circles) and those 20–59 year old women who are married (diamonds). Female employment is high, peaking at more than 70% in the late 1990s. Employment has been rising for several decades (e.g. Goldin 2006) but stabilizes over the sample period and even seems to drop in the 2000s. Married women have lower levels of employment, which has traditionally been the case (e.g. Killingsworth and Heckman 1987). Crucial for this study, the participation rates are universally higher for women with access to a computer at home. This is in line with the simple model in section 3.2, where women do not work if their reservation wage exceeds the market wage.[11]

3.5 Empirical specification I

To estimate the impact of computers on employment I use ordinary least squares regressions on several sets of covariates. With a binary dependent variable the ordinary least squares estimator is usually referred to as "linear probability model" (LPM).[12] The model in section 3.2 shows that omitting controls for the woman's

[9] c.f. Rendall (2010)

[10] See e.g. Black and Spitz-Oener (2010)

[11] The model does not distinguish between non-participation and unemployment, i.e. all unemployment is voluntary.

[12] I also used a Probit to estimate the equations but qualitatively the results do not differ. I prefer the LPM since the results are easier to interpret and it connects more directly with the theoretical model.

wage and non-labor income results in biased estimates. By introducing these controls successively I can gauge whether the models predictions are in line with the empirical findings.

A truncation issue arises when controlling for wages, since wages are only observed when a woman is actually working. Therefore, I use a set of covariates to proxy for wages. The set includes age and its square, dummies for education (completed years in 1984 and 1989, degree obtained thereafter), race (three dummies for white, black, any other race), state and MSA dummies.[13] The only direct measure for non-labor income in the CPS is a categorical measure for combined household income from all sources in the last year. This variable has the disadvantage in that it is not possible to separate the woman's contribution from other income. Focussing on married women allows me to treat the husband's earnings as non-labor income.

It is unlikely that the impact of computers has been constant over time. I therefore estimate the model for each cross-section separately.[14] The regression specification is given by

$$y_{it} = \beta_t^0 + \gamma_t pc_{it} + \beta_t^w x_{it}^w + \beta_t^n x_{it}^n + \nu_{it} \tag{3.8}$$

Where i denotes the individual woman, t the different cross-sections and γ, β^0, β^w, β^n the parameters to be estimated. The covariates are split into those that affect the wage rate x^w , and those that account for non-labor income x^n. Finally, ν is the error term. The dependent variable y is employment with non-employment (unemployed or out of the labor force) as base category and pc is a dummy that is equal to one if the household owns at least one computer. I also consider a specification where pc indicates that the household owns a computer and the woman actually uses it. Computer use might be a better indicator for a women having computer skills, but this measure has two main disadvantages. While a woman might not currently use an available computer, she might have used it in the past, thus making computer availability a better indicator for computer use. In addition computer use is not available in two of the eight cross-sections. I therefore focus the discussion on computer availability, reporting results for computer use only for my preferred specification.[15] Finally, I allow for heterogenous effects of home computers by interacting the availability of a computer with the woman's education level. I account for sampling weights in all regressions and I use White (1980) heteroscedasticity robust[16] standard errors.

Following the theoretical arguments from section 3.2, I first consider the unconditional impact before successively introducing additional controls. The first specification (a) does not include any controls, beyond the availability of a computer. The second set (b) adds controls that proxy for the wage rate, these are education (dummy variables[17]), age (and its square), race (two dummies for white and black

[13]Imputing wages in this manner introduces another possible source of bias. The bias arises if the proxy error, i.e. the deviation of the predicted wage based on the set of proxy variables from the true (potential) wage, is correlated with demand for computers.

[14]An additional advantage of not pooling the cross-sections is that variables with different definitions over time, e.g. education or occupation, do not need to be harmonized.

[15]The differences between computer availability and computer use in this specification are representative for the pattern exhibited by the other specifications.

[16]I also ran the regressions using standard errors that are clustered at the state level to account for spatial correlation (Moulton 1986). The results did not differ.

[17]Depending on the survey year, the education dummies are either completed years of education

women), state dummies and a dummy for metropolitan standard area status. For the third set (c) I control for non-labor income. The controls in this specification are a dummy for home ownership, husband's weekly earnings, and its square, as well as an indicator that is equal to one if the husband does not have any earnings (i.e. is unemployed or not in the labor force). Specification (d) then combines both wage and income controls. Specification (e) includes all the controls from specification (d) and, in addition, the husband's education[18], age (and its square), and dummies for the number of 0–5 and 6–15 year old children in the household.

The final specification adds controls that do not affect wages or non-labor income directly, but both are likely to influence labor force participation and might be correlated with the choice to acquire a computer. Child care is one of the main factors that influences labor supply[19] and the household's computer might have been purchased for the child's benefit. Similarly the husband's characteristics might correlate with the demand for computers and the woman's labor supply. For example, employment is less stable for men with lower levels of education and the women's market work acts as an insurance mechanism.[20]

3.6 Empirical results I

Table 3.3 reports the coefficients for home computer availability in the employment regressions. The unconditional impact of having a computer at home (1a) is positive. On average, women in households with a computer are more likely to be employed. The correlation is increasing over time, starting with a 6.6 percentage points higher employment share in 1984 that increases to 15.3 percentage points in 2003. All coefficients are statistically significant at the 1% level.

The estimates capture the causal impact of home computers, if computers were as good as randomly assigned. As I discuss in section 3.2, this is not very likely. Specifications (1b) and (1c) confirm this suspicion. Adding controls that capture productivity differences, i.e. controls that proxy for the potential wage, reduces the impact of computer availability dramatically. The coefficients are only statistically significant from 1998 onwards and the largest effect (in 2003) is reduced to an 8.3 percentage points increase in employment; about half of the unconditional mean difference. Controlling for measures of non-labor income results in much smaller changes in the coefficient estimates. Compared to the unconditional specification (1a), the estimates are slightly smaller in all years except 1989, with most differing by less than a percentage point. The direction of the changes is as expected when adding wage controls. For non-labor income the model predicts an increase in the coefficient estimates. Surprisingly the opposite is true.

This might be due to measurement error. With classical measurement error in PC, adding a correlated control reduces omitted variable bias, but at the same time increases attenuation bias due to measurement error. With computer use as

(1984 or 1989) or degree obtained (1993 and thereafter). For years of education I pool all women with 11 or less years of completed education in one category and all women with 16 or more years in another. For degrees I also pool all women without a high school diploma and those with a master's degree or more.

[18]The same changes apply to husband's education that apply to the woman's own education.

[19]See e.g. Hotz and Miller (1988).

[20]See e.g. Lundberg (1985)

explanatory variable, there should be less attenuation bias; assuming computer use is less affected by measurement error than computer availability. This would be the case if, for example, computer skills are more prevalent among the women who not only have access to but also use a computer at home. When I estimate regressions (1a) and (1c), and substitute computer use for computer availability, I find the same pattern as in table 3.3. Which does not rule out that PC is affected by measurement error, since it might be present in both computer use and availability, but makes this explanation unlikely.

Another possibility is (non-classical) measurement error of non-labor income. Apart from the home ownership dummy, I use only remuneration from the husband's employment. Other sources of income, e.g. earned interest, are not captured. In addition some 15% of the married women live with a spouse who does not have any market earnings (see table 3.1). However, they might receive benefit payments, scholarships or pensions. This means that the proxy underestimates the true value of non-labor income. With positive correlation between the unobserved component of non-labor income and demand for computers in combination with a negative correlation between the unobserved component and employment, this leads to downward biased estimates of the coefficient on PC. To check the plausibility of this explanation I re-estimate the regressions (1a) and (1c) on a constraint sample, which includes only married women whose husbands report positive market earnings. This excludes the group of women for whom the measurement error is likely to be the most severe. With this constraint I find for all years, except 2003, that coefficient estimates increase compared to the unconditional specification when controlling for non-labor income (upper panel of table 3.4). The increase is moderate, ranging from 1–2 percentage points in the late 80s and early 90s to less than 1 percentage point in the 90s and early 2000s. Measurement error in non-labor income seems to be present but its impact is limited.

When controlling for both sets of covariates in (1d) and (1e), the direction of the bias is theoretically indeterminate. It turns out that the unconditional effect overestimates the impact compared to a full specification that includes both (potential) wage and non-labor controls. In both specifications, (1d) and (1e), I find a moderate increase of 1.5–3 percentage points in the employment probability in the data from 1984 to 1997. From 1998 onwards the estimates are larger, averaging at about 6 percentage points. The estimates are statistically significant at the 5% level in all years except 1984.

So far I have considered the impact of the availability of a computer. The simple presence of a computer should not increase a woman's market productivity without her actually making use of it. But far from all women use the available computer. Table 3.2 shows the share of women who use the available computer for three groups, all women, married women and married women with children. For all three groups the user shares are very similar. In 1984 only 42–48% were using the household's computer. The share increased over time with about 86% of women making use of an available computer in 2003. While it might indicate a general disinterest for the available computer, the lack of current use does not rule out that the computer has been used in the past. It still raises the concern, that the estimates suffer from self-selection bias, since women who benefit the most from computer use would choose to use the computer to acquire computer skills.

I therefore consider a specification with computer use, rather than computer availability, as the dependent variable. The middle panel of table 3.4 shows the results for the most comprehensive set of controls. The CPS supplements in 1998 and 2000 did not include questions on computer use at home, so those two years are omitted.[21] The estimated coefficients are in line with the previous results. They are, with 2.5–4 percentage points, slightly higher from 1984 to 1997, and with 5–5.5 percentage points slightly lower in 2001 and 2003, than the comparable estimates for computer availability (specification (1e) in table 3.3). Self-selection does not appear to be a major concern.

Blau and Kahn (2007) show that female employment increased strongly in the 1980s with a slow-down in the increase during the 1990s. However my results indicate that for women who had access to a computer, employment kept rising, even after the 1990s. To put the results into perspective I consider the descriptive trends in employment and computer availability in figure 3.3 and 3.5 again. Computer availability rose from 13 to 60% from 1984 through 1998. The share of married women in employment increased from 60 to 71% over the same period. The coefficient estimates for computer availability (specification (1e) in table 3.3) imply that the computer skills acquired using a computer at home account for 3 percentage points of the 11 percentage point increase in employment.[22]

The timing of the increase in the impact of computer availability in table 3.3 coincides with the rise of the internet and the proliferation of employment websites. While it is tempting to attribute the increase to improved job search options, Kuhn and Skuterud (2004) find, using CPS supplements data, that internet search did not reduce unemployment duration.[23] On the other hand internet related job opportunities and the ability to (partly) work from home might have increased employment.

As discussed in section 3.2, studies considering the impact of computer use at the workplace find that demand for skilled workers increases. To investigate whether home there is a relationship between home computers and skill I allow for heterogeneity in the impact of home computers on employment. The lower panel of table 3.4 reports the coefficient estimates of home computer availability interacted with educational attainment.[24]

While most of the coefficients are positive, few remain statistically significant. Standard errors increase markedly, compared to the regressions that focus on the overall average effect. Interestingly it is both at the lower and the upper end of the educational distribution that computer availability matters most. The estimates are positive and most of them statistically significant for women who dropped out of high school. The size of the coefficients varies across years but averages around 9 percentage points. For women who finished high school but did not pursue any further education the estimates vary around an average of 5 percentage points, excluding 1984 where the coefficient is negative and statistically indistinguishable from zero.

The coefficients at the upper end of educational attainment (Master's, Professional or PhD degrees) are of similar size, but the estimates are not precise enough

[21] The focus in those CPS supplements is on internet use.

[22] $\Delta = 0.056 * 0.6 - 0.014 * 0.13 = 0.032$

[23] There is evidence for positive effects of internet availability, Beard, Ford, Saba, and Seals (2012) using CPS data from the 2007 supplement, find positive effects of internet availability on job search efforts.

[24] For 1984 and 1989 educational attainment is measured in completed years of education. I interpret 11 years or less of completed education and 12 years of education where the 12th year was not completed as "High school drop-out" and 12 years of completed education as "High school graduate".

to distinguish the majority of coefficients from zero. For all other education groups the estimates are mostly insignificant. Autor, Levy, and Murnane (2003) find that computer use at work favors non-routine tasks and increased demand for highly educated employees strongly. The results here suggest that having access to a computer at home, and thereby the opportunity to acquire computer skills, increases employment for women with low levels of education.

One possible explanation is that while computers substituted for many skills at the workplace, they also require employees capable of using the technology. Many tasks performed by, for example, bank tellers[25] might be substituted with Automated Teller Machines, but the remaining tasks bank tellers perform rely heavily on the use of computers. In addition, the relative value of basic computer skills, like touch typing or being able to use standard software packages, is higher for low levels of education. Finally, as Weinberg (2000) points out, computerization reduces the relative value of physical skill. This, in turn, reduces the comparative advantage of men in classically "muscle intensive" occupations and leads to increased demand for female workers.

In this section I establish sizable positive correlation between home computer availability and employment. The interpretation that having a computer at home increases labor supply and employment hinges on the assumption that the computer increases productivity and wages. The next section analyzes this link.

[25] Using an example given by Autor, Levy, and Murnane (2003)

3.7 Empirical specification II

Based on the model in section 3.2 home computers increase employment, if access to a computer increases productivity and thereby the (potential) market wage. Computer skills should also only be valuable if they can be applied at work. Therefore, I estimate in the following whether women who have access to a computer at home have higher wages and whether they are more likely to work in occupations with a high share of computer users.

Ideally I would like to test whether computer skills increase the productivity of, or the wage earned by, a woman, if she was working. But wages are truncated and only observed if the woman actually works. The unobservable factors that determine a woman's decision to work are likely correlated with the unobservable factors that determine a woman's earnings. This leads to (selection) bias in the simple OLS framework. Heckman (1979) suggests a control function approach to account for this bias. Wages (lhw) are observed only if a person is working ($y = 1$), and they are missing otherwise.

$$lhw_{it} = \begin{cases} lhw^*_{it} & \text{if } y_{it} = 1 \\ \text{missing} & \text{otherwise} \end{cases} \tag{3.9}$$

Where employment (y) is determined by the same process as in equation 3.8. The potential market wage lhw^* is given by

$$lhw^*_{it} = \delta_t pc_{it} + \alpha^w_t x^w_{it} + \eta_{it} \tag{3.10}$$

If the error terms ν in equation (3.8) and η in equation (3.10) are correlated, a simple regression of (log) hourly wages on computer availability and covariates x^w will be biased. Heckman (1979) shows that, if both error terms are normally distributed, the selection bias is given by the covariance of the two error terms multiplied with the inverse Mills ratio. To correct for the bias either a two-step procedure, first estimating the inverse Mills ratio using a Probit model, and then controlling for it explicitly in a second stage OLS regression for wages, or a (partial) maximum likelihood (MLE) approach that accounts for the truncation, can be used. The MLE requires stronger distributional assumptions and tends to have problems with convergence (Wooldridge 2002, p. 566), which makes the two-step procedure more robust. However, in Stata the two-step estimator does not allow for sampling weights nor for non-homoscedastic standard errors. Consequently I use the MLE estimator.

Other than the computer indicator, which is used in both the wage and the selection equation, the two models include the same controls x^w as above (education, age, race, state and MSA status) for both the wage and the selection equation. For the selection model (3.8) I use the full set of exclusion restrictions, given by the earnings and family measures used in the previous section, i.e. a dummy for home ownership, the husband's education, age (and its square), weekly earnings (and its square), a dummy if the husband does not report any earnings, and dummies for the number of 0–5 year old and 6–15 year old children.

3.8 Empirical results II

The estimated coefficients on *PC* in the selection corrected wage equation are reported in table 3.5. Underneath the coefficient and the standard error (in parenthesis) I report the p-value of a test for correlation between the error terms of the wage and selection equation, which are uncorrelated under the H0. In two out of eight years accounting for selection into employment is warranted.[26] Non-correlation can be rejected at least at the 1% level (5% level in 2003). Convergence is achieved in all specifications.

The first panel shows the coefficient for computer availability, the second for computer use. In both cases the computer at home is associated with higher wages. These findings are in line with the increase in demand for women with computer skills (Weinberg 2000). For 1984 the coefficients are small and not statistically significant, however the coefficients increase over the following years and become statistically significant . The estimates from 1989 onwards indicate large returns in the range of 5 to 10% higher wages. The results are smaller than those found by Krueger (1993) for computer use at the workplace and larger than the findings by Zoghi and Pabilonia (2007). Using the 1999–2002 Canadian Workplace and Employee survey, they estimate a 3.6 percent wage premium for adopting a computer at work, accounting for both employee and establishment fixed effects. Zoghi and Pabilonia (2007) also find that returns increase with education levels. I cannot confirm the same for home computers. As can be seen in the lower panel of table 3.5. While the estimates in the late 80s show some evidence for returns for highly educated women (16–17 and 18 or more years of education). The stronger effects are at the lower end of the education distribution from the 90s onwards. I do find positive and significant effects for women with low levels of education, high school dropouts, graduates and women with some college. For these women, the returns from 1993 onwards are in the range of 6 to 14%, averaging below 10% across years. While most of the coefficients for women with a Bachelor's degree are significant, the returns are lower, averaging around 8%.

If the computer at home is used to acquire skills and increase productivity at work, I would expect women to choose employment in occupations where they use a computer. To see whether this is the case I estimate the same model as above, but instead of log hourly wages I use the share of computer users in the woman's occupation as dependent variable.[27] The results are reported in table 3.6. Again, all models converge and selection matters in four of the six available years.[28]

I find that in all years women who have (and use) a computer at home work in occupations that have a higher share of computer users. In 1984, women with a computer at home worked in occupations with, on average, a 1.5 percentage points higher computer user share. The estimates increase throughout the 80s and 90s and fall in the 2000s. The peak is in 1997 where women with a computer at home work in occupations with 7.8 percentage points more computer users than comparable women without a computer at home. Disaggregated by level of education I find

[26] While simple OLS for the two other years would be more efficient than the selection correction model, the coefficients are estimated precisely enough to err on the side of caution and stick with the selection model.

[27] The shares are calculated based on computer use at work for both men and women and leaving out the computer use of the respondent.

[28] See the comments in footnote 26.

mostly positive coefficients and again the strongest effects for both women with little formal education and women with Master's degrees and more.

The results are in line with home computers increasing employment, as having a computer at home is associated with finding employment in more computer intensive occupations and higher wages.

3.9 Discussion

In this paper I estimate the impact of home computers on married women's employment. Using data from the U.S. CPS supplements between 1984 and 2003, I find that employment increases with the availability of a home computer. The unconditional impact ranges from 6.5 percentage points higher employment shares in 1984 to 15 percentage points in 2003. I present a theoretical model that shows that the unconditional estimates are misleading and several bias inducing factors are identified. Most importantly adding controls that account for (offer) wage differences and non-labor income reduce the impact to a range of 1.5 to 7 percentage points.

Employment in the model rises due to computer skills leading to improved offer wages. Therefore, I estimate whether wages differ for women with a computer at home. Accounting for selection into employment, I find that wages are indeed higher for women with a computer at home. The returns are lower in the 80s, starting from 2–5% and increasing to 8–10% higher wages in the 90s and 2000s.

Decomposing the effect by education level shows that gains, both in employment and wages, are strong for women with little formal education. Married women with a Master's degree or higher also seem to benefit, but estimates are very imprecise and few coefficients are statistically significant.

How can these results be interpreted? Skills acquired using a home computer are most useful if they are general enough to be of use on a computer at work and scarce enough to warrant a wage premium. In 1984 the most prevalent home computer was the Commodore 64 while commercial use relied on IBM and IBM compatible computers. The late 80s and especially early 90s saw IBM compatible computers running MS-DOS and Windows take over the market for both home and commercial users. Standard software packages became available and affordable for home users and, consequently, skills acquired on a home computer became more easily transferable to the workplace. At the same time computer use became more widespread, rising more steeply than in the 2000s. This might have lead to demand for computer skills rising more quickly than supply, which gave rise to the wage premium.

The main caveat of this study is that a causal interpretation of the coefficients hinges on (conditional) random assignment of computer availability (or use). The results show that it is crucial to control for both income and wage measures. Whether the available information in the CPS used in this paper suffices to adequately control for all selection effects is not clear. Ideally I would find an instrument for having a computer at home.[29] A promising alternative would be the use of a regression discontinuity design as used by Malamud and Pop-Eleches (2011). They exploit an allocation rule for home computer vouchers issued by the Romanian government.

[29] The share of computer users in the husband's occupation yields a strong first stage, but implausible estimates in the second stage.

While the computers were meant to improve the education of children, they should also have an impact on the mothers in the household.

3.10 Appendix

Figures

Figure 3.1: Labor supply and computers

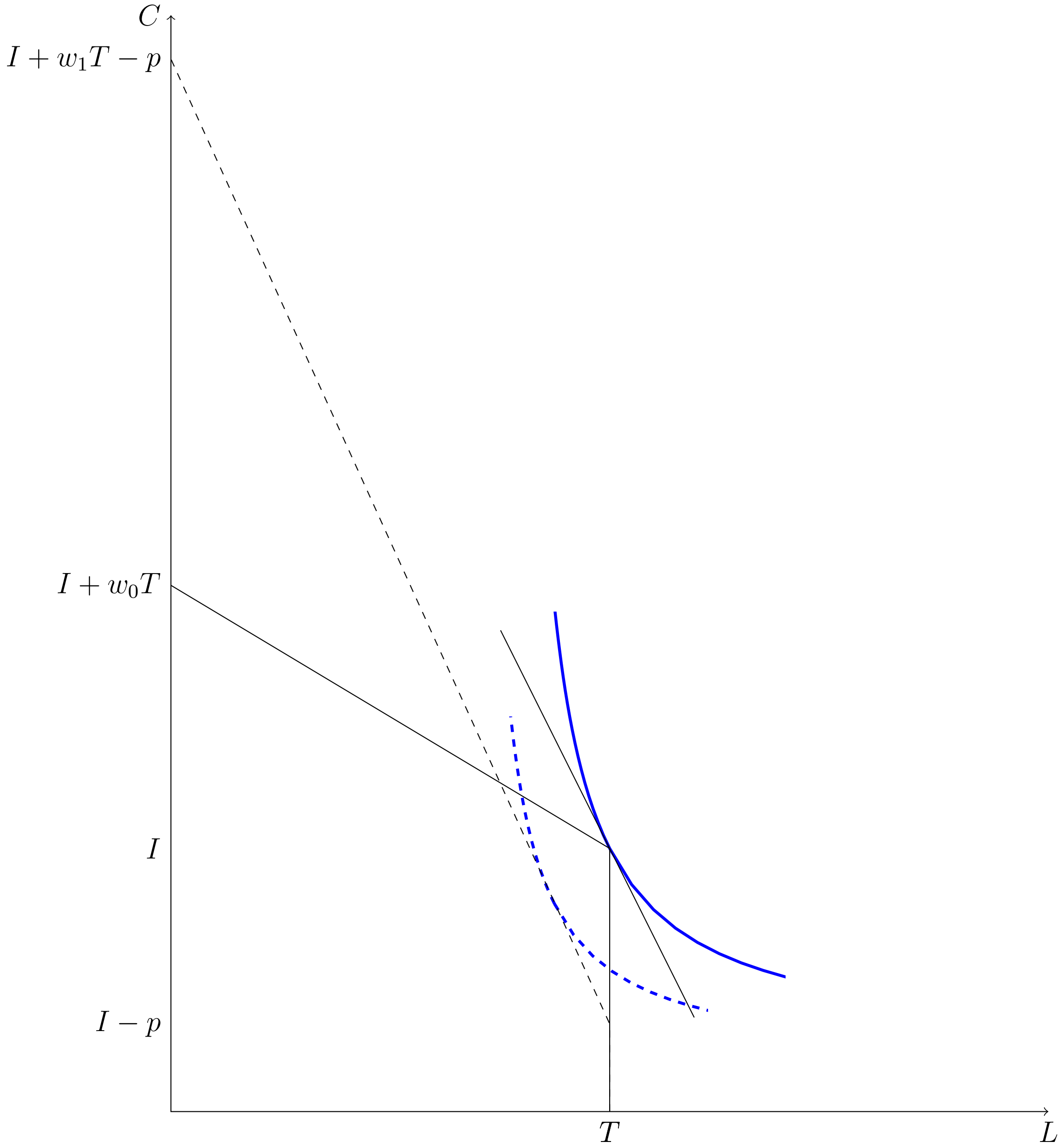
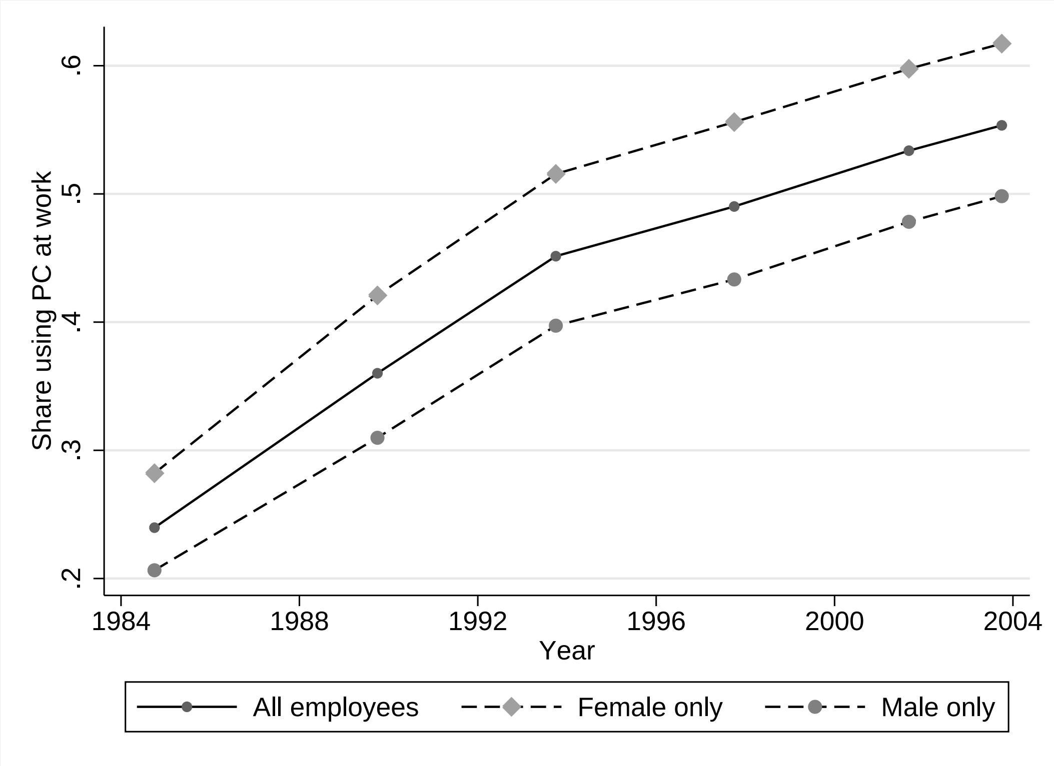


Figure 3.2: Share of employees using a computer at work



Data source: NBER CPS Supplements Oct. 1984, 1989, 1993, 1997, 2003; Sept. 2001

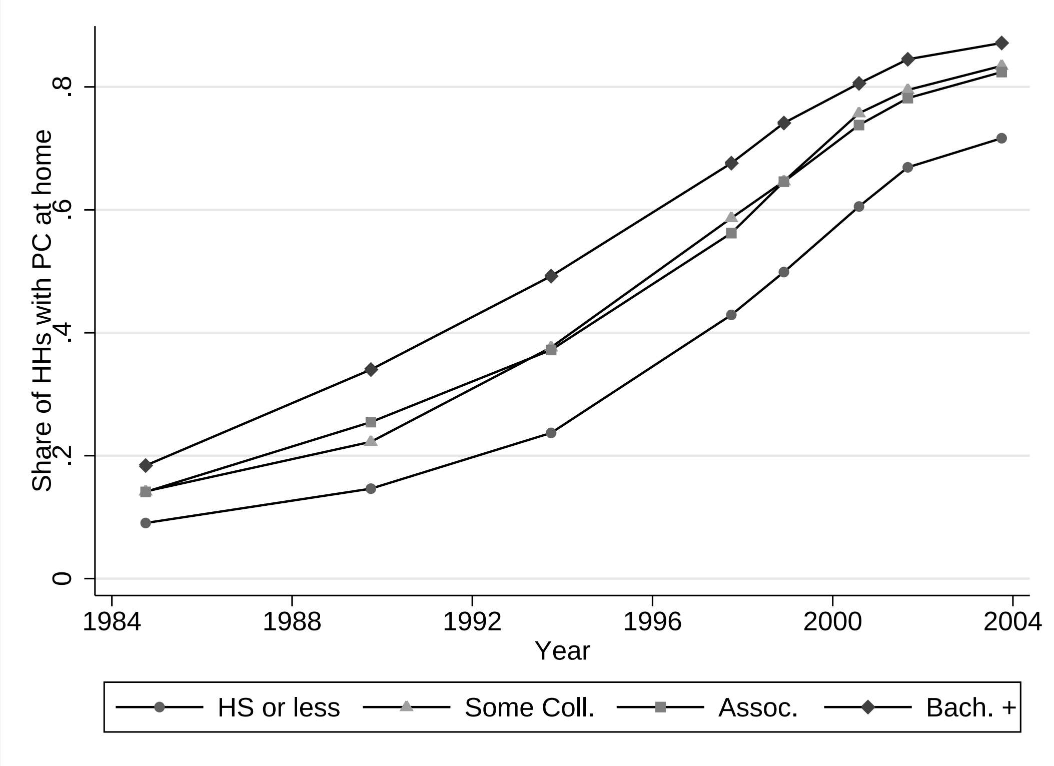
Solid line depicts share of employees that respond “yes” to the question “Does ... directly use a computer at work?”. Dashed lines separate female and male employees. Calculations account for sampling weights.

Figure 3.3: Share of households with access to a computer at home

Data source: CPS Oct. 1984, 1989, 1993, 1997, 2003; Dec. 1998; Aug. 2000; Sept. 2001

Lines depict the share of households that respond "yes" to the question "Is there a computer in this household?" or "Is there a computer or laptop in this household?". Circles indicate all households, diamonds denote households with at least one married female member. Calculations account for sampling weights.

Figure 3.4: Share of married women with access to a computer by educational attainment



Data source: CPS Oct. 1984, 1989, 1993, 1997, 2003; Dec. 1998; Aug. 2000; Sept. 2001

Lines depict the share of households with at least one married woman that respond "yes" to the question "Is there a computer in this household?" or "Is there a computer or laptop in this household?". Education levels are aggregated into four categories:

a) High School graduates and High School drop-outs b) Some College c) Associate Degree (both vocational and academic) d) Bachelor's Degree or more (e.g. Master's, Professional Degree, PhD)

Calculations account for sampling weights.

Figure 3.5: Female employment

Data source: CPS Oct. 1984, 1989, 1993, 1997, 2003; Dec. 1998; Aug. 2000; Sept. 2001

Lines depict the share of 20 to 59 year old women in employment. Circles denote the employment share for all women, diamonds the share of married women. Solid lines are for all households, the dashed line households with a computer. Calculations account for sampling weights.

Tables

Table 3.1: Sample means and standard deviations

	1984	1989	1993	1997	1998	2000	2001	2003
Women's variables								
Employed	0.593	0.666	0.681	0.708	0.706	0.678	0.690	0.685
	(0.491)	(0.472)	(0.466)	(0.455)	(0.456)	(0.467)	(0.463)	(0.465)
Log hourly wage (cent)	6.462	6.698	6.879	7.004	7.062	7.133	7.185	7.230
	(0.488)	(0.521)	(0.536)	(0.533)	(0.537)	(0.541)	(0.536)	(0.612)
PC share in occ.	0.276	0.431	0.527	0.578			0.619	0.634
	(0.230)	(0.290)	(0.306)	(0.299)			(0.269)	(0.271)
PC at home	0.126	0.221	0.333	0.528	0.597	0.694	0.752	0.786
	(0.332)	(0.415)	(0.471)	(0.499)	(0.491)	(0.461)	(0.432)	(0.410)
PC use	0.053	0.111	0.205	0.380			0.637	0.677
	(0.224)	(0.314)	(0.404)	(0.485)			(0.481)	(0.468)
Age	38.5	38.8	39.5	40.1	40.4	40.9	41.2	41.6
	(10.7)	(10.1)	(9.8)	(9.7)	(9.7)	(9.8)	(9.8)	(9.9)
≤11/No Degree	0.183	0.140	0.117	0.112	0.110	0.105	0.100	0.100
	(0.386)	(0.346)	(0.322)	(0.316)	(0.312)	(0.306)	(0.300)	(0.300)
12/High School	0.476	0.452	0.387	0.354	0.344	0.328	0.326	0.316
	(0.499)	(0.498)	(0.487)	(0.478)	(0.475)	(0.470)	(0.469)	(0.465)
13/Some College	0.072	0.077	0.186	0.184	0.184	0.187	0.178	0.174
	(0.258)	(0.267)	(0.389)	(0.388)	(0.387)	(0.390)	(0.383)	(0.379)
14/Assoc. Vocational	0.079	0.101	0.048	0.048	0.051	0.050	0.056	0.056
	(0.270)	(0.302)	(0.214)	(0.213)	(0.220)	(0.218)	(0.230)	(0.230)
15/Assoc. Academic	0.030	0.031	0.041	0.050	0.047	0.052	0.053	0.053
	(0.171)	(0.174)	(0.197)	(0.218)	(0.213)	(0.223)	(0.224)	(0.224)
16-17/Bachelor's	0.125	0.149	0.157	0.179	0.187	0.196	0.204	0.206
	(0.331)	(0.356)	(0.364)	(0.383)	(0.390)	(0.397)	(0.403)	(0.404)
18≥/Master's or more	0.035	0.050	0.063	0.072	0.077	0.082	0.083	0.095
	(0.184)	(0.218)	(0.243)	(0.259)	(0.267)	(0.274)	(0.276)	(0.293)
White	0.890	0.900	0.896	0.890	0.891	0.883	0.884	0.871
	(0.313)	(0.300)	(0.305)	(0.313)	(0.312)	(0.322)	(0.321)	(0.335)
Black	0.078	0.078	0.078	0.078	0.079	0.085	0.083	0.082
	(0.268)	(0.269)	(0.268)	(0.269)	(0.269)	(0.279)	(0.276)	(0.275)
Other	0.032	0.021	0.026	0.032	0.031	0.032	0.033	0.047
	(0.177)	(0.144)	(0.158)	(0.177)	(0.173)	(0.177)	(0.179)	(0.211)
Alabama	0.018	0.018	0.018	0.018	0.018	0.018	0.017	0.017
	(0.132)	(0.134)	(0.131)	(0.135)	(0.133)	(0.133)	(0.129)	(0.128)
Alaska	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.002
	(0.046)	(0.045)	(0.045)	(0.049)	(0.049)	(0.046)	(0.048)	(0.046)
Arizona	0.013	0.014	0.014	0.017	0.015	0.017	0.019	0.018
	(0.114)	(0.117)	(0.116)	(0.129)	(0.121)	(0.129)	(0.135)	(0.134)
Arkansas	0.008	0.010	0.011	0.009	0.010	0.010	0.010	0.009
	(0.091)	(0.100)	(0.102)	(0.094)	(0.101)	(0.097)	(0.099)	(0.097)
California	0.103	0.101	0.110	0.105	0.108	0.109	0.113	0.113
	(0.304)	(0.301)	(0.313)	(0.307)	(0.310)	(0.312)	(0.317)	(0.317)
Colorado	0.015	0.014	0.014	0.016	0.016	0.016	0.014	0.016
	(0.121)	(0.118)	(0.116)	(0.125)	(0.124)	(0.127)	(0.119)	(0.126)
Connecticut	0.013	0.015	0.013	0.011	0.013	0.012	0.011	0.012
	(0.115)	(0.120)	(0.115)	(0.104)	(0.112)	(0.109)	(0.104)	(0.111)
Delaware	0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.003
	(0.052)	(0.054)	(0.054)	(0.051)	(0.051)	(0.053)	(0.053)	(0.052)
District of Columbia	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001
	(0.036)	(0.037)	(0.033)	(0.031)	(0.026)	(0.033)	(0.033)	(0.030)
Florida	0.041	0.049	0.048	0.048	0.048	0.050	0.053	0.053
	(0.198)	(0.216)	(0.213)	(0.214)	(0.214)	(0.219)	(0.223)	(0.224)
Georgia	0.026	0.025	0.025	0.029	0.030	0.030	0.030	0.031
	(0.158)	(0.157)	(0.155)	(0.168)	(0.170)	(0.172)	(0.171)	(0.174)
Hawaii	0.004	0.002	0.002	0.002	0.002	0.002	0.002	0.002
	(0.067)	(0.047)	(0.048)	(0.049)	(0.047)	(0.047)	(0.048)	(0.046)
Idaho	0.004	0.004	0.005	0.005	0.005	0.005	0.005	0.005
	(0.063)	(0.062)	(0.069)	(0.069)	(0.070)	(0.070)	(0.070)	(0.070)
Illinois	0.050	0.048	0.047	0.044	0.045	0.046	0.045	0.046
	(0.218)	(0.213)	(0.212)	(0.204)	(0.208)	(0.208)	(0.207)	(0.209)
Indiana	0.026	0.025	0.025	0.027	0.026	0.025	0.025	0.024
	(0.158)	(0.155)	(0.156)	(0.163)	(0.158)	(0.155)	(0.156)	(0.152)
Iowa	0.012	0.012	0.012	0.011	0.011	0.012	0.012	0.010
	(0.108)	(0.111)	(0.108)	(0.105)	(0.103)	(0.111)	(0.107)	(0.100)
Kansas	0.010	0.011	0.011	0.009	0.010	0.010	0.010	0.010
	(0.097)	(0.103)	(0.105)	(0.095)	(0.100)	(0.100)	(0.100)	(0.099)

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... table 3.1 continued

	1984	**1989**	**1993**	**1997**	**1998**	**2000**	**2001**	**2003**
Kentucky	0.017	0.016	0.016	0.017	0.018	0.016	0.017	0.015
	(0.131)	(0.125)	(0.124)	(0.129)	(0.134)	(0.127)	(0.128)	(0.120)
Louisiana	0.020	0.017	0.016	0.015	0.015	0.015	0.014	0.016
	(0.139)	(0.130)	(0.125)	(0.123)	(0.120)	(0.123)	(0.118)	(0.125)
Maine	0.005	0.005	0.005	0.005	0.005	0.005	0.006	0.004
	(0.072)	(0.074)	(0.072)	(0.070)	(0.067)	(0.073)	(0.074)	(0.066)
Maryland	0.019	0.019	0.019	0.019	0.019	0.018	0.019	0.018
	(0.138)	(0.136)	(0.136)	(0.137)	(0.136)	(0.134)	(0.136)	(0.133)
Massachusetts	0.023	0.023	0.021	0.023	0.022	0.020	0.022	0.022
	(0.150)	(0.151)	(0.144)	(0.151)	(0.146)	(0.139)	(0.147)	(0.147)
Michigan	0.039	0.040	0.038	0.038	0.041	0.038	0.038	0.035
	(0.194)	(0.196)	(0.191)	(0.192)	(0.198)	(0.190)	(0.191)	(0.184)
Minnesota	0.019	0.018	0.016	0.016	0.018	0.019	0.019	0.018
	(0.136)	(0.134)	(0.127)	(0.127)	(0.134)	(0.136)	(0.135)	(0.135)
Mississippi	0.010	0.010	0.010	0.010	0.010	0.010	0.010	0.009
	(0.098)	(0.101)	(0.099)	(0.099)	(0.099)	(0.101)	(0.099)	(0.095)
Missouri	0.021	0.021	0.022	0.022	0.020	0.020	0.019	0.020
	(0.143)	(0.144)	(0.146)	(0.146)	(0.141)	(0.140)	(0.137)	(0.140)
Montana	0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.003
	(0.056)	(0.056)	(0.054)	(0.055)	(0.054)	(0.057)	(0.054)	(0.052)
Nebraska	0.006	0.007	0.006	0.006	0.006	0.006	0.006	0.007
	(0.075)	(0.081)	(0.079)	(0.078)	(0.077)	(0.075)	(0.077)	(0.081)
Nevada	0.004	0.005	0.005	0.006	0.007	0.007	0.007	0.007
	(0.066)	(0.068)	(0.073)	(0.080)	(0.082)	(0.086)	(0.083)	(0.086)
New Hampshire	0.004	0.005	0.006	0.005	0.005	0.004	0.005	0.005
	(0.067)	(0.071)	(0.075)	(0.068)	(0.071)	(0.067)	(0.068)	(0.070)
New Jersey	0.032	0.030	0.029	0.032	0.030	0.030	0.031	0.031
	(0.175)	(0.171)	(0.169)	(0.175)	(0.170)	(0.170)	(0.174)	(0.174)
New Mexico	0.006	0.006	0.006	0.006	0.007	0.006	0.006	0.007
	(0.080)	(0.079)	(0.079)	(0.079)	(0.081)	(0.076)	(0.078)	(0.081)
New York	0.071	0.067	0.065	0.062	0.060	0.060	0.059	0.057
	(0.257)	(0.250)	(0.247)	(0.242)	(0.237)	(0.237)	(0.237)	(0.231)
North Carolina	0.026	0.028	0.029	0.028	0.028	0.029	0.029	0.030
	(0.161)	(0.166)	(0.167)	(0.166)	(0.166)	(0.169)	(0.167)	(0.171)
North Dakota	0.003	0.002	0.002	0.002	0.002	0.002	0.002	0.002
	(0.051)	(0.049)	(0.046)	(0.048)	(0.048)	(0.045)	(0.045)	(0.049)
Ohio	0.048	0.048	0.048	0.046	0.044	0.044	0.043	0.042
	(0.214)	(0.215)	(0.213)	(0.209)	(0.205)	(0.204)	(0.203)	(0.200)
Oklahoma	0.013	0.014	0.014	0.012	0.012	0.013	0.013	0.013
	(0.115)	(0.118)	(0.117)	(0.111)	(0.110)	(0.115)	(0.112)	(0.113)
Oregon	0.010	0.012	0.012	0.012	0.014	0.012	0.013	0.013
	(0.101)	(0.107)	(0.108)	(0.110)	(0.118)	(0.109)	(0.112)	(0.114)
Pennsylvania	0.053	0.050	0.050	0.046	0.044	0.044	0.043	0.046
	(0.223)	(0.218)	(0.219)	(0.209)	(0.204)	(0.206)	(0.203)	(0.208)
Rhode Island	0.004	0.004	0.003	0.003	0.004	0.003	0.004	0.004
	(0.064)	(0.062)	(0.059)	(0.058)	(0.061)	(0.054)	(0.060)	(0.060)
South Carolina	0.014	0.015	0.015	0.014	0.014	0.016	0.014	0.013
	(0.116)	(0.121)	(0.123)	(0.119)	(0.118)	(0.126)	(0.118)	(0.111)
South Dakota	0.003	0.002	0.003	0.003	0.002	0.003	0.003	0.003
	(0.051)	(0.050)	(0.051)	(0.051)	(0.049)	(0.052)	(0.051)	(0.051)
Tennessee	0.021	0.021	0.020	0.023	0.023	0.022	0.022	0.021
	(0.144)	(0.142)	(0.139)	(0.149)	(0.150)	(0.146)	(0.147)	(0.143)
Texas	0.066	0.069	0.075	0.077	0.076	0.076	0.074	0.079
	(0.249)	(0.253)	(0.263)	(0.267)	(0.265)	(0.265)	(0.262)	(0.269)
Utah	0.008	0.008	0.007	0.009	0.008	0.009	0.009	0.010
	(0.087)	(0.086)	(0.082)	(0.092)	(0.089)	(0.092)	(0.093)	(0.098)
Vermont	0.002	0.002	0.003	0.002	0.002	0.002	0.002	0.002
	(0.048)	(0.047)	(0.051)	(0.048)	(0.046)	(0.047)	(0.045)	(0.043)
Virginia	0.028	0.026	0.025	0.025	0.026	0.026	0.027	0.026
	(0.166)	(0.158)	(0.156)	(0.156)	(0.160)	(0.158)	(0.162)	(0.159)
Washington	0.019	0.019	0.021	0.021	0.023	0.021	0.022	0.023
	(0.138)	(0.137)	(0.144)	(0.143)	(0.149)	(0.143)	(0.145)	(0.149)
West Virginia	0.010	0.009	0.009	0.008	0.008	0.008	0.008	0.007
	(0.097)	(0.096)	(0.093)	(0.091)	(0.088)	(0.088)	(0.087)	(0.085)
Wisconsin	0.020	0.022	0.021	0.022	0.022	0.021	0.021	0.020
	(0.139)	(0.145)	(0.144)	(0.146)	(0.145)	(0.145)	(0.145)	(0.141)
Wyoming	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.002
	(0.048)	(0.044)	(0.044)	(0.045)	(0.044)	(0.043)	(0.043)	(0.043)

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... table 3.1 continued

	1984	**1989**	**1993**	**1997**	**1998**	**2000**	**2001**	**2003**
MSA	0.624	0.608	0.599	0.657	0.655	0.659	0.661	0.667
	(0.484)	(0.488)	(0.490)	(0.475)	(0.475)	(0.474)	(0.473)	(0.471)
No MSA	0.287	0.220	0.229	0.201	0.204	0.196	0.191	0.188
	(0.452)	(0.414)	(0.420)	(0.401)	(0.403)	(0.397)	(0.393)	(0.391)
MSA not identified	0.089	0.172	0.172	0.142	0.141	0.145	0.148	0.145
	(0.285)	(0.377)	(0.378)	(0.349)	(0.348)	(0.352)	(0.355)	(0.352)
Own home	0.754	0.770	0.778	0.787	0.798	0.811	0.816	0.813
	(0.431)	(0.421)	(0.416)	(0.409)	(0.402)	(0.391)	(0.388)	(0.390)
Home rented	0.229	0.217	0.209	0.201	0.191	0.180	0.174	0.178
	(0.420)	(0.412)	(0.407)	(0.401)	(0.393)	(0.384)	(0.379)	(0.382)
Lives rent free	0.017	0.013	0.013	0.012	0.011	0.009	0.010	0.010
	(0.130)	(0.115)	(0.114)	(0.108)	(0.103)	(0.094)	(0.099)	(0.098)
Spouse's variables								
Weekly earnings (100USD)	3.980	5.241	5.711	6.730	7.106	7.593	7.826	7.955
	(2.685)	(3.788)	(4.309)	(5.032)	(5.561)	(5.940)	(6.221)	(6.532)
No weekly earnings	0.160	0.133	0.155	0.135	0.137	0.138	0.148	0.166
	(0.366)	(0.340)	(0.362)	(0.342)	(0.344)	(0.345)	(0.355)	(0.372)
≤11/No Degree	0.201	0.169	0.134	0.130	0.128	0.123	0.117	0.119
	(0.401)	(0.374)	(0.341)	(0.336)	(0.334)	(0.328)	(0.321)	(0.324)
12/High School	0.380	0.372	0.345	0.319	0.315	0.303	0.308	0.298
	(0.485)	(0.483)	(0.475)	(0.466)	(0.464)	(0.459)	(0.462)	(0.458)
13/Some College	0.062	0.066	0.177	0.179	0.177	0.179	0.172	0.172
	(0.242)	(0.248)	(0.381)	(0.383)	(0.381)	(0.383)	(0.378)	(0.377)
14/Assoc. Vocational	0.088	0.099	0.041	0.046	0.047	0.049	0.051	0.051
	(0.284)	(0.299)	(0.197)	(0.209)	(0.212)	(0.215)	(0.220)	(0.219)
15/Assoc. Academic	0.032	0.033	0.036	0.037	0.039	0.042	0.040	0.041
	(0.175)	(0.180)	(0.186)	(0.189)	(0.194)	(0.201)	(0.197)	(0.198)
16-17/Bachelor's	0.160	0.171	0.173	0.187	0.190	0.193	0.196	0.206
	(0.367)	(0.377)	(0.378)	(0.390)	(0.392)	(0.395)	(0.397)	(0.404)
18≥/Master's or more	0.076	0.090	0.095	0.102	0.105	0.112	0.116	0.113
	(0.265)	(0.286)	(0.293)	(0.303)	(0.307)	(0.315)	(0.320)	(0.317)
Age	41.3	41.6	42.2	42.5	42.9	43.4	43.6	43.9
	(11.8)	(11.2)	(10.9)	(10.7)	(10.6)	(10.7)	(10.6)	(10.8)
Children's variables								
No kids (0–5)	0.701	0.694	0.708	0.714	0.719	0.719	0.732	0.726
	(0.458)	(0.461)	(0.455)	(0.452)	(0.450)	(0.449)	(0.443)	(0.446)
One kid (0–5)	0.199	0.207	0.193	0.198	0.190	0.191	0.180	0.187
	(0.400)	(0.405)	(0.395)	(0.398)	(0.392)	(0.393)	(0.385)	(0.390)
Two kids (0–5)	0.085	0.086	0.085	0.077	0.077	0.077	0.076	0.074
	(0.280)	(0.281)	(0.278)	(0.266)	(0.267)	(0.267)	(0.264)	(0.262)
Three kids (0–5)	0.012	0.012	0.013	0.010	0.012	0.011	0.011	0.011
	(0.109)	(0.107)	(0.114)	(0.100)	(0.108)	(0.103)	(0.104)	(0.106)
Four or more kids (0–5)	0.002	0.001	0.001	0.002	0.002	0.001	0.001	0.002
	(0.048)	(0.038)	(0.039)	(0.041)	(0.044)	(0.037)	(0.037)	(0.043)
No kids (6–15)	0.593	0.606	0.612	0.592	0.595	0.602	0.605	0.602
	(0.491)	(0.489)	(0.487)	(0.491)	(0.491)	(0.490)	(0.489)	(0.490)
One kid (6–15)	0.223	0.213	0.204	0.220	0.216	0.220	0.213	0.214
	(0.416)	(0.409)	(0.403)	(0.414)	(0.411)	(0.414)	(0.409)	(0.410)
Two kids (6–15)	0.138	0.138	0.138	0.140	0.142	0.132	0.135	0.138
	(0.345)	(0.345)	(0.345)	(0.346)	(0.349)	(0.338)	(0.341)	(0.344)
Three kids (6–15)	0.035	0.035	0.035	0.037	0.038	0.037	0.039	0.037
	(0.185)	(0.183)	(0.185)	(0.190)	(0.190)	(0.189)	(0.193)	(0.188)
Four or more kids (6–15)	0.011	0.009	0.010	0.010	0.009	0.009	0.009	0.010
	(0.103)	(0.093)	(0.101)	(0.101)	(0.093)	(0.097)	(0.097)	(0.098)

Data source: CPS Oct. 1984, 1989, 1993, 1997, 2003; Dec. 1998; Aug. 2000; Sept. 2001; MORG 1983–2005

Table 3.2: Share of women using the available computer

Year	All women	Married women	Married with kids
1984	0.481	.462	.420
1989	0.583	.531	.519
1993	0.663	.632	.625
1997	0.753	.728	.716
2001	0.847	.858	.846
2003	0.864	.876	.858

Share of women that use the available computer for any purpose.
First column shows shares for all 20–59 year old women, second column the share for 20–59 year old women who live with their spouse and the third column refers to 20–59 year old women who live with their spouse and at least one 6–15 year old child.
Data source: CPS Oct. 1984, 1989, 1993, 1997, 2003; Sept. 2001
Calculation of shares accounts for sampling weights.

Table 3.3: Home PC availability and Employment

	1984	**1989**	**1993**	**1997**	**1998**	**2000**	**2001**	**2003**
(1a) ∅	0.066	0.074	0.078	0.102	0.112	0.123	0.124	0.153
	(0.011)	(0.009)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)
(1b) Wage	0.002	0.014	0.008	0.027	0.050	0.057	0.064	0.080
	(0.011)	(0.009)	(0.008)	(0.008)	(0.009)	(0.010)	(0.010)	(0.011)
(1c) Income	0.059	0.077	0.072	0.089	0.104	0.113	0.110	0.132
	(0.011)	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)	(0.011)
(1d) b) & c)	0.011	0.029	0.018	0.032	0.054	0.059	0.063	0.074
	(0.011)	(0.009)	(0.008)	(0.008)	(0.009)	(0.010)	(0.010)	(0.011)
(1e) d) & Kids)	0.014	0.027	0.016	0.029	0.056	0.060	0.060	0.069
	(0.011)	(0.009)	(0.008)	(0.008)	(0.009)	(0.010)	(0.010)	(0.011)
N	20,885	19,421	18,735	16,287	16,107	16,225	19,249	18,401

OLS regressions (weighted to account for sampling probabilities) with employment (0/1 dummy) as dependent variable. The table shows the coefficient estimate on a dummy that indicates that the household owns a computer (or laptop) with the coefficient's heteroscedasticity robust standard errors below (in parenthesis).
Upper panel reports results for all 20–59 year old women, lower panel reports results for 20–59 year old women who live with their spouse.
Specification a) does not include any controls.
Specification b) includes controls for education (dummies), age, age squared, race (white, black, other dummies) state dummies and msa dummies.
Specification c) includes controls for husband's weekly earnings, weekly earnings squared, a dummy if the husband does not have any earnings and a home ownership dummy.
Specification d) combines b) and c).
Specification e) adds dummies for the number of 0–5 and 6–15 year old children in the household, husband's education (dummies), age, age squared.
Data source: CPS Oct. 1984, 1989, 1993, 1997, 2003; Dec. 1998; Aug. 2000; Sept. 2001; MORG 1983–2005

Table 3.4: Home PC use and Employment; Heterogenous impact of home PC availability

	1984	1989	1993	1997	1998	2000	2001	2003
(1'a) ∅	0.046	0.049	0.053	0.076	0.095	0.096	0.105	0.132
	(0.012)	(0.009)	(0.009)	(0.008)	(0.009)	(0.010)	(0.010)	(0.012)
(1'c) Income	0.059	0.068	0.062	0.080	0.099	0.103	0.108	0.120
	(0.012)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.011)	(0.012)
N	17,518	16,821	15,816	14,078	13,897	13,972	16,497	15,414
(2e) PC use & all	0.026	0.041	0.039	0.035			0.051	0.055
	(0.016)	(0.011)	(0.009)	(0.008)			(0.009)	(0.009)
N	20,809	19,305	18,518	16,282			19,235	18,377
(3e) ≤11/No Degree	0.034	0.145	0.040	0.107	0.062	0.094	0.099	0.092
	(0.048)	(0.045)	(0.043)	(0.034)	(0.031)	(0.030)	(0.027)	(0.028)
(3e) 12/High School	-0.004	0.041	0.021	0.040	0.065	0.080	0.051	0.062
	(0.017)	(0.014)	(0.014)	(0.013)	(0.014)	(0.015)	(0.015)	(0.016)
(3e) 13/Some College	0.030	0.041	0.017	-0.010	0.026	0.021	0.060	0.012
	(0.037)	(0.027)	(0.018)	(0.018)	(0.019)	(0.021)	(0.023)	(0.025)
(3e) 14/Assoc. Vocational	0.048	-0.035	0.004	0.043	0.045	0.039	0.063	0.122
	(0.032)	(0.024)	(0.030)	(0.032)	(0.032)	(0.038)	(0.040)	(0.048)
(3e) 15/Assoc. Academic	0.055	-0.013	0.046	0.043	0.039	0.009	0.000	0.106
	(0.055)	(0.042)	(0.035)	(0.033)	(0.034)	(0.040)	(0.043)	(0.051)
(3e) 16-17/Bachelor's	-0.005	0.009	-0.001	-0.000	0.077	0.038	0.042	0.090
	(0.023)	(0.018)	(0.017)	(0.017)	(0.021)	(0.025)	(0.027)	(0.032)
(3e) 18≥/Master's or more	0.074	0.050	0.014	0.062	0.057	0.082	0.154	0.119
	(0.032)	(0.026)	(0.024)	(0.031)	(0.032)	(0.045)	(0.053)	(0.055)
N	20,885	19,421	18,735	16,287	16,107	16,225	19,249	18,401

OLS regressions (weighted to account for sampling probabilities) with employment (0/1 dummy) as dependent variable. Upper panel replicates specifications (1c) and (1e) in table 3.3 omitting women with husbands that report zero earnings. The middle panel of the table shows the coefficient estimate on a dummy that indicates that the woman uses the available computer (or laptop). The lower panel show the coefficients of the interaction of a set of dummies for educational attainment and a dummy that indicates that the household owns a computer (or laptop). Educational attainment in 1984 and 1989 is measured in (completed) years of education and in terms of highest degree obtained thereafter. Below the coefficients the table reports heteroscedasticity robust standard errors (in parenthesis).
All regressions in both panels include the same covariates as in specification e) in table 3.3.
Data source: CPS Oct. 1984, 1989, 1993, 1997, 2003; Dec. 1998; Aug. 2000; Sept. 2001; MORG 1983–2005

Table 3.5: Home PC and (log) hourly wage

	1984	**1989**	**1993**	**1997**	**1998**	**2000**	**2001**	**2003**
(1) PC available	0.018	0.047	0.064	0.100	0.089	0.863	0.086	0.098
	(0.014)	(0.012)	(0.011)	(0.010)	(0.011)	(0.012)	(0.012)	(0.015)
	[0.053]	[0.405]	[0.042]	[0.001]	[0.000]	[0.048]	[0.000]	[0.149]
N	20,885	19,421	18,735	16,287	16,107	16,225	19,249	18,401
(2) PC used	0.041	0.088	0.077	0.117			0.099	0.099
	(0.021)	(0.015)	(0.013)	(0.011)			(0.011)	(0.013)
	[0.050]	[0.400]	[0.010]	[0.000]			[0.000]	[0.160]
N	20,809	19,305	18,518	16,282			19,235	18,377
(3) ≤11/No Degree	0.121	0.076	0.063	0.099	0.107	0.114	0.083	0.130
	(0.063)	(0.068)	(0.063)	(0.041)	(0.042)	(0.035)	(0.035)	(0.043)
(3) 12/High School	0.024	0.036	0.099	0.138	0.090	0.099	0.076	0.109
	(0.021)	(0.017)	(0.017)	(0.016)	(0.016)	(0.017)	(0.017)	(0.023)
(3) 13/Some College	0.046	0.050	0.076	0.073	0.103	0.054	0.130	0.109
	(0.044)	(0.037)	(0.025)	(0.022)	(0.027)	(0.025)	(0.030)	(0.037)
(3) 14/Assoc. Vocational	0.054	0.016	0.047	0.033	0.077	0.017	0.079	-0.002
	(0.041)	(0.033)	(0.044)	(0.039)	(0.043)	(0.049)	(0.049)	(0.057)
(3) 15/Assoc. Academic	-0.093	-0.079	0.052	0.045	0.105	0.114	0.008	0.172
	(0.076)	(0.067)	(0.046)	(0.051)	(0.047)	(0.058)	(0.050)	(0.072)
(3) 16-17/Bachelor's	-0.048	0.084	0.051	0.086	0.060	0.088	0.079	0.040
	(0.033)	(0.025)	(0.024)	(0.025)	(0.029)	(0.038)	(0.034)	(0.042)
(3) 18≥/Master's or more	0.089	0.091	-0.045	0.129	0.088	0.111	0.102	0.053
	(0.046)	(0.038)	(0.040)	(0.050)	(0.056)	(0.084)	(0.072)	(0.093)
	[0.050]	[0.380]	[0.030]	[0.000]	[0.000]	[0.000]	[0.000]	[0.110]
N	20,885	19,421	18,735	16,287	16,107	16,225	19,249	18,401

Heckman selection model estimations (weighted to account for sampling probabilities) with the log of hourly wages as dependent variable. The table shows the coefficient estimate on a dummy that indicates that the household owns a computer (or laptop) with the coefficient's heteroscedasticity robust standard errors below (in parenthesis). In brackets the table reports the p-value of a test for statistical significance of the selection term.

First panel reports results for pc ownership, second panel for PC use, and the third panel shows the coefficients for pc ownership interacted with education.

The estimates include controls for for education (dummies), age, age squared, race (white, black, other dummies) state dummies and msa dummies in both the selection and the wage equation. Husband's education (dummies), age, age squared, weekly earnings and weekly earnings squared, a dummy if the husband does not have any earnings, a home ownership dummy, and dummies for the number of 0–5 and 6–16 year old children in the household are used in the selection equation only.

Data source: CPS Oct. 1984, 1989, 1993, 1997, 2003; Dec. 1998; Aug. 2000; Sept. 2001; MORG 1983–2005

Table 3.6: Home PC and PC use in occupation

	1984	**1989**	**1993**	**1997**	**1998**	**2000**	**2001**	**2003**
(1) PC available	0.015	0.031	0.057	0.076			0.050	0.025
	(0.006)	(0.006)	(0.006)	(0.006)			(0.010)	(0.009)
	[0.000]	[0.000]	[0.030]	[0.190]			[0.000]	[0.000]
N	20,885	19,421	18,735	16,287			19,249	18,401
(2) PC used	0.036	0.069	0.075	0.087			0.092	0.040
	(0.009)	(0.008)	(0.007)	(0.006)			(0.019)	(0.007)
	[0.000]	[0.000]	[0.020]	[0.140]			[0.840]	[0.000]
N	20,809	19,305	18,518	16,282			19,235	18,377
(3) ≤11/No Degree	0.003	0.068	0.098	0.044			0.100	-0.022
	(0.019)	(0.031)	(0.035)	(0.024)			(0.021)	(0.029)
(3) 12/High School	0.024	0.051	0.091	0.115			0.098	0.047
	(0.011)	(0.011)	(0.012)	(0.011)			(0.012)	(0.014)
(3) 13/Some College	0.005	-0.007	0.055	0.087			0.078	0.043
	(0.021)	(0.022)	(0.014)	(0.014)			(0.016)	(0.020)
(3) 14/Assoc. Vocational	0.011	0.005	0.031	0.053			0.072	-0.033
	(0.022)	(0.017)	(0.023)	(0.022)			(0.028)	(0.033)
(3) 15/Assoc. Academic	-0.012	-0.019	0.018	0.091			0.030	0.022
	(0.024)	(0.029)	(0.028)	(0.025)			(0.024)	(0.037)
(3) 16-17/Bachelor's	0.007	0.025	0.023	0.008			0.040	0.029
	(0.011)	(0.011)	(0.011)	(0.011)			(0.014)	(0.023)
(3) 18≥/Master's or more	0.028	0.042	0.037	0.043			0.051	-0.005
	(0.018)	(0.015)	(0.014)	(0.014)			(0.037)	(0.032)
	[0.000]	[0.000]	[0.030]	[0.170]			[0.530]	[0.000]
N	20,885	19,421	18,735	16,287			19,249	18,401

Heckman selection model estimations (weighted to account for sampling probabilities) with the share of computer users in the woman's occupation as dependent variable. The table shows the coefficient estimate on a dummy that indicates that the household owns a computer (or laptop) with the coefficient's heteroscedasticity robust standard errors below (in parenthesis). In brackets the table reports the p-value of a test for statistical significance of the selection term.
First panel reports results for pc ownership, second panel for PC use, and the third panel shows the coefficients for pc ownership interacted with education.
The estimates include controls for for education (dummies), age, age squared, race (white, black, other dummies) state dummies and msa dummies in both the selection and the wage equation. Husband's education (dummies), age, age squared, weekly earnings and weekly earnings squared, a dummy if the husband does not have any earnings, a home ownership dummy, and dummies for the number of 0–5 and 6–16 year old children in the household are used in the selection equation only.
Data source: CPS Oct. 1984, 1989, 1993, 1997, 2003; Dec. 1998; Aug. 2000; Sept. 2001; MORG 1983–2005

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