Employment, Careers and Productivity: Lessons From Three E.U. Countries

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Abstract

This thesis is composed of four empirical studies which use data from Portugal, the United Kingdom and Germany to examine four topical aspects of employment, careers and productivity in these countries' labour markets.

The second chapter studies the impact that a 49.3% change in the legal minimum wage for workers aged 18 and 19 in Portugal had on the wages and employment of this age group of workers. It uses firm-level micro data to compare the employment growth of 18-19 year old workers with employment growth of older workers. It also looks separately at firms more and less likely to be affected by the minimum wage shock.

The third chapter studies the impact of foreign direct investment (FDI) on the productivity of domestic firms in the UK. It uses a plant-level panel covering UK manufacturing to find evidence of FDI spillovers. It does so by investigating the correlation between a domestic plant's TFP and the foreign-affiliate share of employment in that plant's industry and, independently, in that plant's region. A number of different specifications are estimated in order to minimise potential endogeneity bias.

The fourth chapter estimates returns to job tenure and labour market experience in the United Kingdom and Germany using various methods to correct for heterogeneity and endogeneity biases. It also estimates the returns to tenure and experience by qualification group. Results are interpreted in light of the differences between the two labour markets' institutions.

The fifth chapter compares returns to tenure and experience in union and non-union jobs in the United Kingdom in the 80s and 90s. It uses longitudinal data and

instrumental variables methods to correct for potential individual and job match heterogeneity biases. Returns are also calculated separately for jobs with and without seniority wage scales.

Ithaca

When you set out on your journey to Ithaca,
pray that the road is long,
full of adventure, full of knowledge.
The Lestrygonians and the Cyclops,
the angry Poseidon -- do not fear them:
You will never find such as these on your path,
if your thoughts remain lofty, if a fine
emotion touches your spirit and your body.
The Lestrygonians and the Cyclops,
the fierce Poseidon you will never encounter,
if you do not carry them within your soul,
if your soul does not set them up before you.
Pray that the road is long.
That the summer mornings are many, when,
with such pleasure, with such joy
you will enter ports seen for the first time;
stop at Phoenician markets,
and purchase fine merchandise,
mother-of-pearl and coral, amber and ebony,
and sensual perfumes of all kinds,
as many sensual perfumes as you can;
visit many Egyptian cities,
to learn and learn from scholars.
Always keep Ithaca in your mind.
To arrive there is your ultimate goal.
But do not hurry the voyage at all.
It is better to let it last for many years;
and to anchor at the island when you are old,
rich with all you have gained on the way,
not expecting that Ithaca will offer you riches.
Ithaca has given you the beautiful voyage.
Without her you would have never set out on the road.
She has nothing more to give you.
And if you find her poor, Ithaca has not deceived you.
Wise as you have become, with so much experience,
you must already have understood what Ithacas mean.

Constantine Cavafy (1863–1933), Greek poet. repr. in Collected Poems, eds. George Savidis, trans. by Edmund Keeley and Philip Sherrard (1975). "Ithaca," (1911).

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Preface

Chapters 2 "The impact of minimum wages on youth employment in Portugal" and 5 "Returns to seniority and experience in union and non-union jobs in Britain in the 80's and 90s" are single authored. Chapter 3 "Does inward foreign direct investment boost the productivity of domestic firms?" is joint work with Jonathan Haskel and Mathew Slaughter. Chapter 4 "An illustration of the role of job mobility in the estimation of returns to job seniority and labour market experience: a comparison of the U.K. and Germany" is joint work with Christian Dustmann.

Data Sources

Data from Quadros de Pessoal in Chapter 2, was kindly provided by the Statistics Department of the Portuguese Ministry of Qualification and Employment. The Annual Census of Production Respondents Database was used in chapter 3 as part of the U.K. Office of National Statistics business-data-linking project. The British Household Panel Study and the Labour Force Survey have been provided by the UK Data Archive. The data for Germany was made available by the German Socio-Economic Panel Study (SOEP) at the German Institute for Economic Research (DIW), Berlin.

Chapter 1. Introduction

This thesis is composed of four empirical studies which use data from Portugal, the United Kingdom and Germany to examine four topical aspects of employment, careers and productivity in these countries' labour markets. The second chapter studies the impact that a 49.3% change in the legal minimum wage (MW) for workers aged 18 and 19 in Portugal had on the wages and employment of this age group of workers. It uses firm-level micro data to compare the employment growth of 18-19 year old workers with employment growth of older workers. In addition, it compares employment outcomes in firms more likely to be affected by the MW change with firms less likely to be affected, where a firm is more likely to be affected if it was paying wages below the adult MW to workers aged 18 and 19 in 1986. The main findings are that the increase in the minimum wage significantly reduced employment of 18 and 19 year-olds, but increased employment of 20-25 year-olds.

The third chapter aims at providing an answer to the following question: are there productivity spillovers from foreign direct investment (FDI) to domestic firms, and, if so, how much should host countries be willing to pay to attract FDI? To examine these questions we use a plant-level panel covering U.K. manufacturing from 1973 through 1992. For empirical purposes, we distinguish two main channels through which spillovers can occur. Along industry lines and regional lines.

Productivity spillovers along industry lines would take place if domestic firms learn from affiliates in the same industry. This can happen via trade shows, supplier/distributor discussions, exposure to affiliate products, marketing, and patents, technical support from affiliates, reverse engineering, etc. Spillover mechanisms along regional lines would be empirically observed if spillovers happened as a result of labour turnover. If at least some of the knowledge particular to foreign affiliates is embodied in their labour force, and if inter-regional labour mobility within a country is low, then these spillovers are likely to be concentrated within regions where the affiliates operate rather than dispersed country-wide. Across a wide range of specifications, estimates show a significantly positive correlation between a domestic plant's TFP and the foreign-affiliate share of activity in that plant's industry. This is consistent with positive FDI spillovers along industry lines. Typical estimates suggest that a 10 percentage-point increase in foreign presence in a U.K. industry raises the TFP of that industry's domestic plants by about 0.5 percent. We do not generally find significant effects on plant TFP of the foreign-affiliate share of activity in that plant's region. Calculations based on our estimates show that per-job incentives governments have granted in recent high-profile cases appear to be higher than the per-job value of these spillovers.

The fourth chapter estimates the returns to tenure and experience in the United Kingdom and Germany using various methods to correct for heterogeneity and endogeneity biases. We show evidence that job mobility is higher in the UK than in Germany, and that job movers are negatively selected in Germany and not in the UK. In our discussion of the results we point out that these results can be driven by "stickier wages" in Germany (in models with employers' learning of workers' ability) as well as by adverse selection of job movers in Germany due to low mobility in a context of asymmetric information between current and prospective employers about workers' ability. After correcting for most bias associated with job and individual heterogeneity, our findings suggest that returns to tenure are close to zero in both countries and returns to experience are substantially higher in the UK than in Germany. According to our estimates, ten years of labour market experience are associated with average wage returns of between 60 and 70 percent in the UK and between 30 and 40 percent in Germany. Separate estimates for different qualification groups show that in Germany, it is the group of workers with apprenticeship training that is driving the low returns to labour market experience. The underlying reason can be that the German Apprenticeship system may provide workers with general skills which accrue already at the starting of the post-apprenticeship wage, resulting in lower wage growth with labour market experience.

The fifth chapter compares returns to tenure and experience in the union and non-union sectors in the United Kingdom in the 90s. Our results show that returns to tenure are only insignificantly different from zero in union jobs and when estimated with least squares. When instrumental variables are used to correct for individual and job match heterogeneity returns to tenure are insignificantly different from zero in both sectors. While there is no evidence of heterogeneity bias in the non-union sector, results suggest positive heterogeneity bias in the union sector. This would seem unconvincing under the usual interpretation of ability and job-match induced biases. However, it is consistent with a plausible hypothesis of union wage mark-up heterogeneity affecting duration of jobs. Returns to experience are lower in union jobs than in non-union jobs, although this difference in not statistically significant. This result is nevertheless rather robust, since it holds for all the period analysed, and with all estimation methods. Finally, contrary to previous evidence and unlike in non-union jobs, we find no evidence that returns to experience are higher in union jobs with pay scales than without.

Chapter 2. The impact of minimum wages on youth employment in Portugal

2.1 Introduction

This chapter evaluates the employment impact of an increase in the minimum wage of teenagers that took place in Portugal on the 1st of January 1987. At that date, workers aged 18 and 19 became entitled to the full minimum wage, instead of 75% of the adult wage as had previously been the case. Thus, the legal minimum wage for this specific age group increased 49.3% between 1986 and 1987 (a 35.5% increase in real terms). Since between 1986 and 1987 the minimum wage for workers aged 20 or more increased by 12% (1.6% in real terms), the wage increase of workers aged 18 and 19 relative to the one of the older workers was 37.3% (33.9% in real terms). The occurrence of such a large shock in the minimum wage potentially offers a "natural experiment" in which the employment of 18 and 19 year olds can be compared with that of older workers.

This methodology is not new in evaluating the impact of the minimum wage (MW) on employment. Earlier studies on the MW by Richard Lester (1946) and others used "natural experiments" to study the effect of the introduction of the federal minimum wage in the US[1]. This approach has more recently been revived by Card (1992a), Card and Krueger (1994) using MW cross state variations in the US and by Card (1992b), Katz and Krueger (1992) and Bernstein and Schmitt (1998) to look at the effect of changes in the US federal law.

The "natural experiment" approach has a clear advantage over other most common methodologies since it makes use of well identified exogenous variation in the minimum wage. In many time-series and cross-section studies, which use the ratio of the MW to average wages or the Kaitz index (H. Kaitz, 1970), there is typically little variation. Moreover, MW variation may be endogenous, to the extent it depends on political decisions which may be based on employment expectations. This potentially biases estimates of employment effects.

The existing evidence fails to give a clear-cut answer about the employment effects of minimum wage policies. In their thorough survey, Brown, Gilroy and Kohen (1982) concluded (mainly based on time-series studies using US data before the 1980s) that a 10% increase in the minimum wage reduces teenage employment by 1 to 3%, though with no identifiable effect on the adult labour market. This apparent consensus has been challenged by subsequent research well represented in Card and Krueger (1995). By using different datasets and methodologies as well as reinterpreting and re-examining previous studies they concluded that "the new evidence points towards a positive effect of the minimum wage on employment; most shows no effect at all" (page 1).

Their work has been a source of controversy (see the reviews of Card and Krueger in the *Industrial and Labor Relations Review*, 1995; papers on the minimum

[1] The US Federal Minimum Wage was introduced in the Fair Labor Standards Act of 1938.

wage in the *American Economic Review*, 1995; Neumark and Washer, 1995) and has been followed by a resurgence of work on this topic. In his recent re-evaluation of the minimum wages empirical literature using US data, Brown (1999) concluded that "the short-term effect of the minimum wage on teenage employment is small [...] (and zero is often hard to reject)" (page 2154).[2]

The Portuguese MW change is of particular interest since it is so large and aimed directly at teenagers. Most theoretical frameworks predicting non-negative employment elasticities do not apply to "large enough" minimum wage increases. Also, it is generally accepted that youths are more likely to have larger minimum wage employment elasticities (Brown et al., 1982, Dolado et al., 1996, Abowd et al., 1997, OECD, 1998). In addition, employment effects of minimum wage changes may depend strongly on the particular context in which they are implemented. In the period from 1984 to 1986 the average ratio of minimum wages to average wages in Portugal was still 56%. In the US this ratio was already below 50% during the 70's, and went well below 40% during the 80s[3]. In addition, the share of workers under 20 paid close to the minimum in that period is considerably higher (see Section 3.4 below). Finally, the fact that Portugal is a small open economy with little possibility of adjusting to such a shock via product price increases adds to the reasons for a potentially strong negative employment impact. In fact, Portugal was already part of the European Union and the secondary sector was still strong relative to the services sector. It is well known that services sector industries, many of which are not traded across

[2] This conclusion is based on recent studies using CPS data that use year dummies to control for macroeconomic conditions. Burkauser, Couch and Wittenburg (2000) argue nevertheless that given the discrete nature of the minimum wage variation, this procedure eliminates the federal minimum wage variation and by using other ways to control for macroeconomic effects they find that "the elasticity of teenage employment with respect to the minimum wage lies in the range of –0.2 to –0.6", which is rather close to the range of values found in the present study.

international borders, have a much better chance of insulating themselves from shocks due to less international competition.

This study finds that in Portugal the 1987 abolishment of the sub-minimum wage for teenagers had a negative impact on their employment. The estimated employment elasticity is in the range between –0.2 and –0.4. For the factors set out above, these values are at the top of the range (in absolute terms) of values usually found in the MW literature. There is also evidence of some substitution towards older workers (i.e. young adults' relative employment seems to have risen with this MW policy).

The structure of this chapter is as follows. The next section summarises the conditions under which the change in the law took place. Section 2.3 describes the data used. Section 2.4 inspects the wages of the various age groups of workers before and after the MW change. Section 2.5 presents the difference in differences analysis which is extended by using firms as control groups in section 2.6 and by including firm entrants and exitors in section 2.7. Section 2.8 concludes.

2.2 "The Quasi-Experiment"

Monthly statutory minimum wages were introduced in Portugal in 1974 following the democratic revolution in the same year. Up until 31st December 1986, workers aged 20 years or older were entitled to the full statutory minimum wage, while 18 and 19 year old workers were entitled to 75% of the adult minimum wage. Changes in MW levels were then expected to occur every January, as since 1983 this

[3] Source: OECD, Minimum Wage database.

had been the common practice[4]. In August 1986 there was for the first time news in the Portuguese press about changing the starting age for full MW entitlement. The change to the law was announced in the daily papers on the last day of that year. Thus, from 1st January 1987, workers became entitled the full minimum wage from the age of 18[5]. The minimum wage change was remarkably large. Between December 1986 and January 1987, the younger workers saw their MW increase by 49.3%, while workers aged 20 or more had an increase in their MW of only 12%. The corresponding percent increases in real terms are 35.5% for the former group and 1.6% for the latter one. This implied that workers aged 18 and 19 experienced a wage increase relative to the older workers of 37.3% (33.9% in real terms).

The MW change under analysis is likely to have been exogenous with respect to employment. The logic behind the new law had to do with legal rights and citizenship. In the past, an individual had been considered an adult for legal matters at the age of 21. During the eighties it had been already established that 18 would be the age at which an individual would be entitled to full rights and duties in the legal system. This law generalised this principle to the statutory minimum wage.

This study looks at the short and medium term impacts of this change, using a five year panel of firms, from 1985 to 1989. The information collected at each wave refers to March of each year. Thus, the 1986 wave corresponds to a point in time 9 months before the change, the 1987 wave to 3 months after, and the 1988 wave to 15 months after. The analysis mostly uses data for 1986 and 1988. The time elapsed

[4] The minimum wage levels were revised on a year basis to take into account the evolution of inflation and average wages.

[5] Workers aged 17 years old had even lower sub-minimum wages: they were entitled 50% the statutory minimum up to 1986 and 75% from 1987. Workers younger than 17 were entitled 50% of the statutory minimum up to 1987 and 75% from 1988. Because of the few number of workers in this age

between March 1986 and the change in the law is long enough to ensure that no anticipated adjustments were made.

To identify the employment effects of the MW shock, the employment of workers aged 18 and 19 is compared with that of older workers. This procedure hinges on the assumption that in the absence of this shock, employment growth during the period under analysis would not differ with age. In other words, the employment impact will be measured as the difference between the change in the 18 and 19 year olds' employment stock and the change in the older workers' employment stock. Since we had to formulate a parsimonious firm-level data request, we chose two age bands of older workers: 20 to 25 and 30 to 35.

Given the specific context of this quasi-experiment, a distinction must be made between employment flows and stocks. The data used contains the stock of employment of different ages in firms at a given time. It does not have information at the individual level nor on how individuals' employment varies as they age. Thus, because we use a random sample of 30% of the Portuguese firms[6], it is as if we conduct a comparative static analysis of the three age groups' employment in the entire working population. In summary, this experimental design will provide an answer to the following question: did the Portuguese firms change on average the age structure of their workforce in response to the increase in the relative cost of the young workers?

In comparing the employment evolution of the three groups of workers it is important to note that only the relative employment effect on the younger group can

ranges as well as the few number of firms employing these workers, the analysis focus only on 18 and 19 year olds.

[6] Entrants and exitors are added to the panel of firms in Section 2.7, to ensure representativeness of the sample before and after the change in the MW.

be identified. Thus the measured employment effect understates the true employment effect if firms reduce production and therefore reduce employment at all age levels. But the impact on teenagers could overstate the overall MW effect if firms substitute away from employing teenagers, while favouring the recruitment of older and therefore more experienced or qualified workers. In order to identify the employment impact on the younger group, we assume that while firms might substitute teenagers for 20 to 25 year olds, the substitutability between 18 and 19 year olds and 30 to 35 year olds is negligible, as these workers are much more likely to differ in terms of individual and job characteristics. As the older workers' employment is a reference measure against which employment of both younger groups is compared, both direct employment effect on the 18 and 19 year olds and substitution effect towards workers aged 20 to 25 can be identified.

2.3 The data

The data used in this work was computed from Quadros de Pessoal (QP), an extensive data source produced by the Statistics Department of the Portuguese Ministry of Qualification and Employment[7]. All firms established in Portugal with paid workers are legally required to report to this database[8]. A random sample of 30% of the Portuguese firms in 1986[9] was drawn. The panel was then constructed by

[7] The data was kindly supplied by the Statistics Department of the Portuguese Ministry of Qualification and Employment.

[8] See Cardoso (1997) for more details.

[9] 30% of the population corresponds to 32031 firms in 1986. However, firms located in the islands were excluded, as they have regional governments, with autonomous policies. Public administration firms were also excluded as well as firms belonging to the primary sector or whose economic activity is domestic work as they have specific minimum wage regimes. The resulting sample has 29,250 firms (see Table 2-5 in Appendix 2.A).

following these firms up to 1989. Entrants and exiting firms were added to the panel to ensure representativeness [10].

For each of the age groups, the following firm level variables were collected: number of workers on the payroll of the firm, average monthly hours, average overtime hours, total wages for normal hours and total wages for overtime. The following set of firm characteristics[11] was also collected: size, district and industry.

2.4 Minimum wages and wage changes

This section investigates the minimum wage impact on average wages and wage distributions of the three age groups. Figure 2-1 depicts the monthly wage[12] distributions in 1986 and 1987[13]. For the workers aged 18 and 19 (Figure 2-1a) there are two spikes in 1986: one at the interval 16-18 and another one at the interval 22-24 thousand escudos. The former is at the minimum wage for this age group (16875 escudos)[14]. The latter is at the general compulsory minimum wage for non-agricultural workers aged 20 or older (22500 escudos). Since the wage distributions include part time workers, the "true" spike at the minimum wage should be even higher than the one depicted. As the minimum wage is set monthly, the relationship between these

[10] A random sample of 30 percent of the firms that started activity in 1987 was drawn and these firms were followed until 1988. Additionally, random samples of 30 percent of the firms that started activity in 1988 and of 30% of the firms that ceased or started activity from 1985 to 1986 were collected.

[11] For the panel, this information refers to the year of 1986; for entries and exits, refers to the relevant years. Date of creation of the firm was not available in the survey in those years, and other financial and ownership indicators were information not disclosed because of confidentiality concerns.

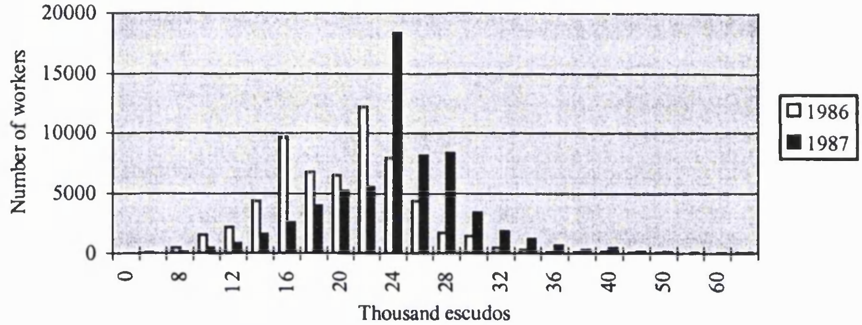
[12] These are the gross monthly wages for normal hours.

[13] The statistics in these distributions were ordered from the Statistics department of the Ministry of Qualification and Employment, and are therefore computed directly from the Universe of Portuguese workers for the three age groups. This was the most desegregated wage data possible by age group, since the wage intervals depicted were the ones made available by the Statistics department of the Ministry of Qualification and Employment, and not the ones chosen by the author.

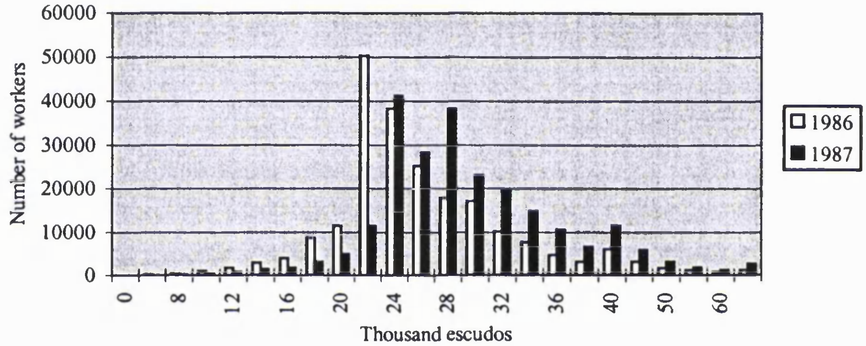
[14] This value is obtained by: 22500x0.75.

workers' wages and the minimum wage is lost. This helps explain the existence of workers paid below the 18-19 year old minimum. Other possible explanations are partial minimum wage exemptions for handicapped workers, apprentices and very small firms[15] and non-compliance (the distributions shown include all these). 15% of the workers lie in the 16-18 interval, while 20% lie in the 22-24 interval. This indicates that the sub-minimum for 18-19 year olds imposes a binding restriction on their wage distribution, although part of these workers are already paid at least the full MW. Efficiency wages, insider-outsider theories, and internal markets may all help explain why a considerable number of 18-19 year old workers are paid above their minimum wage. Employers may find it advantageous to pay their 18 and 19 year old workers the full minimum wage if it enhances their morale, increases productivity or reduces turnover. In addition, employers may encounter difficulties in implementing pay discrimination based on age among workers performing similar tasks, which would explain the spike at the full MW.

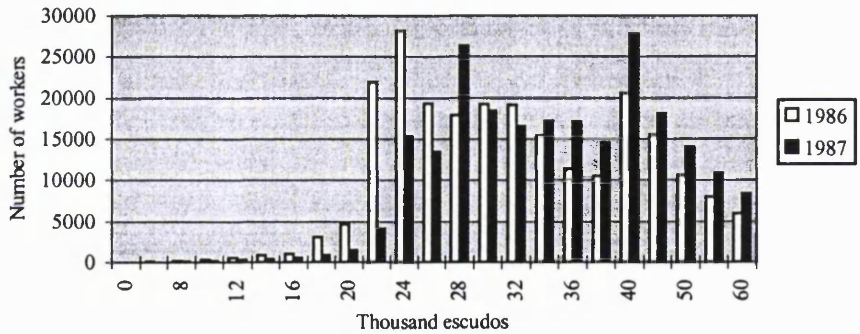
[15] Firms with 5 or less workers or firms with less than 50 workers who claimed to be subject to an unbearable rise in labour costs enjoyed partial exemptions, for they would just be enforced to pay the agricultural minimum wage which is lower than the non-agricultural one.



(a)



(b)



(c)

Source: Quadros de pessoal, DEMQE.

Figure 2-1(a-c): Wage distributions in 1986 and 1987. (a) 18-19 year old workers; (b) 20-25 year old workers; (c) 30-35 year old workers.

In 1987 however, a very sharp spike can be observed at the 24-26 interval, which encloses the new minimum (25200). Twenty nine percent of the 18-19 year olds received wages within this interval[16]. Looking at the wage distributions for the 20-25 year olds in 1986 (Figure 2-1b) it is clear that the compulsory minimum wage cuts the wage distribution. The minimum wage spike in 1987 is somewhat smaller than the one in 1986 so, if anything, the minimum wage lost some of its bite between 1986 and 1987. Moreover, the wage distribution shows some signs of having moved up independently of the minimum wage change. The same applies to the evolution of 30-35 year old workers wage distributions (Figure 2-1c).

To examine this visual impression more formally the following equation is estimated:

$$\Delta W_{ijt} = \alpha + \beta_1 D^{20} + \beta_2 D^{30} + \gamma X_j + \varepsilon_{ijt} \tag{2.1}$$

The dependent variable is the proportional change in the real hourly wage of workers of age group i in firm j between 86 (the year before the MW change) and year t, after the MW change, where t may be 87, 88 or 89. D^{20} is a dummy variable equal to 1 for the 20-25 year old group, and D^{30} is 1 for workers aged 30 to 35. X_j is a vector with the firm characteristics at time t=0. β_1 and β_2 are the difference in difference estimators: they measure the difference in the proportional wage change between each of the older groups and the omitted younger group of workers.

As the new law was enforced from January 1987, the average wage of the 18-20 group is expected to rise markedly between March 1986 and March 1987. β_1 and β_2 are therefore expected to be negative, given the lower wage growth of the 20-25 and 30-35 year olds.

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[16] 20000 out of 63578 workers are at the interval close the minimum wage. Note that 25200 is not in the beginning of the interval, as it was with the previous values.

Table 2-1: Differences in the proportional hourly wage growth

	1986 and 1987	1986 and 1988	1986 and 1989
Dummy = 1 if age is [20-25]	-0.074**	-0.059**	-0.057**
	(0.005)	(0.012)	(0.010)
Dummy = 1 if age is [30-35]	-0.067**	-0.059**	-0.044**
	(0.005)	(0.012)	(0.010)
Constant	0.112**	0.122**	0.256**
	(0.012)	(0.019)	(0.038)
Number of firms	15258	13286	11952
Number of observations	25388	22084	19937

Note: The dependent variable is the time difference in the average hourly wage divided by the hourly wage in 1986. Hourly wage used is for normal time, but the results remain unchanged if overtime pay is included. Robust standard errors in parenthesis. Other regressors are: size, 19 industry dummies and 7 region dummies. The number of observations is less than three times the number of firms because not all firms employ workers of all age groups, thus the wage variable has missing values.
* - Significant at 5% level., ** - Significant at 1% level.

Table 2-1, column 1 shows that between 1986 and 1987 the proportional wage growth of workers aged 20 to 25 was 7.4% below the wage growth of workers aged 18 and 19 and that the wage growth of workers aged 30 to 35 was 6.7% below that of teenagers. The other columns show that wage growth of the older groups was also significantly negative for 1986-88 and 1986-89 but by less than in the 1986-87 interval[17], giving evidence that the differences in the wage growth took place between 1986 and 1987 only.

How can the wage growth of 18 and 19 year olds be only around 7% higher than the wage growth of older workers, if they experienced a relative minimum wage increase of 33.9 percent? A possible explanation could be the presence of ripple effects, i.e., the wages of older workers could have increased with the 18 and 19 year olds' wage increase as an indirect effect of the minimum wage shock. Ripple effects

[17] We actually estimated (2.1) for the intervals 1987 to 1988 and 1988 to 1989. None of the relevant coefficients was significantly different from zero, except the one that compared teenagers with young adults' proportional wages for the period 1987 to 1988. This difference was estimated to be -0.0165

could occur due to internal markets, with relative wages inside the firm playing an important role in wage determination. By comparing the wage growth rates of older workers in 1986-87 with the other years for which there is data available, we find evidence of what can be interpreted as a mild ripple effect. Table 2-6 in the appendix shows the real hourly wage growth for the panel of firms sampled in 1986 by age group. The real hourly wage growth for the 20-25 year olds is 3.7 percent between 1995 and 1996, 4.5 between 1996 and 1997, 0.8 between 1997 and 1998, and 2.4 between 1998 and 1999. For the 30-35 year olds the corresponding wage growth rates are 3.5, 5.2, 0.9 and 4.9 percent. The hourly wage growth rates for the 20-25 and 30-35 year olds seems to have been slightly higher between 1986-87 than in the remaining available years, which could be a consequence of the minimum wage shock.

It seems plausible therefore that the major explanation for the difference found between relative minimum and average wage increases of the 18 and 19 year olds in 1986-87 is the fact that in 1986 a considerable proportion of these workers were already paid at least the full minimum wage. In any case, both the wage distributions in figure 2.1 (a-c) and the wage regressions in table 2.1 give evidence that the abolishment of the wage reduction for teenagers in 1987 had a significant impact on their relative wage increase.

(with a standard error of 0.005), and so was very low when compared to the one of the 1986 to 1987 interval.

2.5 Estimates of employment effects

The formulation used to estimate the employment effects is similar to the one for the wages, where now the dependent variable, ΔN_{ijt}, is the change in the number of workers of age group i in firm j between 86 (the year before the MW change) and year t, after the MW change, where t may be 87, 88 or 89:

$$\Delta N_{ijt} = \alpha + \beta_1 D^{20} + \beta_2 D^{30} + \gamma X_j + \varepsilon_{ijt} \qquad (2.2)$$

X_j is a vector of the firm characteristics in 1986. D^{20} is a dummy variable equal to 1 for the 20-25 year old group, and D^{30} is 1 for workers aged 30 to 35. β_1 and β_2 are the difference in difference estimators: they measure the difference in the employment change between each of the older groups and the omitted younger group of workers. The results of estimating (2.2) are set out in the first 3 columns of Table 2-2. Each column estimates (2.2) for different year intervals. Looking at the first row, sign and standard errors of the coefficients show that employment of 20-25 year olds rose significantly relative to teenagers in every period. Looking at the second row, the coefficients for the older workers are also positive but smaller and only significant for the difference between 1986 and 1988. This suggests a negative impact on the 18-20 year olds employment and a substitution effect between 18-20 year olds and 20-25 year olds.

Table 2-2: Differences in the number of workers and the number of hours

	Difference in the number of workers			Difference in the number of hours		
	1986	1986	1986	1986	1986	1986
	1987	1988	1989	1987	1988	1989
Dummy = 1 if age is [20-25]	0.087**	0.196**	0.223**	14.53**	34.73**	31.93**
	(0.020)	(0.033)	(0.041)	(2.96)	(4.72)	(5.89)
Dummy = 1 if age is [30-35]	0.025	0.107**	0.010	4.62	23.94**	9.62
	(0.020)	(0.033)	(0.056)	(3.30)	(5.48)	(8.67)
Constant	0.011	0.025	0.178	2.48	-1.32	24.14
	(0.052)	(0.111)	(0.152)	(7.55)	(14.84)	(21.11)
Number of Firms	23879	22014	20895	23879	22014	20895
Number of Observations	71637	66042	62685	71637	66042	62685

Note: The dependent variable in columns 1 to 3 is the difference in the total number of workers in the payroll of the firm and in columns 4 to 6 is the difference in the total number of hours worked (normal hours and overtime). Robust standard errors in parenthesis (random effects). Other regressors are: size, 19 industry dummies and 7 region dummies. Region dummies are in most cases not significant, while other controls are significant. Firm controls just reduce slightly standard errors, not affecting the coefficients. * - Significant at 5% level, ** - Significant at 1% level.

The above results point to an employment-minimum wage elasticity of -0.4 for the 18-19 year olds in the period 1986-88. This elasticity is computed based on the differences-in-differences estimation with the 30-35 year olds for the period 1986-1988. The elasticity's numerator is the percent relative employment change, i.e., the ratio between –0.107 and the 18-19 year olds average employment in 1986 in the firms used in the regression, 0.785[18]. The elasticity's denominator is the percent variation in the real minimum wage for the same group of workers relative to older workers (35.5-1.6)[19]. The gross elasticity of substitution between 20-25 year olds and teenagers can be calculated in a similar way. The elasticity's numerator is the percent employment change of 20-25 year olds net of employment change of 30-35 year olds. This elasticity uses the difference between the difference-in-differences estimation

[18] 0.785 is the ratio between the total number of 18-19 year olds in 1986 in the firms surviving until 1988 (row 6, column 3 of table 2.5 in the appendix) 17,291, and the total number of firms present in 1986 and 1988 (row 1, column 3 of table 2.5 in the appendix) 22,014.

[19] –0.004 = (–0.107/0.785)/(35.5-1.6)

with the 20-25 year olds (0.196) and with the 30-35 year olds (0.107) for the period 1986-1988. For the numerator this difference, 0.089, is divided by the 20-25 year olds average employment in 1986 in the firms used in the regression, 2.798[20]. The denominator is again the percent variation in the real minimum wage for the 18-19 year olds relative to that of older workers (35.5-1.6). The gross elasticity of substitution between 20-25 year olds and teenagers is 0.09[21]. Similar calculations for the interval 1986-89 would yield a negligible 'direct' elasticity for the 18-19 year olds and a somewhat larger gross substitution elasticity between the 20-25 year olds and the younger workers. This will be discussed in section 6 where additional results will help shed some light on this apparent puzzle. It is worth noting that although we favour a specification which uses employment levels instead of logarithms[22], when differences in log-employment are used as dependent variable, D^{20} and D^{30} coefficients are positive and significantly different from zero for all year intervals.

The impact of a minimum wage shock on wages and employment depends on the initial distribution of wages. One may for this reason be interested in calculating the average wage-employment elasticities in addition to the minimum wage-employment elasticities. Similar calculations to the ones described above apply, where now the denominator is the difference between the 18-19 year olds and the older groups' proportional hourly wage growth for the period 1986-88, i.e. minus the

[20] 2.798 is the ratio between the total number of 20-25 year olds in 1986 in the firms surviving until 1988 (row 7, column 3 of table 2.5 in the appendix) 61,615, and the total number of firms present in 1986 and 1988 (row 1, column 3 of table 2.5 in the appendix) 22,014 .

[21] 0.0009 = (0.089/2.798)/(35.5-1.6).

[22] Using $\Delta ln(N_{ijt})$ is consistent with assuming that in the absence of the MW change firms would vary the employment of all age groups in similar percentages and using $\Delta(N_{ijt})$ is consistent with assuming that the structure of firms' workforce before the MW change is such that in the absence of the MW change, firms would be indifferent between a marginal increase in the employment of any of the age groups.

estimated coefficients of the older groups' dummies shown in Table 2-1. The employment-average wage elasticity for the 18-19 year olds based on differences-in-differences estimation with the 30-35 year olds for the period 1986-88 is 23[23] percent and the gross elasticity of substitution between the 20-25 year olds employment and teenagers average wage is 5.4[24] percent.

Finally, the key identifying assumption of the difference-in-differences estimator is that in the absence of the minimum wage shock, the average employment change in all groups would be the same:

$$\beta_g = 0, g=1,2 \quad \text{or} \quad E\left[\varepsilon_{ijt} \middle| D^{20}, D^{30}\right] = 0 .$$

Any other labour market shock that took place between 1986 and 1989 and that affected the three age groups' employment growth in a similar way, is differenced out by the difference-in-differences estimator and does not bias the results. However, if there were other shocks with different impacts on the three age groups' employment growth, these would violate this estimator identifying assumption and would bias the estimates. We are not aware of other demand side shocks that could have had an asymmetric affect in any of the three age groups. On the supply side, during the 80s Portugal saw an increase in the number of university vacancies. This would be more likely to affect employment and wages in the absence of unemployment. This was not the case, however. 1986 is only the beginning of the economic expansion following the harsh times that took place in Portugal during the first half of the decade. Still, even in the presence of youth unemployment, the increase in the number of university vacancies could have affected the composition and quality of the labour supply of 18 and 19 year olds and could potentially bias the above results. As a robustness check,

[23] $-0.023 = (-0.107/0.785)/(5.9)$

we extend the difference-in-difference estimator, by adopting a different experimental design, which hinges on different assumptions in the next section.

Columns 4 to 6 of Table 2-2 show the coefficients for the impact on the number of hours worked by the relevant age groups. The results mirror those for employment. From the comparison of the coefficients for the two measures of labour input (number of workers and number of hours worked) one can infer whether firms adjust mainly through firing and (not) hiring or through adjusting their workforce's working time. Between 1986 and 1988 the average growth of the number of teenagers per firm was 0.196 workers lower than the growth of young adults. These workers would have to be working on average 177 hours per month to generate an average loss of 34.73 in the number of hours if all employment adjustment had been made through hiring and firing. For the results obtained with the 30-35 year olds this number is considerably higher (223.7 hours a month). In the sample, workers aged 18 and 19 work on average 165 hours per week in 1986, so the employment impact is not completely captured by changes in the number of people working, but also through the reduction of individuals' working time.

It is worth noting that since hours of work are reported by the employer, they may understate actual hours. For our analysis this could affect the above findings if, for example, following the minimum wage rise workers faced increased pressure to work longer hours. This could lead to a stronger understatement of the number of hours worked after the minimum wage shock. The above findings of a negative impact of the minimum wage change on the working hours would then be downward biased. However, the reported hours of work for teenagers' are on average very high

[24] 0.0054 = (0.089/2.798)/(5.9).

when compared to older groups at the time of the MW change. For the years 1986-1988 18-19 year olds average monthly hours are between 165 to 169, while older individuals work on average 153 to 158 hours. This goes against the possibility of a large understatement of the hours variable.

2.6 Second experimental design

One difficulty with the previous results is that different trends could be affecting the different age groups, thus accounting for different employment paths, rather than employment paths being a consequence of the MW. To investigate this we adopt a second experimental design which groups firms according to their likelihood of being affected by the MW change. A firm is more likely to be affected if the average wage paid to teenagers in 1986 is at or above the MW due to teenagers in 1986 (the firm is complying with the minimum wage law) and not higher than the MW for adults in 1986. A dummy variable A_j is introduced which takes the value 1 for firms more likely to be affected by the MW and 0 otherwise. The regression also requires the interaction dummies $A_j \times D^{20}$ and $A_j \times D^{30}$:

$$\Delta N_{ijt} = \alpha_0 + \alpha_1 D^{20} + \alpha_2 D^{30} + \alpha_3 A_j + \beta_1 A_j \times D^{20} + \beta_2 A_j \times D^{30} + \gamma X_j + \varepsilon_{ijt} \quad (2.3)$$

The parameters of interest, β_1 and β_2 test whether the difference in differences done in (2.2) are different between firms more likely to be affected by the MW change and firms less likely to be affected by the MW change. The identifying assumption is that if the difference between the employment changes of teenagers and older workers is not related with the change in the law, then $\beta_1=\beta_2=0$. In other words, assuming that certain firms are more likely to be affected by the change in the law than others, then employment effects suggested by the previous results should be stronger among those firms. α_3 captures the average difference in employment changes common to all age groups between firms more likely to be affected by the minimum wage shock and firms less likely to be affected. α_1 captures the average difference between the 20-25 year olds' and the 18 and 19 year olds' employment change that is common to the two "types" of firms. α_2 is similar to α_1 but for the difference between the 30-35 year olds and the 18 and 19 year olds'.

Table 2-3: Impact on the number of workers according to firm type

Dummy variables:	1986 1987	1986 1988	1986 1989
D^{20}	-0.004	0.053	0.087*
	(0.020)	(0.032)	(0.040)
D^{30}	-0.023	0.017	-0.082
	(0.021)	(0.036)	(0.062)
A_j	-0.495**	-0.762**	-0.854**
	(0.051)	(0.080)	(0.114)
$A_j \times D^{20}$	0.830**	1.256**	1.192**
	(0.086)	(0.140)	(0.179)
$A_j \times D^{30}$	0.427**	0.795**	0.821**
	(0.069)	(0.098)	(0.125)
Constant	0.062	0.108	0.267
	(0.052)	(0.110)	(0.149)
Number of Firms	23879	22014	20895
Number of observations	71637	66042	62685
Number of observations with A_j =1	7887	7515	7098
Number of observations from Firms employing teenagers in 1986	20118	19026	18093

Note: The dependent variable is the difference in the total number of workers in the payroll of the firm.
Robust standard errors in parenthesis. Other regressors are: size, 19 industry dummies and 7 region dummies.
* - Significant at 5% level, ** - Significant at 1% level.

Table 2-3 shows the results of estimating (2.3). With the inclusion of the interaction dummies, the coefficients of D^{20} and D^{30} became insignificant (only one out of six is significant at the 5 percent level). In contrast, the interaction dummies' coefficients are large and positive, and the coefficients of the interaction dummies with D^{20} are significantly larger than the ones of the interaction with D^{30}. These results show that both the negative employment effect among teenagers and the substitution effect towards 20-25 year olds is concentrated in the firms more likely to be affected by the minimum wage change. Among those firms, the employment effects persist until 1989 for the two older groups of workers. This partially solves the previous puzzle in section 2.5, Table 2-2 in which the employment growth of 30 to 35 year olds exceeded the one of teenagers for the 1986-88 interval only. What is behind the previous insignificant coefficients for the intervals 1986-87 and 1986-89 is a lower growth (though the difference is not significantly different from zero) of the 30 to 35 year olds' employment compared to that of teenagers in firms less likely to be affected by the MW change[25].

Finally, the A_j coefficient is negative, significantly different from zero and its absolute value is increasing with the length of the year interval considered. This implies that firms more likely to be affected by the minimum wage shock have significantly lower employment growth of all age groups than firms less likely to be affected. This can be a consequence of the minimum wage shock, since the minimum wage increase has a negative effect on the overall resources of the firms employing

[25] Similarly, the larger gross substitution elasticity between the 20-25 year olds and the younger workers for the period 1986-89 is caused by higher employment growth of the 20 to 25 year olds relatively to teenagers in the firms less likely to be affected by the MW change (the difference is only significant at 5 percent level). Given that these firms are relatively less likely to be directly affected by the MW change, these disparities would probably be best explained by supply side effects in the

affected workers (income effect). Alternatively, it can also be that the unobservables that lead some firms pay only 75 percent of the full minimum wage to their 18 and 19 year olds are also related to their lower overall employment growth. The crucial assumption for our analysis is that these unobservables do not lead to asymmetric employment growth of the three age groups.

2.7 Firms' entries and exits

The fact that difference in differences are entirely based on firms that were observed before and after the change in the MW may be a source of possible bias, as it excludes from the analysis firms that died or were born after 1986. Entry and exit of firms is an important phenomenon in the Portuguese economy during the 80's[26] and new firms are likely to absorb a disproportionately high share of younger workers, as there are no tenure gains associated with its workforce. In addition, because the workforce in a panel of firms is likely to age, the employment of 18-19 year olds could fall relatively to that of older workers, independently of the MW, and the estimated employment effect of this age group would be biased downwards[27].

In order to deal with this difficulty, the previous analysis is extended to using representative samples of firms before and after the MW change. As new firms are not

teenagers and young adults labour markets. Worker-level data would be required to investigate this issue further.

[26] During the 80's the share of entrants at any given year varied between 10 and 20 percent, while the share of firms exiting at any given year was around 10 percent.

[27] Further problems arise when omitting entrants and exitors from the analysis. In particular there may be an attrition bias as firms' destruction may not be an exogenous phenomenon. Firms may face unbearable wage pressure arising from the new minimum wage which forces them to shut down. In this case difference in differences would give upward biased estimates. Similarly, firms' creation may be endogenous. There may be less new firms after 1986 than otherwise would have been without the wage

observed before 1987, neither are exitors observed after 1986, as the difference-in-differences estimator would require, a 2-stage method is used to proxy the "missing" employment values. This procedure takes firms that exit before the MW change (i.e. between 1985 and 1986) and regresses the employment of each of the age groups on the firms' characteristics, size, region[28] and industry[29]. The resulting estimates are used to predict the values of the various age groups' employment of firms that exit between 1986 and subsequent years, given their characteristics. The idea is to produce a counterfactual for what would have been the distribution of the age groups of the exiting firms before the MW change. With this procedure, if for example firms that exit when the minimum wage shock takes place have a higher fraction of 18-19 year olds than the predicted share for firms with similar characteristics, that would be measured as a negative employment effect.

The same is done with firms born before the MW change (1986) to produce a counterfactual for what would have been the age groups' employment of entrants before the MW change. If, for example entrants that enter after the minimum wage change have a lower fraction of 18-19 year olds than that predicted by their characteristics, that will be measured as a negative employment effect. The two stage procedure aims at reducing the effect of firm and/or group specific unobserved variables. An alternative simpler method is to input zeros on the "missing" employment values of entrants and exitors.

change. Unfortunately, it will not be possible to correct for these biases as there is no suitable instrumental variable to provide identifying restrictions to model attrition.

[28] The regional index at the *distrito* level divides Inland Portugal in 18 area locations. We've however reordered them into 7 larger regions that we believe give a good picture of the economic regional differences across the country: The country is partitioned into Northern Coastal region, Northern Inland region, Central Coastal region, Central Inland region, Lisbon and Tagus Valley, Alentejo and Algarve.

Table 2-4: Impact on the Number of workers: panel of firms and representative samples' results compared

Age Groups	original results		zeros for counterfactual		2-stage Procedure	
	1986 1987	1986 1988	1986 1987	1986 1988	1986 1987	1986 1988
Dummy = 1 if age is [20-25]	0.087**	0.196**	0.083**	0.204**	0.075**	0.144**
	(0.020)	(0.033)	0.016	0.022	(0.016)	(0.021)
Dummy = 1 if age is [30-35]	0.025	0.107**	-0.013	0.036	0.012	0.056**
	(0.020)	(0.033)	(0.016)	0.022	(0.016)	(0.022)
Constant	0.011	0.025	-0.018	0.066	0.030	0.044
	(0.052)	(0.111)	(0.043)	(0.071)	(0.042)	(0.064)
Number of Firms	23879	22014	32871	37461	32871	37461
Number of exiting firms	0	0	4063	5881	4063	5881
Number of entrant firms	0	0	4929	9566	4929	9566
Number of Observations	71637	66042	98613	112383	98613	112383

Note: The dependent variable is the difference in the total number of workers in the payroll of the firm. Robust standard errors in parenthesis. Other regressors are: 8 size dummies, 19 industry dummies and 7 region dummies.
* - Significant at 5% level, ** - Significant at 1% level.

Table 2-4 compares these two methods' results with the ones obtained when only the observations belonging to the panel were used. The coefficients are lower when entrants and exiting firms are included. This suggests that excluding exitors and entrants from the analysis tends to overstate the employment effects. Representative sample results provide the upper boundaries for teenagers' employment elasticity[30] which is now –0.2 for the period 1986-88 when zeros are used as counterfactual and

[29] Firms' economic activity classification (CAE 6Digit) was rearranged into 18 broader groups, defined according to National Institute of Statistics (INE) criteria.

[30] When zeros were given to the counterfactuals, the elasticity was calculated using the ratio given by –0.036 over the average number of 18-19 year old workers average employment in 1986 (because of the zeros introduced, the mean is smaller than the one in previous calculations: 0.519) and the percent variation in the real minimum wage for the same group of workers (35.5), to which we subtracted the percent change in the real MW for other workers (1.6): –0.2 = (-0.036 /0.519)/(35.5-1.6)x100. Similar calculations give the following result for the two-step methodology: –0.26 = (-0.056 /0.627)/(35.5-1.6)x100.

-0.26 with the 2-step methodology. Given the possibility of selective attrition, the true employment elasticity is expected to lie somewhere in the interval between –0.2 and the original value, –0.4.

2.8 Conclusions

This chapter estimates the wage and employment effects of a 35.5% increase in the real MW of workers aged 18 and 19 that took place in Portugal on the 1st of January of 1987. The main findings are the following. First, wages of workers aged 18-19 rose approximately 7% more than that of older workers. Second, employment of workers aged 18-19 fell relatively to that of older workers with an estimated employment-MW elasticity in the range of –0.2 to –0.4. Third, there was a substitution effect towards workers aged 20 to 25. Fourth, firms' adjusted their teenagers' employment both through reducing the number of individuals employed, and through reducing their average working time.

Appendix 2.A

Table 2-5: Summary statistics for the panel of firms sampled in 1986

		1986 all firms	1986 firms surviving until 1987	1986 firms surviving until 1988	1986 firms surviving until 1989	1987	1988	1989
		1	2	3	4	5	6	7
1	Number of firms	29,250	23,879	22,014	20,895	23,879	22,014	20,895
	Number of firms employing:							
2	18-19 year olds	7,806	6,706	6,342	6,031	6,630	6,372	5,998
3	20-25 year olds	15,759	13,405	12,582	12,027	13,387	12,559	11,863
4	30-35 year olds	13,204	11,475	10,777	10,332	11,728	11,471	10,603
5	Average size	16.6	18.8	19.3	20.1	19.2	20.3	21.6
	Employment by age:							
6	18-19 year olds	20,315	18,308	17,291	16,715	18,371	18,232	17,871
7	20-25 year olds	71,637	65,567	61,615	60,480	67,714	66,878	66,287
8	30-35 year olds	72,065	67,671	63,825	63,831	68,318	67,123	65,206
	Monthly hours worked by:							
9	18-19 year olds	164.9	165.0	165.2	165.4	165.8	169.4	168.5
10	20-25 year olds	153.4	153.1	153.2	152.9	154.1	158.2	156.0
11	30-35 year olds	153.5	152.8	152.9	152.6	153.2	158.0	156.1
	Monthly wages of:							
12	18-19 year olds	18,861	19,047	19,106	19,219	22,704	25,509	29,495
13	20-25 year olds	21,794	22,023	22,106	22,127	25,225	28,605	32,569
14	30-35 year olds	26,249	26,415	26,542	26,661	30,381	34,642	40,481

Table 2-6: Real hourly wage growth for the panel of firms sampled in 1986

Age group	85-86	86-87	87-88	88-89
18-19 year olds	0.0344	0.0857	0.0037	0.0310
20-25 year olds	0.0368	0.0452	0.0084	0.0240
30-35 year olds	0.0352	0.0518	0.0087	0.0490

Each cell presents yearly growth rates of the real hourly wage for each age group in the panel of firms sampled in 1986. Real hourly wage growth is obtained by dividing deflated monthly gross wages for normal hours and overtime by the sum of the monthly normal hours and the monthly overtime hours.

Chapter 3. Does inward foreign direct investment boost the productivity of domestic firms?

3.1 Introduction

An important part of globalisation in recent years has been the ongoing rise in foreign direct investment (FDI). UNCTAD (2000) reports that from 1979 to 1999, the ratio of world FDI stock to world gross domestic product rose from 5% to 16% and the ratio of world FDI inflows to global gross domestic capital formation rose from 2% to 14%. One consequence is that an increasing share of countries' output is accounted for by foreign affiliates of multinational firms. The foreign-affiliate share of world production is now 15% in manufacturing and other tradables (Lipsey, et al, 1998).

An obvious policy issue for governments is whether incentives should be offered to multinational firms to induce local affiliate production. In recent decades dozens of countries have altered laws to at least grant multinationals national treatment, if not to favour these firms via policies such as subsidies and tax breaks

(UNCTAD, 2000).[31] Policy promotion of FDI is now common not just in developing countries but in many developed countries as well. The exact values of FDI incentive packages are typically hard to know, but the values of many well-known FDI packages appear very high. In the late 1980s the U.S. state of Kentucky offered Toyota an incentive package worth (in present value) $125-$147 million for a plant planning to employ 3,000 workers (Black and Hoyt, 1989). In 1994 the state of Alabama offered Mercedes an incentive package of approximately $230 million for a new plant planning to employ 1,500 workers (Head, 1998). In 1991 Motorola was paid £50.75 million to locate a mobile-phone plant in Scotland, employing 3,000 workers. The factory closed in 2001, and Motorola paid back £16.75 million in grants. Siemens was offered £50 million in 1996 to locate a 1000-worker semiconductor plant in Tyneside, in Northeast England. The factory closed 18 months later, at which point Siemens had to repay £18 million in grants.

Is there economic justification for this kind of policy promotion? There would be if the social returns to FDI exceed the private returns. One often-claimed possibility is that inward FDI raises the productivity of domestic plants by bringing new knowledge into the host country that is, at least partly, a public good.

There are thus two empirical questions that we seek to shed light on in this chapter. First, are there productivity spillovers from FDI to domestic firms? Second, if so, how much should host countries be willing to pay to attract FDI? Despite the public interest and policy importance of these two questions, there is very little empirical evidence offering answers.

[31] For example, as Aitken and Harrison (1998) document, before 1989 foreign firms in Venezuela were taxed at a higher rate than domestic firms (50% versus 35%), were forced to repatriate profits at officially fixed exchange rates and could not enjoy confidentiality privileges in joint ventures. See Hanson (2001) for an overview of issues involved in FDI policy.

Existing evidence on whether there are productivity spillovers is of three types. The first are case studies. Cases can offer rich description about episodes and exemplify general issues, but do not always offer quantitative information and do not easily generalise. Second, there are industry-level studies (e.g., Caves, 1974; Blomstrom, 1986; and Driffield, 2000). Many have documented a positive industry-level correlation between FDI inflows and productivity. However, the causal meaning of this correlation is unclear. It may be that inward FDI raises host-country productivity via spillovers. But it may also be that inward FDI raises host-country productivity by forcing the exit of low-productivity domestic plants, or simply by raising the market share of foreign firms who are, on average, more productive. Or it may be that multinationals tend to concentrate in high-productivity industries. This latter interpretation is consistent with recent "knowledge-capital" models of multinational firms, in which these firms generate knowledge assets that can be deployed in different countries (e.g., Carr, et al, 2001).

The third set of studies are micro-level analyses. These studies examine whether the productivity of domestic plants (or firms) is correlated with FDI presence in the industry and/or region of the domestic plants. Of the few micro-level studies we are aware of, only one finds any evidence of positive spillovers. Haddad and Harrison (1993) find increased industry-level FDI is correlated with lower domestic-plant productivity in Moroccan manufacturing plants. Aitken and Harrison (1999) find the same negative result for Venezuelan manufacturing. They suggest these negative spillovers reflect adverse effects of FDI due to competition and further that FDI spillovers might not be positive in developing countries whose firms do not have the absorptive capacity. Chung, et al (1998) find that Japanese automobile firms operating in the United States did not boost the productivity of their American component-supplier firms via technology spillovers. Girma and Wakelin (2001) look at one industry, U.K. electronics, and find a positive correlation between domestic-firm productivity and regional Japanese FDI.[32]

To bring some fresh evidence to bear on this issue, we use a plant-level panel for all U.K. manufacturing from 1973 through 1992, where each plant reports information on nationality of ownership. Our main innovation is that we are, to the best of our knowledge, the first study looking at FDI spillovers using plant-level data spanning the entire manufacturing sector of a developed country. The U.K. is of interest for a number of reasons. First, by virtue of being a high-income country that is among the top-five R&D producers in the world (Keller, 2001), there is *ex ante* reason to suppose that it has sufficient absorptive capacity to realise FDI spillovers. Second, in recent decades the U.K. has seen substantial inflows of FDI. In our panel the foreign-affiliate share of manufacturing employment has risen from 12% in 1973 to 23% in 1992. Third, in recent years the U.K. government has spent hundreds of millions of pounds in incentives for foreign firms both to locate in the U.K. and to expand existing U.K. production. With estimates of spillovers, we can undertake some simple calculations to evaluate these actual government outlays.[33]

We study whether domestically owned plants are more productive when foreign-owned plants are present. We can measure foreign "presence" in the domestic firm' industry and region. Thus, our general approach will be to regress domestic

[32] Using data not on firms or plants but rather data on patent citations, Branstetter (2001) looks for spillovers of Japanese FDI into the United States. Subsequent to our work in this paper, Harris and Robinson (2001) look for spillovers in a collection of 20 detailed U.K. industries. In footnote 50 we compare our methods with theirs.

[33] The Appendix 3.B describes how the U.K. government subsidises inward FDI. In general, the government offers incentives to many types of foreign-affiliate activity deemed worthy, where employment protection/expansion is a prominent criterion. Between 1985 and 1988, 58% of Regional Selective Assistance

plant-level output on domestic plant-level inputs, measures of foreign presence in the plant's industry and region, and other control regressors. We interpret coefficient estimates on our FDI regressors as evidence consistent with spillovers from inward FDI to domestic-plant total-factor productivity (TFP). As we will discuss, this rich data set raises a number of estimation issues regarding endogeneity, measurement, and selection. We will exploit the panel nature of our data in various ways to try to address these issues and thereby gauge the robustness of our results. In addition, we will examine if FDI spillovers vary across dimensions including absorptive capacity of domestic plants and nationality of foreign investors.

Our main finding is evidence consistent with FDI spillovers along industry lines. Across a wide range of specifications, on our full sample we estimate a significantly positive correlation between a domestic plant's TFP and the foreign-affiliate share of activity in that plant's industry. Typical estimates suggest that a 10 percentage-point increase in foreign presence in a U.K. industry raises the TFP of that industry's domestic plants by about 0.5 percent. Our estimates suggest this TFP/foreign-affiliate correlation to be stronger for plants that are smaller, less technologically advanced, and less skill intensive. Spillovers seem to accrue predominantly to "lagging" domestic plants, not "leading" ones. We also find this correlation to be stronger for U.S. and French FDI, suggesting different spillover potentials for different parent countries. We find no significant correlation between plant TFP and FDI presence by region.

We then use our typical estimates of FDI spillovers to calculate the amount by which an additional foreign job in a U.K. industry boosts the output of domestic plants

(RSA, the major source of U.K. government support for firms) went to plant expansions and 25% to new plants, and foreign firms received 60% of the value of RSA (PA Consultants, 1993, Tables 2.3 and11.1, respectively).

in that industry. This amount is about £2000 per year at 1992 prices. We then compare these spillover benefits with the per-job incentives governments have granted in several recent high-profile cases. The spillover magnitudes appear to be less than actual per-job incentives, in some cases several times less. This suggests that productivity spillovers alone might not justify some of the recent high-profile policy initiatives.

There are five sections to the rest of this chapter. Section 3.2 briefly discusses the theory of productivity spillovers. Section 3.3 discusses our data, measurement, and estimation issues. Section 3.4 presents our empirical findings, and section 3.5 discusses their public-finance implications. Section 3.6 concludes.[34]

3.2 Multinationals and theories of productivity spillovers

Many standard models of multinational firms assume they possess knowledge assets (e.g., patents, proprietary technology, trademarks, etc.) that can be deployed in plants outside the parent country. This knowledge aspect of multinationals is a key feature of recent general-equilibrium models such as Carr, et al (2001) and earlier work such as Dunning's (1981) "OLI" framework, in which a necessary condition for a firm to become multinational is that it possess an "ownership advantage" over some mobile knowledge asset. This knowledge-asset view is supported empirically. For example, multinationals are much more R&D-intensive than are purely domestic firms (e.g., Griffiths, 1999).

[34] Beyond knowledge spillovers, foreign presence may raise aggregate U.K. productivity by inducing exit of domestic firms and/or by exerting competitive pressure on domestic firms. Our focus on knowledge spillovers is for *surviving* domestic plants, but we consider foreign presence when addressing selection issues. We also try to

If multinationals transfer knowledge from parents to their foreign affiliates, then it is possible that some of this knowledge "spills over" to domestic firms in the host country through non-market transactions. The general idea that interaction among firms can generate spillovers dates back to at least Marshall (1920). Mansfield and Romeo (1980) present survey evidence in which U.S. multinationals reported the frequency and pace at which their technology deployed in foreign affiliates reached host-country competitors, all evidence consistent with multinational spillovers.

Theoretical work on the mechanics of spillovers ranges from general discussions, often leavened with anecdotes, to formal general-equilibrium models. For empirical identification, spillovers can be of two types. Spillovers falling along industry lines and spillovers along regional lines. An example of multinational spillovers along industry lines is Rodriguez-Clare (1996), in which affiliates increase a host country's access to specialised varieties of intermediate inputs, the improved knowledge of which raises the TFP of domestic producers. Less formally, it is often hypothesised that domestic firms learn from affiliates in the same industry via a range of informal contacts (e.g., trade shows; supplier/distributor discussions; exposure to affiliate products, marketing, and patents; technical support from affiliates; reverse engineering).

Other spillover mechanisms may operate along regional lines. One commonly proposed avenue (since at least Marshall, 1920) is via labour turnover. If at least some of the knowledge particular to foreign affiliates is embodied in their labour force, then as affiliate employees leave to work for domestic firms this knowledge may move as well. For example, Song, et al (2001) use U.S. patent records to trace the movement of

control for competitive pressures. Relatedly, our analysis is only for domestic plants, and does not address the relative performance of foreign and domestic plants (e.g., Griffith, 1999; Oulton, 2000; Harris, 2001).

scientists between domestic and foreign firms (also see Motta, et al, 1999, and Moen, 2000). This knowledge need not be firm-specific (e.g., inventory-control or management techniques). If inter-regional labour mobility within a country is low, then these spillovers are likely to be concentrated within regions where the affiliates operate rather than dispersed country-wide. More generally, regional labour-market spillovers can be thought of as one important kind of agglomeration economy that can induce firms to locate near each other in space. Krugman (1991) offers some formal models of agglomeration issues.

Overall, then, there is reason to suppose that inward FDI may boost the productivity of domestic plants either along industry lines or along regional lines. Accordingly, we plan to investigate both empirically by looking for a correlation between domestic-plant productivity and industry and regional measures of foreign-plant presence. Such a correlation we will interpret as evidence consistent with the presence of productivity spillovers.[35]

[35] If multinational firms are aware of their ability to generate spillovers, then their operational decisions may be endogenous to this possibility—e.g., they may attempt to minimise spillovers' benefits to competitors. Evidence consistent with this appears in Mansfield and Romeo (1980), where the age of technology transferred to affiliates varies with mode of foreign entry, and in Shaver and Flyer (2000), where larger foreign firms are found to be less likely to build U.S. plants near other competitors. See our discussion below for our treatment of endogeneity; other discussion of these issues appears in Kugler (2001).

3.3 Data, measurement, and econometrics

3.3.1 Overview of the ARD data set

Details of our data can be found in Griffith (1999), Oulton (1997), Disney, et al (2000), and in Appendix 3.A. Here we briefly set out the main features of the data, and concentrate on issues involved in calculating productivity and foreign presence.

Our main data set is the ARD (Annual Census of Production Respondents Database), which is the micro-data underlying the U.K. Census of Production. The basic unit on the ARD is a production facility at a single mailing address, which corresponds to a "production unit" or "plant." Each unit is assigned a unique identification number, which allows units to be linked over time into a panel. Units also have another identification number corresponding to the firm who owns them, where units under common ownership share the same firm identifier.

To maintain the ARD data, the Office for National Statistics (or ONS, previously the Central Statistical Office, or CSO) maintains a register of businesses designed to capture the universe of production-sector activity. The register is drawn from a variety of sources including historical records, tax returns and other surveys.[36] This register is the basis upon which the Census forms are sent out, response to which is mandatory under the 1947 Statistics of Trade Act. These forms request extensive operational information on inputs and outputs, which as discussed below we use to

[36] Thus, for example, the 1983 Value Added Tax Act allowed the CSO to start using VAT information in compiling the register. In 1994, the CSO moved to a completely new register. See Perry (1985) for details on the ARD's sampling methods.

estimate productivity. Crucially for our purposes, the ONS also collects information on plants' industry, region, and nationality of ownership.

In at least two ways, the U.K. government has reduced the reporting burden on firms. First, it has not required all smaller plants to fill out Census forms. Each year, all plants with employment over some minimum size (100 in most years) are sampled. Plants with employment below this threshold are sampled with probabilities decreasing in size: in most years, 50% of plants with employment from 50 to 100 are sampled, and 25% of plants with employment from 20 to 50. The very smallest plants each year are excluded from the Census. Thus, each year's sample consists of a mix of larger plants sampled with certainty and smaller plants sampled with varying probabilities. The sampled plants altogether are referred to as the "selected sample," while all non-sampled plants constitute the "non-selected sample." Each year the selected sample accounts for around 90% of total U.K. manufacturing employment (Oulton, 1997).

A second reporting-burden issue is that multi-plant firms have some latitude in the level of aggregation at which they report plant information. If a multi-plant firm considers some of its individual plants to be too small to complete a full Census form, it may report an amalgamation of plants. This reporting level is called an "establishment."

Computerised ARD records go back to 1972; paper records for earlier years have been destroyed. In 1993 and 1994 there was a complete recoding of the variable which uniquely identifies establishments overtime. This recoding has not been fully documented, and matching plants between 1992 and the following years can only be done by resorting to other variables such as post-code, industry, etc. Not only that would be a major task in itself, but also could not be done without some degree of error. In this chapter we therefore only use the years up to 1992, a period which fortunately covered a substantial increase in FDI inflows.

The ARD structure raises many issues for our data analysis. Here we highlight two, with these and additional issues—e.g., sample selection—addressed more in the next sub-section. First is the level of aggregation at which to investigate productivity spillovers. In principle, the ARD panel can be configured for plants, establishments, or firms. However, at the level of firms, spillovers might be obscured for multi-plant firms in multiple regions and/or industries. And since multi-plant firms that aggregate operations into establishments do not report data for each separate plant, at the level of plants we cannot measure TFP for all observations. Accordingly, we choose to work at the level of establishments, which is the most-disaggregated level at which we can measure TFP. For brevity, we will use the terms establishments and plants interchangeably. That said, it is important to remember that because of firms' reporting latitude, ARD establishments can consist of more than one plant. For the cleaned data used in the regression analysis, 65% of establishments are single plants, and 35% have multiple plants. To the extent that some of the multiple-plant establishments have plants in more than one region/industry, the share of foreign employment in the region/industry will suffer from measurement error. This would result in a downward bias of the spillover effects. We will check the robustness of our estimation results to this in our set of robustness checks.

A second issue is what information, if any, can be used from the non-selected data. Since these businesses are not sent a full Census form, we have no information on their inputs (such as material and investment). They do report on nationality of ownership. The ONS imputes their employment levels using turnover data from tax records. The ONS does check employment for plants with imputed employment of over 11. However, due to time lags in the provision of tax data and processing of imputations, such information is typically refers to data from two years earlier (Perry, 1985). In addition, these checked plants are only around 20% of the non-selected sample. In sum, we cannot use the non-selected data for productivity calculations. But we could potentially use the employment data to measure foreign presence and/or to weight the selected sample. We address both these issues below.

Finally, before our analysis we cleaned the data via extensive checks for nonsense observations, outliers, coding mistakes, and the like. This task is important in itself, but takes on additional significance for any analysis on time-differenced data, as differencing tends to magnify the role of measurement error. For example, plant identification numbers are supposed to die with the plant, so we deleted any observations where plant identifiers returned after dropping out of the entire data set.[37] We dropped publicly owned plants (mainly in utilities), and plants that seemed to change ownership, industry, or region in unusual fashion. Finally, when running regressions we deleted plants in the top and bottom percentiles of changes in all plant-specific output and input variables.

[37] A plant might truly do this if it happens not to be sampled for full Census information for some period because of its small size, but we can check on this using the plant records for those who do not fill out the full Census form.

3.3.2 Specification, measurement, and estimation issues

Specification

To investigate whether inward FDI generates productivity spillovers for domestic plants, we estimate variations of the following basic equation specification.

$$\ln Y^d{}_{it} = \alpha \ln INPUT^d{}_{it} + \sum_{k=0}^{T} \gamma_1{}^k FOR_{R,t-k} + \sum_{k=0}^{T} \gamma_2{}^k FOR_{I,t-k} + \delta Z^d{}_{it} + \varepsilon_{it} \qquad (3.1)$$

In (3.1), subscripts *i*, *t*, *k*, *R* and *I* denote plant, time, lag length, region, and industry; α, γ, and δ are parameters to be estimated; and the superscript *d* denotes that plants are domestically owned. Output of domestic plants is denoted Y^d, their inputs denoted $INPUT^d$, foreign presence in the region and industry FOR_R and FOR_I, Z^d are other control regressors, and ε is an unobserved influence on domestic plant productivity. Thus (3.1) is a production function for domestic plants, augmented by measures of foreign presence and other controls, where coefficient estimates on the non-input regressors capture their contribution to TFP. As written in (3.1), these estimates are the same across all panel dimensions; in our robustness checks we relax this in various ways.

An alternative strategy to (3.1) would be to calculate TFP using data on outputs, inputs, and input-cost shares, and then regress calculated TFP on the non-input regressors in (3.1). In our robustness checks we report results from this alternative.

As in all micro-level empirical work with production functions, we face important concerns involving measurement and also estimation. We discuss each of these issues in turn, with additional measurement discussion in Appendix 3.A.[38]

Measurement

Output is measured as gross output. For *INPUT* we use capital, *K*; production and non-production labour, L^U and L^S (for unskilled and skilled); materials, *M*; and hours, *h*. L^U, L^S and *M* are available directly from the *Census* full-form surveys. L^U and L^S count employment of both part-time and full-time workers, and *M* measures the value of both energy and non-energy materials purchases. The hours variable available to us is manual hours at the two-digit industry level, published in the Department of Employment Gazette. The underlying data is the New Earnings Survey, which is an employer-based survey. This data, due to the aggregation level, is likely to suffer from measurement error if there are differences in hours between establishments in the same industry. Also, recorded hours may not reflect underworking (overworking) in recessions (booms) (Muellbauer, 1984), thus understating (overstating) changes in TFP. In addition, since they are reported by the employer they may be underestimated. Output and materials are deflated using industry-level price indexes as detailed as possible.[39] The ARD does not ask plants to report capital stocks, so we used plant investment data to calculate capital stocks. We chose industry-level starting capital-stock values and depreciation rates for buildings, plant and machinery, and vehicles taken from O'Mahony and Oulton (1990). We

[38] See Bartelsman and Doms (2000) for a detailed discussion of data issues specific to micro-level data sets.

[39] Our lack of plant-level prices is a pervasive problem in the literature on micro panels. To preview our interest in the correlation between foreign presence and productivity, if inward FDI lowers industry prices then there may be a spurious correlation between foreign presence and our measure of plant productivity. Without plant-level

deflated each component of investment by ONS industry-year investment deflators. We experimented with different capital-stock computations (the two main variables affecting the capital-stock path are starting values and depreciation rates), but these did not overly affect the results.

The FOR_R and FOR_I terms in equation (3.1) are foreign presence by region and by industry. The information on foreign multinationals in the ARD is provided by Dun and Bradstreet and gives, for each firm, the nationality of the ultimate beneficial owner whenever he owns more than 20% of the enterprise shares. In our data, then, foreign-affiliate plants are those plants owned at least 20% by an overseas business interest. Note that beyond this 20% cut-off, the ARD does not measure the degree of foreign ownership. Also note that domestic plants mix both U.K.-headquartered multinational firms and purely domestic U.K. plants, as the ARD does not provide any ownership distinction among domestically owned plants. Despite these caveats, one important advantage of the ARD over similar data sets for most other countries is that it reports nationality of ownership in every year.[40]

Given this information on nationality of ownership, we measure FOR_R as the share of total employment in region R accounted for by foreign-owned plants. FOR_I is constructed analogously, as the share of total employment in industry I accounted for by foreign-owned plants. For the differenced regressions we take, for each establishment, the difference between FOR_R (FOR_I) at time t and FOR_R (FOR_I) at time t-s. If an establishment moves region or changes industry, the differenced FOR_R

prices we cannot assess the importance of this effect. But if it were important, then all plant-level studies should automatically find this correlation.

[40] In contrast, the widely used analogous U.S. data base, the Longitudinal Research Database, does not track nationality of ownership. The only year in which nationality information was merged in (from the U.S. Bureau of Economic Analysis) was 1987 (see examination of this one year in Doms and Jensen, 1998). For the countries providing information and data to the current OECD micro-data project (Finland, Holland, France, U.S., U.K.,

(FOR_I) will measure the corresponding difference between the share of foreign employment in the two different regions (industries). There are several points to make regarding measurement of these important variables.

First, these shares capture the idea that what matters for spillovers is how prevalent foreigners are in the domestic region or industry, scaling for the overall size of that industry or region. Other micro-level spillover studies have used share measures of foreign presence.[41] To examine the separate role of each share's two components, total foreign employment and total employment, we also estimate specifications that decompose the shares.

Second, to construct the shares we prefer employment as the activity measure because many spillover theories (section 3.2) involve interpersonal contacts. One obvious alternative is to use capital, the other primary factor. Another possibility is employment of a particular skill group. More-skilled non-production workers might embody most of the spillovers, e.g., due to their greater knowledge of technology innovations. Or production workers might be those most familiar with specific production techniques (e.g., leaner assembly-line operations) that boost productivity. Below, we report results for these alternatives.

Third, our baseline specifications measure FOR_R and FOR_I using plants in the ARD's selected sample. As discussed in section 3.3.1, we can also measure these shares using both the selected and non-selected samples. The trade-off is comprehensiveness against data quality. The non-selected sample does cover around

Germany and Italy), nationality of ownership data is missing for Germany, Holland, Italy, and the U.S.; the French data are incomplete; and only the U.K. and Finland have such data.

[41] Different papers have used slightly different specifications of foreign presence, though. For example, Aitken and Harrison (1999) use FOR_I and also the interaction of foreign ownership in the same industry and region. One advantage of separating our foreign-presence measures by industry and region is that if spillovers along these different dimensions take different times, then our separated terms can be entered with different lag lengths. We tried various specifications with interacted measures, but these were consistently insignificant.

10% of total U.K. manufacturing employment. But 80% of the non-selected employment data are imputed, not reported, and thus introduce greater measurement error into FOR_R and FOR_I. Concern about this measurement error leads us to use just the selected sample as our baseline. In our robustness checks we report the alternative of measuring foreign presence using employment from both the selected and non-selected samples. Using the non-selected data also raises the estimation issue of weighting observations, as the ARD is a size-based sample. We address this below.

Fourth, as indicated in equation (3.1) we allow these foreign-presence measures to enter both contemporaneously and with lags. This is because although theory suggests that FDI spillovers may take time to arise (e.g., labour turnover to domestic plants), there is not sharp empirical evidence on this issue as to exactly how long. Our specifications will try many lag structures.[42]

Fifth, theory offers no sharp prediction as to how narrowly or broadly regions and industries should be measured. We distinguish 11 different U.K. regions. These are commonly used regions originally identified in the U.K. censuses of population, and they fall across conventional political and other boundaries. For FOR_I we distinguish 22 different manufacturing industries; these are roughly comparable to two-digit Standard Industrial Classification industries for U.S. manufacturing. There was a major revision to the U.K. industry classifications in 1980. These reclassifications make it difficult to separate industries in greater detail with confidence, so to minimise potential measurement error our baseline is to use the 22 two-digit industries. This practical issue aside, there may be reason to think industry-mediated spillovers are not "too narrow". For example, inventory-management techniques in apparel production might apply to a wide range of apparel goods—men's, women's, and children's. Or, as discussed in section 3.2, spillovers may arise from supplier and/or customer interactions—e.g., windshield producers learning from automobile firms.

Table 3-1 reports some basic ownership information in our ARD panel. As column 1 shows, we have usable data on 13,000-23,000 plants per year. Columns 2 and 3 show the bulk of those are British owned, but column 4 shows that the fraction of manufacturing employment accounted for by foreign affiliates grew from 12% in 1973 to 23% in 1992. The general decline in the number of British plants in Table 3-1 is consistent with the general decline during our sample period in overall U.K. manufacturing activity.[43] Note that given how we construct FOR_R and FOR_I, this decline will tend to increase our foreign-presence measures even if there is no change in FDI activity. To control for this, we will estimate specifications that add to equation (3.1) the lagged number of British plants by region and industry. Entering separately the numerators and denominators of FOR_R and FOR_I will also control for this.

Table 3-2 A. and B. show the regional and industrial variation, respectively, in foreign-employment shares for 1977 and 1992. By region, foreign presence was highest in the South East in the 1970s, but by 1992 Wales was the highest. By industry, foreign presence was generally highest in office machinery, motor vehicles, and chemicals. But the ranking of foreign presence in regions and industries is not fixed, and the panel nature of our data allows us to exploit this variation.

[42] In Mansfield and Romeo's (1980) surveys, U.S. multinationals report that their technology deployed in foreign affiliates reached host-country competitors in anywhere from zero to over 6.5 years, with a modal response of 0.5 to 1.5 years and a mean response of about four years.

[43] Office of National Statistics (1998) reports that total U.K. manufacturing employment fell from 6.446 million in 1980 to 4.084 million in 1992. There is a spike in the number of plants in 1984 and 1989 because the Central Statistical Office changed the compilation method of the register (see Disney, et al, 2000).

Turning to the control regressors Z in equation (3.1), one important set of controls is for product-market competition. There is now a large literature suggesting that competition affects the productive efficiency (i.e., X-inefficiency) of firms (for a theory review see Vickers, 1995; for empirical evidence see Nickell, 1996). The idea that foreign competition through FDI can exert competitive pressures has been both discussed and empirically analyzed in many studies (e.g., Caves, 1974; Blomstrom, 1986; Chung, et al, 1998). More generally, Baily and Solow (2001) survey a wide range of micro evidence that international competition of many forms—including both FDI and trade—spurs competitive responses in exposed firms.

It seems reasonable that the entry of foreign firms might raise the degree of competition and hence the effort level that domestic firms must exert to remain viable. This pro-competitive effect might be regarded as a spillover effect, but the welfare consequences of this are different from the knowledge spillovers that theory tends to focus on. Knowledge spillovers are Pareto-improving positive externalities, whereas increased effort represents a welfare transfer away from the harder-working employees to shareholders and/or customers. Hours is our only possible effort measure thus far, so without direct controls for competition the coefficient on FOR_I might reflect both knowledge spillovers and the effects of competition. Indeed, Aitken and Harrison (1999) ascribe their finding of negative spillovers to competition: foreign entrants take domestic firms' market shares, and thereby force domestic incumbents up their average-cost curves. All this suggests the need to control for product-market competition.[44]

[44] Note that including inputs in equation (3.1) should help control for the output consequences of plants moving along their average-cost curves. Also, it seems unlikely that manufacturing plants compete along regional lines. This suggests that the coefficients on FOR_R are unlikely to reflect increased effort.

Following Nickell (1996), we use four potential measures of product-market competition: industry concentration (*$CONC_{It}$*), import penetration (*$IMPORT_{It}$*), market share (*$MSHARE_{it}$*) and rents (*$RENTS_{it}$*). *IMPORT* is available at the industry-level as imports as a share of domestic production. *MSHARE* is measured as plant output as a proportion of four-digit-industry output.[45] This is unlikely to be a reliable cross-section measure of market power, since it is affected by technological differences between industries (e.g., capital intensity) which also likely affect productivity. Accordingly, we use changes in market share, *ΔMSHARE,* to measure changes in competitive pressure. *RENTS* aims to capture *ex ante* rents potentially available to workers and managers to take as increased leisure. It is defined as sales less material, capital and labour costs, expressed as a proportion of net output (where we measure labour cost using industry-region average wages instead of actual plant wages).

Estimation issues

One important estimation issue is endogeneity. This is a particular concern for our key regressors of interest, *FOR_R* and *FOR_I*. Foreign firms may be attracted to regions and/or industries with high-productivity domestic plants—e.g., perhaps learning spillovers flow in both directions. To address this possibility, we use lagged measures of *FOR_R* and *FOR_I*. Above, we argued that lags may be appropriate because spillovers take time to materialise. Lagged foreign presence is also predetermined relative to current plant productivities. We also suspect that the competition regressors may be endogenous: e.g., higher plant efficiency might raise rents and market share.

[45] We also calculated market shares for three- and two-digit industries. The coefficient standard error rose as we did this, suggesting that the measure becomes increasingly inaccurate as we use a broader base, which is plausible.

We therefore lag *RENTS* and *ΔMSHARE* by two years.[46] Unfortunately, endogeneity can also be due to the foreign acquisition of domestic firms, and this would not be mitigated with lags. If domestic firms with high productivity growth are selected for foreign acquisition, growth in foreign presence would be associated with lower domestic productivity growth. Conversely if domestic firms with low productivity growth are selected for foreign acquisition, growth in foreign presence would be associated with higher domestic productivity growth. To the extent that foreign acquisitions select on the productivity growth of domestic firms, our estimates may suffer from bias.

A second estimation issue is omission of unobserved variables. There are likely to be a host of plant, industry, time, and region-specific influences that are unobservable to the econometrician but are known to the plant. These unobservables might underlie any observed correlation between productivity and foreign presence. For example, sound infrastructure, high-quality management, proximity of suppliers or availability of skilled labour might all raise domestic productivity and attract foreign firms.

We attempt to address this omitted-variables problem via time differencing and fixed effects. First, we estimate (3.1) on time-differenced data. In addition to removing any fixed plant-specific unobservable variation, differencing also removes fixed regional and industrial effects such as indicators of global engagement (e.g., tariffs), infrastructure, technological opportunity. One well-known cost of differencing is that it can aggravate measurement error in the regressors, and thereby introduce

[46] The other obvious option would be to instrument for foreign presence, using some variable correlated with foreign presence but uncorrelated with unobservable determinants of plant productivity. In our data we know of no good candidates. Government policies (and changes therein) are one common candidate set of instruments. We do

biases. In a multivariate setting, the downward bias in the variables measured with error may in turn impart biases in the other variables, if they are correlated. Longer time differences tend to attenuate this problem (Griliches and Hausman, 1986), so we report results for one-year, three-year, and five-year differences. Longer time differences may also be more appropriate if spillovers take time to materialise.

Second, in our differenced specifications we also include full sets of time, industry, and region fixed effects. These additional fixed effects control for unobservables that may be driving changes in key variables (e.g., we control not just for "Wales is an attractive region" but also for "the attraction of Wales is rising over time," or not just for "computers is a large industry" but also for "computers is a booming industry"). Thus, our findings rely not on differences in plant productivity and differences in foreign presence but on the deviation of differences in plant productivity and foreign presence from their year, region, and industry means.

If our differencing and fixed effects are sufficient, then in equation (3.1) the error term ε is left uncontaminated by omitted variables. This will not be the case, however, if there are important unobservables that vary both across plants and over time. For example, managerial talent may not be fixed over time within plants. Without measures of these plant-and-time-varying factors, estimates from (3.1) may still be biased. Olley and Pakes (1996) show that these remaining unobservable shocks can be proxied from investment behaviour, on the assumption that these shocks influence current investment but, since investment takes time, not current output. Olley and Pakes (1996) implement their method on telecommunication plants, as does Pavcnik (2000) on Chilean manufacturing plants.

have data since 1980 on U.K. government support for firms by region, but this support was available for all firms—domestic and foreign, manufacturing and services.

As Griliches and Mairesse (1995) discuss, however, this structural approach depends on a number of assumptions: e.g., plants cannot undertake zero investment, other factors besides capital fully adjust to shocks each period, and markets are perfectly competitive. The sensitivity of this approach to violations of assumptions is an ongoing research question. For example, Levinsohn and Petrin (2000) propose using intermediate inputs rather than investment to address the underlying omitted-variables problem. For our purposes, we prefer not to assume perfect competition in light of the emphasis in the micro-spillovers literature on the competitive effects of foreign entrants.[47]

A third estimation issue is selection bias. Plants can choose to exit each period, but our data contains only the surviving plants. This might bias our estimates for foreign presence. Suppose that foreign presence truly does boost domestic-plant productivity, and thereby domestic-plant survival chances. In regions and/or industries with low foreign presence, we will observe only those plants whose unobservable offsetting benefits—e.g., good management—allow them to survive. But in regions and/or industries with high foreign presence, we are much more likely to observe all plants. This suggests that selection bias may understate the true relationship between inward FDI and productivity. Conversely, if firms with lower productivity growth are less likely to survive when foreign presence is high, selection may overestimate the relationship between inward FDI and productivity. Therefore, the direction of the overall potential bias is unknown.

A standard approach to handling the selection issue is to condition (3.2) on an auxiliary equation containing variables that capture the probability of the

[47] Girma and Wakelin (2001) analyze productivity spillovers using both a specification similar to ours and the Olley-Pakes specification, and find that both approaches yield qualitatively identical results about spillovers.

establishment surviving. Olley and Pakes (1996) attempt to model selection structurally by postulating an explicit model of exit (see also Pavcnik, 1999, and Levinsohn and Petrin, 1999). In Olley and Pakes' model, exit depends on an unobserved shock (to the econometrician) to productivity. This shock would be entirely captured by the investment (and capital) variables that would affect the entry/exit decision. Current output would not be affected by current investment since it is assumed that investment takes time materialise into additional capital. Griliches and Marisse (1995) argue nevertheless that the structural approach followed by Olley and Pakes depends on strong assumptions: the probability of exit depends only on the current realisation of productivity shocks not on its whole history, and the determinants of unobserved shocks (investment in their model) is measured without error. In our case we find it hard to argue that investment could work as an exclusion restriction, since capital stock is itself estimated from establishment level investment.

A final estimation issue is weighting. Since we have the selected and non-selected data, we can construct sampling weights and run weighted regressions on the selected sample. However, there are at least two reasons why weighted regressions might be misleading. One is that the true marginal effects may differ across size groups. As DuMouchel and Duncan (1983) show, only in special cases do weighted regressions return an estimate of the average effect across groups.[48] A second issue is that if the sampling weights are measured with error, then weighted least squares can yield biased coefficient estimates. This is a real concern, both because the precise

[48] DuMouchel and Duncan (1983) consider the following. Suppose one is trying to estimate a marginal effect β between Y and set of variables X, where the data has been sampled and weights w_i are assigned to the ith observation. The OLS estimator of β is $\beta_{OLS}=(X'X)^{-1}X'Y$. The weighted least squares estimator is given by $\beta_{WLS}=(X'WX)^{-1}X'WY$ where W is a diagonal matrix whose ith diagonal element is w_i. Suppose, however, that the β varies across size strata so that the model is $Y=X\beta(j) + \varepsilon$. A marginal effect of interest would be the weighted average marginal effect namely $\beta_{AVG}=\Sigma w_i\beta(j)/ \Sigma w_i$ where the summation is over strata. DuMouchel and Duncan

details of the sampling rules used by the ONS every year are no longer on record and because employment in the non-selected data from which weights can be approximated is in most cases imputed.

Because of concerns about these complications, our baseline estimates all use unweighted least squares. When using the non-selected sample in measuring foreign presence, however, for robustness we report results for weighted least squares as well, where the weights are constructed using employment bands by year, region, and industry.[49]

Summary

In light of these various measurement and estimation issues, we estimate variations of this basic differenced equation.

$$\begin{aligned}\Delta \ln Y^d{}_{it} &= \alpha_1 \Delta \ln K^d{}_{it} + \alpha_2 \Delta \ln M^d{}_{it} + \alpha_3 \Delta \ln S^d{}_{it} + \alpha_4 \Delta \ln U^d{}_{it} + \alpha_1 \Delta \ln h^d{}_{it} + \\ &\sum_{k=0}^{T} \gamma_1{}^k \Delta FOR_{R,t-k} + \sum_{k=0}^{T} \gamma_2{}^k \Delta FOR_{I,t-k} + \\ &\delta_1 \Delta MSHARE^d{}_{it-2} + \delta_2 \Delta RENTS^d{}_{it-2} + \delta_3 RENTS^d{}_{it-2} + \lambda_t + \lambda_I + \lambda_R + \nu_{it}\end{aligned} \tag{3.2}$$

Equation 3.2 includes our variables for inputs, foreign presence, competition, and time, regional, and industry dummies (λ_t, λ_R, and λ_I). We tried all the competition variables discussed above in both levels and changes, but only those shown in (3.2)

(1983) show that β_{WLS} is a biased estimate of β_{AVG} (unless all the regressors are constant), and so there is no reason to prefer weighting. In fact β_{OLS} is also biased, but there is no general result that one is less biased than another.

[49] The size bands are based on the following employment intervals: first less than 20; then eight intervals of 10 additional workers up to 100 (i.e., 20-29, 30-39, etc.); 100-199; 200-299; 300-399; and 400 or larger. The reason we include intervals for plants over 100 workers is that for various reporting reasons, some of these observations actually appear in the non-selected data. Although plants with less than 20 employees are not sampled, our data contain a few observations with less than 20 employees (this may be because large firms may choose to report by small local units). We dropped these observations in our weighted regressions because they would be given very large weights, which would exacerbate error in the weights.

were consistently significantly different from zero. We now turn to our estimation results.[50]

3.4 Estimation results

Baseline results

Table 3-3 reports our baseline OLS estimates of equation (3.2) using short and long differences in combination with various lag structures. Each column reports a different difference length and lag structure, with robust standard errors reported below coefficient estimates[51]. Column 1 shows the simplest specification, namely, current FOR_R and FOR_I with one year differences. Both coefficient estimates are positive, consistent with positive productivity spillovers from foreign plants to domestic plants at both the regional and industry level, but the regional coefficient is insignificantly different from zero. The coefficient on FOR_I suggests that a rise of 10 percentage points in FOR_I for some industry, *ceteris paribus*, would raise output in each domestic plant in that industry by about 0.5%. Because we control for inputs in estimating (3.2), this output increase is a TFP increase.

Since this magnitude is common to a number of the specifications we report below, it is worth trying to put it in some context. The observed rise in FOR_I over the sample period 1973-1992 is about 11 percentage points. By our estimates of the

[50] For a sub-sample of 20 four-digit SIC industries in the ARD, Harris and Robinson (2001) estimate productivity equations somewhat like equation (3.1). Their industries include cement and plaster; preparation of milk products, cocoa, etc; and steel wire. One important difference is their observations include plants in the non-selected sample, where the authors impute all unreported output and inputs data for these observations based on the non-selected employment information.

previous paragraph, this implies that industry spillovers raised UK manufacturing industry TFP by about 0.5%. Since actual TFP in U.K. manufacturing rose by about 10% over the estimation period, our estimates suggest that spillovers explain about 5% of the observed 1973-1992 rise in U.K. manufacturing TFP.[52]

Returning to Table 3-3, column 2 shows both foreign-presence measures dated *t-2* and *t-3*, which are predetermined relative to the differenced dependent variable.[53] The second lag of FOR_I is positive and the most significant. While both lags of FOR_R are positive, neither is very significant. To see the magnitude of the overall effects across all lags, two lower rows also report the sum of the individual coefficients for the industry (ΣFOR_I) and region (ΣFOR_R). The P-value for the joint significance of the summed coefficients is reported in the next two rows, entitled P(ind) and P(reg). We see that the net industry effect is about the same magnitude as in column 1, and remains significant. The net regional effect is larger than in column 1, but remains insignificant.

Column 3 reports a specification using all lagged terms. Looking at the P-values, the regional effects are again jointly insignificant whereas the industrial effects are jointly significant. The net regional effect is now larger than in columns 1 and 2. The net industrial effect is smaller, apparently because of a negative but insignificant (t-3) effect.

Columns 4 to 6 set out the three-year differences, and columns 7-9 the five-year differences. Comparing columns 4 and 7 with column 1, the results are similar: a

[51] Underlying robust standard errors of reported t-values are not adjusted for industry-level clustering. Unfortunately, since FOR_R (FOR_I) only vary between regions (industries) and we use individual data, reported t-statistics are likely to be overstated.

[52] To undertake this calculation, we needed to calculate total manufacturing TFP in a manner consistent with the regression from which we use the coefficients for FOR_I. We do this by subtracting from the change in log real output the weighted changes in the logs of K, M, S, U and H with the weights being the coefficients taken from estimates of (3.2).

significantly positive coefficient on FOR_I, with about the same magnitude as column 1, and an insignificant coefficient on FOR_R. Columns 5 and 8, using the $(t\text{-}2)$ and $(t\text{-}3)$ lags, also give similar results, with a coefficient of around 0.05 for FOR_I. Finally, columns 6 and 9 both give jointly insignificant effects for FOR_R and significant effects for FOR_I. It is also worth noting that the longer differences raise slightly the coefficients on FOR_I. This is consistent with the theory discussed earlier about measurement error and length of differences.

Taken together, the results in Table 3-3 suggest that industry-mediated productivity spillovers are positive and significant, with a semi-elasticity of 0.05 as our central estimate. Applied to actual data on foreign presence and TFP, this semi-elasticity suggests that spillovers explain about 5% of the actual rise in U.K. manufacturing TFP over our sample period. Our estimates of spillover effects along regional lines are less consistent. These estimates are generally positive, but are also mostly insignificantly different from zero.

These are our basic results. Table 3-4 and Table 3-5 next show a set of extensions, first of splitting our data by groups of plants and second of decomposing foreign presence by country. Table 3-6 then shows a large number of robustness checks for various measurement and estimation issues.

Extension: spillovers by absorptive capacity

It has been argued that the ability of domestic plants to realise FDI spillovers might depend on their absorptive capacity. Absorptive capacity may have something to do with the overall level of economic development in the host country. For example, in discussing their inability to find any FDI spillovers among Venezuelan

[53] We also experimented with lags dated four years and beyond, but they were not significant.

plants, Aitken and Harrison (1999, p. 617) conjecture that "the economy [might] not [be] sufficiently developed or diversified, to receive large benefits from foreign presence." If there is indeed some minimum level of development countries need to realise spillovers, conditional on a country reaching that level there may also be variation in absorptive capacity among domestic plants due to differences in plant size, skill intensity, or technological sophistication. Perhaps only the "best practice" plants can take advantage of FDI spillovers. Conversely, perhaps best-practice plants, by definition, have already implemented foreign ideas and methods, such that spillovers accrue mainly to other plants with more to learn.

There is no obvious single measure of a plant's absorptive capacity. We proxy for it by splitting our sample into three groups based on their location in the distribution of three different performance measures: total employment, TFP, and skill intensity (i.e., non-production share of total employment). Consider the example of employment. Within each industry-year, we separated all plants into three groups based on their total plant employment: those below the 25^{th} percentile, those between the 25^{th} and 75^{th} percentile, and those above the 75^{th} percentile. Note that we separate by industry and by year, which accounts for cross-industry variation in total employment due to factors like underlying technology differences. We pool across all industry-years to obtain our three sub-samples, and then estimate (3.2) separately on each sub-sample. This process was repeated for our TFP and skill-intensity performance measures. The three different criteria seemed to generate broadly similar sub-samples, consistent with the micro evidence from several countries that best-practice plants appear as such along several dimensions.[54]

[54] Since employment in any year may be measured with error, in making the rankings we averaged employment in year t over years (t-1) and (t-2).

Table 3-4 reports our estimation results for these various sub-samples, using the same specification as in column 1 of Table 3-3. There are two features of note. First, the results are consistent with Table 3-3 in that FOR_I is generally positive and significant and FOR_R insignificant. Second, there is a suggestion that spillovers are somewhat larger at lower points in the performance distribution. For all three performance measures, the coefficient on FOR_I is insignificant and small for the best-practice plants above the 75th percentile. For the lower two groups in the distribution, for all three performance measures the coefficient on FOR_I is larger and is near or above standard significance levels. These differences we consider to be suggestive, as pairwise F-tests show the coefficients on FOR_I to be significantly different (at the 10% level) in only two of the nine possible comparisons: within the skill-intensity distribution, between plants above the 75th percentile and those between the 25th and 75th percentiles and between plants above the 75th percentile and those below the 25th percentile. Overall, we think Table 3-4 offers suggestive evidence consistent with the hypothesis that FDI spillovers accrue predominantly to plants further away from the best-practice frontier.[55]

Extension: spillovers by nationality of foreign ownership

Since the ARD reports the country of ownership of foreign plants, we can examine variation in spillovers with nationality. We are particularly interested in inward FDI from the world's other high-R&D countries: the United States, France,

[55] One explanation of Table 3-4's patterns may be that the best-practice centiles contain most of the U.K.-headquartered multinationals. Recall that our ARD sample has no way to separate U.K.-owned plants between those that are part of U.K.-headquartered multinationals and those that are part of purely domestic U.K. firms. If U.K. multinationals have little to learn from other multinationals, then this might help explain Table 3-4. Also, note that across the three different absorptive-capacity metrics in Table 3-4, each centile range does not contain the exact same number of plants because the extent of missing data varies across these metrics.

Germany, and Japan. We need, however, enough number of observations for these four countries and with enough variation. For the overall period, the average number of firms across all 2-digit industries is 1035 for the US, 89 for Germany, 64 for France and 28 for Japan. The median is 303, 28, 22 and 4, respectively. The average number of firms across all regions is 542 for the US, 47 for Germany, 34 for France and 15 for Japan. The median is 672, 76, 43 and 18, respectively.

The average FOR_I for the US increased from 9.9 percent in 1977 to 10.5% in 1992. For France it increased from 0.4% to 1%, for Germany, from 0.7% to 1.7% and for Japan from 0.0% to 1.4%. Behind these apparent average small changes, there is considerable time-variation in the industry shares of US, French and Japanese employment. The data for German FDI shows less time variation, which may help explain the non-significant coefficient reported below. For example, the absolute value of the change in FOR_I between 1977 and 1992 is 8% on average for the US, 0.8% for Germany, 1.8% for France and 1.3% for Japan.

The average FOR_R for the US remained at 10.3% between 1977 and 1992. However in 4 regions FOR_R increased at least 3 percentage points. For France FOR_R increased from 0.7% to 2.2%, for Germany from 0.3% to 1.3% and for Japan from 0.0% to 1.8%. Again, behind these average small changes, there is considerable time-variation in the region shares of US, French, Japanese and German employment. For example, the regional mean of the absolute value of the change in FOR_R between 1977 and 1992 is 3% for the US, 1.1% for Germany, 2.1% for France and 1.7% for Japan.

Table 3-5 reports estimation results where FOR_R and FOR_I are constructed for each country separately. As with earlier tables, regional effects remain generally insignificant. For industry effects, our estimates are consistent with significantly positive spillovers from U.S. and French FDI, insignificant spillovers from German FDI, and significantly negative spillovers from Japanese FDI. The U.S. finding is consistent with both aggregate and micro-level evidence that the United States is at or near the world technology frontier.[56] And the overall ranking of the four countries is strikingly consistent with Doms and Jensen (1998, Table 7.6). Looking at foreign affiliates in the United States, they find that relative to U.K. plants French plants have higher TFP, German plants have about the same TFP, and Japanese plants have lower TFP.[57]

Robustness checks

To verify the robustness of our main results we performed a large number of checks. Table 3-6 reports ten important checks, all using the column 1, Table 3-3 specification of contemporaneous foreign-presence measures and one-year time differences. It is important to note that many robustness checks were estimated for a wide range of specifications, but that we report just one specification for brevity. In many cases this one specification actually yields weaker results than others not reported; we will highlight some important instances.

[56] An example of country-level comparisons is Davis and Weinstein (2001). At the micro-level, Doms and Jensen (1998) document that parents of U.S. multinationals are more productive than U.S. affiliates of foreign-owned multinationals.

[57] One interpretation of the large negative coefficient estimate for Japanese presence might be that Japanese firms exert particularly strong competitive pressures that our competition regressors do not fully capture. To test this idea we examined the industry distribution of Japanese FDI; over our sample period, about two-thirds of all Japanese employment in the U.K. was in electrical and electronic engineering. Excluding this industry from our measure of Japanese FDI presence reduced our coefficient estimate somewhat (to –0.183), but did not reduce it to zero. An alternative explanation would be if Japanese FDI occurs primarily in the form of mergers and acquisitions of the most productive domestic firms. Though in the current chapter we do not look specifically at mergers and acquisitions, this topic is of interest for future research.

The first five columns of Table 3-6 involve checks on measurement of our key foreign presence regressors, FOR_R and FOR_I. Column 1 addresses what activity is used to calculate foreign presence. In section 3.3 we argued that employment is our baselines measure of foreign-affiliate presence, but other options include capital stocks or employment by skill group. Column 1 reports estimation results measuring FOR_R and FOR_I using non-production employment. As before, industry effects are significant but region effects are not. Qualitatively identical results were obtained using production employment or capital, all of which reflects the high sample correlations in levels and in differences among these different variables.[58]

Columns 2 and 3 introduce the non-selected sample into the analysis. In column 2, we measure FOR_R and FOR_I using not just the selected sample but also the non-selected sample as well. Industry effects remain significant, with the slight decline in the coefficient estimate consistent with attenuation bias due to measurement error in the imputed employment values in the non-selected sample. This slight decline generally disappears for alternative specifications with lagged regressors and/or longer time differences, so we regard the industry results to be entirely robust to this measurement issue.[59]

In column 3 we again use the non-selected sample to measure foreign presence; we also use weighted rather than ordinary least squares as outlined in section 3.3.2. As in column 2, there is little evidence of spillovers along regional lines. The industry coefficient estimate is virtually unchanged from column 2, albeit now insignificant at standard levels. This decline in significance may reflect problems with

[58] For example, the skilled-labour and unskilled-labour activity measures have sample correlations in levels and in differences (three-year and five-year) that range from 0.81 to 0.97. Because of these very high correlations, multicollinearity problems inhibit attempts to enter both employment measures in the same regression to see if one employment group matters more.

measurement error in the sampling weights, discussed earlier. We note that this particular specification of WLS actually gives among the weakest evidence of industry spillovers. Weighted estimates from alternative specifications with lagged regressors and/or longer time differences—including those that omit the smallest plants, as discussed earlier—almost all yield larger, more significant industry coefficient estimates.[60]

Our baseline measures of FOR_R and FOR_I are disaggregated between their numerators and denominators in column 4. This is to check that foreign presence matters in absolute levels as well as in shares. This appears to be the case. The coefficient estimate for total foreign employment by industry is significantly positive, while that for total foreign employment by region is basically zero. As discussed earlier (see Table 3-1), one reason to worry about differences between employment levels and shares might be the generally declining number of British plants over our sample period. To control for this directly, column 5 returns to using foreign-presence shares but adds regressors controlling both for the number of British plants by industry and for the number by region (with both controls lagged one year). As column 5 shows, accounting for declines in total U.K. employment has no substantive impact on our estimates of the role of foreign presence by industry and region.

The second row of Table 3-6 addresses other measurement and estimation issues. Column 6 reports results for a more-general specification of equation (3.2) in which we allow the various α coefficients on inputs to vary across all two-digit

[59] For example, the specification with contemporaneous regressors and five-year time differences yields a coefficient estimate on FOR_I of 0.107 (t-statistic of 5.30) and on FOR_R of –0.052 (t-statistic of 1.64).

[60] For example, the WLS specification with contemporaneous regressors and five-year time differences yields a coefficient estimate on FOR_I of 0.087 (t-statistic of 3.08) and on FOR_R of –0.091 (t-statistic of 0.98). For the analogous WLS specification that excludes all plants with fewer than 20 employees, the coefficient on FOR_I is 0.110 (t-statistic of 4.45) and on FOR_R is –0.017 (t-statistic of 0.46).

industries (by interacting input terms with industry dummies). One might worry that the assumption of equal α coefficients is not warranted, in a way that might bias our estimates for spillovers. This does not appear to be the case, however, as the coefficient estimates on FOR_R and FOR_I are very similar to the basic results in Table 3-3. Column 7 addresses a related specification issue; here, we modify our estimation equation by dropping the input regressors and replacing the regressand with TFP calculated from sample data on inputs, outputs, and cost shares. Again, estimates are consistent with positive spillovers from foreign industry presence but not regional presence.

The fact that some observations actually represent multi-plant establishments is addressed in column 8. Recall that this issue arises because firms are granted some latitude in completing ONS Census forms. To the extent that our data contain multi-plant establishments with constituent plants spanning multiple industries or regions, then our foreign-presence measures will contain some error relative to the ideal of fully separating out each plant. This measurement error might bias downward our baseline coefficient estimates; were this the case, our industry results would be a lower-bound estimate of spillover magnitudes, but our regional results thus far might be obscuring true regional spillovers. Column 8 estimates our baseline specification on the sub-sample of single-plant establishments. This group constitutes about two-thirds of our full sample, broadly consistent with Oulton's (1998) facts on this issue for the 1980s. For this sub-sample we continue to find estimates consistent with industry but not regional spillovers.

Finally, column 9 reports results for the sub-sample that excludes all plants located in Wales. As reported in Table 3-2 A., Wales was one of the regions with the largest increase in foreign presence during our sample period, and we wanted to check that our main results were robust to excluding apparently important regions and/or industries. As demonstrated by the Wales exclusion, the results do seem robust in this way.

We also conducted other robustness checks that, for brevity, we do not report in Table 3-6. For example, as a further check on our industry estimates we excluded all observations that switched industry (at our baseline two-digit level) in either 1979 or 1980, the period of the large redefinition of industry classifications. One additional issue we mention is there may be serial correlation in the short-differenced residuals if there are short-run adjustments to shocks. Long-differencing the data is one possible treatment for smoothing out short-run shocks. For our one-year differences, we also added to equation (3.2) a lagged dependent variable and once-lagged input variables. We estimated this equation using the GMM method of Arellano and Bond (1991). The coefficient on $\Delta FOR_{I,\,t}$ was 0.054 (t=3.10) and on $\Delta FOR_{R,\,t}$ was 0.027 (t=1.15); the coefficient on the lagged dependent variable was 0.37 (t=7.47), which implies a long-run effect of $FOR_{I,\,t}$ of 0.085. The p value for the test of no MA(1) error in the residuals was zero, rejecting the null of no autocorrelation, which is to be expected since first differencing should induce MA(1) residual autocorrelation. However, the p value for the test of no MA(2) error in the residuals was 0.30, which fails to reject the null of no autocorrelation. Thus, the test statistics indicate this dynamic specification is acceptable. In summary, we think our findings are robust to more complicated dynamic specifications.[61]

[61] We also ran the same specification with $\Delta FOR_{I,\,t-2}$, $\Delta FOR_{I,\,t-3}$ and $\Delta FOR_{R\,t-2}$, and $\Delta FOR_{R,\,t-3}$. Just as Table 3-3, column 2, the coefficient on $\Delta FOR_{I,\,t-2}$ was significantly positive (coefficient 0.080, t=4.15) with the other variables insignificant (and with p values of zero and 0.16 for MA(1) and MA(2) autocorrelation, respectively).

3.5 Public-finance implications: how much should governments pay to attract FDI?

In the introduction we reported estimated costs of government FDI subsidies for several high-profile cases in the United Kingdom and United States. Tables 3.3 through 3.6 report our estimates of the spillover benefits to a host country from FDI. In this sub-section we attempt some calculations to compare these costs and benefits on a present-value, per-worker basis. We have in mind that subsidy costs are incurred at the start of a foreign plant's life (and perhaps thereafter as well), after which that plant delivers a flow of productivity-spillover benefits as long as it continues to operate. In performing the calculations, we reiterate the caveat that our estimation results are best interpreted as suggestive evidence consistent with productivity spillovers: we perform these calculations assuming that spillovers actually do exist.

The subsidy costs per worker can be easily calculated given reports of subsidy values and jobs covered. However, it is important to note the uncertainty surrounding both these quantities, as the reports are culled mainly from press reports without systematic verification of either values or jobs involved. For the four cases mentioned in the introduction, the costs per worker (all expressed in 2000 U.K. pounds) are Siemens (UK) £35,417; Motorola (UK) £14,356; Toyota (Kentucky, USA) £39,827; and Mercedes (Alabama, USA) £117,178.[62]

The subsidy benefits per worker arise from the TFP boost enjoyed by the affected domestic plants thanks to the inward FDI. Because our estimates of productivity spillovers are for each year, they accumulate over the duration of foreign

[62] We converted U.S. dollars to U.K. pounds using market exchange rates, and then converted all values into 2000 prices using the U.K. GDP deflator.

presence. This means we need to calculate the per year output boost for domestic plants per extra foreign job, and then discount these output boosts over the length of that job.

Consider a foreign plant coming into a particular industry *I*. If this new plant raises our foreign-presence measure FOR_I by $\Delta\phi_I$, then the percentage rise in output in each domestic plant in that industry is equal to $(\gamma_1)(\Delta\phi_I)$, where (γ_1) is the spillover coefficient in equation (3.1). If the initial output across all domestic plants in that industry is given by Y_{I0}^d, then the level rise in domestic output in that industry, ΔY_I^d, is given by $\Delta Y_I^d = (Y_{I0}^d)(\gamma_1)(\Delta\phi_I)$.

ΔY_I^d gives the rise in output per rise in the foreign-employment *share*, $\Delta\phi_I$. To transform this into the rise per foreign *worker*, we need to calculate the relation between the rise in foreign employment share, $\Delta\phi_I$, and the rise in foreign employment, ΔN_I^f. This relation is given by

$$\Delta\phi_I = \frac{\Delta N_I^f}{(N_{I0}^f + N_{I0}^d)} \frac{1}{\left(1 + N_{I0}^f / N_{I0}^d + \Delta N^f / N_{I0}^d\right)} \qquad (3.3)$$

where N_{IO}^d and N_{IO}^f are the number of domestic and foreign jobs in the industry in the base period. The intuition behind this is as follows. Recall that $\phi_I = (N_{IO}^f)/(N_{IO}^d + N_{IO}^f)$. Thus, an increase in N_{IO}^f raises both the numerator and denominator of ϕ_I. This accounts for the two terms on the right-hand side of (3.3). The first term shows the direct effect on ϕ_I from ΔN_I^f via the numerator of ϕ_I. The second terms shows the effect on ϕ_I from ΔN_I^f via the denominator of ϕ_I. The second term shows that the higher is foreign employment, the more is N_{IO}^f/N_{IO}^d and so the less a given rise in N_I^f raises ϕ_I.

Combining (3.3) with the expression for ΔY_I^d, we can write the extra domestic output per foreign job, $\Delta Y_I^{\,d}/\Delta N_I^{\,f}$, as follows:

$$\frac{\Delta Y_I^d}{\Delta N_I^f} = \gamma_1 Y_{I0}^{\ d} \frac{1}{(N_{I0}^f + N_{I0}^d)} \frac{1}{(1 + N_{I0}^f / N_{I0}^d + \Delta N_I^f / N_{I0}^d)} \qquad (3.4)$$

The extra domestic output per extra foreign job consists of four terms. The first, γ_1, is the estimated coefficient from equation (3.1) that gives the percentage change in domestic-plant output in response to a rise in foreign-employment share. The second term in (3.4), Y_{I0}^d, converts this percentage change into a level change. The third and fourth terms convert the rise in foreign employment share to rise in foreign employment in actual levels.

An expression similar to (3.4) would hold for productivity spillovers along regional lines, and we could therefore calculate the extra domestic output in region per foreign job created in a region. Our estimates of regional productivity spillovers were mostly small and insignificant, however, so we do not attempt any regional calculations.[63]

Using data for the last year of our sample, 1992, we apply equation (3.4) to calculate the extra domestic output per foreign job. This quantity $\Delta Y_I^d/\Delta N_I^f$ varies by industry: we estimated γ_1 to be the same across industries, but each industry has different values of the other three components of the right-hand side of (3.4). Averaging our calculations across all industries, we obtain an average value of $\Delta Y_I^{\,d}/\Delta N_I^{\,f}$ of £2,097 in 1992 prices. This figure says that, *ceteris paribus*, each new

[63] If spillovers truly operated along both industry and region lines, then a new foreign plant would necessarily stimulate spillovers along both lines. Our industry calculations ignore regional effects, consistent with the

foreign worker stimulates an extra £2,097 in output across all domestic plants in that worker's industry. This amount is about £2,440 at 2000 prices.[64]

We can now compare our calculations of subsidy costs and benefits. To do this, we need to remember that the subsidy benefits accrue per year, and accordingly measure costs and benefits over the same time spans. For the two U.S. cases, note that we are assuming that our estimates of U.K. productivity spillovers apply in the same way to the United States. We have no way to evaluate this assumption, but maintain it simply for the sake of discussion.

The U.K. Siemens plant stayed open 18 months. At a discount rate of 5%, £2,440 for 18 months is £3,430: this is the value of spillover benefits per worker at this plant. The subsidy cost £35,417 per worker, an order of magnitude more than our best guess as to its spillover benefits. The U.K. Motorola plant survived 10 years. At a discount rate of 5%, this translates into a present-value spillover benefits of £18,841 per worker. The subsidy cost £14,356 per worker, so in this case the government cost of the subsidy was about equal to its estimated productivity benefits.

The two U.S. cases are harder to judge, both because of the spillover caveat mentioned above and because the plants remain open today. The Toyota plant opened in 1988, and so thus far has generated a present-value spillover benefit of £22,920 per worker. The subsidy cost per worker is £39,827 in this case. This amount would be the present value of spillover benefits if the plant operates for 35 years, suggesting the Toyota plant must remain open 22 more years to "break even." The Mercedes plant opened in 1994, with an implied spillover benefit of £14,119 for its seven years of

evidence in Tables 3.3 through 3.6. Alternatively, once could assume that industries and their owners are distributed evenly throughout regions, so that any new foreign plant would have a negligible impact on FOR_R.

[64] As a benchmark, in 1992 gross output per domestic worker averaged about £73,000, with domestic wages averaging about £15,000.

operation. This is an order of magnitude smaller than our calculated subsidy cost per worker of £117,178, which suggests that for this case the subsidy cost will exceed its productivity-spillover benefits.

A number of comments regarding these calculations are worth making. The first and most important is to reiterate that these calculations are only suggestive, as they rely on many assumptions and caveats. In particular, we have not considered benefits to foreign presence beyond the single issue of productivity spillovers. Foreign plants may bring benefits we have not considered (e.g., civic benefits of "good citizen" employers). We also have not specified from where new foreign employees come. A new employee at a foreign plant may come from abroad, or from employment in a different domestic plant, or from unemployment. In the last case, the social value of the new foreign job may be higher.

A second comment is to stress the *ceteris paribus* nature of these calculations. For a foreign plant to continue generating spillovers over time, it needs to maintain its boost to the foreign-affiliate share of its industry employment. It is not length of plant life that is at issue, strictly speaking, but rather the length of increase in foreign-affiliate employment share. These calculations assume no other growth or decline in employment among all other plants. In reality, this may not be the case. For example, if over time spillovers stimulate hiring at domestic plants, then a foreign plant's boost to the foreign-affiliate employment share declines over time.

A final consideration is the incidence of subsidy costs and benefits. In the four cases we considered, host-country governments directly pay the subsidy costs. But these governments do not directly realise the subsidy benefits. Productivity spillovers accrue to domestic firms, not domestic governments. In principle, subsidies could be paid by coalitions of domestic firms that organise to pool contributions used as incentives to foreign firms. In practice, the standard collective-action problem of free riding may make such coalition-forming difficult.

Governments may be willing to pay subsidy costs based on the tax revenues they gain from the domestic-output boost. But if governments care only about their tax-revenue gain, then the cost they should be willing to incur equals just their share of the output bonus. In 1992 the maximum corporate tax rates were 33% in the United Kingdom and 34% in the United States. This means that spillover benefits accruing to governments are only about 1/3 the total benefits calculated above, which makes the cost-benefit calculations even more unfavourable. Alternatively, governments might care about more than their tax-revenue gain, and thus may somehow internalise the spillover benefits enjoyed by domestic firms.[65]

3.6 Conclusions

A large number of countries pay subsidies to attract FDI. One justification is that the social returns to FDI exceed the private returns, because of productivity spillovers from FDI to domestic firms. In this chapter we therefore examined two issues. First, are there productivity spillovers from FDI to domestic firms? Second, if there are such spillovers, what level of subsidies would be justified? Using a plant-level panel for U.K. manufacturing covering 1973-1992, we estimated production functions for domestic plants augmented with terms measuring foreign presence in the industry and region. Our major findings are as follows.

(a) We estimate a significantly positive correlation between a domestic plant's TFP and the foreign share of employment in that plant's industry. Typical estimates suggest that a 10 percentage-point increase in foreign presence in a U.K. industry raises the TFP of that industry's domestic plants by about 0.5 percent. This correlation is consistent with productivity spillovers from inward FDI to domestic plants. We do not find significant effects for foreign share of employment by region. Our estimates are robust across several issues regarding measurement and specification.

(b) These estimates suggest that the per-job value of spillovers appear to be less than per-job incentives governments have granted in recent high-profile cases, in some cases several times less.

We have also found some evidence that spillovers take time to permeate to domestic plants, that they are more important for plants at the lower end of the performance distribution, and that they are the largest from U.S.- and French-owned plants.

Ours is the first micro-level study we are aware of to find broad evidence of FDI spillovers. In future work there are at least two additional questions we plan to investigate. One important issue is the channels of productivity spillovers—e.g., access to suppliers, labour-market turnover. Another is whether different modes of FDI activity—greenfield investments, acquisitions of British firms, expansions of existing affiliates—have different impacts on domestic producers.

[65] If part of the subsidy package governments offer is tax breaks, then the relevant effective tax rates are even lower. Courant (1994) surveys how to evaluate tax policy when used to foster economic development.

Appendix 3.A Variable definitions and sources

$\Delta \ln Y_t$ The log change in total manufacturing real gross output (£s in 1980) (direct from ARD), deflated by 4 digit annual output price deflators supplied by the ONS.

$\Delta \ln K_t$ The log change in total manufacturing real net capital stock (£s in 1980). Capital stock is estimated from establishment level investment in plant and machinery, vehicles and buildings, using perpetual inventory methods with the starting values and depreciation rates taken from O'Mahony and Oulton (1990) using the selected sample only. Depreciation rates: buildings 2.91%, plant and machinery 11.097%, and vehicles 28.1%. Buildings and plant and machinery are deflated by two digit industry deflators, vehicles by annual deflators. Deflators were supplied, by Rachel Griffith at IFS. In addition, establishments may disappear and appear from the ARD data due to sampling. This clearly creates problems for the perpetual inventory method. If we drop all establishments that disappear and reappear for at least one year we lose almost 50% of our selected sample. To fill in the missing year's investment data, we multiplied that year's industry investment by the establishment's average share of industry investment over the establishment's lifetime. After some experimentation we used this method to interpolate for establishments with at most three year's missing data. This means we only lose 10% of the sample. Although investment is of course volatile, establishments' investment shares by industry are in fact extremely stable and so we feel the induced inaccuracies are likely to be small relative to very large gain in sample size.

$\Delta \ln L_t$ The log change in total manufacturing employment (direct from ARD).

$\Delta \ln S_t$ The log change in total manufacturing non-manual employment (direct from ARD).

$\Delta \ln U_t$ The log change in total manufacturing manual employment (direct from ARD).

$\Delta \ln M_t$ The log change in total manufacturing real intermediate inputs (£s in 1980) (direct from ARD), deflated by four digit input price deflators supplied by the ONS.

$\Delta MSHARE_{it-2}$. The lagged change in market share, *(t-2)-(t-3)* . The market share is establishment nominal gross output as a share of four digit industry nominal gross output.

$RENTS_{it-2}$. Rents lagged twice. It is defined as rents over net output, where rents are net output less material, capital and labour costs, expressed as a proportion of net output. Labour costs are the region- and four digit industry specific average manual and non-manual wage.

$\Delta RENTS_{it-2}$. The lagged change in rents, *(t-2)-(t-3).*

ΔFOR_{It} The change in employment in a foreign-owned plant as a share of total employment in the industry. Industry is defined at the two-digit level, there are 22 two-digit industries.

ΔFOR_{Rt} The change in employment in a foreign-owned plant as a share of total employment in the region. There are 11 standard regions in the United Kingdom.

h Manual hours at the two-digit industry level from the New Earnings Survey, as published in the Department of Employment Gazette.

Appendix 3.B Payments to foreign firms operating in the United Kingdom

The U.K. Government supports firms in many ways.[66] EU legislation restricts such support to special cases, such as investment that can be shown to be of social benefit in low-income areas designated by the EU as Assisted Areas. There are thus two main sources of support which are available in these areas.[67]

1. EU money from the European Structural funds. This money is mostly paid out to large infrastructure projects.
2. Money from the U.K. government. These are discretionary grants made to support both small (i.e., less than £500,000) and large (i.e., above £500,000) private investment projects.

Most funding for foreign investment is for larger projects, and comes from Regional Selective Assistance (RSA). The projects must either create new employment or safeguard existing employment in the Assisted Areas. To be eligible for RSA, before investment goes ahead applicants have to disclose the investment size as well as its expected employment creation and duration. Foreign companies are eligible for RSA for greenfield investments as well as expansions or modernisations of existing operations. RSA is available for up to 15% of eligible project costs (mostly the costs of capital investment).

A government official judges whether an RSA-applied investment will create jobs and for how long. It is difficult to assess exactly how this judgement is made. An indication of the process involved is given by the following excerpt from the standard

[66] For more information see www.Invest-in-the-UK.com. General information about grants is at www.dti.gov.uk/support and www.invest.uk.com.
See <http://news.bbc.co.uk/hi/english/business/the_company_file/newsid_332000/332560.stm> for information on the Siemens case and <http://news.bbc.co.uk/hi/english/uk/scotland/newsid_1294000/1294662.stm> for information on the Motorola case.

RSA application form; it states that all of the listed criteria must be met for the grant application to be considered.

The project:
Takes place in an Assisted Area. Is aimed at more than a local market. Is based on forecast growth in the market sector to ensure that displacement is not an issue. Will involve a minimum capital expenditure of £500,000 on fixed assets. Will directly create or safeguarded job in the business. Expects the business as a whole to be financially viable and profitable within three years. If the project is undertaken by a member of a group, the group will be financially stable. Needs RSA as essential for the project to proceed.

[67] Assisted Areas are designated as Tier 1, 2, or 3 depending on their deprivation level. U.K. examples of Tier 1, i.e., poorest, areas are the Sheffield and Liverpool areas.

Appendix 3.C Tables

Table 3-1: Basic Facts of the ARD Panel

Year	# Plants	# British Plants	# Foreign Plants	% Employment in Foreign Plants
	(1)	(2)	(3)	(4)
1973	21,413	20,418	995	0.12
1974	23,486	22,333	1,153	0.13
1975	21,798	20,665	1,133	0.13
1976	21,820	20,582	1,238	0.14
1977	21,860	20,363	1,497	0.16
1978	18,823	17,426	1,397	0.15
1979	17,965	16,441	1,524	0.16
1980	14,901	13,432	1,469	0.17
1981	14,717	13,155	1,562	0.18
1982	14,468	12,920	1,548	0.18
1983	14,046	12,493	1,553	0.17
1984	18,352	16,793	1,559	0.17
1985	13,783	12,416	1,367	0.17
1986	13,192	11,927	1,265	0.16
1987	13,316	12,026	1,290	0.16
1988	13,460	12,161	1,299	0.16
1989	18,982	17,370	1,612	0.18
1990	14,036	12,544	1,492	0.20
1991	13,926	12,319	1,607	0.22
1992	13,449	11,826	1,623	0.23

Note: In each year, a foreign-owned plant is defined as one in which a foreign business entity has at least a 20% ownership stake. All plants not meeting this criterion are defined as British owned. The employment shares in the final column report the share of overall U.K. manufacturing employment accounted for by foreign-owned plants. The sample of plants used for each year is the entire ARD selected sample, unweighted. See text for details on ownership and sampling issues.

Table 3-2 A. e B: Share of Foreign Employment by Region (A.) and by Industry (B.)

A. By Region

Region	1977	1992
South East	0.26	0.31
East Anglia	0.23	0.27
South West	0.12	0.18
West Midlands	0.08	0.22
East Midlands	0.08	0.14
Yorkshire /Humberside	0.11	0.16
North West	0.12	0.20
North	0.11	0.23
Wales	0.18	0.33
Scotland	0.19	0.29
N. Ireland	0.22	0.27

Note: Each cell reports the share of that region-year's total manufacturing employment accounted for by foreign-owned plants. The sample of plants used for each year is the entire ARD selected sample, unweighted. See text for details on sampling issues.

B. By Industry

Two-digit industry	1977	1992
21 Extraction and preparation of metalliferous ores	0.00	0.00
22 Metal manufacturing	0.05	0.19
23 Extraction of minerals not elsewhere specified	0.02	0.00
24 Manufacture of non-metallic mineral products	0.11	0.13
25 Chemical industry	0.29	0.38
26 Production of man-made fibres	0.16	0.20
31 Manufacture of metal goods not elsewhere specified	0.10	0.19
32 Mechanical engineering	0.18	0.28
33 Manuf. of office machinery and data processing equipment	0.41	0.68
34 Electrical and electronic engineering	0.22	0.31
35 Manufacture of motor vehicles and parts thereof	0.34	0.48
36 Manufacture of other transport equipment	0.02	0.11
37 Instrumental engineering	0.40	0.29
41 Food and drink manufacturing industries[1]	0.12	0.09
42 Food, drink and tobacco manufacturing industries[2]	0.11	0.25
43 Textile industry	0.04	0.08
44 Manufacture of leather and leather goods	0.04	0.00
45 Footwear and clothing industries	0.04	0.06
46 Timber and wooden furniture industries	0.03	0.06
47 Manuf. of paper and paper products; printing and publishing	0.16	0.22
48 Processing of rubber and plastics	0.23	0.28
49 Other manufacturing industries	0.14	0.13

Note: Each cell reports the share of that industry-year's employment accounted for by foreign-owned plants. The sample of plants used for each year is the entire ARD selected sample, unweighted. See text for details on sampling issues. Industries are by the U.K. Standard Industrial Classification.
1 - Oils, margarines, milk products; freezing, processing and preserving of meat, fish, fruit and vegetables; grain milling, bread and flour confectionery.
2 - Sugar and sugar confectionery, cocoa, coffee, tea, animal feeds and pet foods, and all others.

Table 3-3: The Effect of Foreign-Affiliate Presence on Productivity
Baseline Specifications of Equation (3.2)

	1-Year Differences			3-Year Differences			5-Year Differences		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\Delta FOR_{I,t}$	0.049		0.052	0.053		0.086	0.053		0.022
	(3.70)**		(3.08)**	(3.16)**		(3.58)**	(2.88)**		(0.85)
$\Delta FOR_{I,t-1}$			-0.060			0.003			0.068
			(3.42)**			(0.10)			(2.44)*
$\Delta FOR_{I,t-2}$		0.057	0.043		0.026	-0.005		0.052	0.013
		(3.39)**	(2.31)*		(1.14)	(0.17)		(1.95)	(0.45)
$\Delta FOR_{I,t-3}$		-0.006	-0.028		0.030	0.079		0.011	0.010
		(0.39)	(1.68)		(1.33)	(3.01)**		(0.40)	(0.37)
$\Delta FOR_{R,t}$	0.004		0.015	-0.011		0.044	-0.018		-0.035
	(0.23)		(0.71)	(0.45)		(1.25)	(0.56)		(0.91)
$\Delta FOR_{R,t-1}$			0.006			0.002			-0.020
			(0.25)			(0.05)			(0.46)
$\Delta FOR_{R,t-2}$		0.026	0.030		-0.076	-0.089		0.001	0.019
		(1.25)	(1.30)		(2.34)*	(2.35)*		(0.03)	(0.43)
$\Delta FOR_{R,t-3}$		0.029	0.029		0.075	0.103		0.036	0.034
		(1.28)	(1.24)		(2.28)*	(2.71)**		(0.82)	(0.77)
Σ FOR_I	0.049	0.051	0.007	0.053	0.056	0.163	0.053	0.063	0.113
P (ind)		0.0009	0.0000		0.0491	0.0003		0.0467	0.0067
Σ FOR_R	0.004	0.055	0.080	-0.011	-0.001	0.060	-0.018	0.037	-0.002
P (reg)		0.3026	0.5544		0.0228	0.0452		0.6635	0.6848
Observations	74,615	54,481	54,481	57,057	40,485	40,485	35,260	26,287	26,287
R-squared	0.56	0.58	0.58	0.70	0.71	0.71	0.76	0.76	0.76

Note: Robust t-statistics in parentheses. * significant at 5% level; ** significant at 1% level. The rows Σ FOR_I and Σ FOR_R report the sum of the relevant coefficient estimates in that column; P(ind) and P(reg) report the p-value for testing the joint significance of the relevant estimates. The dependent variable is the difference of the log real output. Other regressors are the differenced logs of capital, materials, skilled employment, unskilled employment and hours, year dummies, 20 two-digit industry dummies, 10 region dummies and competition control variables. For brevity, these coefficient estimates are not reported.

Table 3-4: The Effect of Foreign-Affiliate Presence on Productivity
Specifications of Equation (3.2) By Absorptive Capacity

	Employment within each two digit industry/year			TFP within each two digit industry/year			Skill intensity within each two digit industry/year		
Centile Range	Below 25th	Between 25^{th} & 75^{th}	Above 75^{th}	Below 25th	Between 25^{th} & 75^{th}	Above 75^{th}	Below 25th	Between 25^{th} & 75^{th}	Above 75^{th}
$\Delta FOR_{I,t}$	0.074	0.053	0.027	0.059	0.033	0.022	0.062	0.070	-0.009
	(1.75)	(3.08)**	(1.17)	(1.95)	(1.75)	(0.88)	(2.31)*	(4.03)**	(0.29)
$\Delta FOR_{R,t}$	0.054	-0.014	0.000	0.004	0.033	-0.047	-0.008	0.000	0.033
	(1.08)	(0.60)	(0.01)	(0.12)	(1.38)	(1.31)	(0.24)	(0.00)	(0.89)
Observations	10,143	40,965	23,507	16,284	34,350	23,981	18,059	39,214	17,342
R-squared	0.51	0.58	0.57	0.60	0.60	0.51	0.56	0.58	0.54

Note: Robust t-statistics in parentheses. * significant at 5% level; ** significant at 1% level. The percentiles use the average employment, TFP and skill share in years (t-1) and (t-2) in the corresponding two digit industry. Skill intensity is the share of skilled employment over total employment. The dependent variable is the difference of the log real output. Other regressors are the differenced logs of capital, materials, skilled employment, unskilled employment and hours as described in appendix, year dummies, 20 two-digit industry dummies, 10 region dummies and competition control variables. For brevity, these coefficient estimates are not reported.

Table 3-5: The Effect of Foreign-Affiliate Presence on Productivity
Specifications of Equation (3.2) By Different Source Countries

United States	
$\Delta FOR_{I,t}$	0.063
	(3.95)**
$\Delta FOR_{R,t}$	-0.005
	(0.20)
France	
$\Delta FOR_{I,t}$	0.106
	(2.43)*
$\Delta FOR_{R,t}$	-0.055
	(0.86)
Germany	
$\Delta FOR_{I,t}$	0.048
	(0.42)
$\Delta FOR_{R,t}$	-0.159
	(1.05)
Japan	
$\Delta FOR_{I,t}$	-0.275
	(2.59)**
$\Delta FOR_{R,t}$	-0.070
	(0.57)
Observations	74,615
R-squared	0.56

Note: Robust t-statistics in parentheses. * significant at 5% level; ** significant at 1% level. The dependent variable is the difference of the log real output. Other regressors are the differenced logs of capital, materials, skilled employment, unskilled employment and hours as described in appendix, year dummies, 20 two-digit industry dummies, 10 region dummies and competition control variables.

Table 3-6: The Effect of Foreign-Affiliate Presence on Productivity
Specifications of Equation (3.2) With Various Robustness Checks

Robustness Check	Foreign Presence Uses Skilled Employment (1)	Foreign Presence Uses Non-Selected Sample (2)	Use Non-Selected and Sampling Weights (3)	Foreign Presence Uses Levels, Not Shares (4)	Control for Number of U.K.-Owned Plants (5)
$\Delta FOR_{I,t}$	0.048	0.038	0.039	2.47 x E-7	0.051
	(3.80)**	(2.63)**	(1.16)	(4.62)**	(3.84)**
$\Delta FOR_{R,t}$	-0.010	0.001	-0.057	-6.89 x E-9	0.002
	(0.65)	(0.08)	(1.03)	(0.18)	(0.11)
Observations	74,615	74,615	74,615	74,615	74,615
R-squared	0.56	0.56	0.53	0.56	0.56

Robustness Check	Vary by Each Sector All Input Coefficients (6)	Use Constructed TFP as Regressand (7)	Exclude Observations with Multiple Plants (8)	Exclude Observations in Wales (9)
$\Delta FOR_{I,t}$	0.048	0.029	0.044	0.050
	(3.65)**	(2.08)*	(2.73)**	(3.68)**
$\Delta FOR_{R,t}$	0.004	0.002	-0.011	0.004
	(0.24)	(0.14)	(0.54)	(0.21)
Observations	74,615	75,157	48,787	71,985
R-squared	0.57	0.03	0.56	0.56

Note: Robust t-statistics in parentheses. * significant at 5% level; ** significant at 1% level. All regressions shown here use one-year time differences, analogous to column 1 of Table 3-3. The dependent variable is the difference of the log real output. Other regressors are the differenced logs of capital, materials, skilled employment, unskilled employment and hours as described in appendix, year dummies, 20 two-digit industry dummies, 10 region dummies and competition control variables. For brevity, these coefficient estimates are not reported. Column 1 measures foreign activity using skilled employment rather than total employment. Column 2 measures foreign presence accounting for employment estimates from the non-selected sample. Column 3 does the same, but also weights observations using sampling weights. Column 4 measures foreign presence using the absolute number of foreign employees, rather than employment shares. Column 5 adds as control regressors the lagged number of U.K.-owned plants by industry and region. Column 6 estimates a separate set of the five input coefficients for each two-digit industry. Column 7 uses as the regressand TFP calculated by the standard method that assumes perfect competition, and thus omits the input regressors. Column 8 excludes all observations that cover multiple plant locations. Column 9 excludes all observations located in Wales. For all these robustness checks, see text for details.

Chapter 4. An illustration of the role of job mobility in the estimation of returns to job seniority and labour market experience: a comparison of the U.K. and Germany

4.1 Introduction

This study uses worker-level data, the *British Household Panel Survey* and the *German Socio-economic Panel* to compare the U.K. and Germany[68], in terms of the importance of tenure in the firm and experience in the labour market for the wage profile of workers. There has been an extensive debate on the measurement of the impact of seniority on wages. This has been the case because of the difficulties in eliminating estimation biases associated with individual and job-match heterogeneity and endogeneity of job mobility. We compare two countries with very different labour market institutions and patterns of job mobility. For example, while according to the OECD Employment Outlook (1999) the UK is among the countries with least restrictive employment protection legislation, Germany stands out for having

[68] Because for Germany our data runs from 1984 to 1996, East Germany is excluded from our analysis.

relatively strict employment protection[69], along with France and Southern European countries. Moreover, unlike the UK, Germany has a tight corporatist labour market[70], which implies that even though there are no legal minimum wages in Germany, there are contractual wages per hour or month which are applied to all job categories, cover over 90 percent of the working population, and which are re-negotiated in most instances on an yearly basis. In the UK, and the for the period under analysis (1991-1999) wages are much less regulated due to unions' weakened power and the absence of minimum wages. These institutional disparities may play an important role in facilitating or hampering wage flexibility and job mobility. We show evidence that not only is job mobility higher in the UK than in Germany, but also that job movers are negatively selected in Germany and not in the UK. In our discussion of the results we point out that these results can be driven by "stickier wages" in Germany (in models with employers' learning of workers' ability) as well as by adverse selection of job movers in Germany due to low mobility in a context of asymmetric information between current and prospective employers about workers' ability. After correcting for most bias associated with job and individual heterogeneity, our findings suggest that returns to tenure are close to zero in both countries and returns to experience are substantially higher in the UK than in Germany. According to our estimates, ten years of labour market experience are associated with average wage returns of between 60 and 70 percent in the UK and between 30 and 40 percent in Germany. Separate

[69] They report indicators of strictness of employment protection based on the regulations concerning firing, e.g., redundancy procedures, mandated pre-notification periods and severance payments, special requirements for collective dismissals and short-time work schemes.

[70] There are strong unions and employers' associations with autonomy to conclude collective agreements virtually on all matters of labour relations. The Federal Minister of Labour and Social Affairs estimates that, in 1990, the number of collective agreements in force was about 32000, encompassing almost all industries and services and about 90% of all employees (K.-L. Paque', 1993). Typically, these collective agreements fix contractual minima for wages and working conditions, and in

estimates for different qualification groups show that in Germany, it is the group of workers with apprenticeship training that drives down the returns to labour market experience in Germany. Our interpretation of this result is that much of the learning that seems to take place in the first few years in the UK labour market, is provided through the apprenticeship training in Germany. In fact, both the unskilled and the university graduates' returns to experience do not differ much between the two countries.

Our empirical section starts with the estimation of the very simple wage equation using OLS and three other estimation methods used in the previous returns to tenure literature and which aim at correcting (at least partially) for some of the estimation bias. These estimation methods are the instrumental variable estimator suggested by Altonji and Shakotko (1987), Finnie's (1993) modification of the Altonji and Shakotko's estimator and the 2-stage method proposed by Topel (1991). These are not the only methods in the literature to attempt correction of some of the inherent problems of estimating returns to seniority. The Abraham and Farber (1987) method of using complete tenure (or an estimate of it) to capture the unobserved dimensions of job or worker quality has been shown by Topel (1991) to be inconsistent. Dustmann and Meghir (2001) estimate a more flexible model[71], but which requires a sufficient large sample of separations that can be assumed as exogenous, such as from establishments closures. They use a 1 percent sample of the German Social Security records (IAB data) for the period 1975-1995, and therefore their study focus on young workers only[72]. Buchinsky et al (2001) explicitly model the participation and mobility

practice virtually all organised employers offer the same wage and working conditions to union members and non-members alike.

[71] Wages are match specific and workers move jobs as a result of identifying a better match.

[72] The oldest worker in their sample is 35 years old.

decisions together with the wage equation. In order to compute the parameter estimates they adopt a Bayesian approach and employ methods of Markov Chain Monte Carlo.

A clear attraction of the methods we chose to re-visit in this paper is their simplicity and low data requirements. For this reason the Altonji and Shakotko (1987) instrumental variable estimator has also been used to study extensions of the standard wage model such as the returns to industry specific capital (Parent, 1999) and the impact of employer-provided training on wages (Parent, 2000), as well as to investigate the evolution of the wage premium for job seniority in the US (Marcotte, 1998).

At least two other studies estimate returns to seniority with the employer and to labour market experience in the UK[73]. Paull (1998) studies the pattern of wage growth of the low paid men and women. She uses the first four waves of the BHPS and the Topel 2-stage method, but because the results are reported separately for jobs preceded by an unemployment spell and jobs beginning directly after previous employment, they are not directly comparable with the ones we present in this study. Swaffield (2000) studies the female wage equation and the gender wage differential using the 1991-1997 waves in the BHPS. Since the paper's emphasis is on the female wage equation, only least squares returns for tenure and experience for men are shown, and these include jobs in both the public and private sectors.

[73] Brunello and Ariga's (1997) compare earnings and seniority in Japan and the UK, but they do not distinguish between firm seniority and labour market experience. Their specification includes polynomials in firm tenure and age net of tenure, implying that the coefficient of their tenure variables measure returns to both tenure and a rough proxy for experience. They use the years 1975, 1976 and 1979 of the New Earnings Survey to construct their units of observation – cell means cross-classified by age group, industry and socio-economic group. Their data does not have information on education.

This chapter is organised as follows. Section 4.2 sets out the wage growth model and the estimation methods used. Section 4.3 describes the data. Section 4.4 provides descriptive statistics of job mobility, within and between jobs wage growth, and reasons for job separation. Section 4.5 presents the results, section 4.6 discusses them in light of the institutional differences between the two countries, and section 4.7 concludes.

4.2 Methods

4.2.1 The empirical model

The empirical analysis is based on the standard wage model described in Topel (1991) and Altonji and Shakotko (1987) in which workers wages depend on aggregate real wage growth, years of experience in the labour market and seniority with the firm.

$$W_{ijt} = \beta_0 \gamma_t + \beta_1 X_{ijt} + \beta_2 T_{ijt} + \varepsilon_{ijt} \qquad (4.1)$$

The dependent variable, W_{ijt}, is the log of the gross real hourly wage of individual i on job j at time t. γ_t is the time dummy, X_{ijt} denotes actual experience in the labour market and T_{ijt} is seniority with the current employer. The empirical regression also includes individual controls and higher order terms of the tenure and experience variables.

The error term ε_{ijt} is decomposed in three orthogonal components, A_i, θ_{ij} and v_{ijt}. The individual fixed effect A_i captures unmeasured differences in ability, the job-match effect θ_{ij} is fixed during the course of a job and allows for heterogeneity in the quality of the job matches, and the transitory component v_{ijt} accounts for idiosyncratic shocks and measurement error:

$$\varepsilon_{ijt} = \theta_{ij} + A_i + v_{ijt} \tag{4.2}$$

The wage equation in (4.1) can be re-written using (4.2):

$$W_{ijt} = \beta_0 \gamma_t + \beta_1 X_{ijt} + \beta_2 T_{ijt} + \theta_{ij} + A_i + v_{ijt} \tag{4.3}$$

Both the individual and job match effects can be correlated with years of seniority and labour market experience as represented in the auxiliary regressions (4.4) and (4.5):

$$A_i = b_1 X_{ijt} + b_2 T_{ijt} + \xi_{ijt} \tag{4.4}$$

$$\theta_{ij} = c_1 X_{ijt} + c_2 T_{ijt} + \omega_{ijt} \tag{4.5}$$

Individuals with high unobserved ability are usually assumed to experience less layoffs and quits due to some inherent characteristic such as perseverance, motivation, or health status. This is usually assumed by analogy to the empirical positive relationship found between job tenure and other observable measures of ability such as education. Thus, unobserved ability A_i is likely to be positively correlated with the tenure variable. Schönberg (2002) has shown that employers' asymmetric learning about workers' ability may provide an explanation for more able individuals having longer job spells. If the firm has discretion with respect to whom lay off, the market infers that laid-off workers are of low ability. Wages offered by prospective employers reflect this expectation and high ability individuals have no incentive to move jobs due to adverse selection. (Gibbons and Katz, 1991, Acemoglu and Pischke, 1998). These models predict that high ability workers are less likely to be laid off by current employers and, because of adverse selection, are also less likely to quit.

Experience in the labour market X_{ijt} may be positively correlated with the individual fixed effect A_i. Workers' experience is the result of successive decisions in and out of employment. If high ability individuals have longer job spells, they are for this reason likely to experience less unemployment spells throughout their lives, accumulating more labour market experience than low ability individuals. However, since tenure and experience are positively correlated, the positive correlation between tenure and the individual fixed effect induces a negative correlation between experience and the individual fixed effect (see Altonji and Williams, 1997 for a formal derivation). Intuitively, this is due to returns to experience being identified from returns to tenure when workers change jobs. If low ability workers are over-represented among job movers due to the positive correlation between job duration and the individual fixed effect, this would both overstate the effect of job tenure on wages and understate the effect of labour market experience on wages. The positive correlation between experience and the individual fixed effect would also induce, again because tenure and experience are positively correlated, a negative correlation between tenure and the job match component. In conclusion, both tenure and experience are likely to be positively correlated with A_i. However, because these two variables are positively correlated, the sign of the overall effect of A_i on the tenure and experience slopes is unclear.

Tenure can also be positively correlated with the job-match effect θ_{ij} since workers may be less likely to quit high wage jobs. A "good match" is a worker-job pair with a high wage relatively to the distribution of wages the worker faces. Workers in good matches a less likely to receive a better offer than the current one and therefore are less likely to quit. In addition, if firms share the returns to a "good match", workers in jobs with a high θ_{ij} are also less likely to be laid off.

Search theory and matching models predict a positive correlation between experience and the job match effect, since job shopping over a career implies that the longer an individual spends in the labour market, the higher the probability of having received above average wage offers. Again, even though both tenure and experience are likely to be positively correlated with the job match effect θ_{ij}, given that tenure and experience are positively correlated with each other, the overall signs of the effect of θ_{ij} on tenure and experience are unknown. Formally, our model can now be re-written using (4.3), (4.4) and (4.5):

$$W_{ijt} = \beta_0 \gamma_t + (\beta_1 + b_1 + c_1) X_{ijt} + (\beta + b_2 + c_2) T_{ijt} + \xi_{ijt} + \omega_{ijt} + v_{ijt} \qquad (4.6)$$

Least squares estimation of (4.6) is likely to produce biased estimates of returns to seniority and experience and according to our previous discussion, the signs of these biases are unknown.

4.2.2 Altonji and Shakotko (A+S) Instrumental Variable procedure

The first method we use to correct for some of the potential biases described above was suggested by Altonji and Shakotko (1987). They follow an instrumental variable approach in which each of the tenure variables is instrumented with its deviations from *job means* DT_{ijt}. Let $\overline{T}_{ij}$ be the job mean of the tenure variable, then $DT_{ijt} = T_{ijt} - \overline{T}_{ij}$. The empirical section also includes higher order terms in tenure that are instrumented in the same way. If $\overline{T_{ijt}^{P}}$ is the job mean of a higher order term of the tenure variable, then $DT_{ijt}^{P} = T_{ijt}^{P} - \overline{T_{ijt}^{P}}$ is its deviation from the job mean. As this variables have zero average over each job, they are by construction orthogonal to the fixed individual and job match components. Hereafter we will call the estimation method of instrumenting tenure with its deviations from job means IV-tenure.

Experience cannot be instrumented with deviations from its job means as well, because deviations from job means of the linear terms of tenure and experience are perfectly collinear. The authors note that the potential positive correlation between experience and the job match effect would not only bias the experience effect upwards, but would also bias the tenure effect downwards: "Intuitively, the downward bias rises as a partial correction for the overstatement of the effect of additional labour market experience on wages during years in which the job remains the same" (p. 440). However, returns to experience can also be biased due to the individual fixed effect being correlated with experience (this in turn can bias returns to tenure) and these bias can be of either sign, depending on whether the positive correlation due to higher ability workers experiencing less or shorter unemployment spells outweighs the negative correlation due to negative selection of job movers.

4.2.3 Finnie's extension of Altonji and Shakotko Instrumental Variable procedure

Finnie (1993) extends the IV-tenure estimator by instrumenting the tenure variables with their deviations from *job means* and the experience variables with their deviations from *individual means.* Let $\overline{Exp_{ijt}}$ and $\overline{Exp^{P}_{ijt}}$ be the individual means of the experience variables, then $DExp_{ijt} = Exp_{ijt} - \overline{Exp_{ijt}}$ and $DExp^{P}_{ijt} = Exp^{P}_{ijt} - \overline{Exp^{P}_{ijt}}$ are the deviations from their individual means. As this variables have zero average over each individual, they are by construction orthogonal to the individual fixed effect. Because tenure is instrumented with a variable that is uncorrelated with the individual and job match effects, and experience is instrumented with a variable that is uncorrelated with the individual fixed effect, we expect this estimator to be free from most bias. Experience instruments can still nevertheless be correlated with the job match component. Theory predicts a positive correlation between experience and the job match component. Thus, with this estimation method returns to experience can still be positively biased and returns to tenure negatively biased.

4.2.4 Topel 2-stage estimation procedure

The method put forward by Topel (1991) is to use the 2-stage procedure to estimate a lower bound to returns to job seniority. The first stage consists of estimating the sum of the tenure and experience linear terms from a regression in which first differences are applied to observations of the same job in (4.3). Because first differences of the linear tenure and experience terms are perfectly collinear, they cannot be identified separately from the differenced regression. Given that first differences are applied to observations of the same job only, fixed individual and job effects are eliminated, thus returning consistent estimates of $\beta = \beta_1 + \beta_2$.

$$\Delta W_{ijt} = (\beta_1 + \beta_2) + \xi_{ijt} \tag{4.7}$$

For ease of exposition we drop the time dummy from (4.7). In the empirical section, we subtract from differenced log wages the first difference of the time dummies' coefficients obtained from a least squares estimation of (4.1). Time effects obtained from the least squares estimation of (4.1) should be free from bias under the assumption that the covariance between the time effects and each of the unobservables is zero, conditional on X_{ij} and T_{ij} (Altonji and Williams, 1997).

Topel notes that the following relationship holds between tenure and experience:

$$X_{ijt} = X_{0ij} + T_{ijt} \tag{4.8}$$

where X_{0ij} is experience at the start of a job. Using the first step estimate of $\beta = \beta_1 + \beta_2$ and (4.8), β_1 can be obtained from initial wages on new jobs in a second step:

$$W_{ijt} - \hat{\beta}T_i = \beta_1 X_{0ij} + A_i + \theta_{ijt} + \zeta_{ijt} \tag{4.9}$$

One could use only observations at the start of a job, with zero tenure. Due to efficiency concerns, Topel favours an estimation equation that uses all observations, in which tenure multiplied by the unbiased estimate of β obtained from the first stage is subtracted from the log wage on the left hand side[74]. An estimate of returns to tenure β_2 is obtained by subtracting the estimate of the linear experience coefficient β_1 from the first step unbiased estimate of $\beta_1 + \beta_2$.

Topel notes that in (4.8) initial experience X_{0ij} is correlated with θ_{ijt}. Assuming for the moment that experience is uncorrelated with the individual fixed effect A_i, the biases associated with the job match effect θ_{ijt} can be easily calculated by using (4.5):

$$E(\hat{\beta}_1 - \beta_1) = b_1 + \gamma_{X_0 T}(b_1 + b_2) \qquad (4.10a)$$

$$E(\hat{\beta}_2 - \beta_2) = -b_1 - \gamma_{X_0 T}(b_1 + b_2) \qquad (4.10b)$$

Where $\gamma_{X_0 T}$ is the least squares coefficient from a regression of tenure T_{ijt} on initial experience X_{0ij}. Since experience and the job-match effect are positively correlated, the experience effect is overestimated (4.10a), thus returning an underestimation of the tenure effect (4.10b).

Topel acknowledges that the potential positive correlation between tenure and the individual fixed effect may also bias the experience effect downwards when experience at the start of the job is used: more able individuals change jobs less often and so, on average they started their jobs earlier. This implies that controlling for total experience, more able individuals have on average lower levels of experience when they started their current job. To correct for this bias, Topel suggests instrumenting

[74] In the empirical section we include higher terms in tenure and experience. We estimate the following modification of (4.7) to (4.9):

$$\Delta W_{ijt} = (\beta_1 + \beta_2) + \beta_1^P \Delta X_{ijt}^P + \beta_2^P \Delta T_{ijt}^P + \xi_{ijt} \qquad (4.7a)$$

$$X_{ijt} = X_{0ij} + T_{ijt} \qquad (4.8a)$$

initial experience in the second stage with total experience. However we've argued before that total experience itself can be positively correlated with the individual fixed effect. Experience is therefore also likely to be positively biased due to its correlation with the individual fixed effect, which would re-enforce the underestimation of the tenure effect.

4.3 The Data

In this section we describe the two data sets used in this chapter. Further details can be found in the appendix 4.C. This study uses the first 9 waves of the British Household Panel Survey (1991-1999) and the first 14 waves of the German Socio-Economic Panel (1984-1997).

4.3.1 The British Household Panel Survey

The BHPS was designed as an annual survey of all adult (16+) members of a nationally representative sample of more than 5,000 households, making a total of approximately 10,000 individual interviews. The same individuals are followed in the successive waves and, if they split-off from original households, all adult members of their new households are also interviewed. Children are interviewed once they reach the age of 16. Thus the sample should remain broadly representative of the population of Britain as it changes through the 1990s. However, in order to construct tenure and experience we need to use the retrospective data on past jobs collected in the second

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$$W_{ijt} - \hat{\beta}T_i - \hat{\beta}_1^P X_{ijt}^P - \hat{\beta}_2^P T_{ijt}^P = \beta_1 X_{0ij} + A_i + \theta_{ijt} + \zeta_{ijt} \qquad (4.9a)$$

and third waves (1992 and 1993). For this reason, we may not be able to include adults of newly formed households with members that split-off from the original households. We assume that this sample selection is random and does not affect the wage regressions as long as tenure and experience are included in the regressions.

At each wave the interviewees are asked to state the beginning date of the ongoing job spell, which is defined by a change of employer or a change of job within the same employer. Previous literature has focused on returns to tenure with the employer. We follow the same approach because promotions and job changes within the employer are likely to be associated with wage changes, and therefore must be considered as part of the same spell, for the purpose of the measurement of wage returns to job seniority. For this reason, the construction of both the tenure and experience variables require the use of the retrospective data on the labour market histories, for even if one chose to use potential experience instead of actual experience, this would be needed for the accurate construction of the variable "tenure with the employer".

When linking the job spell information in the various yearly questionnaires and the retrospective data collected in waves 2 and 3 one is confronted with the overlapping of more than one source of information for the same spell. Conflicting answers are resolved by giving priority to the information collected closest to the event occurrence. This is because recall error is likely to increase with the time elapsed between an event and the time of interview.

With the purpose of minimising endogeneity and unobserved heterogeneity, our analysis restricts the sample to observations of non-self-employed white males aged between 16 and 60 with jobs in the private sector. Self-employed wages may be misreported and loosely related with the individual productivity, non-white and female wages may suffer from discrimination, and the latter are also likely to be highly affected by endogenous labour force participation. By restricting the age interval, we avoid individuals without strong labour force attachments. We exclude the public sector where wages are regulated and may not reflect accumulation of human capital and worker productivity increases.

The earnings variable used is real hourly wage. Nominal hourly wage is obtained by first dividing the current job usual gross monthly pay by 4.33 to obtain weekly wage and then by weekly hours, to obtain hourly wage. Weekly hours is the sum of the number of hours normally worked per week and the number of overtime hours in normal week. The nominal hourly wage is then deflated with the Retail Prices Index[75] to obtain real hourly wages.

Some of the analysis in the next sections the will divide workers into three skill groups: *unskilled*, *medium skilled* and *university graduates*. For the UK data the unskilled include those which report the following qualifications: no qualifications, other qualifications, apprenticeship, CSE, commercial qualifications, no O levels. The medium skilled include those with O levels or equivalent, nursing qualifications, teaching qualifications and A- levels. Finally, the university graduates are those with a higher degree, a first degree, or other higher qualification.

[75] Monthly values are averaged for each year with 1991 as base year. Source: Economic Trends, Annual Supplement, 1998, Office for National Statistics.

4.3.2 The German Socio-Economic Panel

The GSOEP started in 1984 as an yearly longitudinal survey of 4298 private households[76] and around 9000 individuals in the Federal Republic of Germany (FRG). Although from 1990 data is also collected for the German Democratic Republic (GDR), we restrict our analysis to the FRG since the GDR labour market is likely to behave very differently. Similar to the BHPS, in the GSOEP all household members are interviewed individually from the age of 16. In principle, all persons who took part in the very first wave of the survey as well as their children whenever born, are surveyed in the following years whether or not they remain in the household. Third persons moving into an existing GSOEP household are also followed-up. For the same reasons stated in section 4.3.1, the analysis is restricted to observations of non-self-employed white males aged between 16 and 60 with jobs in the private sector.

The number of years of labour market experience is constructed in two stages. The first stage uses the yearly biographical scheme containing employment information from the age of 16 to the first wave of the panel to construct total experience at the entry of the panel. Both part-time and full-time spells are taken into account. The second stage uses the calendar available for each wave listing all labour market activities for each month in the year preceding the interview. This information is added to the information computed in the first stage to construct experience at each wave. The tenure variable is constructed from the information about the exact year and month the individual has started current job (i.e., the employment spell with the current employer), up to the time of interview.

Wages are computed by dividing reported gross earnings in the month before the interview by the number of hours worked for pay.

For the German data, given the apprenticeship system, the three skill groups considered are the following: workers with no apprenticeship training and no university – the *unskilled*, workers with apprenticeship training but no university degree – the *medium skilled* or *apprenticeship trainees*, and workers with university degree – the *university graduates*.

4.3.3 Sample statistics

Columns 1 and 3 of Table 4-1 show the mean sample characteristics for the two full samples. There is a high degree of similarity between the two data sets. The average age is 37 in the UK and 39 in Germany and the mean experience is 20 years in the UK and 19 in Germany. When constructing labour market experience in Germany we did not include the apprenticeship training period, hence the larger age-experience gap in Germany than in the UK. Average tenure is two years longer in Germany than in the UK. The remaining columns present the summary statistics for the differenced sub-samples used in the first stage of Topel 2-stage model. Workers in the differenced sub-samples are older, more experienced and have been in their current jobs longer.

[76] These numbers are for the GSOEP sub-sample A - "Residents in the FRG", 95% scientific-use version.

Table 4-1: Summary Statistics - Mean sample characteristics for white males

	U. K. - BHPS		Germany - GSOEP	
	Full sample	Topel differenced sample	Full sample	Topel differenced sample
	1	2	3	4
Hours worked per week	39.8	39.8	40.8	40.7
	(6.7)	(5.9)	(6.2)	(5.7)
Tenure (years)	8.2	9.8	10.0	11.8
	(7.9)	(7.9)	(9.5)	(9.4)
Experience (years)	19.6	21.2	18.9	21.0
	(11.6)	(11.1)	(12.0)	(11.5)
Age	36.9	38.4	39.1	41.0
	(10.9)	(10.4)	(10.7)	(10.2)
Percent married	63.8	68.3	74.9	80.2
Number of observations	7073	4572	12302	8818
Number of individuals	1502	1079	2209	1673
Number of jobs	2259	1345	3053	1993
Number of waves	9	8	14	13

Note: Standard errors in parenthesis

4.4 Descriptive statistics

4.4.1 Wage growth: total, within jobs and between jobs

Figure 4-1 depicts the average yearly wage growth in both data sets for the years available[77]. Wage growth between two adjacent years is computed by averaging the difference in the log of real hourly wage for all individuals observed in both periods. This does not necessarily coincide with total wage growth across all individuals, since it does not include those that enter and exit the panel[78]. In the years

[77] Nominal wages were deflated with the retail price index for each country. All figures and tables use real wages.

[78] Wage growth between 1984 and 1985 in Germany may be excessively low since unlike the other years where 80 to 95 percent of interviews take place between February and April, in 1984 by May only 50 percent of the interviews had taken place, and the remaining took place between May and September. Measurement error in yearly wage growth is therefore likely to be higher between 1984 and 1985. We expect the time dummies in our regression analysis to pick the potential downward bias.

1984-97, the real gross hourly wages in the German sample grew on average 3.23 per year and in the period 1991-99 the real gross hourly wages in the UK sample grew on average 2.87 per year. However, during the years for which there is data for the two countries – 1991 to 1997, the yearly wage growth was very similar in the two data sets (2.81 in Germany against 2.58 in the UK).

Figure 4-2 shows the yearly average wage growth within and between jobs. The between jobs wage growth is much more volatile than the within jobs wage growth. This is probably related with the differences in the causes for separation during periods of low and high wage growth. It is a well known fact that one of the characteristics of recessionary years is the increase in the number of laid off workers. Conversely, during periods of higher wage growth when it is easier for workers to find jobs, there should be a higher share of voluntary moves. Volatility is however likely to be exacerbated by the reduced number of individuals changing jobs each year[79].

While between jobs wage growth is clearly above within jobs wage growth in Germany, this is only true in the UK in the second half of the nineties. For both Germany and the UK, however, the overall average of between jobs wage growth (5.66 and 3.25) is higher than within jobs wage growth (3.23 and 2.82). Figure 4-3 plots within and between jobs wage growth by experience interval. For both countries between jobs wage growth is higher than within jobs wage growth in the first 10 to 15 years of workers' careers. After that, wage gains at job changes fall below within jobs wage growth becoming negative towards the end of individuals' careers. This is consistent with decreasing marginal returns to job search.

[79] In the German sample each year between 40 and 60 individuals are observed in a different job from the previous year, while in the BHPS sample we observe between 60 and 80 job changes in two consecutive years.

Figure 4-4 shows the cumulative wage growth with years of experience. For each year of experience, the cumulative wage growth is computed by summing up the average yearly wage growth for that and all lower years of experience. The figure shows cumulative wage growth averaged over the entire sample period. The concave relationship between wages and experience is very similar in both countries. In the first 10 years in the labour market workers have a 77.3 percent average wage increase in the UK and a 73.7 percent in Germany and the cumulative wage growth of the first 20 years in the labour market is close to one hundred percent in both countries.

Figure 4-5 shows cumulative wage growth for different sub-periods. For Germany there is no evidence of significant changes during the period 1984-1997. However in the UK there is some evidence that average wage growth with have become higher at higher levels of experience overtime. This is consistent with the rise in wage inequality that took place in the UK in the 1990s. However, a note of caution is worth making with respect to the figure for the UK. The data is likely to be noisy at low levels of experience since our BHPS sample has few observations of individuals who just entered the labour market each year. In fact, in order to construct actual labour market experience, individuals had to either be in the panel at wave 2 where that information was collected, or not have previous job history. Although we include children who reach the age of 16 in households previously interviewed, we are not able to include new members of newly formed households. So, for example, while in the period 1994-96 wage growth between the first and the second year of experience seems remarkably high, in the period 1997-99 wage growth seems to be higher between the third and the fourth year of experience.

Figure 4-6 shows that in the UK university graduates experience higher wage growth than medium educated workers which in turn have higher wage growth than unskilled workers. By contrast, university graduates and workers with apprenticeship training in Germany have very similar wage growth profiles, being the unskilled workers the ones whose wages fall increasingly behind during their first ten years in the labour market.

4.4.2 Reasons for job separation, education and experience

Figure 4-7 plots the share of voluntary separations (quits), employer originated separations (layoffs) and separations for other reasons on the total number of job separations by experience interval in the UK and Germany[80]. Both countries exhibit higher share of quits than layoffs at all experience intervals. However, the share of quits is between 50 and 60% of total separations in Germany while in the UK it is between 40 and 50%. This is consistent with higher firing costs in Germany. In fact, the average share of employer originated separations is 30% in the UK and 25% in Germany. Both in Germany and the UK the share of quits increases during the first ten years of labour market experience and the share of other reasons decreases during the same period. The latter is probably associated with increased workers' attachment to the labour market.

Figure 4-8 shows the evolution of the shares of quits, employer originated and other separations in total separations in the UK and Germany for the period with data

[80] These graphs use for the UK our cleaned BHPS data for which we could match "reason for separation". For Germany it uses the 1990 to 1997 waves of our cleaned GSOEP data, because before 1990 GSOEP respondents were given the possibility of choosing the answer "separation by common agreement" which makes it particularly difficult to identify quits from employer related separations.

available in both data sets: 1991-1997[81]. In the UK the share of quits increases during the period, while the share of employer originated separations falls. Quits dominate employer originated separations from 1993 onwards only. Possible explanations for this are the decline in the unemployment rates (OECD, 1999) and increase in GDP growth rate (OECD) that took place during this period. In Germany, and during the same period there is a steady decline in the share of quits and an increase in the share of employer originated separations. Again, the increase in the unemployment rates (OECD, 1999) and the decline in GDP growth that took place in Germany (OECD) during the period may explain, at least partially, the evolution observed. Figure 4-9 shows the average number of jobs by years of experience in the UK and Germany. In calculating the number of jobs we add to the number of jobs before the panel to the number of different jobs at the interview dates. We are therefore not accounting for the number of short job spells between interviews, which is likely to be higher in the UK. British workers hold on average more jobs during their careers than German workers and the difference between the two countries is likely to be even larger than the one depicted, due to the omission of short spells between waves.

[81] See in data appendix why for the GSOEP we do not use the information on the reason for separation prior to 1991.

4.5 Results

4.5.1 Returns to tenure and experience

Table 4-2 A. and B. show the coefficients and cumulative returns to tenure and experience in the two countries using the four estimation methods described: OLS, IV-tenure, IV-tenure-experience and the 2-stage method. All regressions use fourth degree polynomials in tenure and experience to allow for non-linear returns[82]. Columns 1 and 5 of Table 4-2.B. show that according to OLS estimates ten years of tenure generate a 9.1 percent wage increase in the UK and a 8 percent wage increase in Germany. These values are considerably smaller than typical estimates with US data (Topel 1991, Altonji and 1987, Altonji and Williams, 1997). Moreover, with the exception of the Topel 2-stage method for the UK, which estimates a 13.8 percent cumulative returns to 10 years of tenure, returns to 10 years of tenure are always below 10 percent in both countries. When tenure is instrumented with deviations from job means, returns to 10 years of tenure decline to 6.1 percent in the UK and to 2.2 percent in Germany, and loose statistical significance. This suggests that the correlation between tenure and the individual and job match effects generate a positive but small bias in the returns to tenure estimated by OLS. As it has been pointed out before, because experience may be correlated with the individual and job unobservables, returns to experience are likely to be biased, which may in turn bias

[82] We decided to include a fourth degree polynomial in tenure for both countries though only for Germany with the least squares estimator the non-liner terms in tenure are significantly different from zero (at the 5 percent confidence level). Similarly, we also chose a specification with a fourth degree polynomial in experience for both countries, though for Germany the 3rd and 4th order experience terms

returns to tenure. Columns 3 and 7 show that for the IV-tenure-experience estimator, returns to experience increase only slightly in the UK and almost 10 percentage points at 10 years of experience in Germany in relation to the IV-tenure estimations. What can we conclude from the IV-tenure-experience estimator results about the likely bias in the OLS estimates? We have argued before that unobserved ability can be positively correlated with labour market experience if low quality workers have more or longer unemployment/non-participation spells. However ability can also be negatively correlated with experience due to returns to experience relying on wage gains at job changes, and low quality workers being more likely to move. The results suggest that the individual effect induces a downward bias in the returns to experience. Thus, the potential underestimation of returns to experience due to low quality workers moving more seems to outweigh the potential overestimation resulting from differences in unemployment histories. Our results show a slightly larger difference between OLS and IV-tenure returns to tenure estimates in Germany than in the UK. They also show a larger difference between IV-tenure and IV-tenure-experience returns to experience estimates in Germany and in the UK. Both differences suggest that the role of unobserved ability on mobility is stronger in Germany. We will discuss possible underlying causes for the differences in the ability bias in the two countries in section 6.

The over all picture given by Table 4-2 A. and B. points to higher returns to experience in the UK than in Germany. For the UK, and according to all of the four estimation methods, the first year in the labour market yields a return of roughly 8 percent, and by the 10th year in the labour market the resulting average cumulative

are insignificantly different from zero with all estimation methods. We have checked that eliminating insignificant terms in tenure and experience does not affect results.

return lies somewhere between 63 and 74 percent. The marginal returns decrease with experience and the following 10 years generate a another 10 to 30 percent points increase. In Germany, the first year in the labour market yields a return in the interval 3.3 to 4.7 percent, the first 10 years yield a cumulative wage gain of roughly 30 to 40 percent, and the first 20 years yield a 35 to 57 percent wage gain.

Though the four estimation methods do not give dramatically different results, the 2 stage method gives higher returns to tenure than the remaining estimators for both countries. One reason could be the underestimate of the experience coefficient in the second step, namely β_1 in equation (4.9), suggested by Topel (1991) due to the possible negative correlation between the individual fixed effect A_i and experience at the start of the job X_{0ij}. This is due to the fact that, controlling for total experience, more able individuals may have lower experience at the start of a job due to longer job durations. Columns (1) and (3) of Table 4-3 A. and B. show that instrumenting initial experience with total experience does not change the results in the 2-stage method[83]. This is consistent with our previous findings suggesting that the potential individual heterogeneity bias in the OLS returns to tenure estimates is not very important.

To further test the role of individual heterogeneity bias in OLS results, we estimate a variation of the instrumental variables estimator in which tenure is instrumented with its deviations from *individual means* only. Column 2 of Table 4-3.B. (coefficients in Table 4-3.A.) shows that in the UK, returns to tenure and experience given by this IV method differ trivially from those given by OLS. In contrast, column 4 shows that with the German data, using as instruments for tenure deviations from its *individual means* gives lower returns to tenure and higher returns to experience than with OLS. In fact, with the German data returns to ten years of tenure fall from 8 percent points with least squares to an insignificantly different from zero 2.2, and returns to ten years of experience increase from 27.8 percent points with least squares to 30.2 percent with IV. Note that for Germany, instrumenting tenure with its deviations from individual means gives virtually the same results as instrumenting tenure with its deviations from job means. This suggests that in Germany it is unobserved individual ability that causes the positive bias in the OLS estimates of returns to tenure, and in the UK it is the job match effect that causes the small positive bias in the OLS estimates of returns to tenure.

Topel (1991) shows that in a linear specification for the tenure and experience variables, the IV-tenure and the 2-stage methods are equivalent to each other. In our results, the 2-stage method gives higher returns to tenure than the remaining estimators, and this difference appears to be larger in the UK than in Germany. We note that Topel showed that the IV-tenure and the 2-stage methods are equivalent only for a *linear specification* of the tenure and experience variables. Since in the 2-stage method all higher order terms of tenure and experience are estimated free from bias in the first stage, and the estimate of the linear term in experience from the second stage is likely to be biased, it is not very clear how that can affect the computation of the full experience and tenure effects from the complete polynomial.

We conclude that 10 years of job seniority generate a wage return of between 4 and 14 percent in the UK, and between zero and 6 percent in Germany, while the returns to 10 years of experience are between 63 and 73 in the UK and between 30 and 40 percent in Germany.

[83] Topel (1991) obtains similar findings for the US.

4.5.2 Cumulative returns to tenure and experience by skill group

Section 4.4 gave evidence that mobility and wage growth differ by skill group. These differences may have an impact on the estimations of the returns to job seniority and general labour market experience. Table 4-4. and Table 5-6 display the returns to tenure and experience by qualification group for the UK and Germany. These results were obtained by interacting qualification dummies for the medium skilled and university graduates with the tenure and experience polynomials. So, for example, results in columns 1,4, and 7 are from a single least squares regression in which the tenure and experience polynomials are interacted with the qualification dummies. Similarly, columns 2, 5 and 8 are from a single IV regression and columns 3, 6 and 9 from an IV regression in which both tenure and experience are instrumented. For the UK, returns to tenure given by least squares are somewhat higher for the high skilled. For example, though returns to 10 years of tenure are 10.6 percent, which is not much larger than 7.4 and 8.7 for the medium and low skilled, returns to 15 years of experience are 18.8 percent for the high skilled, which is clearly higher than 7.3 (medium skilled) and 11.4 (low skilled). In fact, with least squares only the coefficients of the tenure polynomial interacted with the high skilled dummy are significantly different from zero (Table 4-8 in Appendix 4.C). However, similar to the findings for the full sample, once returns to tenure are instrumented with deviations from job means none of the skill groups has returns to tenure significantly different from zero. Returns to experience in the UK seem to rise moderately with education for all estimation methods. For example, with the IV-tenure, returns to 10 years of experience are 60 percent for the unskilled (column 2), 74 percent for workers with medium skills (column 5) and approximately 80 percent for university graduates (column 8). The various estimation methods differ in roughly the same way for each qualification group as for the whole sample of workers.

For Germany the main difference among the three education groups' results is that for workers who went through apprenticeship training (Table 4-5, columns (4) to (6)) returns to experience are substantially lower than for the other two qualification groups, with returns to ten years of experience between 17 and 28 percent, depending on the estimation method. The unskilled have remarkably similar returns to experience to their counterparts in the UK, and the university graduates have between 45.6 and 64.5 percent of cumulative wage returns to ten years of experience.

4.5.3 Negative selection of job movers

Our results so far suggested that job movers are negatively selected in Germany, but not in the UK. As a robustness check, we conduct a similar exercise to Dustmann and Meghir (2001) by looking at how wages vary with the number of jobs held, in order to look for evidence of negative selection of job movers in any of the two countries. Table 4-7.A shows the results for Germany of regressing the log wage on job number dummies, an age polynomial and year dummies. The last two columns show that for the whole sample, wages do not increase with the number of jobs, and if anything they decrease with the number of jobs. In fact most coefficients are negative, though only a few are significantly different from zero at 1 percent significance level. The results for university graduates (columns 5 and 6) and workers with apprenticeship training (columns 3 and 4) do not differ much from the ones for the whole sample. For unskilled workers (columns 1 and 2), however, positive coefficients suggest that the number of jobs are positively associated with higher wages, though most coefficients are not significantly different from zero.

How can wages decrease with the number of jobs conditional on age and aggregate wage growth if we showed in section 4.4 that wage growth between jobs is on average higher than within jobs? One possible explanation would be if individuals who change jobs are of lower unobservable ability. If individuals who have a large number of jobs conditional on age are of lower ability, their wages are likely to be lower than those of "stayers". This would explain why in spite of wage gains at job changes, wages seem to decrease with the number of jobs. One way of testing this is to run the same regressions with individual fixed effects: after controlling for unobservables, wages should increase with the number of jobs. Columns 7 and 8 of Table 4-7.B confirm this hypothesis. Overall, wages increase steadily with the number of jobs (wages at the 8^{th} and subsequent jobs are 20% higher than the wage at the first job). Columns 1 to 6 break the analysis by qualification group. University graduates' wages increase significantly with the job number, unskilled workers wage gains seem substantially higher than the ones given by OLS estimates, and workers with apprenticeship training have now positive wage coefficients, though most are not significantly different from zero. This confirms that our previous finding of negative selection of job movers in the GSOEP[84] applies to all skill groups.

Table 4-6 (parts A and B) shows the results for the United Kingdom which are in sharp contrast with the ones described above. Least squares estimates show evidence of positive wages gains for up to three jobs, with an eventual decline suggested by the negative but not significantly different from zero coefficient for 7

[84] Dustmann and Meghir (2001) obtain similar evidence using the German Social Security records (IAB data) for the years 1975-1995.

and more jobs. The unskilled workers seem to be the ones with positive wages gains, since for the more qualified groups' only the coefficient for job number 2 is positive and significantly different from zero. With fixed effects on the full sample, the signs of job number dummies become negative and insignificantly different from zero. The major difference between fixed effects estimations and OLS is that most wage coefficients for the unskilled become negative and insignificantly different from zero, suggesting positive selection of job movers among the unskilled. We conclude that on the overall there is no evidence of negative selection among job movers in the UK[85] (one could argue that there is at most a very modest suggestion of negative selection among university graduates since wage gains at job changes are somewhat higher with fixed effects than with OLS), and if anything, results point to some positive selection among unskilled job movers.

4.6 Discussion of results

In this section we discuss the various findings of the preceding section. In terms of returns to tenure, our estimates point to low average returns to tenure in both countries. If these estimates are unbiased, this implies that either the component of workers' skills that is not transferable across employers is unimportant, or that workers do not share the returns to this training in the form of increasing wages. Unfortunately, since our estimates could not correct for the potential upward bias in

[85] In Section 4.4.1 we showed that in both countries between jobs wage growth is higher than within jobs wage growth only in the first 10 to 15 years of workers' careers. We therefore also looked separately at the relationship between wages and the number of jobs for the first and latter years of workers' careers. We could find no evidence of negative selection of job movers in the UK in any of the periods.

the experience effect due to its possible positive correlation with accumulation of search capital, our estimates of the tenure effect may still be downward biased.

Average returns to labour market experience are, according to our results, markedly higher in the UK than in Germany. The estimates by skill group suggest that at least some of this difference is likely to be due to higher "entry wages" for workers who have been through apprenticeship training, since this group of workers' returns to experience estimates are substantially lower than the other two. Workers who undergo the apprenticeship training are known to receive general or transferable skills (Acemoglu and Pischke, 1999), and their productivity and corresponding wage may increase less since their first work period after the apprenticeship training, simply because much of the learning is concentrated during the apprenticeship period. In fact, for all other qualification groups in Germany and all qualification groups in the UK, returns to experience have a very steep slope during the first few years. Estimated returns to experience among unskilled workers are similar in the two countries, but returns to experience among university graduates seem to be somewhat higher in the UK than in Germany.

By exploring the differences between OLS and IV results, we concluded from our results that "more able" workers have longer job durations in Germany, but not in the UK. We discuss two models that could explain why "more able" workers experience longer job tenures, or, what is the same, why "less able" workers are laid off more often or have more incentives to quit.

A possible source of negative selection of job movers consistent with our wage model is the presence of "sticky wages". In a context of learning with sticky wages, where in a first stage the employer's information about workers' ability is imperfect, sticky wages may imply that the firm is forced to layoff workers' whose productivity turns out to be "too low". An assumption behind this model is that both current employers and prospective employers learn about the worker's ability after a job spell. This is only plausible if prospective employers have access to direct information from the previous/current employer.

It is reasonable to accept that individual real wages of German workers may be stickier than those of British workers. Although nominal wage rigidity probably holds in the two countries, British employers may have more discretion with respect to individual wage increases and promotions than German employers who face wage tariffs for different occupations and industries which are often renegotiated on a yearly basis.

An alternative possible source of negative selection of job movers is asymmetric information between the current and prospective employers about workers' ability, as mentioned in section 4.2.1 (see Schönberg (2002) for a formal derivation of why asymmetric information leads an ability bias in the estimation of returns to job tenure). In a context of asymmetric information, adverse selection is less important the higher the job mobility (Acemoglu and Pischke, 1998). The higher the job mobility, the lower the expected difference in the average ability of job stayers and job movers. Since job mobility is higher in the UK than in Germany, if negative selection is due to asymmetric information, it should be stronger in Germany than in the UK.

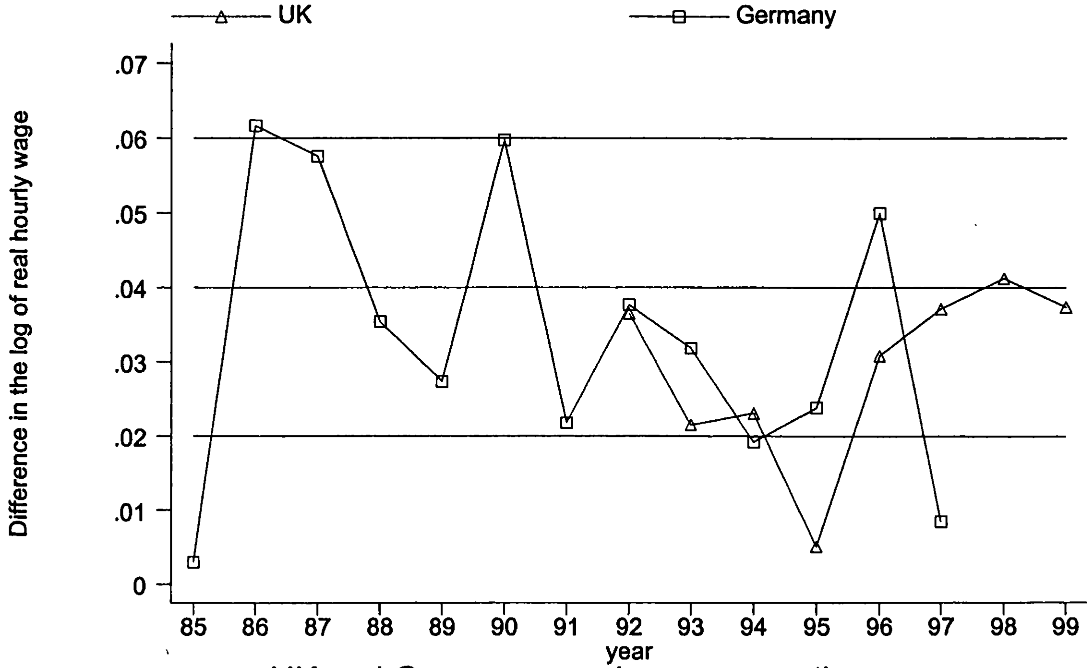
4.7 Conclusions

In this study we compare the returns to tenure and experience in the UK and Germany using least squares, instrumental variables estimations and the Topel (1991) 2-stage method. Our results show that returns to tenure are low in both countries. Returns to experience are higher in the UK than in Germany. We estimate that 10 years of job seniority generate a wage return of between 4 and 14 percent in the UK, and between zero and 6 percent in Germany. Returns to 10 years of experience are between 63 an 73 percent in the UK and between 30 and 40 percent in Germany.

We also estimate separate regressions for three different qualification groups. We find that workers who went through the apprenticeship training system in Germany have substantially lower returns to labour market experience than all other groups. This suggests that a large part of difference between the two countries' returns to experience can be attributed to the apprenticeship training. Workers with these qualifications probably receive a relatively high entry wage in their first employment spell after apprenticeship, to reflect the productivity gains associated with the acquisition of skills during the apprenticeship period.

The differences between OLS and IV estimates show some evidence of stronger heterogeneity biases in Germany than in the UK. These are interpreted as being suggestive of negative selection of job movers in terms of unobserved ability in Germany. Finally, we point out that the institutional differences between the two countries may be the source behind the differences in the selection of jobs movers in Germany. It can both be driven by wage tariffs in Germany (in models of imperfect information about the workers' ability with "sticky wages") or by stronger adverse selection of job movers in Germany induced by the lower job mobility in a context of asymmetric information between current and prospective employers about workers' ability.

Figure 4-1



UK and Germany: yearly wage growth

Figure 4-2

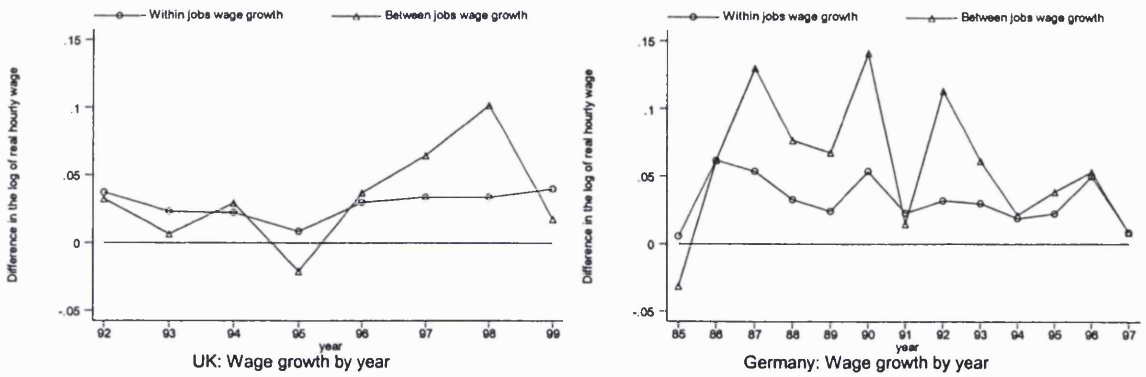


Figure 4-3

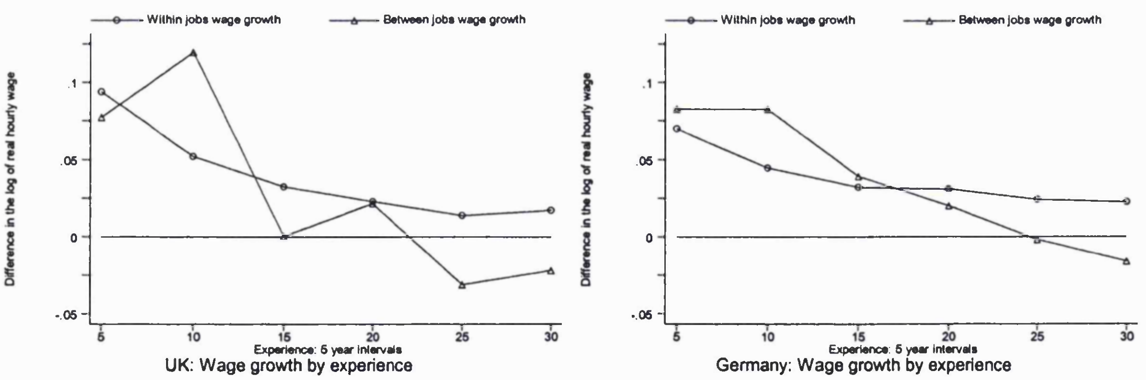


Figure 4-4

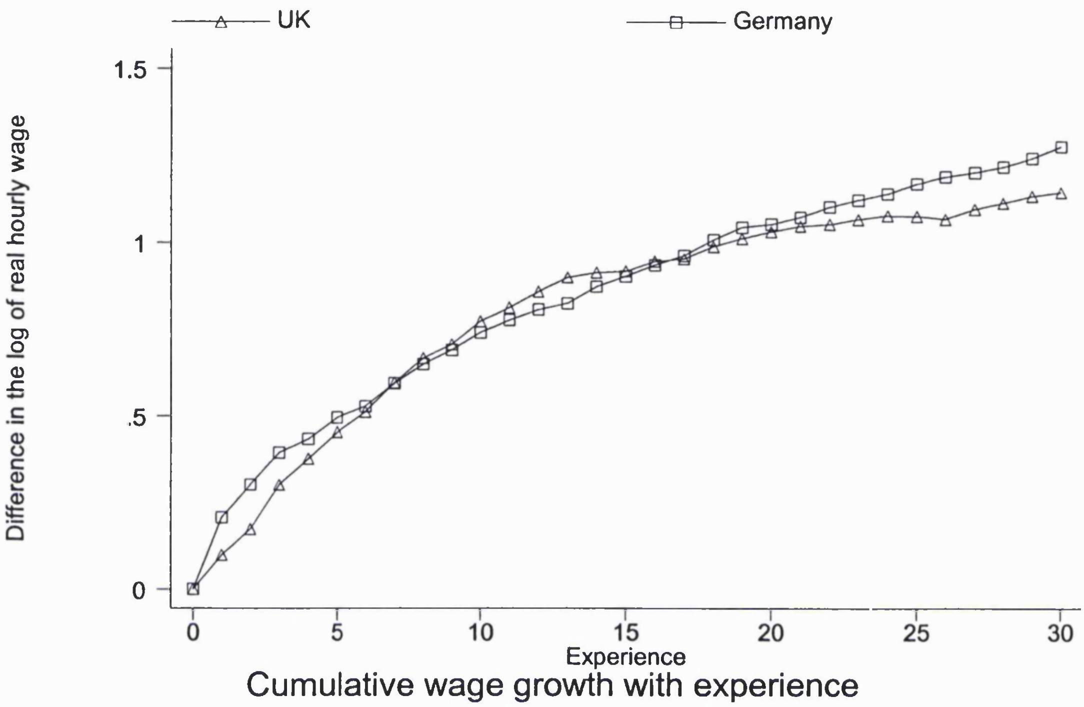


Figure 4-5

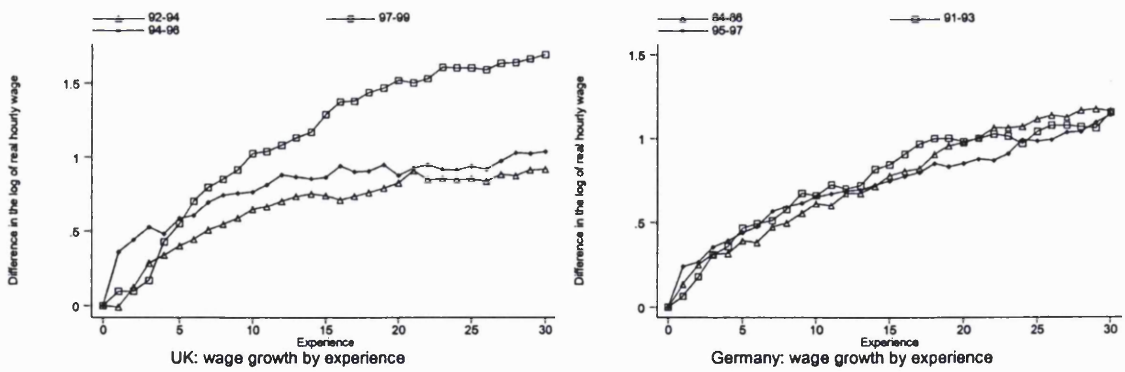


Figure 4-6

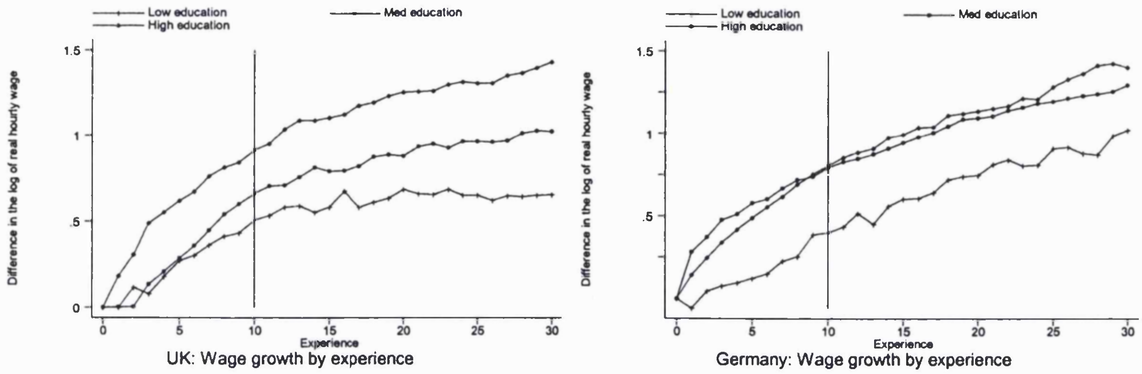


Figure 4-7

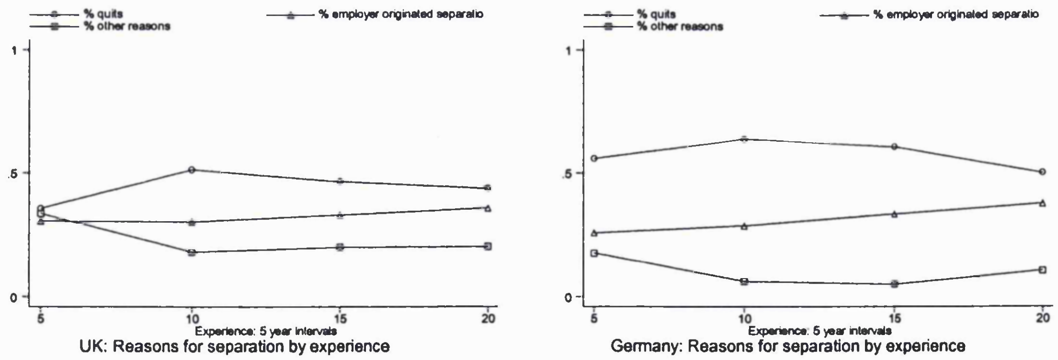
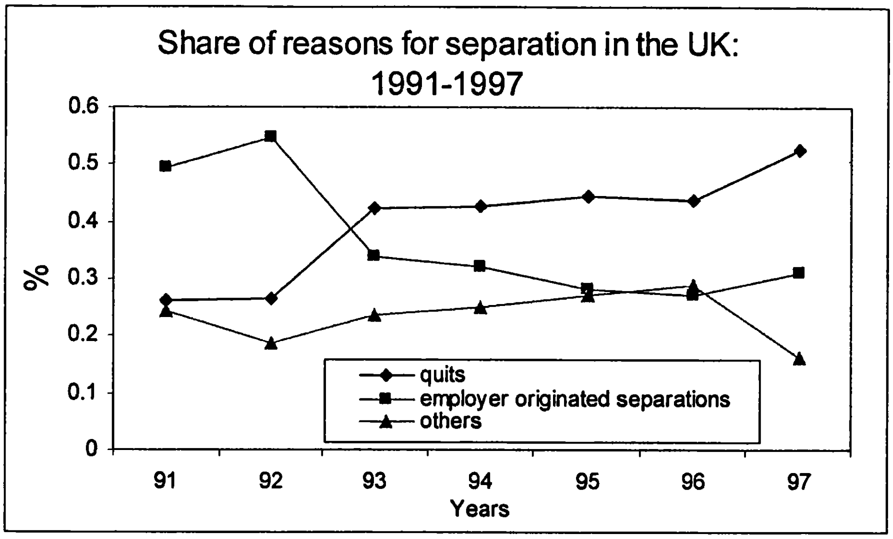
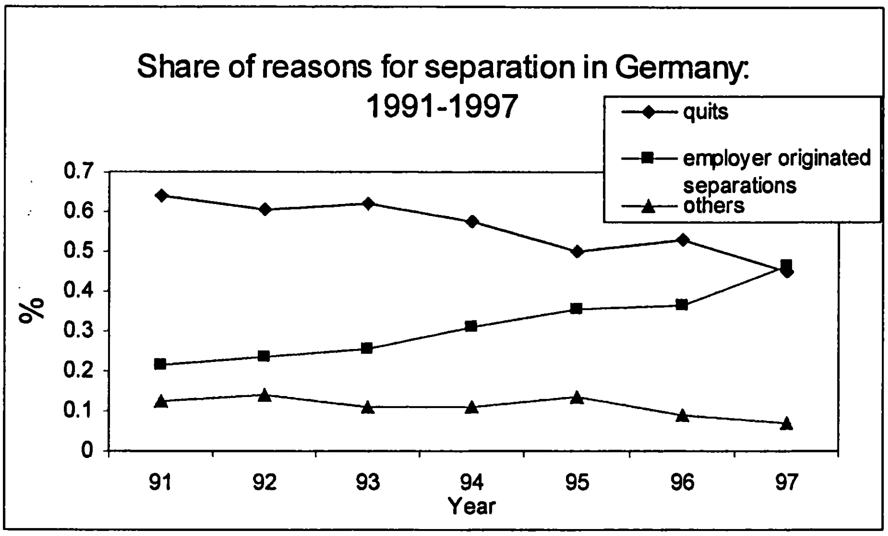


Figure 4-8


Share of reasons for separation in the UK: 1991-1997
0.6
0.5
0.4
%
0.3
0.2
0.1
0
quits
employer originated separations
others
91
92
93
94
95
96
97
Years


Share of reasons for separation in Germany: 1991-1997
quits
employer originated separations
others
0.7
0.6
0.5
0.4
%
0.3
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91
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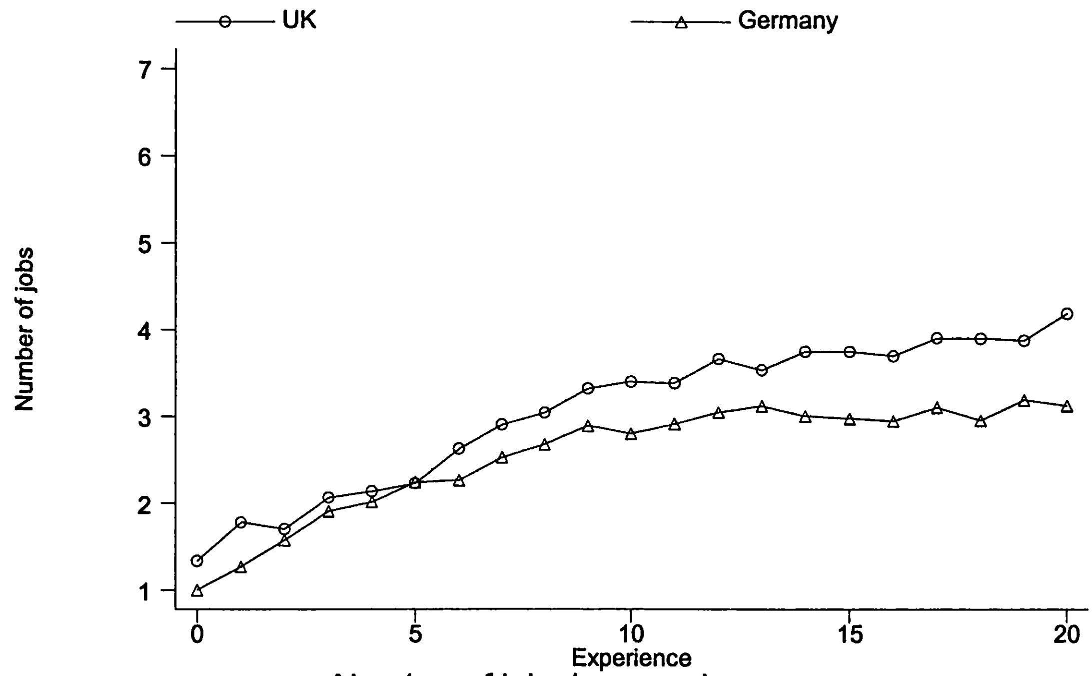
94
95
96
97
Year

Figure 4-9



Number of jobs by experience

Appendix 4.B Tables

Table 4-2: Returns to tenure and experience - OLS, IV and 2-Stage method

A. Coefficients

	UK				Germany			
	OLS	Ten IV	Ten IV Exp IV	Topel 2-stage	OLS	Ten IV	Ten IV Exp IV	Topel 2-stage
Ten.+Exp.	-	-	-	0.1023	-	-	-	0.0511
				(0.0156)**				(0.0094)**
Tenure	0.0116	0.0142	0.0150	0.0217	0.0136	0.0038	-0.0013	0.0060
	(0.0053)*	(0.0104)	(0.0102)	-	(0.0034)**	(0.0051)	(0.0051)	-
Tenure ^{2}x10	-0.0040	-0.0129	-0.0164	-0.0126	-0.0094	-0.0030	-0.0019	0.0014
	(0.0065)	(0.0140)	(0.0141)	(0.0124)	(0.0042)*	(0.0062)	(0.0061)	(0.0084)
Tenure ^{3}x100	0.0014	0.0054	0.0066	0.0044	0.0040	0.0017	0.0012	-0.0012
	(0.0028)	(0.0065)	(0.0065)	(0.0060)	(0.0018)*	(0.0026)	(0.0026)	(0.0038)
Tenure ^{4}x1000	-0.0002	-0.0007	-0.0008	-0.0006	-0.0005	-0.0002	-0.0001	0.0003
	(0.0004)	(0.006)	(0.006)	(0.0009)	(0.0003)*	(0.0004)	(0.0004)	(0.0005)
Experience	0.0885	0.0904	0.0856	0.0807	0.0345	0.0369	0.0472	0.0451
	(0.0062)**	(0.0071)**	(0.0132)**	(0.0005)**	(0.0039)**	(0.0044)**	(0.0063)**	(0.0003)
Experience^{2}x10	-0.0452	-0.0454	-0.0374	-0.0388	-0.0110	-0.0105	-0.0160	-0.0194
	(0.0050)**	(0.0056)**	(0.0111)**	(0.0136)**	(0.0032)**	(0.0036)**	(0.0052)**	(0.0093)*
Experience^{3}x100	0.0096	0.0097	0.0075	0.0077	0.0005	0.0001	0.0020	0.0031
	(0.0015)**	(0.0017)**	(0.0036)*	(0.0044)	(0.001)	(0.0011)	(0.0017)	(0.0031)
Experience^{4}x1000	0.0008	-0.0008	-0.0006	-0.0006	0.0001	0.0001	0.0001	-0.0002
	(0.0002)**	(0.0002)**	(0.0004)	(0.0005)	(0.0001)	(0.0001)	(0.0002)	(0.0003)
P-value (tenure)	24.85	0.83	0.71	-	58.44	1.53	1.14	-
P-value (exp.)	143.25	89.01	23.56	-	104.59	73.10	46.60	-
N. obsv. (1st)	7073	7073	7073	4572	12302	12302	12302	8818
N. obsv. (2nd)	-	-	-	7073	-	-	-	12302
R2 (1st step)	0.3012	0.2985	0.2880	0.0226	0.4004	0.3979	0.3864	0.0089
R2 (2nd step)	-	-	-	0.8494	-	-	-	0.6594

Note: All regressions include marital status, 2 qualification dummies and year dummies. Standard errors in parenthesis.
**- Significant at 1 percent level; *- Significant at 5 percent level.

B.: Cumulative returns to tenure and experience - OLS, IV and 2-Stage estimations

	UK				Germany			
	OLS	Ten IV	Ten IV Exp IV	Topel 2-stage	OLS	Ten IV	Ten IV Exp IV	Topel 2-stage
1 year ten	0.0113	0.0131	0.0135	0.0207	0.0128	0.0035	-0.0015	0.0061
	(0.0047)	(0.0093)	(0.0091)	-	(0.0031)	(0.0046)	(0.0046)	-
5 years ten	0.0508	0.0461	0.0425	0.0856	0.0506	0.0134	-0.0097	0.0324
	(0.0149)	(0.0290)	(0.0269)	-	(0.0098)	(0.0157)	(0.0149)	-
10 years ten	0.0911	0.0611	0.0442	0.1378	0.0803	0.0224	-0.0211	0.0659
	(0.0168)	(0.0371)	(0.0297)	-	(0.0114)	(0.0228)	(0.0196)	-
15 years ten	0.1253	0.0683	0.0358	0.1730	0.1067	0.0352	-0.0280	0.0973
	(0.0166)	(0.0485)	(0.0347)	-	(0.0118)	(0.0311)	(0.0239)	-
1 year exp	0.0877	0.0898	0.0854	0.0799	0.0340	0.0366	0.0467	0.0442
	(0.0063)	(0.0072)	(0.0132)	-	(0.0038)	(0.0042)	(0.0061)	-
5 years exp	0.4065	0.4192	0.4101	0.3711	0.1566	0.1721	0.2196	0.1983
	(0.0296)	(0.0344)	(0.0629)	-	(0.0152)	(0.0173)	(0.0259)	-
10 years exp	0.6856	0.7141	0.7353	0.6327	0.2718	0.3067	0.3932	0.3314
	(0.0473)	(0.0571)	(0.1076)	-	(0.0221)	(0.0266)	(0.0408)	-
15 years exp	0.8191	0.8623	0.9449	0.7670	0.3374	0.3913	0.5107	0.3984
	(0.0519)	(0.0664)	(0.1348)	-	(0.0237)	(0.0317)	(0.0484)	-
20 years exp	0.8472	0.9020	1.0530	0.8032	0.3553	0.4238	0.5738	0.4119
	(0.0500)	(0.0697)	(0.1541)	-	(0.0230)	(0.0364)	(0.0527)	-

Note: *Log-wage returns* to *k* years of tenure (experience) with *k=1,5,10,15,20* is the cross-product of the row vector of the tenure (experience) polynomial coefficients with a column vector of the form (k, k^2, k^3, k^4). Values presented are the *wage returns* and are obtained by applying an exponential transformation to the log wage returns minus 1. Standard errors are the square root of a 1st order Taylor approximation of the corresponding variance.

Table 4-3: Returns to tenure and experience - other specifications

A. Coefficients

	UK		Germany	
	2-stage diffs IV exp0 w/ exp	IV-tenure w/ dev. ind. means	2-stage diffs IV exp0 w/ exp	IV-tenure w/ dev. ind.means
Ten.+Exper.	0.1023	-	0.0511	-
	(0.0156)**		(0.0094)**	
Tenure	0.0223	0.0135	0.0064	0.0061
	-	(0.0083)	-	(0.0045)
Tenure ^{2}x10	-0.0126	-0.0083	0.0014	-0.0064
	(0.0124)	(0.0120)	(0.0084)	(0.0057)
Tenure ^{3}x100	0.0044	0.0040	-0.0012	0.0028
	(0.0060)	(0.0058)	(0.0038)	(0.0025)
Tenure ^{4}x1000	-0.0006	-0.0006	0.0003	-0.0003
	(0.0090)	(0.0009)	(0.0005)	(0.0003)
Exper.	0.0800	0.0886	0.0447	0.0357
	(0.0006)**	(0.0066)**	(0.0006)**	(0.0043)**
Exper.^{2}x10	-0.0388	-0.0450	-0.0194	-0.0093
	(0.0136)**	(0.0053)**	(0.0093)*	(0.0035)**
Exper^{3}x100	0.0077	0.0095	0.0031	-0.0001
	(0.0044)	(0.0016)**	(0.0031)	(0.0011)
Exper^{4}x1000	-0.0006	-0.0007	-0.0002	0.0020
	(0.0005)	(0.0002)**	(0.0003)	(0.0001)
P-value (ten.)	-	0.0001	-	0.0975
P-value (exp.)	-	0.0000	-	0.0000
N. obsv. (1st)	4572	7073	8818	12302
N. obsv. (2nd)	7073	-	12302	-
R2 (1st step)	0.0226	0.3008	0.0089	0.3966
R2 (2nd step)	0.8493	-	0.6593	-

Note: All regressions include marital status, 2 qualification dummies and year dummies. Standard errors in parenthesis. **- Significant at 1 percent level; *- Significant at 5 percent level.

B. Returns to tenure and experience - other specifications

	UK		Germany	
	2 stage diffs IV exp0 w/ exp	IV-tenure w/ dev. ind.means	2 stage diffs IV exp0 w/ exp	IV-tenure w/ dev. ind.means
1 year ten	0.0213	0.0128	0.0066	0.0055
		(0.0073)		(0.0041)
5 years ten	0.0891	0.0525	0.0348	0.0179
		(0.0217)		(0.0131)
10 years ten	0.1450	0.0888	0.0709	0.0222
		(0.0252)		(0.0172)
15 years ten	0.1841	0.1244	0.1050	0.0268
		(0.0301)		(0.0214)
1 year exp	0.0792	0.0879	0.0437	0.0354
		(0.0067)		(0.0041)
5 years exp	0.3668	0.4079	0.1955	0.1678
		(0.0314)		(0.0167)
10 years exp	0.6226	0.6889	0.3252	0.3022
		(0.0505)		(0.0249)
15 years exp	0.7505	0.8230	0.3887	0.3901
		(0.0562)		(0.0281)
20 years exp	0.7808	0.8496	0.3988	0.4279
		(0.0555)		(0.0298)

Note: *Log-wage returns* to k years of tenure (experience) with $k=1,5,10,15,20$ is the cross-product of the row vector of the tenure (experience) polynomial coefficients with a column vector of the form (k, k^2, k^3, k^4). Values presented are the *wage returns* and are obtained by applying an exponential transformation to the log wage returns minus 1. Standard errors are the square root of a 1st order Taylor approximation of the corresponding variance.

Table 4-4: Cumulative returns to tenure and experience by qualification – UK

	Unskilled			Medium qualified			University graduates		
	OLS	IV tenure	IV-tenure IV-exp w/ dev. ind. m.	OLS	IV tenure	IV-tenure IV-exp w/ dev. ind. m.	OLS	IV tenure	IV-tenure IV-exp w/ dev. ind. m.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1 year ten	0.0135	0.0212	0.0190	0.0228	0.0076	0.0073	-0.0054	0.0082	0.0107
	(0.0090)	(0.0205)	(0.0186)	(0.0083)**	(0.0159)	(0.0150)	(0.0084)	(0.0161)	(0.0150)
10 years ten	0.0870	0.0691	0.0398	0.0742	0.0042	-0.0172	0.1055	0.0818	0.0885
	(0.0341)*	(0.0936)	(0.0621)	(0.0268)**	(0.0672)	(0.0490)	(0.0273)**	(0.0655)	(0.0471)
15 years ten	0.1140	0.0438	-0.0011	0.0728	-0.0043	-0.0480	0.1880	0.1114	0.1095
	(0.0326)**	(0.1181)	(0.0724)	(0.0260)**	(0.0929)	(0.0596)	(0.0283)**	(0.0826)	(0.0535)
1 year exp	0.0787	0.0791	0.0775	0.0809	0.0819	0.0679	0.1164	0.1150	0.1068
	(0.0167)**	(0.0193)**	(0.0336)*	(0.0094)**	(0.0117)**	(0.0181)**	(0.0120)**	(0.0136)**	(0.0268)**
10 years exp	0.5794	0.6133	0.6712	0.7069	0.7419	0.6681	0.7808	0.7951	0.7195
	(0.1176)**	(0.1507)**	(0.2593)*	(0.0701)**	(0.0948)**	(0.1381)**	(0.0832)**	(0.1000)**	(0.2077)**
20 years exp	0.6964	0.7834	1.0033	1.0283	1.1043	1.1154	0.7702	0.8234	0.7722
	(0.1185)**	(0.1873)**	(0.3271)**	(0.0772)**	(0.1375)**	(0.2004)**	(0.0742)**	(0.1031)**	(0.2507)**
N. obsv. (1st)	7073	7073	7073	7073	7073	7073	7073	7073	7073
R2 (1st step)	0.3136	0.2903	0.279	0.3136	0.2903	0.279	0.3136	0.2903	0.279

Note: *Log-wage returns* to *k* years of tenure (experience) with *k=1,5,10,15,20* is the cross-product of the row vector of the tenure (experience) polynomial coefficients with a column vector of the form (k, k^2, k^3, k^4). Values presented are the *wage returns* and are obtained by applying an exponential transformation to the log wage returns minus 1. Standard errors are the square root of a 1st order Taylor approximation of the corresponding variance. Results in columns (1), (4) and (7) are obtained by running a least squares regression on the tenure and experience polynomials, interacted with dummies for medium and high skilled workers. Similarly, columns (2), (5) and (8) are obtained by running an IV regression with instruments for tenure and columns (3), (6) and (9) by running an IV regression with instruments for tenure and experience. All regressions include marital status, 2 qualification dummies and year dummies. **- Significant at 1 percent level; *- Significant at 5 percent level.

Table 4-5: Cumulative returns to tenure and experience by qualification – Germany

	Unskilled			Apprenticeship training			University graduates		
	OLS	IV tenure	IV-tenure IV-exp w/ dev. ind. m.	OLS	IV tenure	IV-tenure IV-exp w/ dev. ind. m.	OLS	IV tenure	IV-tenure IV-exp w/ dev. ind. m.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1 year ten	-0.0107	-0.0204	-0.0247	0.0140	0.0088	0.0043	0.0211	-0.0154	-0.0174
	(0.0082)	(0.0148)	(0.0137)	(0.0036)**	(0.0055)	(0.0053)	(0.0097)*	(0.0147)	(0.0142)
10 years ten	-0.0263	-0.0446	-0.1199	0.0939	0.0353	-0.0022	0.1032	0.0036	-0.0329
	(0.0313)	(0.0828)	(0.0548)*	(0.0133)**	(0.0276)	(0.0227)	(0.0320)**	(0.0837)	(0.0555)
15 years ten	0.0088	-0.0090	-0.1279	0.1216	0.0423	-0.0108	0.1132	0.0465	-0.0268
	(0.0340)	(0.1118)	(0.0649)*	(0.0135)**	(0.0375)	(0.0275)	(0.0353)**	(0.1465)	(0.0737)
1 year exp	0.0989	0.1030	0.0901	0.0174	0.0190	0.0312	0.0586	0.0781	0.0859
	(0.0111)**	(0.0131)**	(0.0184)**	(0.0045)**	(0.0049)**	(0.0071)**	(0.0137)**	(0.0169)**	(0.0211)**
10 years exp	0.6024	0.6281	0.6838	0.1694	0.1978	0.2846	0.4557	0.5803	0.6451
	(0.0692)**	(0.0984)**	(0.1231)**	(0.0250)**	(0.0298)**	(0.0457)**	(0.0664)**	(0.0971)**	(0.1289)**
20 years exp	0.5505	0.5870	0.8509	0.2506	0.3121	0.4452	0.6228	0.7544	0.8925
	(0.0652)**	(0.1496)**	(0.1499)**	(0.0258)**	(0.0402)**	(0.0593)**	(0.0680)**	(0.2075)**	(0.1833)**
N. obsv. (1st)	12302	12302	12302	12302	12302	12302	12302	12302	12302
R2 (1st step)	0.4063	0.4027	0.3904	0.4063	0.4027	0.3904	0.4063	0.4027	0.3904

Note: *Log-wage returns* to k years of tenure (experience) with $k=1,5,10,15,20$ is the cross-product of the row vector of the tenure (experience) polynomial coefficients with a column vector of the form (k, k^2, k^3, k^4). Values presented are the *wage returns* and are obtained by applying an exponential transformation to the log wage returns minus 1. Standard errors are the square root of a 1st order Taylor approximation of the corresponding variance. Results in columns (1), (4) and (7) are obtained by running a least squares regression on the tenure and experience polynomials, interacted with dummies for medium and high skilled workers. Similarly, columns (2), (5) and (8) are obtained by running an IV regression with instruments for tenure and columns (3), (6) and (9) by running an IV regression with instruments for tenure and experience. All regressions include marital status, 2 qualification dummies and year dummies. **- Significant at 1 percent level; *- Significant at 5 percent level.

Table 4-6: Wages and job number in the United Kingdom

A. OLS

Job Number	Unskilled		Medium Educated		Univ. Graduates		Total	
	Coef.	t-ratio	Coef.	t-ratio	Coef.	t-ratio	Coef.	t-ratio
2	0.122	4.03	0.046	2.14	0.063	2.14	0.074	4.46
3	0.108	3.21	0.033	1.40	0.018	0.67	0.079	4.53
4	0.050	1.52	0.033	1.50	0.055	1.51	0.049	2.65
5	0.081	2.42	-0.025	-1.08	0.001	-0.27	0.006	0.13
6	0.091	2.17	0.062	1.85	-0.114	-2.85	0.019	1.06
7+	0.058	1.95	0.005	0.19	-0.163	-5.06	-0.043	-2.44
N. Observ.	1818		2495		2403		6716	

Note: Coefficients shown for the regression of log-wages on job number dummies. Separate regressions for each education group. The regression for the total of workers includes medium and high education dummies. All regressions include third order age polynomial and year dummies. Job one dummy and year one dummy are omitted. For comparison with the German data in these regressions we restrict the data to workers who had no more than 10 years of labour market experience at the time of the first wave.

B. Fixed Effects

Job Number	Unskilled		Medium Educated		Univ. Graduates		Total	
	Coef.	t-ratio	Coef.	t-ratio	Coef.	t-ratio	Coef.	t-ratio
2	-0.070	-1.73	-0.013	-0.50	0.109	3.15	-0.008	-0.52
3	-0.102	-1.93	0.005	0.19	0.136	2.99	0.000	-0.04
4	-0.015	-0.21	0.018	0.29	0.096	1.78	-0.007	-0.35
5	0.041	0.36	-0.011	-0.13	0.019	0.13	-0.039	-1.43
6	-0.017	-0.61	0.079	1.43	0.039	0.66	-0.024	-0.72
7+	0.129	1.34	-0.038	-0.18	0.103	0.33	-0.022	-1.20
N. Observ.	1818		2495		2403		6716	

Table 4-7: Wages and job number in Germany

A. OLS

Job Number	Low Education		Medium Education		High education		Total	
	Coefficient	t-ratio	Coefficient	t-ratio	Coefficient	t-ratio	Coefficient	t-ratio
2	0.075	2.130	-0.037	-2.460	-0.017	-0.680	-0.031	-2.220
3	0.138	3.350	-0.028	-1.620	-0.045	-1.260	-0.045	-2.710
4	0.160	2.270	0.008	0.380	-0.001	-0.010	-0.006	-0.280
5	0.069	0.940	-0.044	-1.650	-0.012	-0.280	-0.017	-0.690
6	0.265	1.950	-0.038	-1.030	0.299	3.590	0.005	0.130
7	0.254	0.770	-0.146	-2.860	-0.034	-0.380	-0.105	-2.020
8+	-0.032	-0.390	-0.170	-4.430	0.028	0.400	-0.167	-4.640
N. Observ.	550		2430		760		3740	

Note: Coefficients shown for the regression of log-wages on job number dummies. Separate regressions for each education group. The regression for the total of workers includes medium and high education dummies. All regressions include third order age polynomial and year dummies. Job one dummy and year one dummy are omitted. The sample used is smaller because number of jobs can only be computed for workers who entered the labour market not more than 10 years before the first wave.

B. Fixed Effects

Job Number	Low Education		Medium Education		High education		Total	
	Coefficient	t-ratio	Coefficient	t-ratio	Coefficient	t-ratio	Coefficient	t-ratio
2	0.146	3.310	0.033	1.350	0.071	2.540	0.100	5.720
3	0.152	2.750	0.033	1.110	0.094	2.630	0.106	4.800
4	0.303	3.730	0.017	0.470	0.240	5.240	0.120	4.370
5	0.497	4.530	0.078	1.820	0.247	4.740	0.187	5.570
6	0.622	3.610	0.033	0.600	0.305	3.900	0.157	3.500
7	0.802	2.390	0.040	0.540	0.210	2.230	0.155	2.510
8+	(dropped)		0.109	1.430	(dropped)		0.202	2.950
N. Observ.	550		2430		760		3740	

Note: Coefficients shown for the regression of log-wages on job number dummies. Separate regressions for each education group. The regression for the total of workers includes medium and high education dummies. All regressions include third order age polynomial and year dummies. Job one dummy and year one dummy are omitted. The sample used is smaller because number of jobs can only be computed for workers who entered the labour market not more than 10 years before the first wave.

Appendix 4.C Coefficients of regressions by qualification

Table 4-8: Coefficients of tenure and experience by qualification – UK

	OLS	IV-tenure	IV-tenure IV-exp w/ dev. ind. m.
Ten	0.0143	0.0234	0.0214
	(0.0099)	(0.0225)	(0.0207)
Ten2x10	-0.0092	-0.0250	-0.0260
	(0.0113)	(0.0282)	(0.0274)
Ten3x100	0.0037	0.0098	0.0100
	(0.0045)	(0.0126)	(0.0122)
Ten4x1000	-0.0005	-0.0015	-0.0015
	(0.0006)	(0.0018)	(0.0017)
TenxEd2	0.0111	-0.0145	-0.0124
	(0.0136)	(0.0291)	(0.0267)
Ten2xEd2x10	-0.0213	0.0105	0.0089
	(0.0169)	(0.0374)	(0.0365)
Ten3xEd2x100	0.0107	-0.0030	-0.0027
	(0.0074)	(0.0172)	(0.0167)
Ten4xEd2x1000	-0.0017	0.0007	0.0005
	(0.0010)	(0.0025)	(0.0024)
TenxEd3	-0.0228	-0.0153	-0.0107
	(0.0138)	(0.0293)	(0.0267)
Ten2xEd3x10	0.0415	0.0267	0.0253
	(0.0173)**	(0.0373)	(0.0366)
Ten3xEd3x100	-0.0199	-0.0124	-0.0122
	(0.0077)**	(0.0173)	(0.0170)
Ten4xEd3x1000	0.0028	0.0022	0.0021
	(0.0011)**	(0.0026)	(0.0025)
Exp	0.0800	0.0801	0.0778
	(0.0168)**	(0.0192)**	(0.0336)**
Exp2x10	-0.0436	-0.0402	-0.0323
	(0.0133)**	(0.0148)**	(0.0270)
Exp3x100	0.0101	0.0086	0.0063
	(0.0041)**	(0.0046)	(0.0085)
Exp4x1000	-0.0009	-0.0006	-0.0005
	(0.0004)**	(0.0005)	(0.0009)
ExpxEd2	0.0010	0.0016	-0.0104
	(0.0191)	(0.0226)	(0.0384)
Exp2xEd2x10	0.0103	0.0094	0.0151
	(0.0153)	(0.0177)	(0.0313)
Exp3xEd2x100	-0.0041	-0.0037	-0.0055
	(0.0047)	(0.0056)	(0.0099)
Exp4xEd2x1000	0.0004	0.0003	0.0006
	(0.0005)	(0.0006)	(0.0011)
ExpxEd3	0.0378	0.0362	0.0308
	(0.0204)	(0.0235)	(0.0428)
Exp2xEd3x10	-0.0364	-0.0364	-0.0408
	(0.0170)**	(0.0190)	(0.0350)
Exp3xEd3x100	0.0119	0.0123	0.0145
	(0.0055)**	(0.0062)**	(0.0113)
Exp4xEd3x1000	-0.0013	-0.0014	-0.0017
	(0.0006)**	(0.0007)**	(0.0012)
Number of observ.	7073	7073	7073
R-squared	0.3136	0.2903	0.279

Note: All regressions include marital status, 2 qualification dummies and year dummies. Ed2 is a dummy variable equal to 1 for the medium qualified, and Ed3 is equal to 1 for the highly qualified. (definitions in data appendix).Standard errors in parenthesis. **- Significant at 1 percent level; *- Significant at 5 percent level.

Table 4-9: Coefficients of tenure and experience by qualification – Germany

	OLS	IV-tenure	IV-tenure IV-exp w/ dev. ind. m.
Ten	-0.0119	-0.0233	-0.0269
	(0.0093)	(0.0165)	(0.0155)
Ten2x10	0.0110	0.0279	0.0198
	(0.0108)	(0.0167)	(0.0169)
Ten3x100	-0.0018	-0.0105	-0.0064
	(0.0044)	(0.0067)	(0.0067)
Ten4x1000	0.0000	0.0013	0.0007
	(0.0006)	(0.0009)	(0.0009)
TenxEd2	0.0266	0.0330	0.0321
	(0.0101)**	(0.0177)	(0.0166)
Ten2xEd2x10	-0.0193	-0.0382	-0.0288
	(0.0119)	(0.0183)**	(0.0184)
Ten3xEd2x100	0.0046	0.0151	0.0105
	(0.0050)	(0.0074)**	(0.0074)
Ten4xEd2x1000	-0.0003	-0.0019	-0.0012
	(0.0007)	(0.0010)	(0.0010)
TenxEd3	0.0347	0.0048	0.0066
	(0.0144)**	(0.0237)	(0.0226)
Ten2xEd3x10	-0.0307	0.0028	0.0093
	(0.0200)	(0.0295)	(0.0297)
Ten3xEd3x100	0.0095	-0.0033	-0.0077
	(0.0099)	(0.0153)	(0.0150)
Ten4xEd3x1000	-0.0010	0.0006	0.0012
	(0.0016)	(0.0026)	(0.0025)
Exp	0.1013	0.1054	0.0912
	(0.0111)**	(0.0130)**	(0.0184)**
Exp2x10	-0.0727	-0.0768	-0.0501
	(0.0106)**	(0.0133)**	(0.0174)**
Exp3x100	0.0206	0.0223	0.0121
	(0.0037)**	(0.0046)**	(0.0058)**
Exp4x1000	-0.0020	-0.0023	-0.0011
	(0.0004)**	(0.0005)**	(0.0006)
ExpxEd2	-0.0841	-0.0868	-0.0599
	(0.0120)**	(0.0140)**	(0.0198)**
Exp2xEd2x10	0.0734	0.0790	0.0443
	(0.0112)**	(0.0139)**	(0.0184)**
Exp3xEd2x100	-0.0232	-0.0256	-0.0127
	(0.0039)**	(0.0048)**	(0.0061)**
Exp4xEd2x1000	0.0024	0.0027	0.0012
	(0.0004)**	(0.0005)**	(0.0007)
ExpxEd3	-0.0417	-0.0259	-0.0039
	(0.0182)**	(0.0218)	(0.0282)
Exp2xEd3x10	0.0451	0.0324	0.0000
	(0.0197)**	(0.0248)	(0.0296)
Exp3xEd3x100	-0.0144	-0.0105	0.0019
	(0.0080)	(0.0115)	(0.0120)
Exp4xEd3x1000	0.0014	0.0010	-0.0003
	(0.0011)	(0.0017)	(0.0016)
Number of observ.	12302	12302	12302
R-squared	0.4063	0.4027	0.3904

Note: All regressions include marital status, 2 qualification dummies and year dummies. Ed2 is a dummy variable equal to 1 for the medium qualified, and Ed3 is equal to 1 for the highly qualified. (definitions in data appendix).Standard errors in parenthesis. **- Significant at 1 percent level; *- Significant at 5 percent level.

Appendix 4.D Data Notes

British Household Panel Survey

Tenure: is the total number of years in which the individual works for the same employer. It is constructed for all individuals that are in paid employment. It is not constructed for self-employed, since these are excluded from the sample. Individuals are asked to give the starting date of the job spell, and not the spell with employer[86]. For example, if the individual is promoted, the date collected is the date of promotion. In order to track down the starting date with the present employer, we go back as many spells as there are jobs changes with the same employer, which involves using the information of the inter-wave history files and the retrospective data in many instances. We therefore add the time spent in the various spells within the same employer in order to compute tenure with the employer. When linking the job spell information in the various yearly questionnaires and the retrospective data collected in waves 2 and 3 one is confronted with the overlapping of more than one source of information for the same spell, or part of it. Conflicting answers are resolved by giving priority to the information collected closest to the event occurrence. This is because recall error is likely to increase with the time elapsed between an event and the time of interview. In addition, in some cases in two

[86] Question Text: "What was the date you started working in your present position? If you have been promoted or changed grades, please give me the date of that change. Otherwise please give me the date when you started doing the job you are doing now for your present employer."

consecutive waves although the job starting date given in the later wave takes place before the previous wave interview, the discrepancy between the two start dates makes it clear that they refer to two different job spells. We therefore also adopted the following rule: if the starting date of a given spell occurs just before the previous wave interview date (i.e., during the previous year) and it is more than 1 year apart from the starting date recorded in the previous interview, then it is assumed that this spell started just after the previous wave interview.

Experience: sums the individual's time spent in paid employment or as a self-employed since leaving full time education. Similar to the tenure variable, it combines the information from the various yearly questionnaires and the retrospective data collected in waves 2 and 3.

Skills: The skill variable is constructed from the information on the individuals' highest educational qualification. We classified workers into three skill groups as follows. *Unskilled:* No qualifications, other qualifications, apprenticeship, CSE, commercial qualifications, no O levels. *Medium skilled:* O levels or equivalent, nursing qualifications, teaching qualifications, A levels. *University Graduates:* Higher degree, 1st Degree, other higher.

Reason for separation: The information on the reasons for job separations is collected every year and refers to the jobs over the period from 1st

September of the preceding year to the date of interview. The interviewees are asked, for each job during that period, to choose from a number of possible statements the one that best describes why he/she stopped doing that job. For the construction of our variable the answers were grouped in as follow. *Quits:* left for better job. *Employer originated separations:* made redundant, dismissed or sacked, temporary job ended. *Others:* took retirement, stopped for health reasons, left to have baby, children/home care, care of other person, other reason, moved away, started college or university. Since for each year the question refers to the jobs that ended since the 1st of September of the previous year, the information on the reason for separation had to be matched with the job spell of the previous interview, using the spell dates.

Wage: Nominal hourly wage is obtained by first dividing the current job usual gross monthly pay by 4.33 to obtain weekly wage and then by weekly hours. Weekly hours is the sum of the number of hours normally worked per week and the number of overtime hours in normal week. The nominal hourly wage is then deflated with the Retail Prices Index[87] to obtain real hourly wages.

[87] Monthly values are averaged for each year with 1991 as base year. Source: Economic Trends, Annual Supplement, 1998, Office for National Statistics.

German Socio-economic Panel

Tenure: The variable tenure in the job was constructed using the information about the exact year and month the individual has started current job, up to the time of interview. This variable was rounded to the nearest year.

Experience: The number of years of labour market experience is constructed in two stages. The first stage uses the yearly biographical scheme containing employment information from the age of 16 to the first wave of the panel to construct total experience at the entry of the panel. Both part-time and full-time spells are taken into account. The second stage uses the calendar available for each wave listing all labour market activities for each month in the year preceding the interview. This information is added to the information computed in the first stage to construct experience at each wave. This variable was rounded to the nearest year.

Skills: Given the apprenticeship training system in Germany, workers are divided workers into those with no apprenticeship training and no university degree – *Unskilled*, those with apprenticeship training – *Medium skilled or apprenticeship trainees*, and those with a university degree – *University graduates.*

Reason for separation: constructed from the answers to the question: *Why did you leave this job? Which one of the following points applies to you?* The interviewee can then choose from a number of options. For this particular variable we only use the years 1991-97 for two reasons. First, there were various changes in code in the preceding years. Second, before 1991 one of the possible answers was separation by agreement between the worker and the employer, which is difficult to classify as either a quit or a layoff. We grouped the answers for the period 1991-97 as follows. *Quits:* resigned, employee requested transfer within the company. *Employer originated separations*: company closed down/ was laid off, job ended automatically/time limit agreed on beforehand, business relations ended, company transferred employee, leave of absence/ laid-off. *Others:* retired, took early retirement, training/education completed, other.

Wage: Real hourly wage was constructed using the information on the reported gross earnings in the month preceding the interview. These excluded any additional payments, e.g., holiday money or back-pay and included money earned for overtime. This amount was divided by 4.33 to obtain weekly wage and then by weekly hours. Weekly hours are a derived variable with the actual number of hours worked per week. This is based on the information given at the question: *And how much on average does your actual working week amount to, with possible overtime?* Gross nominal hourly wages were deflated by the German consumer price index.

Chapter 5. Returns to seniority and experience in union and non-union jobs in Britain in the 80's and 90s

5.1 Introduction

This chapter looks at returns to job and labour market seniority in union and non-union jobs in Britain in the 80's and 90s. This is an important issue for a number of reasons. First, because no other study that we are aware of estimates returns to job seniority in union and non-union jobs in the United Kingdom using longitudinal data[88]. Most studies focus on US data, which can be hard to generalise as American unions may differ from the institutional and historical setting of other unions. Second, because of the strong decline in the union sector in Britain since the 80s it is of interest to examine whether there has been a change in the seniority remuneration during this period. Third, because both theory and evidence failed to produce a clear cut answer as to which of the two sectors rewards seniority the most.

[88] Booth and Frank (1996) use the first wave of the British Household Panel Survey to estimate the returns to seniority in unionised and non-unionised workplaces in the United Kingdom. In their study seniority refers to labour market experience, because their estimates of returns to job tenure are insignificant.

Most empirical studies using cross sectional data have found that returns to seniority are larger in non-union jobs than in union jobs[89]. This has puzzled some among the economics profession for at least two reasons. First, because there is wide evidence that with respect to other key factors for workers' welfare, seniority is substantially more important in unionised workplaces than in non-unionised workplaces. This is the case of layoffs, promotions and fringe benefits. Second, because a large body of literature on union behaviour is either based on median voter models which stress the influence of senior workers on union objectives (see, for example Grossman, 1983), or draw on optimum price discrimination theory to provide a rationale for rising wage-seniority wage profiles in union jobs (examples are Kuhn, 1988 and Kuhn and Robert, 1989).

Various explanations have been put forward to solve this apparent paradox. Abraham and Farber (1988) argue that the upward biases in returns to seniority due to worker and/or jobs match heterogeneity are likely to be larger in the non-union sector than in the union sector. Using US data for the years 1968 through 1980, they find that estimated returns to seniority in the union sector become larger once they are corrected for heterogeneity. Freeman and Medoff (1984) show that including the value of fringe benefits in the wage calculations partly corrects for the decrease in union advantage with age. Topel (1991) calls attention for the conceptual problem of defining returns to seniority in the union sector. Because union jobs are usually rationed, the relevant alternative may be employment in a non-union job. After correcting for the fact that the alternative job for the union sector may be a non-union job, returns to tenure become higher in the union sector. Finally, Booth and Frank

[89] See Lewis (1986) for an overview.

(1996) bring a new insight to this topic by taking into account unions' heterogeneity in terms of existence of pay scales. They use the data from the 1991 British Household Panel Survey wave to show that returns to labour market experience are higher in unionised jobs with pay scales than in non-unionised jobs. They suggest that union heterogeneity in terms of pay scales may explain why one fails to find higher returns to seniority in union jobs. In fact, they find no significant difference in terms of returns to labour market experience between unionised jobs with no pay scales and non-unionised jobs.

Not all economists see lower returns to seniority in union jobs as a paradox. Most empirical studies on the impact of unions on wage dispersion find that unions significantly reduce wage dispersion within the union sector and within the establishment (Freeman 1980, 1982, Hirsch 1982, Goslin and Machin, 1993). Returns to seniority may therefore be flatter in the union sector. The usual assumption used in conjunction with median voter models that senior workers are insulated from all but the extreme falls in demand has been questioned and may not hold in practice. In his discussion of trade union objectives, Pencavel (1991) points out that in spite of the inverse seniority layoff rates among union workers, senior workers with less outside job opportunities seem to be more willing to sacrifice income for job security. Furthermore in Britain, as Turnbull (1988, p.61) has documented, "when selection for redundancy is at issue alternatives or caveats to *last in, first out* are the norm rather than the exception". This may work as an opposing force to steep wage-seniority profiles in the union sector. In fact, a recent paper by Kuhn and Sweetman (1999) using US and Canadian data shows evidence that "in contrast to non-union workers, reemployment wages of workers displaced from unionised jobs decline with tenure on the lost job". The authors' interpretation of this finding gives support to the idea that senior union workers seem to have lower outside options than non-union workers[90].

This chapter uses the first nine waves of the British Household Panel Survey (BHPS) which run from 1991 through 1999. In order to compare with the 80's it also uses data from the 1983 General Household Survey (GHS).

This study aims at providing answers to the following questions. How do returns to firm tenure and labour market experience differ between the union and non-union sectors in Britain? Is there more heterogeneity bias in any of the sectors and why? Given the decline in the union bargaining power over the period and the documented failure of unions to organise in new establishments[91] is there a pattern of change in the returns from the early eighties into the late nineties? Are seniority returns different between union jobs with and union jobs without pay scales?

The chapter proceeds as follows. Section 5.2 describes the wage model used, section 5.3 describes the data, section 5.4 presents the results and section 5.5 concludes.

5.2 The model

The empirical analysis' starting point is the standard wage model in which the workers' wage depends on aggregate real wage growth, years of experience in the labour market and seniority with the firm.

[90] According to the authors this could either be due to negative selection of senior union workers or to a negative causal relationship of unionism on workers' alternative skills.
[91] See Machin (2000) for recent evidence.

$$W_{ijt} = \alpha + \beta_0\gamma_t + \beta_1 X_{ijt} + \beta_2 T_{ijt} + \varepsilon_{ijt} \qquad (5.1)$$

The dependent variable, W_{ijt}, is the log of the gross real hourly wage of individual i on job j at time t, γ_t is the time dummy, X_{ijt} denotes actual experience in the labour market, T_{ijt} is seniority with the current employer. Higher order terms of the tenure and experience polynomials and a vector with worker and job characteristics will be included in the empirical model, and are now omitted for simplicity.

The error term ε_{ijt} is decomposed into an individual fixed effect A_i which captures unmeasured differences in ability, a time-invariant job-match effect θ_{ij} which allows for heterogeneity in the quality of the job matches, and a transitory component ν_{ijt} to account for idiosyncratic shocks as well as measurement error:

$$\varepsilon_{ijt} = \theta_{ij} + A_i + \nu_{ijt} \qquad (5.2)$$

The least squares estimation of β_1 and β_2 is likely to give biased estimates due to the correlation of the individual fixed and job match effects with years of seniority and experience (Abraham and Farber, 1987 and 1988, Altonji, 1987 and Topel, 1991). In fact, tenure is believed to be positively correlated with the unobserved ability. Highly productive individuals are usually assumed to experience less layoffs and quits due to some inherent characteristic such as perseverance, motivation, or health status. This assumption is justified in the analogy to the empirical positive relationship found between job tenure and other observable measures of ability such as education. Tenure is also likely to be positively correlated with the job-match effect as one expects that better matched workers are less likely to be laid off, and if firms share returns to a good match, workers are also less likely to quit those jobs. Topel (1991) argues nevertheless that in survey data tenure may be negatively correlated with the job match effect. According to search models, the longer an individual spends in the labour market, the higher the probability of having received above average wage offers. Consequently, controlling for experience, individuals with shorter tenures have on average received above average wage offers more recently, and so have on average better job-matches.

Experience is also likely to be correlated with the unobservable effects. Search theory and matching models predict a positive correlation between experience and the job market effect, and while most authors tend not to worry[92] about the potential correlation between experience and the individual specific effect, as workers' experience is the result of successive decisions in and out of employment, and as more able individuals are expected to experience less unemployment spells throughout their lives, experience may be for this reason positively correlated with the unobserved fixed effect as well.

From what has been said, least squares estimation of the returns to firm seniority and labour market experience are likely to be biased. Moreover, as experience and tenure are positively correlated the possibility of tenure being overestimated may produce a negative bias in the experience coefficient. The signs of both the bias of returns to tenure and experience are therefore not known.

In order to correct for some of the above potential biases we use the instrumental variable approach suggested by Altonji and Shakotko (1987)[93]. They

[92] See Dustmann and Meghir (2002) for an exception.

[93] Other methods have been used to estimate returns to seniority and experience. Abraham and Farber (1988) estimate returns to seniority in union and non-union jobs using as an instrument for seniority the residual from regressing seniority on completed job duration. This residual would be by construction uncorrelated with completed duration and correlated with seniority. In a balanced panel where all durations were completely observed

instrument tenure with its deviations from job means[94]. Let $\overline{T}_{ij}$ be the job mean of the tenure variable, then its deviations from job means are $DT_{ijt} = T_{ijt} - \overline{T}_{ij}$. The higher order terms in tenure included in the empirical section are instrumented in the same way. If $\overline{T^P_{ijt}}$ is the job mean of a higher order term of the tenure variable, then $DT^P_{ijt} = T^P_{ijt} - \overline{T^P_{ijt}}$ is its deviation from the job mean. As this variables have zero average over each job, they are by construction orthogonal to the fixed individual and job match components. Returns to experience are still likely to be overestimated due to the potential positive correlation between experience and the job match and individual fixed effects. This would result in a downward bias of the tenure effect since the two are positively correlated.

Since deviations from job means of the linear terms of tenure and experience are perfectly collinear, it is not possible to instrument both tenure and experience with deviations from job means. Finnie (1992) suggests instrumenting experience with its deviations from individual means. This corrects the estimation from the potential bias due to the correlation between experience and the individual fixed effect. Let $\overline{Exp_{ijt}}$ be the individual mean of the experience variable, then $DExp_{ijt} = Exp_{ijt} - \overline{Exp_{ijt}}$ are

$$E(T_{ijt}) = E(T_{ijt} | \theta_{ij}, A_i) = 1/2 \cdot D_{ij}$$

and therefore this residual would not be correlated with the individual and job fixed effects. Topel (1991) noted that because in short panels with incomplete longitudinal histories $\overline{T_{ij}}$ - the average observed value of tenure on job j conditional on estimated duration is potentially correlated with the individual and job fixed effects this will yield biased estimates. Topel (1991) uses a two-stage model to produce a lower bound to returns to firm seniority, which is he shows is equivalent to the Altonji and Williams IV procedure for the linear case.

[94] With the aim of increasing efficiency, Altonji and Shakotko enlarge the set of instrumental variables for tenure. First, they include deviations from job means of the experience variable and its higher order and interaction terms as instruments. Then, they include deviations from job means of all time varying regressors, which corresponds to the Hausman and Taylor's estimators for the case of time varying endogenous regressors. They find that the efficiency gains are quite small.

the deviations from individual mean[95]. As this instrument has zero average over each individual, it is by construction orthogonal to the individual fixed effect. It is nevertheless still correlated with the job match component. As theory predicts a positive correlation between experience and the job match component, this estimation method is still likely to overestimate returns to experience, and therefore to provide a lower bound to returns to tenure.

5.3 The data

This study uses the first 9 waves of the British Household Panel Survey as its main data source, which cover the survey window from 1991 to 1999. The BHPS was designed as an annual survey of each adult (16+) member of a nationally representative sample of more than 5,000 households, making a total of approximately 10,000 individual interviews. The same individuals are followed in the successive waves and, if they split-off from original households, all adult members of their new households are also interviewed. Children are interviewed once they reach the age of 16. Thus the sample should remain broadly representative of the population of Britain as it changes through the 1990s. However, in order to construct tenure and experience we need to use the retrospective data on past jobs collected in the second and third waves (1992 and 1993)[96]. For this reason, we may not be able to include adults of

[95] The higher order terms are instrumented in a similar fashion. Let $\overline{Exp_{ijt}^{P}}$ be the individual means of a higher order term of the experience polynomial, then $DExp_{ijt}^{P} = Exp_{ijt}^{P} - \overline{Exp_{ijt}^{P}}$ are its deviations from the individual means. This variables have zero average over each individual and they are therefore orthogonal to the individual fixed effect.

[96] Even if one chooses to use potential experience instead of experience, the accurate construction of the variable "tenure with the employer" requires the information contained in the retrospective files. In

newly formed households with members that split-off from the original households. We assume that this sample selection is random and does not affect the wage regressions as long as age, tenure and experience are included in the regressions.

At each wave the interviewees are asked to state the beginning date of the ongoing job spell, which is defined by a change of employer or a change of job within the same employer. Previous literature has focused on returns to tenure with the employer. We follow the same approach because promotions and job changes within the employer are likely to be associated with wage changes, and therefore must be considered as part of the same spell, for the purpose of the measurement of wage returns to job seniority. For this reason, the construction of both the tenure and experience variables require the use of the retrospective data on the labour market histories, for even if one chose to use potential experience instead of actual experience, this would be needed for the accurate construction of the variable "tenure with the employer". When linking the job spell information in the various yearly questionnaires and the retrospective data collected in waves 2 and 3 one is confronted with the overlapping of more than one source of information for the same spell. Conflicting answers are resolved by giving priority to the information collected closest to the event occurrence (see Appendix 5.B)[97]. This is because recall error is likely to increase with the time elapsed between an event and the time of interview.

With the purpose of minimising endogeneity and unobserved heterogeneity, our analysis restricts our analysis the sample to observations of non-self-employed

fact, at each wave the individual is asked to state the date of the beginning of the ongoing job spell, where the job spell is considered to start when there is a change of employer or a change of job within the same employer. For example, if the individual is promoted, the date collected is the date of promotion. In order to track down the starting date with the present employer, one needs to go back as many spells as there are jobs changes with the same employer, which involves using the information of the retrospective data in many instances.

white males aged between 16 and 60 with jobs in the private sector. Self-employed wages may be misreported and loosely related with the individual productivity, non-white and female wages may suffer from discrimination, and the latter are also likely to be highly affected by endogenous labour force participation. By restricting the age interval, we avoid individuals without strong labour force attachments. We exclude the public sector where wages are regulated and may not reflect accumulation of human capital and worker productivity increases.

Nominal hourly wage is obtained by first dividing the current job usual gross monthly pay by 4.33 to obtain weekly wage and then by weekly hours. Weekly hours is the sum of the number of hours normally worked per week and the number of overtime hours in normal week. The nominal hourly wage is then deflated with the Retail Prices Index[98] to obtain real hourly wages. Wage outliers were dropped[99]. The resulting sample has 6750 observations with information on hours, wage, tenure with the employer, labour market experience, qualifications, occupation, union status, industry and size of employer. This corresponds to 1455 individuals with a total of 2177 jobs.

In order to compare the results for the nineties with the previous decade we use the General Household Survey which has information on union membership in 1983. Both the BHPS and the GHS-83 ask whether there is a trade union or staff

[97] This method ensures longitudinal consistency of the tenure and experience variables. In fact, tenure and experience do not increase more than 20 months and less than 6 months in two consecutive years.

[98] Monthly values are averaged for each year with 1991 as base year. Source: Economic Trends, Annual Supplement, 1998, Office for National Statistics.

[99] 7 observations were excluded with hwage<£0.5. Hourly wages higher than £70 were investigated, corrected when there was an obvious misplacement of the decimal cases (3 observations) and 7 observations were excluded. Five other observations were dropped because they showed a cut in the hourly wage of 85% from one year to the other, and 8 others because they represented an increase in the hourly wage of 500% from one year to the other.

association at the individual's workplace and whether the individual is a member[100]. Similarly to previous studies, we shall focus on the variable that asks about the existence of a union at the workplace that negotiates pay for workers with that type of job, since wages and working conditions resulting from bargaining between employers and unions are usually equally applied to all workers inside the firm irrespectively of membership. All the analysis in this study will therefore use union coverage as a measure of union presence.

In the BHPS, this question was not asked in waves 2 to 4 to those employees who did not change jobs since the previous wave. In those waves we assumed that the union status was the same as in wave 1. (In addition, whenever the union variable was missing for a particular year, and there was a non-missing value is the previous or the following year, we input that union code). This can be a strong assumption given the strong de-unionisation that took place during the period. However, misclassification of the union status reported by workers raises concerns about the use of longitudinal variation in the union variable to identify the union effect (Card, 1996, Freeman, 1984). We are therefore not too confident that we would be able to extract a great deal of information in exploring that variation in the data. In fact, a substantial number of jobs have more than 1 union status change, which is likely to be due to reporting error. We follow Abraham and Farber's (1988) decision rule to assign jobs that were coded non-union and union in different years. A job was considered a union job if (1) in at least two thirds of the observed years it was coded union, (2) there were no runs of

[100] The questions in the BHPS are TUJBPL, "Is there a trade union, or a similar body such as a staff association, recognised by your management for negotiating pay or conditions for the people doing your sort of job in your workplace?" and TUIN1 "Are you a member of this trade union/association?". In the GHS-83 the questions are TU "Thinking about your present job again, is there a Trade Union or Staff Association where you work, which people in your type of job can join if they want to?" and TUMEM "Are you currently a member of (that or) any Trade Union or Staff Association?"

three or more years coded non-union, (3) the first and last years on the job were coded union. An analogous decision rule lead to the assignment of mixed jobs into the non-union sample.

In our data there were 241 changes in the union status during the course of a job, with 114 of them being a change from non-existence to existence of a union at workplace. This corresponded to 154 jobs with changes in the union status variable and 917 observations. After assigning mixed jobs according to the above rule, only 113 jobs with union status changes were left, 91 of each with only one union status change. These 91 jobs were kept since genuine union status change might have occurred. 171 observations corresponding to the remaining 22 other jobs were dropped.

In addition, the BHPS asks whether pay includes annual increments: JBRISE – "Some people can normally expect their pay to rise every year by moving to the next point on the scale, as well as receiving negotiated pay rises. Are you paid on this type of incremental scale?". This information will be used in our empirical section to test whether returns to seniority are different in the presence of pay scales[101].

5.3.1 Descriptive analysis of union and non-union sectors

Table 5-1 shows the mean sample characteristics of workers and jobs in our final sub-sample of the BHPS. Workers in union jobs are on average around two and a half years older, are more likely to be married, have on average three more years of experience and their jobs last over three and a half years longer than their counterparts

[101] This question is not asked to individuals who did not change jobs since the previous wave in waves 2 to 4. Data from the non-missing observations of those jobs were inputted in those years.

in the non-union sector. Workers in the non-union sector are on average more qualified, work in smaller firms, are more likely to work in service sectors and less likely to work in manufacturing sectors and are more likely to have managerial and administration occupations and less likely to have blue collar occupations, in comparison with workers in union jobs. This simple analysis is consistent with the historical role of unions in the manufacturing industries which have seen a strong decline in the last decades and with the empirical evidence that unions have had difficulty in organising in new workplaces (Disney et al., 1994, 1995, Machin, 2000, Stewart, 1995).

The final union sub-sample has 2971 observations, 760 individuals and 907 jobs, and the non-union sub-sample has 3958 observations, 1001 individuals and 1448 jobs, which implies that the share of union jobs is just over 40 percent.

5.4 Results

5.4.1 Returns to tenure and experience

Tables 5.2.A. and B. show the coefficients and cumulative returns to tenure and experience in the two sectors using the three estimation methods described earlier: OLS, IV-tenure, IV-tenure-experience. All regressions use four degree polynomials in tenure and experience to allow for non-linear returns. In addition, they include individual controls – marital status dummy, two education dummies (low education is omitted), and firm controls – four firm size dummies, nine 1-digit industry dummies and eight 1-digit occupation dummies. Columns 1 and 5 of Table 5-2.B. show the OLS results. While returns to tenure are somewhat higher in the union than in the non-union sector, returns to experience are considerably lower in union jobs. Least squares estimates a 10.3 percent wage return to ten years of tenure in union jobs and a 2.4 percent wage return to ten years in non-union jobs. Ten years of labour market experience generate a cumulative wage gain of 50.8 percent in the union sector and of 72.3 percent in the non-union sector. In order to investigate whether these differences are statistically significant, we estimated a pooled regression with both union and non-union jobs. The highest significance for the interaction variables were obtained with a specification allowing different impact for all controls and intercept in the two sectors. The tenure variables interacted with a non-union dummy had a P- value of 0.0167 and the interacted experience variables had a P-value of 0.0505. This implies that the differences between the two sectors are not statistically significant at the significance level of 1 percent (this is also the case with the instrumental variables estimators).

Column 2 shows that when in the union sector job tenure is instrumented with deviations from job means, returns to tenure become very low and insignificantly different from zero, and returns to ten years of experience increase to 61.1 percent. In the non-union sector, however, both returns to tenure and to experience remain unchanged. This suggests only in the union sector there is overestimation in the returns to tenure due to longer individual and/or job heterogeneity bias. In other words, unlike in non-union jobs, in union jobs better matches last longer and/or more able individuals have longer job tenures.[102]

[102] This evidence goes against Abraham and Farber's (1988) work which shows that under certain assumptions the tenure heterogeneity bias is likely to be larger in the non-union sector. One of their assumptions is that the extent to which jobs matches improve with experience is similar in union and non-union jobs (pp. 6). Given mobility between the two sectors this is unlikely to hold. For example,

In the non-union sector results point to job duration being independent of the worker's ability. This implies that high ability workers are not more stable, and that low ability workers are not more likely to being laid off. This is consistent with a competitive labour market with workers being paid their marginal productivity and no adverse selection of job movers. In addition, in the non-union sector there is no evidence of "better matches" lasting longer. It can be that workers in better matches do not necessarily have to wait longer for another job offer if being better matched increases the probability of finding another good match (a good match may enhance the knowledge ones' preferences or other potential employers). In addition, if jobs are heterogeneous in terms of characteristics, what seems like a better match is simply a job with a higher wage but less attractive in terms of things such as training / work environment / responsibility / hours / location / etc.

Given the absence of heterogeneity bias in non-union jobs, how can we explain the apparent existence of heterogeneity bias in union jobs? Workers paid union negotiated wages are less likely to be rewarded for individual ability, as wages are set for all workers with similar observable characteristics. Assuming this holds, how can there be individual and job match heterogeneity among workers paid negotiated wages? The answer may be in the union bargaining structure in the UK, which is complex, with bargaining often taking place at the plant level and with many bargaining units within the plant. What looks like a good match in the data, may simply be that that the worker's wage is higher because its employer has higher rents and/or the relevant union's bargaining power is higher. Then workers with union

the increase in the job match may be higher for workers moving from non-union to union jobs, which could result in a higher heterogeneity bias in the union sector. Their findings are consistent with their claim. They use the PSID for the years 1968 through 1980, and they restrict their analysis to blue-collar workers.

wages in those employers have little incentive to change jobs as they are less likely to find a better paid job. Workers paid union wages in employers where the union mark up is lower will be observed as being in a lower match. These workers are more likely (than the ones in better paid employers) to find a better paid job in the future and to move jobs. If workers paid wages with higher union mark up are not more likely to being laid off[103], then these workers are more likely to move only to other high paid jobs, which would be picked up in the data as being high ability workers.

Column 3 shows the results for the union sector when tenure is instrumented with deviations from job means and experience is instrumented with deviations from individual means. Returns to tenure remain as low as when only tenure is instrumented (column 2) but returns to experience decrease substantially. For example, ten years of experience decrease to 39.27. This result suggests that previous estimates overestimate returns to experience due to individual heterogeneity bias. Using a similar reasoning to the one for the tenure heterogeneity bias, this suggests that individuals in jobs with a higher wage mark-up are more experienced. This is consistent with job shopping among union jobs, and with large job match wage gains with experience. A second mechanism that would re-enforce a higher wage mark-up for more experienced workers, would be that when workers experience an unemployment spell, they are more likely to loose their "search capital" and forced to accept a job with a lower wage.

The results in column 6 however show that in the non-union sector when both tenure and experience are instrumented, returns to experience are slightly higher than when only tenure is instrumented. This suggests that in the non-union sector if

[103] This would not be the case if those particular firms would be at a higher risk of closing down.Stewart (1995), finds no link between the magnitude of an establishment's union wage

anything, individual heterogeneity bias the returns to experience down, i.e., unobserved ability and experience are negatively correlated. Although the difference between the two estimators is not large enough to deserve much attention, a possible explanation could be due to differences in the quality of education across cohorts of workers, with younger workers enjoying better education conditional on type of qualification.

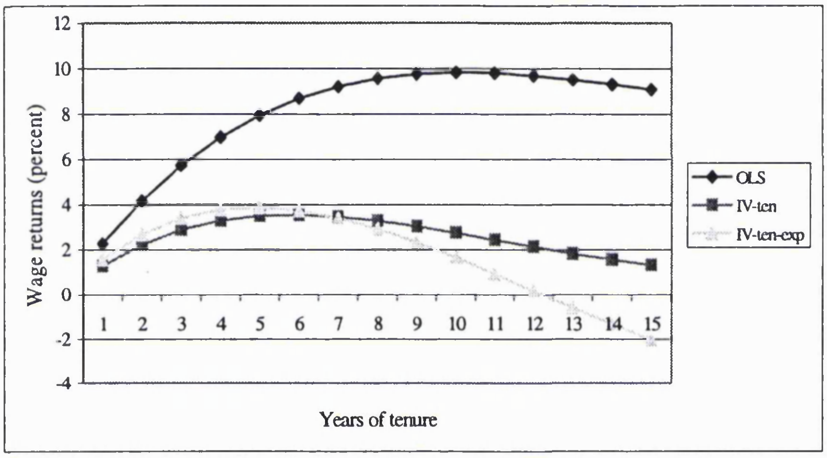


Figure 5-1: Returns to tenure in union jobs

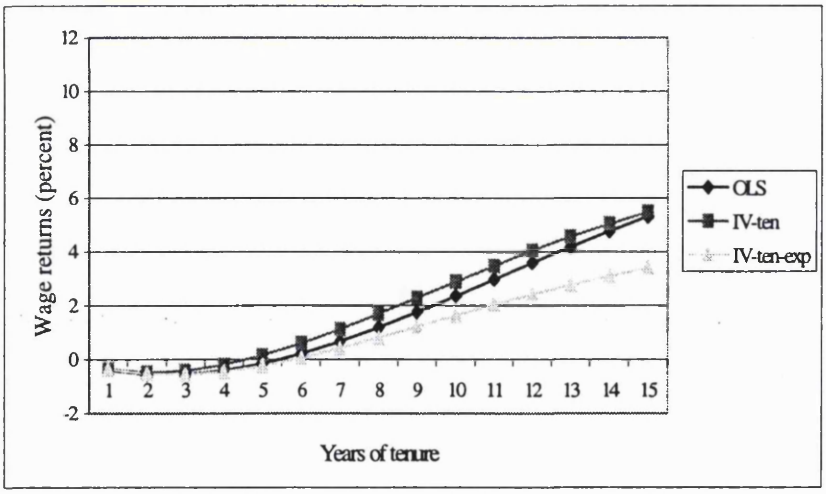


Figure 5-2: Returns to tenure in non-union jobs

differential in 1984 and the probability of closure in the next six years.

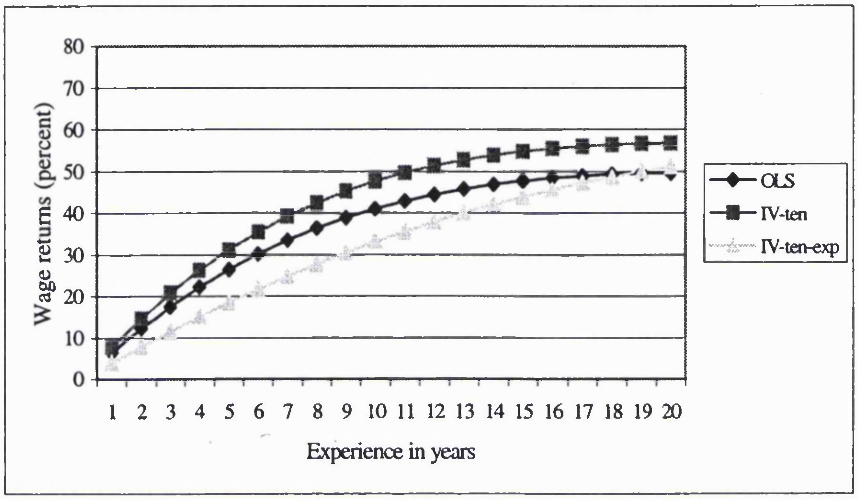


Figure 5-3: Returns to experience in union jobs

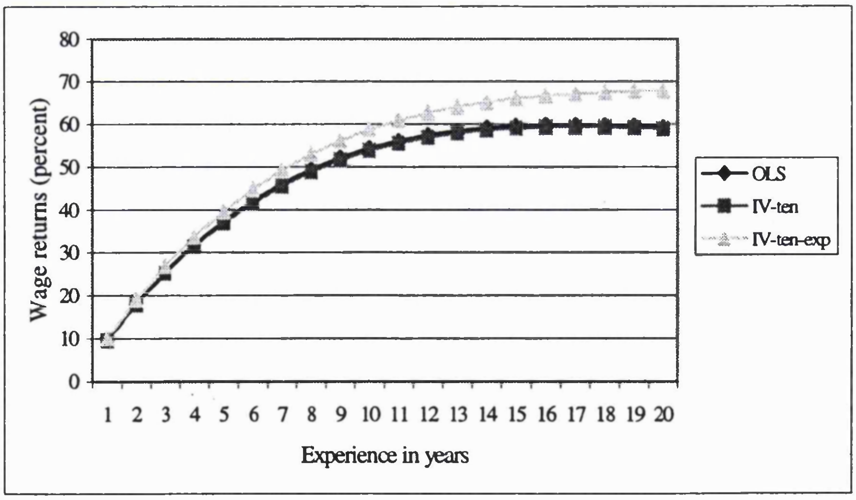


Figure 5-4: Returns to experience in non-union jobs

Figures 5.1 to 5.4 give a visual impression of the returns to tenure and experience in the two sectors. The graph of returns to experience has a concave shape, suggesting decreasing wage returns to labour market experience. Returns to tenure in union jobs are also concave, and in non-union jobs returns are zero for the first 7 years.

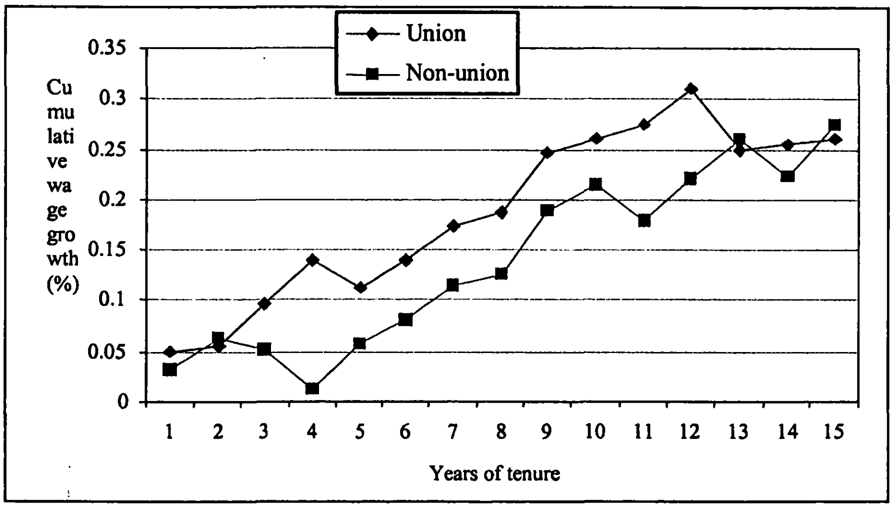


Figure 5-5: Mean sample wage growth by years of tenure - union and non-union jobs

Figure 5-5 shows cumulative wage growth with tenure in the raw data for union and non-union jobs. For union jobs, the graph is consistent with the previous suggestion of concave returns to tenure (Figure 5-1): in the raw data wages in union jobs do not seem to grow after the 9th year of tenure. For non-union jobs, similar to the estimated returns (Figure 5-2) wages do not seem to grow until the 5th year of tenure.

British unions have lost a lot of their bargaining power during the eighties and early nineties. It is therefore interesting to examine whether returns to seniority and experience have changed during this period. Table 5-3, Table 5-4 and Table 5-5 show the wage returns and coefficients of least squares regressions for the years 1983, 1991/92 and 1998/99. For the 90s we use the BHPS and for 1983 we use the GHS. As job seniority in the GHS is a categorical variable, regressions include three dummy variables according to the duration of the job (the dummy for jobs with less than 1 year is omitted). The coefficients presented estimate how much the current wage exceeds that of the first year in a job. Given that the BHPS has much less observations per wave, we pool the first two waves and the last two waves.

Although only in 1983 returns to tenure in the union sector are significantly different from zero, that can be at least partly due to the smaller number of observations for the latter years. In fact, the magnitude of the coefficients remains rather similar across the period. After 5 years of tenure, union workers are paid on average 12 to 17% more than non-union workers in their first year. In the non-union sector there is some indication that returns to tenure fell during the period under analysis. While in 1983 after the first year of tenure workers would be paid 10 to 20% more, depending on duration, in 1998/99 significance and magnitude of coefficients point to zero returns to seniority in the non-union sector. Cross section estimates of returns to tenure and experience may however vary with the economic conditions of the specific year used. In recessionary years the number of laid of workers is likely to be higher, and the number of voluntary moves is likely to be lower. In the data we would observe for those years many workers with zero tenure and low wages, which would overstate the tenure effect. The opposite would happen in years of boom. This could be driving the apparent decline in the tenure effect in the non-union sector. In addition, the data is likely to be noisy at low levels of experience since our BHPS sample has few observations of individuals who just entered the labour market each year. In fact, in order to construct actual labour market experience, individuals had to either be in the panel at wave 2 where that information was collected, or not have previous job history. Although we include children who reach the age of 16 in households previously interviewed, we are not able to include new members of newly formed households, which means that in the sample used mean tenure and experience increase somewhat during the course of the panel[104]. This could impart some measurement error in the latter years due to smaller number of observations at low levels of tenure and experience, and explain part of the decline in the returns to tenure overtime in the non-union sector.

Table 5-4 shows that returns to experience seem to have declined in both sectors between 1983 and 1999. For example in the union sector ten years of experience would give a percent wage return of 73 in 1983, 59 in 1991/92 and 52 in 1998/99. In the non-union sector the experience wage returns are 80 percent in 1983, 71 in 1991/92 and 69 in 1998/99. From the analysis of Table 5-4 and Table 5-5 it can be concluded that least squares estimates of returns to tenure give similar returns to tenure in the two sectors for the first part of the period, after which least squares estimates zero returns to tenure in non-union jobs. Returns to experience are always comparatively lower in union jobs than in non-union jobs.

5.4.2 Seniority pay scales and returns to experience

Firms may have incentives to use pay scales even in the absence of union presence. However, even though when human capital is acquired with time in the firm, there has to be at least another reason for firms to choose to pay according to a

[104] In the union sector mean tenure is 9.95 years in 1991 and 11.24 in 1999. In the non-union sector mean tenure is 5.58 years in 1991 and 7.04 in 1999.

scale, since if the firm can pay according to the individual's human capital, pay scales could distort incentives for human capital acquisition and productivity. Other reasons are if individual output is inherently hard to measure, and if pay scales entice workers to accept lower initial wages in a context of deferred compensation policies.

On the other hand, independently of whether firms have or not incentive to use pay scales, according to discrimination monopoly trade union models (Frank 1985, Kuhn and Robert 1989, Frank and Malcomson 1994), for a given bargaining power, unions can bargain for a higher total wage mark-up in the presence of pay scales, since firms are allowed to pay the higher wage rates in the discounted future. A corollary would be that union pay scales would be likely to exist where workers bargaining position is higher (this could be the case even in the absence of a union).

Next we examine whether the above results differ between jobs with and without pay scales.

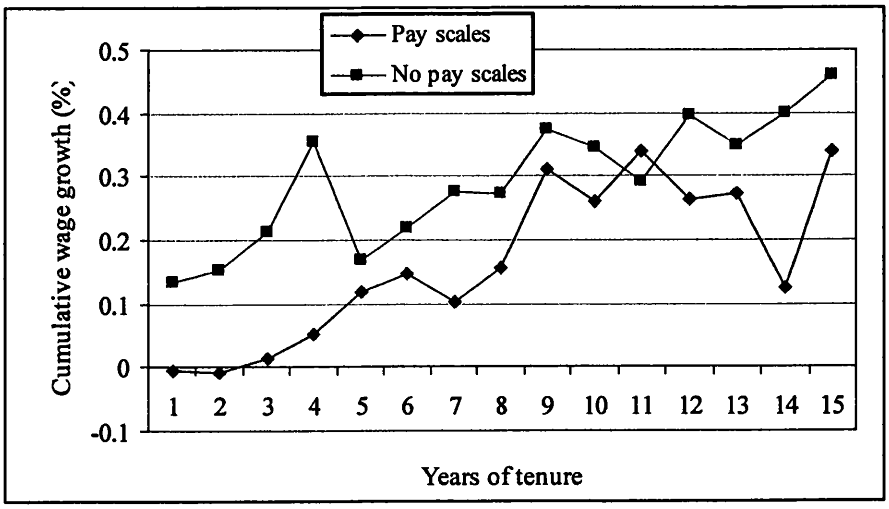


Figure 5-6: Mean sample wage growth by years of tenure - union jobs with and without pay scales

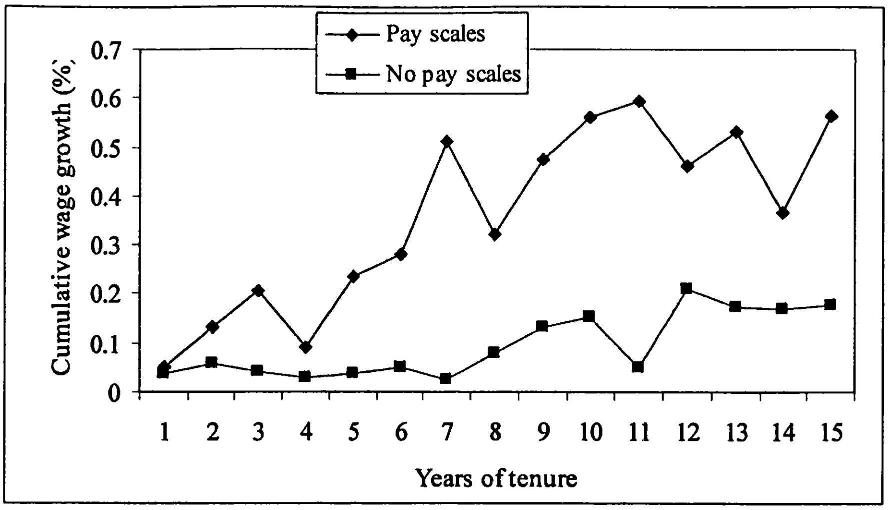


Figure 5-7: Mean sample wage growth by years of tenure - non-union jobs with and without pay scales

Figure 5-6 shows cumulative wage growth with tenure in the raw data for union jobs with and without pay scales. With the exception of the first year of tenure, during which wages seem to grow more in jobs without pay scales, wage growth does not seem to be different whether the wage is set according to a pay scale or not. Figure 5-7 shows that in the raw data cumulative wage growth with tenure is higher for non-union jobs with pay scales than for non-union jobs without pay scales.

Samples used in the regression analysis that distinguishes jobs with pay scales from jobs without are slightly smaller due to missing responses to the pay-scale question. There are 107 missing responses in union jobs and 179 in non-union jobs. Table 5-6 shows the results in the union sector for the three estimation methods. According to least squares estimations (columns 1 and 4) returns to tenure are positive but small and insignificantly different from zero in both cases and returns to experience are steeper in union jobs without pay scales than in union jobs with pay scales. The latter is true for all estimation procedures. This does not support Booth and Frank's (1996) finding that returns to labour market experience are steeper in union jobs with pay scales than in both non-union jobs and union jobs without pay scales. There are however a number of differences between my analysis and theirs. First, they only use the first wave of the BHPS which collects data for 1991. Second, they exclude tenure from their regressions on the grounds of insignificance. Third, experience is specified in their regression as a second degree polynomial, and here as a fourth degree polynomial. Fourth, the experience variable is constructed differently. We use the retrospective information only collected at the second and third waves (not available then) in order to obtain actual labour market experience which should be more precise than their potential experience variable. In addition, we exclude public sector jobs. These last two differences imply that for 1991 we have less observations. Finally, due to the smaller sample we include less controls in my regressions.

Other columns of Table 5-6 show that returns to tenure are insignificantly different from zero across all specifications except with least squares in jobs with no pay scales. When tenure is instrumented with deviations form job means in jobs with pay scales returns fall and became insignificantly different from zero. In addition, when experience is instrumented with deviations from individual means, in jobs with pay scales returns to experience are somewhat higher than when only instruments for tenure are used. Following our earlier interpretation of heterogeneity bias in the union sector, this suggests that union wage mark-ups are more heterogeneous in jobs without pay scales. Unfortunately, we cannot give much credit to these estimations, since standard errors in columns 3 and 6 are very high and returns to experience are in fact insignificantly different from zero. This is probably due to the low number of observations.

Table 5-7 replicates the analysis of Table 5-6 for the non-union sector. As before, returns to tenure are close to zero for all specifications, though it is interesting that most values are positive in jobs with pay scales and negative in jobs without pay scales. Consistently with the mean sample wage growth shown in Figure 5-7, returns to experience are steeper in jobs with pay scales than without. For example, least squares estimates a return to ten years of experience for workers in jobs with a pay scale of 59%, and for workers in jobs without pay scales of 45.6%.

The preceding analysis suggests that in the union sector, wages from jobs with pay scales grow with labour market experience at least as much as wages from jobs without pay scales. In contrast, in the non-union sector pay scales are clearly associated with steeper wage-experience profiles, and may serve the purpose of deferring compensation as a means of preventing quits, financing initial training, etc. My interpretation of Tables 5.5 and 5.6 is that in the union sector jobs with pay scales do not have higher returns to experience than jobs without pay scales.

5.5 Concluding remarks

This chapter estimates returns to firm seniority and labour market experience in union and non-union private sector jobs in the 80's and 90's in Britain and offers a number of interesting results. First, returns to tenure are only insignificantly different from zero in union jobs and when estimated with least squares. When instrumental variables are used to correct for individual and job match heterogeneity returns to tenure are insignificantly different from zero in both sectors. While there is no evidence of heterogeneity bias in the non-union sector, results suggest positive heterogeneity bias in the union sector. This would seem unconvincing under the usual interpretation of ability and job-match induced biases. However, it is consistent with a more plausible hypothesis of union wage mark-up heterogeneity affecting duration of jobs. Second, returns to experience are lower in union jobs than in non-union jobs, although this difference in not statistically significant. This result is rather robust, since it holds for all the period analysed, and with all estimation methods. Finally, contrary to previous evidence and unlike non-union jobs, returns to experience do not seem to be higher in union jobs with pay scales.

Appendix 5.A Tables

Table 5-1: Summary Statistics for white males in the British Household Panel Survey

	1991-1999		
	Union	Non-union	All
Hourly wage	6.7	6.3	6.5
	(2.7)	(3.0)	(2.9)
Hours (average)	39.0	40.6	39.9
	(5.8)	(7.2)	(6.6)
Tenure (years)	10.4	6.5	8.2
	(8.8)	(6.7)	(7.9)
Experience (years)	21.5	18.4	19.7
	(11.3)	(11.5)	(11.5)
Age	38.3	35.9	36.9
	(10.6)	(10.9)	(10.8)
Percent married	68.8	60.5	64.1
Distribution by qualification			
Low Qualification	29.4	24.2	26.4
Medium Qualification	39.4	36.2	37.5
High Qualification	31.2	39.7	36.0
Distribution of workers by size of employer			
1 to2	1.4	2.9	2.2
3 to 9	5.1	18.2	12.6
10 to 49	18.9	35.1	28.2
50 to 99	12.0	12.2	12.1
100 to 499	49.7	27.3	36.9
1000 or more	13.0	4.3	8.0
Distribution of workers by 1 digit industry			
Agriculture, forestry & fishing	1.8	2.3	2.1
Energy & water supplies	8.7	1.2	4.4
Extraction of minerals & ores other tha	8.5	5.6	6.8
Metal goods, engineering & vehicles ind	19.7	20.6	20.2
Other manufacturing industries	21.5	13.8	17.1
Construction	3.8	6.8	5.5
Distribution, hotels & catering (repair	7.8	23.8	17.0
Transport & communication	13.0	6.0	9.0
Banking, finance, insurance, business s	12.0	15.9	14.3
Other services	3.3	3.9	3.6
Distribution of workers by occupation groups			
Managers & administrators	10.7	23.1	17.8
Professional occupations	6.3	9.3	8.0
Associate professional & technical occu	8.1	8.8	8.5
Clerical & secretarial occupations	10.0	8.7	9.3
Craft & related occupations	25.3	22.2	23.5
Personal & protective service occupatio	2.1	3.0	2.6
Sales occupations	3.6	6.6	5.3
Plant & machine operatives	28.7	14.2	20.4
Other occupations	5.2	4.1	4.6
Number of observations	2891	3859	6750
Number of individuals	728	960	1455
Number of jobs	871	1397	2177

Table 5-2 A. and B.: OLS and Instrumental Variables
Returns to Tenure and Experience

A. Coefficients

	Union			Non-union		
	OLS	Ten IV	Ten IV Exp IV	OLS	Ten IV	Ten IV Exp IV
Tenure	0.0248	0.0147	0.0182	-0.0048	-0.0049	-0.0047
	(0.0069)**	(0.0145)	(0.0145)	(0.0069)	(0.0126)	(0.0125)
Tenure 2/10	-0.0209	-0.0197	-0.0259	0.0111	0.0135	0.0112
	(0.0077)**	(0.0182)	(0.0184)	(0.0098)	(0.0182)	(0.0183)
Tenure 3/100	0.0066	0.0091	0.0109	-0.0045	-0.0068	-0.0059
	(0.0031)*	(0.0081)	(0.0082)	(0.0048)	(0.0088)	(0.0088)
Tenure 4/1000	-0.0007	-0.0014	-0.0016	0.0050	0.0110	0.0010
	(0.0004)	(0.0012)	(0.0012)	(0.0007)	(0.0013)	(0.0013)
Experience	0.0676	0.0811	0.0406	0.1020	0.1003	0.1068
	(0.0083)**	(0.0100)**	(0.0215)	(0.0084)**	(0.0092)**	(0.0182)**
Experience2/10	-0.0326	-0.0420	-0.0071	-0.0618	-0.0604	-0.0627
	(0.0062)**	(0.0075)**	(0.0155)	(0.0074)**	(0.0078)**	(0.0160)**
Experience3/100	0.0066	0.0093	-0.0006	0.0156	0.0152	0.0162
	(0.0018)**	(0.0022)**	(0.0045)	(0.00246)**	(0.00262)**	(0.00547)**
Experience4/1000	-0.0005	-0.0007	0.0002	-0.0014	-0.0014	-0.0015
	(0.0002)**	(0.0002)**	(0.0005)	(0.0003)**	(0.0003)**	(0.0006)*
P-value (tenure)	0.0001	0.5743	0.4341	0.0023	0.7800	0.7521
P-value (exp.)	0.0000	0.0000	0.0023	0.0000	0.0000	0.0000
N. obsv.	2891	2891	2891	3859	3859	3859
R2	0.4678	0.4494	0.4278	0.5068	0.5057	0.5008

Note: All regressions include marital status, two qualification dummies (low education is omitted), four firm size dummies, nine 1-digit industry dummies, eight 1-digit occupation dummies and year dummies. Standard errors in parenthesis.
**- Significant at 1 percent level; *- Significant at 5 percent level.

A. Cumulative returns

	Union			Non-union		
	OLS	Ten IV	Ten IV Exp IV	OLS	Ten IV	Ten IV Exp IV
	(1)	(2)	(3)	(4)	(5)	(6)
1 year ten	0.0230	0.0129	0.0158	-0.0038	-0.0036	-0.0037
	(0.0063)**	(0.0130)	(0.0131)	(0.0060)	(0.0110)	(0.0108)
5 years ten	0.0827	0.0356	0.0396	-0.0016	0.0015	-0.0023
	(0.0212)**	(0.0412)	(0.0408)	(0.0169)	(0.0330)	(0.0307)
10 years ten	0.1032	0.0277	0.0166	0.0238	0.0293	0.0163
	(0.0244)**	(0.0494)	(0.0459)	(0.0178)	(0.0448)	(0.0373)
15 years ten	0.0951	0.0131	-0.0200	0.0547	0.0565	0.0345
	(0.0226)**	(0.0574)	(0.0500)	(0.0186)	(0.0639)	(0.0521)
1 year exp	0.0665	0.0801	0.0407	0.1007	0.0990	0.1059
	(0.0082)**	(0.0100) **	(0.0210)	(0.0085)**	(0.0093) **	(0.0186) **
5 years exp	0.3024	0.3660	0.2028	0.4537	0.4458	0.4863
	(0.0373)**	(0.0475) **	(0.0927)*	(0.0393)**	(0.0439) **	(0.0905) **
10 years exp	0.5076	0.6110	0.3927	0.7232	0.7117	0.7990
	(0.0592)**	(0.0781) **	(0.1562)*	(0.0595) **	(0.0687) **	(0.1517) **
15 years exp	0.6118	0.7298	0.5520	0.8141	0.8035	0.9343
	(0.0658)**	(0.0910) **	(0.1976)**	(0.0617) **	(0.0749) **	(0.1850) **
20 years exp	0.6418	0.7634	0.6682	0.8091	0.8002	0.9698
	(0.0645)**	(0.0962) **	(0.2268)**	(0.0577) **	(0.0744) **	(0.2092) **

Note: *Log-wage returns* to *k* years of tenure (experience) with *k=1,5,10,15,20* is the cross-product of the row vector of the tenure (experience) polynomial coefficients with a column vector of the form (k, k^2, k^3, k^4). Values presented are the *wage returns* and are obtained by applying an exponential transformation to the log wage returns minus 1. Standard errors are the square root of a 1st order Taylor approximation of the corresponding variance. **- Significant at 1 percent level; *- Significant at 5 percent level.

Table 5-3: Evolution of returns to tenure in the union and non-union sectors (OLS)

Tenure dummies	GHS	BHPS	
	1983	1991/92	1998/99
Union			
1 to 5 years	0.1042	0.0458	0.1178
	(0.0319)**	(0.0653)	(0.0679)
5 to 10 years	0.1465	0.1157	0.1680
	(0.0341)**	(0.0677)	(0.0727)*
>= 10	0.1610	0.1115	0.1686
	(0.0332)**	(0.0673)	(0.0686)*
N. of observ.	1392	868	502
R squared	0.4005	0.4029	0.5174
Non-union			
1 to 5 years	0.1272	0.0579	0.0089
	(0.0358)**	(0.0422)	(0.0452)
5 to 10 years	0.0974	0.0733	0.0072
	(0.0430)*	(0.0459)	(0.0500)
>= 10	0.2097	0.1466	0.0030
	(0.0441)**	(0.0490)**	(0.0485)
N. of observ.	964	1061	686
R squared	0.5121	0.4861	0.5385

Note: Values presented are the exponential of the changes in log wages minus 1. Standard errors are the square root of a 1st order Taylor approximation of the corresponding variance. Dummy for job tenures shorter than 1 year omitted.
**-significant at 1 percent level; *-significant at 5 percent level

Table 5-4: Evolution of returns to experience in the union and non-union sectors (OLS)

	GHS	BHPS	
	1983	1991/92	1998/99
Union			
1 year exp	0.1041	0.0811	0.0803
	(0.0137)**	(0.0166)**	(0.0249)**
5 years exp	0.4661	0.3630	0.3420
	(0.0657)**	(0.0767)**	(0.1078)**
10 years exp	0.7334	0.5879	0.5161
	(0.1030)**	(0.1202)**	(0.1533)**
15 years exp	0.8152	0.6822	0.5618
	(0.1098)**	(0.1305)**	(0.1531)**
20 years exp	0.8051	0.6991	0.5520
	(0.1046)**	(0.1263)**	(0.1421)**
N. of observ.	1392	868	502
R squared	0.4005	0.4029	0.5174
Non -Union			
1 year exp	0.1105	0.1059	0.0876
	(0.0183)**	(0.0162)**	(0.0200)**
5 years exp	0.5014	0.4660	0.4062
	(0.0863)**	(0.0747)**	(0.0889)**
10 years exp	0.7984	0.7092	0.6875
	(0.1313)**	(0.1100)**	(0.1329)**
15 years exp	0.8921	0.7564	0.8273
	(0.1366)**	(0.1106)**	(0.1395)**
20 years exp	0.8803	0.7172	0.8629
	(0.1287)**	(0.1016)**	(0.1334)**
N. of observ.	964	1061	686
R squared	0.5121	0.4861	0.5385

Note: *Log-wage returns* to *k* years of tenure (experience) with *k=1,5,10,15,20* is the cross-product of the row vector of the tenure (experience) polynomial coefficients with a column vector of the form (k, k^2, k^3, k^4). Values presented are the *wage returns* and are obtained by applying an exponential transformation to the log wage returns minus 1. Standard errors are the square root of a 1st order Taylor approximation of the corresponding variance.
**-significant at 1 percent level; *-significant at 5 percent level

Table 5-5: Evolution of returns to experience in the union and non-union sectors: least squares' coefficients

	GHS		BHPS			
	1983		1991/92		1998/99	
	Union	**Non - Union**	**Union**	**Non - Union**	**Union**	**Non - Union**
Tenure dummies						
1 to 5 years	0.1042	0.1272	0.0458	0.0579	0.1178	0.0089
	(0.0319)**	(0.0358)**	(0.0653)	(0.0422)	(0.0679)	(0.0452)
5 to 10 years	0.1465	0.0974	0.1157	0.0733	0.1680	0.0072
	(0.0341)**	(0.0430)**	(0.0677)	(0.0459)	(0.0727)**	(0.0500)
>= 10	0.1610	0.2097	0.1115	0.1466	0.1686	0.0030
	(0.0332)**	(0.0441)**	(0.0673)	(0.0490)**	(0.0686)**	(0.0485)
Experience Polynomial						
Experience	0.1055	0.1115	0.0825	0.1076	0.0826	0.0884
	(0.0134)**	(0.0180)**	(0.0165)**	(0.0160)**	(0.0251)**	(0.0201)**
Experience2/10	-0.0662	-0.0688	-0.0465	-0.0712	-0.0548	-0.0453
	(0.0110)**	(0.0162)**	(0.0128)**	(0.0144)**	(0.0223)**	(0.0186)**
Experience3/100	0.0173	0.0176	0.0112	0.0190	0.0154	0.0101
	(0.0034)**	(0.0055)**	(0.0038)**	(0.0049)**	(0.0075)**	(0.0065)**
Experience4/1000	-0.0016	-0.0016	-0.0010	-0.0018	-0.0016	-0.0009
	(0.0004)**	(0.0006)**	(0.0004)**	(0.0005)**	(0.0008)**	(0.0007)**
P-value (tenure)	0.0000	0.0000	0.0383	0.0111	0.0622	0.9961
P-value (exp.)	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
N. obsv.	1392	964	868	1061	502	686
R2	0.4005	0.5121	0.4029	0.4861	0.5174	0.5385

Note: Log-wage returns to k years of tenure (experience) with $k=1,5,10,15,20$ is the cross-product of the row vector of the tenure (experience) polynomial coefficients with a column vector of the form (k, k^2, k^3, k^4). Values presented are the *wage returns* and are obtained by applying an exponential transformation to the log wage returns minus 1.**-significant at 1 percent level; *-significant at 5 percent level

Table 5-6: Cumulative returns in jobs with and without pay scales in the union sector

	Pay scales			No pay scales		
	OLS	Ten IV	Ten IV Exp IV	OLS	Ten IV	Ten IV Exp IV
1 year ten	0.0185	0.0023	-0.0105	0.0196	0.0295	0.0257
	(0.0097)	(0.0191)	(0.0221)	(0.0089)*	(0.0188)	(0.0185)
5 years ten	0.0638	0.0124	-0.0417	0.0730	0.0835	0.0655
	(0.0318)*	(0.0599)	(0.0661)	(0.0297)*	(0.0593)	(0.0569)
10 years ten	0.0753	0.0254	-0.0697	0.0960	0.0679	0.0341
	(0.0360)*	(0.0747)	(0.0769)	(0.0339)**	(0.0668)	(0.0592)
15 years ten	0.0658	0.0355	-0.0985	0.0922	0.0400	-0.0096
	(0.0336)	(0.0892)	(0.0851)	(0.0309)**	(0.0735)	(0.0594)
1 year exp	0.0444	0.0520	-0.0002	0.0915	0.0830	0.1360
	(0.0113)**	(0.0157) **	(0.0277)	(0.0154) **	(0.0187) **	(0.0506) **
5 years exp	0.1945	0.2255	0.0424	0.4250	0.3990	0.6668
	(0.0490) **	(0.0679) **	(0.1115)	(0.0770) **	(0.0910) **	(0.3289) *
10 years exp	0.3186	0.3611	0.1683	0.7168	0.7089	1.1680
	(0.0762) **	(0.1046) **	(0.1853)	(0.1350) **	(0.1612) **	(0.7815)
15 years exp	0.3837	0.4236	0.3438	0.8563	0.8858	1.4258
	(0.0847) **	(0.1177) **	(0.2531)	(0.1557) **	(0.1972) **	(1.1174)
20 years exp	0.4111	0.4444	0.5340	0.8872	0.9397	1.5136
	(0.0838) **	(0.1255) **	(0.3306)	(0.1523) **	(0.2068) **	(1.2460)
N. of observ.	1329	1329	1329	1455	1455	1455
R squared	0.5253	0.523	0.4118	0.4487	0.4218	0.4062

Note: *Log-wage returns* to *k* years of tenure (experience) with $k=1,5,10,15,20$ is the cross-product of the row vector of the tenure (experience) polynomial coefficients with a column vector of the form (k, k^2, k^3, k^4). Values presented are the *wage returns* and are obtained by applying an exponential transformation to the log wage returns minus 1. Standard errors are the square root of a 1st order Taylor approximation of the corresponding variance.

Table 5-7: Cumulative returns in jobs with and without pay scales in the non-union sector

	Pay scales			No pay scales		
	OLS	Ten IV	Ten IV Exp IV	OLS	Ten IV	Ten IV Exp IV
1 year ten	0.0096	0.0067	0.0043	-0.0211	-0.0129	-0.0117
	(0.0118)	(0.0229)	(0.0220)	(0.0073)**	(0.0134)	(0.0135)
5 years ten	0.0404	0.0374	0.0254	-0.0577	-0.0274	-0.0301
	(0.0346)	(0.0789)	(0.0711)	(0.0200)**	(0.0384)	(0.0366)
10 years ten	0.0649	0.0775	0.0549	-0.0378	-0.0031	-0.0201
	(0.0366)	(0.1067)	(0.0925)	(0.0211)	(0.0508)	(0.0427)
15 years ten	0.0786	0.1125	0.0799	0.0033	0.0251	-0.0053
	(0.0376)*	(0.1300)	(0.1231)	(0.0220)	(0.0732)	(0.0589)
1 year exp	0.1026	0.1026	0.1056	0.0796	0.0731	0.0772
	(0.0150)**	(0.0167) **	(0.0308) **	(0.0113)**	(0.0125)**	(0.0293)**
5 years exp	0.4025	0.4007	0.4188	0.3116	0.2888	0.3127
	(0.0715) **	(0.0792) **	(0.1532) **	(0.0548)**	(0.0610)**	(0.1635)
10 years exp	0.5912	0.5852	0.6229	0.4560	0.4281	0.4856
	(0.1176) **	(0.1330) **	(0.2689)*	(0.0904)**	(0.1020)**	(0.3363)
15 years exp	0.6547	0.6442	0.6963	0.5029	0.4772	0.5780
	(0.1284) **	(0.1536) **	(0.3200)*	(0.0986)**	(0.1138)**	(0.4720)
20 years exp	0.6597	0.6457	0.7048	0.5045	0.4816	0.6314
	(0.1214) **	(0.1544) **	(0.3276)*	(0.0933)**	(0.1120)**	(0.5789)
N. of observ.	1164	1164	1164	2516	2516	2516
R squared	0.5295	0.5237	0.5119	0.5063	0.5052	0.4665

Note: *Log-wage returns* to *k* years of tenure (experience) with *k=1,5,10,15,20* is the cross-product of the row vector of the tenure (experience) polynomial coefficients with a column vector of the form (k, k^2, k^3, k^4). Values presented are the *wage returns* and are obtained by applying an exponential transformation to the log wage returns minus 1. Standard errors are the square root of a 1st order Taylor approximation of the corresponding variance.

Appendix 5.B Data Notes

British Household Panel Survey

Tenure: is the total number of years in which the individual works for the same employer. It is constructed for all individuals that are in paid employment. It is not constructed for self-employed, since these are excluded from the sample. Individuals are asked to give the starting date of the job spell, and not the spell with employer[105]. For example, if the individual is promoted, the date collected is the date of promotion. In order to track down the starting date with the present employer, we go back as many spells as there are jobs changes with the same employer, which involves using the information of the inter-wave history files and the retrospective data in many instances. We therefore add the time spent in the various spells within the same employer in order to compute tenure with the employer. When linking the job spell information in the various yearly questionnaires and the retrospective data collected in waves 2 and 3 one is confronted with the overlapping of more than one source of information for the same spell, or part of it. Conflicting answers are resolved by giving priority to the information collected closest to the event occurrence. This is because recall error is likely to increase with the time elapsed between an event and the time of interview. In addition, in some cases in two consecutive waves although the job starting date given in the later wave

[105] Question Text: "What was the date you started working in your present position? If you have been promoted or changed grades, please give me the date of that change. Otherwise please give me the date when you started doing the job you are doing now for your present employer."

takes place before the previous wave interview, the discrepancy between the two start dates makes it clear that they refer to two different job spells. We therefore also adopted the following rule: if the starting date of a given spell occurs just before the previous wave interview date (i.e., during the previous year) and it is more than 1 year apart from the starting date recorded in the previous interview, then it is assumed that this spell started just after the previous wave interview.

Experience: sums the individual's time spent in paid employment or as a self-employed since leaving full time education. Similar to the tenure variable, it combines the information from the various yearly questionnaires and the retrospective data collected in waves 2 and 3.

Chapter 6. Summary of findings

In the second chapter we studied the impact on workers' wages and employment of the 35.5% increase in the real minimum wage of workers aged 18 and 19 that took place in Portugal on the 1st of January of 1987. The main findings were the following. First, wages of workers aged 18-19 rose approximately 7% more than that of older workers. Second, employment of workers aged 18-19 fell relatively to that of older workers with an estimated employment-MW elasticity in the range of –0.2 to –0.4. Third, there was a substitution effect towards workers aged 20 to 25. Fourth, firms' adjusted their teenagers' employment both through reducing the number of individuals employed, and through reducing their average working time.

In the third chapter we examined two issues. First, are there productivity spillovers from FDI to domestic firms? Second, if there are such spillovers, what level of subsidies would be justified based on productivity spillovers alone? We found evidence of a significantly positive correlation between a domestic plant's TFP and the foreign share of employment in that plant's industry. This is consistent with the existence of productivity spillovers. Typical estimates suggest that a 10 percentage-point increase in foreign presence in a U.K. industry raises the TFP of that industry's domestic plants by about 0.5 percent. We do not find significant effects for foreign share of employment by region. Our estimates are robust across several issues regarding measurement and specification. These estimates suggest that the per-job value of spillovers appear to be less than per-job incentives governments have granted in recent high-profile cases, in some cases several times less. We have also found some evidence that spillovers take time to permeate to domestic plants, that they are more important for plants at the lower end of the performance distribution, and that they vary across parent country. Ours is the first micro-level study we are aware of to find broad evidence of FDI spillovers. Future work on this issue should investigate channels of productivity spillovers—e.g. access to suppliers, labour-market turnover, and whether different modes of FDI activity—greenfield investments, acquisitions of British firms, expansions of existing affiliates—have different impacts on domestic producers.

In the fourth chapter we compare the returns to tenure and experience in the UK and Germany using least squares, instrumental variables estimations and the Topel (1991) 2-stage method. Our results show that returns to tenure are low in both countries. Returns to experience are higher in the UK than in Germany. We estimate that 10 years of job seniority generate a wage return of between 4 and 14 percent in the UK, and between zero and 6 percent in Germany. Returns to 10 years of experience are between 63 an 73 percent in the UK and between 30 and 40 percent in Germany.

We also estimate separate regressions for three different qualification groups. We find that workers who went through the apprenticeship training system in Germany have substantially lower returns to labour market experience than all other groups. This suggests that a large part of difference between the two countries' returns to experience can be attributed to the apprenticeship training. Workers with these qualifications probably receive a relatively high entry wage in their first employment spell after apprenticeship, to reflect the productivity gains associated with the acquisition of skills during the apprenticeship period.

The differences between OLS and IV estimates show some evidence of stronger heterogeneity biases in Germany than in the UK. These are interpreted as being suggestive of negative selection of job movers in terms of unobserved ability in Germany. Finally, we point out that the institutional differences between the two countries may be the source behind the differences in the selection of jobs movers in Germany. It can both be driven by wage tariffs in Germany (in models of imperfect information about the workers' ability with "sticky wages") or by stronger adverse selection of job movers in Germany induced by the lower job mobility in a context of asymmetric information between current and prospective employers about workers' ability.

The fifth chapter estimates returns to firm seniority and labour market experience in union and non-union private sector jobs in the 80's and 90's in Britain and offers a number of interesting results. First, returns to tenure are only insignificantly different from zero in union jobs and when estimated with least squares. When instrumental variables are used to correct for individual and job match heterogeneity returns to tenure are insignificantly different from zero in both sectors. While there is no evidence of heterogeneity bias in the non-union sector, results suggest positive heterogeneity bias in the union sector. This would seem unconvincing under the usual interpretation of ability and job-match induced biases. However, it is consistent with a more plausible hypothesis of union wage mark-up heterogeneity affecting duration of jobs. Second, returns to experience are lower in union jobs than in non-union jobs, although this difference in not statistically significant. This result is rather robust, since it holds for all the period analysed, and with all estimation methods. Finally, contrary to previous evidence and unlike non-union jobs, returns to experience do not seem to be higher in union jobs with pay scales.

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