TECHNOLOGY AND REGULATION AS DETERMINANTS OF EMPLOYMENT RIGIDITIES AND WAGE INEQUALITY

Thesis submitted for the degree of Ph.D. in Economics,

Candidate: Maarten Goos

Supervisor: Professor Alan Manning

London School of Economics and Political Sciences
University of London

October 2005

ABSTRACT

Chapter I, *"Lousy and lovely jobs: the rising polarization of work in Britain"*, shows that the UK since 1975 has exhibited a pattern of job polarization with rises in employment shares in the highest- and lowest-wage occupations. This is not entirely consistent with the standard view of skill-biased technical change as a hypothesis about the impact of technology on the labor market. However, a more nuanced view of skill-biased technological change recently proposed by Autor, Levy and Murnane [2003] (ALM) is a better explanation of job polarization. ALM argue persuasively that technology can replace human labor in routine tasks, be they manual or cognitive, but (as yet) cannot replace human labor in non-routine tasks. Since non-routine tasks are concentrated at both ends of the earnings distribution, it is shown that ALM's routinization hypothesis can explain one-third of the rise in the log(50/10) and one-half of the rise in the log(90/50) wage differential.

Chapter II, *"The impact of shop closing hours on labor and product markets"*, adds to a small but growing literature related to the idea that product market regulation affects employment. More specifically, it is argued that shop closing hours can affect the level and composition of employment in retail industries. First, this chapter exploits recent changes in US Sunday Closing Laws to find that total employment, total revenue and the number of shops increase in deregulating industries and possibly decrease in non-deregulating industries. Second, building on what we know about retail markets, a model is presented to show how consumer behavior and retail competition can explain the observed impact of deregulation on retail labor and product markets and therefore ultimately employment.

Chapter III, *"The recent expansion of higher education in Britain, college premiums and wage inequality"*, examines the impact of changes in the relative supply of college workers on college premiums and wage inequality between 1975 and 2003 in the UK. First, it provides a test for the hypothesis proposed by Card and Lemieux [2001] (CL) that the inter-cohort slowdown in college attainment growth rates explains the higher college premiums for cohorts born between 1955 and 1970. More precisely, the chapter examines the expansion of Britain's higher education system between 1988 and 1994 to find lower relative earnings for college graduates born between 1970 and 1976, in line with the CL hypothesis. Second, accounting for

a positive time trend in college attainment and a secular increase in the relative demand for college workers, it is shown that the slowdown in educational attainment for cohorts born between 1955 and 1970 can explain an important part of the increase in the average college premium and a significant part of the increase in wage inequality after 1980. Relative to the secular increase in the demand for and supply of college workers, the recent expansion of Britain's higher education system is thus expected to significantly reduce the average college premium and therefore wage inequality.

Chapter IV, *"Cyclicality and fixed effects in gross job flows: a European cross country analysis"*, uses information on manufacturing establishments during the 1990s in Belgium, France, Italy and the UK to examine whether time series of employment dynamics behave differently across countries and whether persistent differences exist in gross job flows that are country or industry specific. The results suggest job destruction is more cyclically volatile in the UK compared to Continental European countries. In the longer-run, a country fixed effect best captures the process of job reallocation whereas industry specific differences are not important. Symmetry of job creation and destruction over the business cycle and the existence of country specific differences in gross job flows most likely reflect the importance of different labor market regulations in Continental European countries.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

CHAPTER IV: CYCLICALITY AND FIXED EFFECTS IN GROSS JOB FLOWS: A EUROPEAN CROSS-COUNTRY ANALYSIS

ACKNOWLEDGMENTS

"The teacher who walks in the shadow of the temple, among his followers, gives not of his wisdom but rather of his faith and his lovingness. If he is indeed wise he does not bid you enter the house of wisdom, but rather leads you to the threshold of your own mind. The astronomer may speak to you of his understanding of space, but he cannot give you his understanding. The musician may sing to you of the rhythm which is in all space, but he cannot give you the ear which arrests the rhythm nor the voice that echoes it. And he who is versed in the science of numbers can tell of the regions of weight and measure, but he cannot conduct you thither."

- Kahlil Gibran (1923) -

The images of that rainy September day in 2000 when I left my family and friends behind and moved to London, will long remain burned in my memory. But little did I know just how an amazing journey this would become.

First I would like to thank those who were not literally my teachers but whose presence I found invaluable. They are my colleague PhD students, my undergraduate students, researchers and support staff at the Centre for Economic Performance and its many visitors.

Second, I have been blessed with some excellent teachers. They are Michael Elsby, Joep Konings, Alan Manning, Barbara Petrongolo and Steve Pischke. Special thanks also goes to Alan Krueger and Hank Farber whose hospitality at Princeton's Industrial Relations Section made it possible for me to write my job market paper. Besides being outstanding economists, they have also given me the moral support whenever I needed it.

Finally and most importantly, I would like to thank my family and friends for their patience. Of course, this thesis is as much my girlfriend's as it is mine. An economist once asked what it is that wakes me up in the morning. After all those years in graduate school I came to realize there can only be one answer: to give to her as much as she has given to me.

GENERAL INTRODUCTION

A complicated model would be needed to explain every possible labor market outcome and only high quality data could support its hypotheses. However, this thesis aims to show that testing the relevance of simple labor market models using publicly available data, already goes some distance towards a better understanding of recent changes in employment shares and wage inequality.

Chapter I, *"Lousy and lovely jobs: the rising polarization of work in Britain"*, starts from a simple framework considering the relative demand for skilled workers to emphasize the role played by technological change. More precisely, this chapter amplifies a recent paper by Autor, Levy and Murnane [2003] (ALM) arguing that technology can replace human labor in routine tasks but (as yet) cannot replace human labor in non-routine tasks. If the ALM hypothesis is correct then we might expect to see evidence for it in other areas.

First consider the employment impact of ALM's routinization hypothesis. The routine tasks in which technology can substitute for human labor include jobs like craft manual jobs and book-keeping jobs that require precision and, hence, were never the least paid jobs in the labor market. The non-routine tasks which are complementary to technology include highly paid professional and managerial jobs but also many of the most unskilled jobs that rely on hand-eye coordination that virtually all humans find easy but machines find enormously difficult. If this is true, the impact of technology will be to lead to rising relative demand in well-paid jobs, but also rising relative demand in low-paid jobs and falling relative demand in middling jobs – a process we call job polarization.

Turning to the wage impact of ALM's routinization hypothesis, it is then shown that job polarization in the UK can explain one-third of the rise in the log(50/10) and one-half of the rise in the log(90/50) wage differential between 1975 and 1999. However, the finding that the wages in the lowest paid jobs are falling relative to those in the middling jobs presents something of a puzzle for the ALM hypothesis as one might expect the opposite to happen if relative demand is rising in the lowest paid jobs relative to the middling jobs.

Chapter II, *"The impact of shop closing hours on labor and product markets"*, argues that, besides technological change, also product market deregulation can increase the relative demand for low-wage work. More precisely, using deregulation of shop opening hours and building on what we know about the structure of retail markets, this chapter examines how deregulation affects retail labor and product markets and therefore ultimately employment.

First, the chapter shows that after 1977 different US states deregulated their Sunday Closing Laws at different points in time. Using a number of specifications and estimators, it is shown that deregulation significantly increased employment, revenue and the number of shops in deregulating industries. However, it is also shown that deregulation most likely decreased employment, revenue and the number of shops in industries exempted from Sunday Closing Laws.

The chapter then presents a model building on standard assumptions about retail markets to explain the observed impact of deregulation on employment, sales and the number of shops in deregulating and non-deregulating industries. For deregulating industries, it is argued that longer shop opening hours will increase employment because labor partially is a quasi-fixed input factor that only varies with opening times (threshold labor effect). In so far the observed increase in revenue due to increased product demand reflects an increase in the volume of sales, employment will further increase (sales effect). Finally, if the increase in revenue offsets the increase in labor costs, retailers will find it profitable to extend their opening hours in the short-run. In the long-run, the number of shops will therefore increase, further increasing employment (entry effect). However, to the extend that consumers substitute income towards deregulating industries, employment will fall in non-deregulating industries because of a decrease in total spending (sales effect) and a decrease in the number of shops (exit effect). In line with the empirical evidence, these are the channels through which it is argued that deregulation affects retail labor and product markets and therefore ultimately employment.

Chapter III, *"The recent expansion of higher education in Britain, college premiums and wage inequality"*, turns to the importance of shifts in the relative supply of skilled workers. More precisely, following Card and Lemieux [2001] (CL), a simple model is presented showing that in a period of accelerating (decelerating) educational attainment, age group specific educational premiums are likely to twist so that inequality among younger workers compresses (expands) relative to the old. Consequently, inter-cohort differences in college attainment growth rates are expected to affect the average college premium and therefore wage inequality over time.

This chapter first provides some further evidence in support of the CL hypothesis that inter-cohort differences in college attainment growth rates can significantly affect college premiums across age groups. Using data for the UK between 1975 and 1996, CL have shown that college premiums for cohorts born between 1955 and 1970 are higher due to a slowdown in the growth of educational attainment between 1973 and 1988. In line with the CL hypothesis, this chapter uses data for the UK up to 2003 to find higher relative earnings for cohorts born between 1955 and 1970 and lower relative earnings for cohorts born between 1970 and 1976 following Britain's expansion in higher education between 1988 and 1994.

The chapter then turns to the question whether a steady trend in the relative demand for or supply of college workers is sufficient to explain the rise in the average college premium and therefore wage inequality after 1980. It is shown that a significant part of the increase in the average college premium and a significant part of the rise in wage inequality can be explained by the slowdown in college attainment growth rates for cohorts born between 1955 and 1970. Relative to the secular increase in the demand for and supply of college workers, the recent expansion of Britain's higher education system is thus expected to significantly decrease the average college premium and therefore wage inequality.

Chapter IV, *"Cyclicality and fixed effects in gross job flows: A European cross country analysis"*, uses an establishment-panel data set to examine the variation in employment flows across four European countries and narrowly defined manufacturing industries during the 1990s. The key issues addressed in this chapter are whether cyclical variation in employment dynamics behaves differently across countries and whether time persistent differences exist that are country or industry specific.

Using comparable information on manufacturing establishments for four European countries (Belgium, France, Italy and the UK), this chapter shows that job destruction is more cyclically volatile than job creation only in the UK. Contrary to this, Baldwin, Dunne and Haltiwanger (1998) use harmonised data for manufacturing industries in Canada and the US to find that job destruction is more cyclically volatile in both countries.

Examining the between-country variation in employment adjustments for the US and Canada, Baldwin, Dunne and Haltiwanger (1998) also find that country specific differences are not important and that the process of job reallocation in both countries is best explained by an industry fixed effect. However, this chapter shows that a country fixed effect best captures the process of job reallocation whereas an industry fixed effect is not important. It is therefore argued that different labor market institutions in Continental European countries could partially explain the absence of higher cyclical volatility in job destruction as well as the existence of persistent country specific differences in employment adjustments.

CHAPTER I

LOUSY AND LOVELY JOBS: THE RISING POLARIZATION OF WORK IN BRITAIN[1]

[1] This chapter is joint work with my supervisor Alan Manning. A statement of co-authorship is attached at the end of this chapter.

INTRODUCTION

Economists writing about the impact of technology on the labor market in recent years have tended to emphasize the role played by skill-biased technical change (SBTC), the idea that technology is biased in favor of skilled workers and against unskilled workers. The idea of SBTC has primarily been used to explain rising wage inequality (see Katz and Autor [1999] for a survey of a very large literature). But a recent paper by Autor, Levy and Murnane (ALM) [2003] has argued for a more nuanced way of understanding the impact of technology in general (and computers in particular) on the labor market[2]. They argue persuasively that technology can replace human labor in routine tasks, be they manual or cognitive, but (as yet) cannot replace human labor in non-routine tasks[3]. The ALM hypothesis is intuitively plausible and they provide evidence that industries in which routine skills were heavily used have seen the most adoption of computers, and this has reduced the extent of routine skills in those industries (see Spitz [2003] for similar evidence for Germany). But, if the ALM hypothesis is correct then we might expect to see evidence for it in other areas: this is the aim of this chapter.

The basic idea is the following. The SBTC hypothesis predicts that demand for 'skilled' jobs is rising relative to that in 'unskilled' jobs, while the ALM hypothesis suggests a more subtle impact of technology on the demand for labor of different skills. The routine tasks in which technology can substitute for human labor include

[2] See also Card and DiNardo [2002] for the argument that SBTC is not as successful in explaining wage inequality as commonly thought.

[3] The idea of jobs as a set of tasks that differ in how easily machines can displace human labour is not new and goes back to at least Herbert Simon [1960]. Unlike more expansive computer scientists of that time, Simon had a clear sense of what computers would and would not be able to do and his predictions are broadly consistent with both the ALM hypothesis and the ideas advanced in this chapter. Simon starts from the prediction that complex information processing programs will supplant labor in routine jobs intense in many relatively simple and repetitive eye-brain-hand sequences. Consequently, workers will sort into non-routine jobs requiring the flexible use of the brain, eyes, hands and legs.

jobs like craft manual jobs and book-keeping jobs that require precision and, hence, were never the least-skilled jobs in the labor market. The non-routine tasks which are complementary to technology include 'skilled' professional and managerial jobs that tend to be in the upper part of the wage distribution. The non-routine manual tasks that make up many of the most 'unskilled' jobs such as cleaning are not directly affected by technology but the impact of technology in other parts of the economy is likely to lead to a rise in employment in these jobs. If this is true then the impact of technology will be to lead to rising relative demand in well-paid skilled jobs (that typically require non-routine cognitive skills) and in low-paid least skilled jobs (that typically require non-routine manual skills) and falling relative demand in the 'middling' jobs that have typically required routine manual and cognitive skills – a process we call job polarization. This chapter documents that the pattern of employment changes in Britain over the period 1975-99 is consistent with job polarization and the consequences of that.

A literature related to the idea of job polarization has emerged more recently – the 'job quality' debate in the US. Some of the early papers on the rise in US wage inequality (e.g. Bluestone and Harrison [1988]) argued that there was an increasing number of low-wage jobs and a shrinking number of 'middling' jobs. This was controversial even at the time (e.g. see Kosters and Ross [1988]) and most labor economists came to the conclusion that the problem for low-skill workers was a declining number of jobs for them rather than an increasing number (see Burtless [1990]). But, in the 1990s one can still find a number of papers continuing to address the major themes of the job quality debate (see, for example, Costrell [1990], Howell and Wolff [1991], Levy and Murnane [1992], Juhn, Murphy and Pierce [1993], Murphy and Welch [1993], Gittleman and Howell [1995], Ilg [1996], Farber [1997], Acemoglu [1999, 2001], Juhn [1999], Ilg and Haugen [2000], Wright and Dwyer [2003]). Although these studies do differ slightly in their conclusions, common themes do emerge, most notably that, in the last 30 years, there has been a very big increase in the number of high-paid jobs and (probably) an increase in the number of low-paid service jobs – this is broadly consistent with the job polarization prediction of the ALM hypothesis although few of these papers offer this interpretation of their results.

The plan of the chapter is as follows. In the first section, we use the US data from Autor, Levy and Murnane [2003] to show that the jobs that require non-routine tasks tend to be at the top and bottom of the wage distribution while the jobs that require routine tasks tend to be in the middle, thus leading to the job polarization prediction. The second section describes the data used for the UK. The third section then documents how job polarization can be observed in the UK between 1975 and 1999 when the quality of jobs is defined by their median wage. There has been a growth in lousy jobs (mainly in low-paying service occupations) together with a (much larger) growth in lovely jobs (mainly in professional and managerial occupations in finance and business services) and a decline in the number of middling jobs (mainly clerical jobs and skilled manual jobs in manufacturing). We document that one sees these trends using all measures of employment, for men and women together or separately and for all definitions of 'jobs' that we use. We also show that a method used by Juhn, Murphy and Pierce [1993] and Juhn [1999] to predict employment growth at each percentile of the wage distribution also supports the hypothesis of job polarization. And although the pattern of changes in the occupational structure of employment is broadly consistent with the ALM hypothesis, other factors may be important and the fourth section considers some of them. We discuss the potential importance of changes in the composition of the labor force (e.g. from the rising labor market participation of women, the changing age and education structure), the structure of consumer demand and trade. It is likely that all of these factors are important for employment changes in at least some occupations but none of these hypotheses seem able to explain the broad sweep of job polarization.

As an increase in the relative demand for low-wage workers (relative to middling workers) is not in line with the predictions of the SBTC hypothesis, sections five and six consider the evidence most commonly cited in favor of that hypothesis. Section five considers the rise in the employment of non-manual workers - we argue that the pattern of within and between industry changes in employment observed at the 1-digit occupation level is consistent with the ALM hypothesis that technical progress has displaced the labor of clerical and manual workers in all sectors of the economy but that differential productivity growth between manufacturing and service sectors has led to the growth in low-wage service employment (as originally proposed by Baumol [1967]). Section six documents that the well-known shift towards more educated labor has largely occurred within jobs and that there has been a rapid rise in educational attainment of workers even in the worst jobs. There are two possible interpretations of this. First, that there has been SBTC within jobs as we define them so that the consensus view on the importance of SBTC is correct. Secondly, that as the educational attainment of all groups in the population has risen but the job distribution has become more polarized, some educated workers are forced into the low-skill jobs at the bottom end of the distribution. The attraction of this view is that it can explain why there has been a simultaneous rise in the returns to education (the demand for educated workers has increased as the number of good jobs has increased) and in the level of over-education as some have claimed. Distinguishing between these hypotheses requires evidence on changing skill requirements within jobs that is hard to find. We review two pieces of evidence that might shed light on these questions although they are somewhat contradictory in their implications.

Section seven considers the extent to which the observed job polarization can explain the rise in wage inequality between the 1970s and 1990s. We find that a modest part of the rise in wage inequality can be explained by the polarization of jobs alone but that once one includes the fact that wage growth seems to be monotonically positively related to the quality of jobs one can explain most of the evolution of wage inequality. The implication is that the rise in 'within-group' wage inequality that others have emphasized is more a product of a restricted definition of a 'group' and that if one includes jobs controls then it largely disappears. However the finding that the wages in the lousy jobs are falling relative to those in the 'middling' jobs presents something of a problem for the ALM hypothesis as one might expect the opposite if relative demand is rising in the lousy relative to the middling jobs. The final section concludes.

I. ROUTINE JOBS, NON-ROUTINE JOBS AND TECHNICAL CHANGE

This section shows how the Autor, Levy and Murnane [2003] view of the impact of technology on the demand for different skills predicts job polarization. ALM use the US Dictionary of Occupational Titles (DOT) to associate particular occupations with the intensity of use of five particular types of task. The types of task included in the analysis are chosen to represent those that are affected in different ways by technology – they label them non-routine cognitive, non-routine interactive, routine cognitive, routine manual and non-routine manual (see Autor, Levy and Murnane [2003] for a more detailed description of the tasks given these labels).

ALM then show that industries that were relatively intensive users of occupations that use routine tasks had more computerization and that the extent of the use of routine skills has fallen in these industries. Here we pursue an angle of the ALM hypothesis that ALM do not develop – namely that jobs that can be routinized are not distributed uniformly across the wage distribution. The central idea is that non-routine manual jobs are concentrated in the lower percentiles of the wage distribution whereas non-routine cognitive and interactive jobs are concentrated in the top end of the wage range with routine jobs concentrated in the middle.

ALM argue that the non-routine cognitive and interactive tasks are complementary to technology, the routine tasks are substitutes and the non-routine manual tasks are not directly affected. However this should not be taken to mean there will be no effects of technology on employment in occupations that primarily consist of non-routine manual tasks. The reason is the general equilibrium effect first identified by Baumol [1967] – employment will shift towards jobs in which productivity growth is low (because technology is not applied there) in order to keep the balance of output in different products. Baumol applied his argument to the shift in employment from manufacturing to services but it is relevant in the current context as well. As a result, technological progress can be expected to result in job polarization with employment growth in lovely and lousy jobs and employment falls in 'middling' jobs.

Table I presents a simple way of showing that the non-routine jobs are concentrated at the top and bottom of the wage distribution. We use wage information from the CPS MORG 1983 file and assign to each individual the five task measures in 1977 used by ALM based on their occupations[4]. All skills are measured on a 10-point scale, although these should not be taken to be comparable across tasks. Table I tabulates the fraction of workers that have DOT scores above the overall mean DOT score for the 5 different tasks as a percentage of total employment within the three terciles of the wage distribution. For example, only 17% of all workers in the lowest paid occupations are in jobs that require above average non-routine cognitive skills. But 88% percent of workers in the highest paid occupations are in jobs that require above average non-routine cognitive skill. A similar picture holds for the non-routine interactive skills: occupations intensive in non-routine interactive skills are concentrated in the upper part of the wage distribution. In contrast, routine-intensive occupations are concentrated in the middle. Of workers in occupations earning between the 33rd and 66th wage percentiles, 63 % require above average routine cognitive and 58 % above average routine manual skills. These numbers are higher than for any other specified wage range. Finally, the lowest paid occupations require a higher fraction of non-routine manual skills and its fraction is higher than for any other occupation paying higher wages.

This section has shown some direct evidence that workers in the middling jobs used to do routine tasks, while workers in lousy and lovely jobs did non-routine tasks. Since non-routine jobs are concentrated in both tails of the wage distribution, the ALM hypothesis predicts an increasing polarization of the workforce into lousy and lovely jobs. This predicted process of polarization provides an explanation for the empirical "facts" in an ongoing debate about the quality of jobs mentioned in the introduction.

[4] We are grateful to David Autor for making the DOT data available to us. The year 1983 is the earliest year for which the DOT occupations can be merged into CPS data.

Table I
DOT Task Density by Wage Percentiles

DOT Task Measure	*Mean DOT Task Measure*	*Fraction of workers above mean DOT task measure by wage percentiles*		
		≤33	*33-66*	*≥66*
Non-Routine Cognitive	3.755	0.17	0.48	0.88
Non-Routine Interactive	2.417	0.03	0.14	0.59
Routine Cognitive	4.582	0.37	0.63	0.43
Routine Manual	3.901	0.28	0.58	0.35
Non-Routine Manual	1.198	0.49	0.33	0.31

Notes: Task inputs are measured as in ALM [2003] and are between 0 and 10. The mean DOT task measure is the 1977 mean across 3-digit occupations. Wage percentiles are taken from the CPS MORG 1983 file.

II. THE DATA

The main data in this chapter comes from Britain but we would expect the task composition of occupations and the impact of technology to be very similar to that observed in the US. The data used in this chapter come from two sources, the New Earnings Survey (NES) and the Labor Force Survey (LFS). The New Earnings Survey is an annual panel dataset that started in 1968 though the first year for which computerized records are available is 1975, the sample being all individuals whose National Insurance number ends in '14'. In April of each year the tax records are used to contact the employer of each of these workers who reports information on pay, hours, and, importantly for this chapter, occupation and industry. Although the

NES is in theory a random sample, it is known to under-sample certain groups in practice, notably part-time workers (if weekly earnings fall below the threshold for paying National Insurance then they are unlikely to appear in the tax records) and those who have changed jobs recently (as the sampling frame is drawn up early in the year and the survey is likely to be sent to the wrong employer in April).

For this reason we supplement the NES with data from the Labor Force Survey. The LFS was first conducted in 1975, then every two years until 1983, then annually until 1992 and quarterly since then (when a panel component was also introduced). The LFS has a much smaller sample than the NES (and until 1993 it did not contain any wage data) but does have the advantage that it is closer to a random sample.

In this chapter we define a 'job' as a particular occupation or as a particular occupation in a particular industry. The occupation part corresponds to the main usage of the term 'job' – the question in the LFS used to obtain the information on occupation is 'what was your main job in the week ending Sunday?'. The industry part of the definition of a job is more problematic but other papers in this area have used a similar definition and there are significant industry effects on wages even once one has controlled for occupation. However, it is important to realize that the occupation part of our definition is much more important than the industry part as one gets very similar results whether a job is defined by occupation alone or by an occupation-industry interaction.

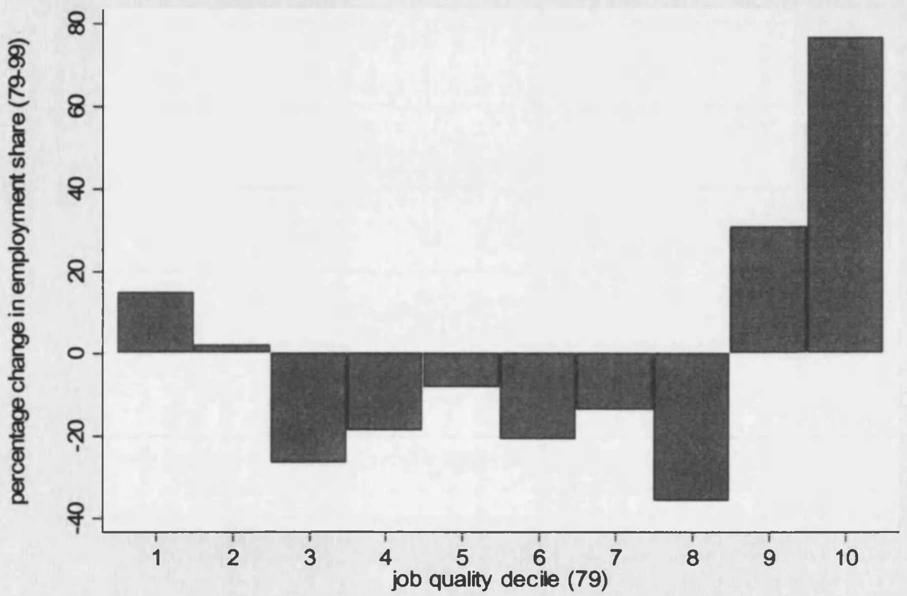
We have explored using different levels of disaggregation by occupation and industry and the results seem robust to the level chosen. We have restricted the results reported in this chapter to using 3-digit occupation codes only (allowing for approximately 370 jobs) as well as the interaction of a 3-digit occupation and 1-digit industry classification (allowing for a maximum of 3700 jobs, although in practice only about 1600 exist as not all occupations are represented in all industries). One might wonder about whether there are jobs that are disappearing and new ones popping up. In practice this does not seem to be a problem: of the occupations that existed in the 1970s all still have workers in them in the late 1990s and there are essentially no new occupations that cannot be put into the 1970s classification.

III. TRENDS IN THE QUALITY OF JOBS

We start by looking at long-term trends in the quality of jobs. To do this, obviously requires a measure of the quality of a 'job'. We first do this in a very simple way by using the median wage in the job at the beginning of the period (see OECD [2001] and Meisenheimer [1998] for a discussion of other ways of discussing the quality of jobs). One can think of it as a 'single-index' model of skill – see Card and Lemieux [1996]. However, we then also take a slightly different approach based on the analysis of Juhn, Murphy and Pierce [1993] with very similar results.

First consider how the proportional change in employment from the late-1970s to the late-1990s is related to the initial level of wages. If the SBTC hypothesis is correct then one would expect to see a monotonic positive relationship between employment growth and initial wages. Figure I groups occupations into the 'lowest 10%', the 'second-lowest 10%', up to the 'top 10%' based on their median wage and cell size in 1979. For example, the worst job quality decile captures 10% of all workers employed in the lowest paid occupations. Figure I shows large growth in the share of employment in the top two deciles, but also growth, albeit smaller, in the share of jobs in the bottom decile. Also, there has been a significant decline in

Figure I
Percentage Change in Employment Share by Job Quality Decile



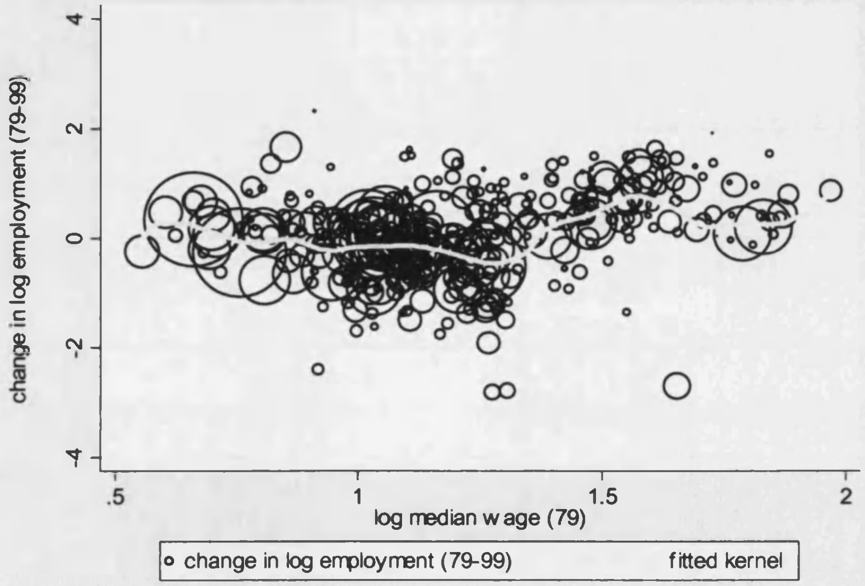
Notes: Employment data are taken from the LFS using 3-digit SOC90 codes. Employment changes are taken between 1979 and 1999. Quality deciles are based on 3-digit SOC90 median wages in 1979 taken from the NES.

middling jobs. Though the increase in the number of workers with bad jobs has been lower than the increase in the number of workers with good jobs, employment polarization into low paid and high paid work is clear from Figure I. It is this process of job polarization that is the central theme of this chapter.

Figure II presents the LFS data for the period 1979-99, where the size of the circles denotes the initial employment level in each occupation. On Figure II, we also include a kernel regression estimate of the mean of employment growth conditional on job quality.[5] There is certainly no striking evidence of a positive monotonic relationship between employment growth and initial log median wages as the literature on SBTC might have led one to expect. Moreover, one can discern the J-shaped relationship that is going to appear in the regression results.

[5] These are Nadaraya-Watson estimates, using a bandwidth of 0.1 and an Epanechnikov kernel.

Figure II
Employment Growth by Job Median Wage



Notes: Employment data are taken from the LFS using 3-digit SOC90 codes. Employment changes are taken between 1979 and 1999. Wages are 3-digit SOC90 median wages in 1979 taken from the NES.

Figure I and Figure II relate to one measure of employment, one definition of a job and to one survey (the LFS). One would like to know whether the results are robust or not. Because it is tedious to present graphs for every possible outcome, we turn to a simple regression to summarize our results.

[redacted]

$$(1) \qquad \Delta n_j = \beta_0 + \beta_1 w_{j0} + \beta_2 w_{j0}^2$$

where Δn_j is the change in log employment in job j and w_{j0} is the initial log median wage in the job.

We experiment with a number of different measures of employment and jobs. Table IIa presents estimates combining employment for men and women. The top half of Table IIa measures employment in terms of bodies using different definitions for a job and different surveys: we report results from the LFS and the NES using either 3-digit occupation codes only or the interaction of a 3-digit occupation code with a 1-digit industry code. But the results tell a similar story. The linear term in (1) is negative and the quadratic term positive implying a U-shaped relationship between employment growth and the initial level of wages. One might be concerned that the downward-sloping part of this relationship contains no data points but, as the final column in Table IIa makes clear, this is not the case: substantial numbers of workers are in the downward-sloping part of the relationship[6]. These regressions support the view that there has been polarization in the quality of jobs, with the employment growth being at the extreme ends of the distribution. It should also be noted that the parameter estimates for the LFS and NES are very similar, which suggests that the non-random sampling in the NES is not too serious a problem. We have also experimented with further aggregation or disaggregation in the jobs classification but this does not seem to make a great deal of difference to the qualitative results.

One might think that these results are misleading because much of the growth in employment has been in part-time jobs and these tend to be low-paid. Hence, the estimates in the top half of Table IIa might be thought to over-state the employment growth in low-paid occupations. However, when we measure employment in terms of total hours, the results are very similar so this does not explain away the observed job polarization. One might also think that the feminization of employment can explain this job polarization, with women accounting for the growth in relatively low-paid

[6] Inspection of the kernel regression line in Figure II should make it clear that this estimate of the proportion in the downward-sloping section is not an artifact of the quadratic specification adopted.

Table IIa
The Relationship between Employment Growth and Initial Median Wage: Men and Women Together

Sample	Sample Period	Data	Employment Measure	β1	β2	Fraction in Declining Section
Men+Women	1979-99	LFS (occ)	Employment	-4.541 (0.700)	2.107 (0.297)	52.93
Men+Women	1976-95	NES (occ)	Employment	-3.412 (0.664)	1.373 (0.267)	72.57
Men+Women	1979-99	LFS (occXind)	Employment	-4.804 (0.472)	2.109 (0.198)	62.80
Men+Women	1976-95	NES (occXind)	Employment	-3.957 (0.378)	1.581 (0.151)	74.69
Men+Women	1979-99	LFS (occ)	Hours	-4.218 (0.785)	2.047 (0.327)	28.42
Men+Women	1976-95	NES (occ)	Hours	-3.603 (0.775)	1.576 (0.319)	56.85
Men+Women	1979-99	LFS (occXind)	Hours	-4.331 (0.514)	1.969 (0.213)	49.67
Men+Women	1976-95	NES (occXind)	Hours	-4.145 (0.435)	1.748 (0.178)	62.22

Notes: Regressions are weighted by job cell size in the initial period. Occupation uses 3-digit SOC90 codes. Industry uses 1-digit SIC80 codes.

occupations. But, as Table IIb and Table IIc show, one observes similar patterns for male and female employment considered separately although, in fact, the trends are more marked for men.[7]

One might also be concerned that the quality ranking of jobs changes a lot over time so that the patterns of employment growth are sensitive to the point in time at which job quality is measured. Table III shows this is not the case.

[7] Note that the last column in Table IIc shows a missing if the quadratic term is not significantly different from zero.

Table IIb
The Relationship Between Employment Growth and Initial Median Wage: Men

Sample	Sample Period	Data	Employment Measure	β1	β2	Fraction in Declining Section
Men	1979-99	LFS (occ)	Employment	-5.807 (1.317)	2.447 (0.482)	39.66
Men	1976-95	NES (occ)	Employment	-3.080 (1.097)	1.267 (0.389)	43.33
Men	1979-99	LFS (occXind)	Employment	-6.039 (0.719)	2.413 (0.265)	55.84
Men	1976-95	NES (occXind)	Employment	-4.697 (0.535)	1.783 (0.191)	68.91
Men	1979-99	LFS (occ)	Hours	-5.022 (1.361)	2.246 (0.502)	27.98
Men	1976-95	NES (occ)	Hours	-4.732 (1.266)	1.981 (0.463)	39.10
Men	1979-99	LFS (occXind)	Hours	-5.622 (0.755)	2.337 (0.281)	45.48
Men	1976-95	NES (occXind)	Hours	-5.906 (0.618)	2.309 (0.226)	64.32

Notes: Regressions are weighted by job cell size in the initial period. Occupation uses 3-digit SOC90 codes. Industry uses 1-digit SIC80 codes.

Table IIc
The Relationship between Employment Growth and Initial Median Wage: Women

Sample	Sample Period	Data	Employment Measure	β1	β2	Fraction in Declining Section
Women	1979-99	LFS (occ)	Employment	-1.580 (1.025)	1.222 (0.505)	-
Women	1976-95	NES (occ)	Employment	-0.657 (0.686)	0.584 (0.310)	-
Women	1979-99	LFS (occXind)	Employment	-3.363 (0.840)	1.942 (0.411)	54.69
Women	1976-95	NES (occXind)	Employment	-2.227 (0.517)	1.256 (0.239)	50.95
Women	1979-99	LFS (occ)	Hours	-1.441 (1.177)	1.415 (0.597)	-
Women	1976-95	NES (occ)	Hours	-0.776 (0.815)	0.887 (0.401)	-
Women	1979-99	LFS (occXind)	Hours	-3.199 (0.934)	2.034 (0.466)	34.17
Women	1976-95	NES (occXind)	Hours	-2.650 (0.618)	1.659 (0.306)	29.58

Notes: Regressions are weighted by job cell size in the initial period. Occupation uses 3-digit SOC90 codes. Industry uses 1-digit SIC80 codes.

Table III
The Relationship between Employment Growth and Terminal Median Wage

Sample	Sample Period	Data	Employment Measure	β1	β2	Fraction in Declining Section
Men+Women	1979-99	LFS (occ)	Employment	-1.915 (0.491)	0.839 (0.166)	29.59
Men+Women	1976-95	NES (occ)	Employment	-2.920 (0.387)	1.090 (0.127)	54.96
Men+Women	1979-99	LFS (occXind)	Employment	-0.581 (0.289)	0.403 (0.097)	-
Men+Women	1976-95	NES (occXind)	Employment	-2.416 (0.231)	0.915 (0.076)	50.36
Men+Women	1979-99	LFS (occ)	Hours	-1.651 (0.519)	0.806 (0.171)	17.22
Men+Women	1976-95	NES (occ)	Hours	-2.770 (0.433)	1.101 (0.140)	38.97
Men+Women	1979-99	LFS (occXind)	Hours	-0.271 (0.286)	0.356 (0.094)	-
Men+Women	1976-95	NES (occXind)	Hours	-2.506 (0.264)	1.003 (0.085)	37.21

Notes: Regressions are weighted by job cell size in the terminal period. Occupation uses 3-digit SOC90 codes. Industry uses 1-digit SIC80 codes.

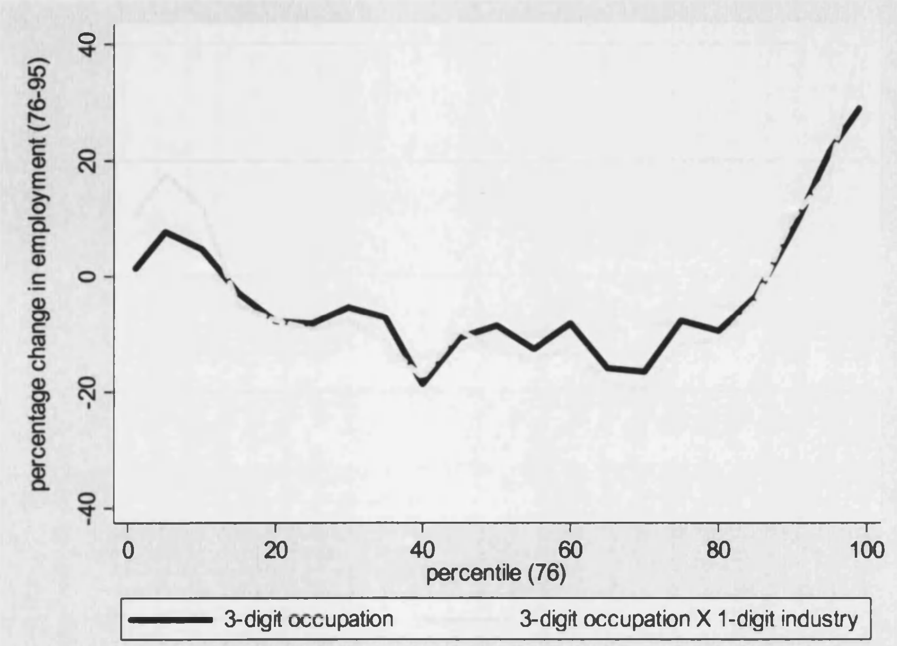
III.B An alternative approach: Juhn, Murphy and Pierce

So far we have defined the quality of a job by the median wage in that job. Although this approach has the virtue of simplicity in that it enables us to label specific jobs as 'good' or 'bad' it does ignore the fact that there is substantial wage dispersion within jobs. One approach to dealing with this is taken by Juhn, Murphy and Pierce [1993] – although that paper is better known for other contributions – and Juhn [1999].

They assume that each job (defined here as an occupation) potentially uses labor from each percentile of the wage distribution. They compute the share of labor from each percentile used in each job in a base year. Assuming that these 'factor shares' remain constant, one can then predict changes in employment for each percentile of the wage distribution by allowing changes in the total levels of employment in each occupation. Note that now the same job potentially contributes to the predicted change in employment for each percentile rather than contributing only once as in the single-index approach taken above.

Figure III plots these predicted employment changes between 1976 and 1995 for different percentiles of the 1976 wage distribution. As Figure III shows, employment growth is positive for all workers earning less than the 11th percentile and more than the 86th percentile. Predicted growth at the top end is strongest, between 35% and 45%. Growth at the 5th percentile is between 8% and almost 20% percent whereas employment in the middling jobs is in decline. The conclusions derived are therefore the same as those derived from our more simplistic approach in the previous section that there has been increased polarization in the quality of jobs. It is noteworthy that only in the top 3 deciles does one see evidence of the positive relationship between skill and employment change as predicted by SBTC.

Figure III
The Impact of Job Polarization on Employment Growth by Wage Percentile



Notes: Data are taken from the NES using 3-digit SOC90 codes. Employment changes are taken between 1976 and 1995. Percentiles are the 1976 wage density percentiles.

III.C. Employment growth by occupation

What are the sorts of jobs that are growing and declining? Table IV presents a 'top 10' by job growth for occupations that have cells of a respectable size using the LFS data[8]. The first column specifies the occupation. Since growth in the best jobs has been stronger than employment growth in the bad jobs, most of the jobs reported in Table IV pay above median hourly wages as can be seen from the second column in the table. Columns three and four report estimated employment levels by occupation and the final column calculates the percentage change in employment between 1979 and 1999.

[8] There are some dangers in doing this as the occupations at the extremes of the employment change distribution are quite likely to be ones for which a number of factors reinforce each other.

Most of the 'top 10' rapidly-growing jobs are specialized occupations mainly in finance and business service industries located at the top end of the wage distribution. But positions 1, 6 and 7 in the 'Top 10' however are taken by low paid jobs – care, education and hospital assistants. And, just outside the top-10 one finds large increases in the number of hotel porters, merchandisers, window dressers and travel and flight attendants, among other low-paid service occupations that are intense in non-routine manual tasks.

To document this, Table V lists the 10 lowest paying jobs given they are of considerable size, their median wage and employment in 1979 and 1999. The biggest absolute increase in those jobs listed has been for sales assistant and checkout operators. Given the emphasis in the literature on SBTC the presence of the good jobs in Table IV is probably no surprise but strong growth in many bad jobs in Table V might be more surprising. However, this pattern is exactly what we would expect to see according to the ALM hypothesis as the rapidly-growing lousy jobs are all ones where it has proved difficult to substitute machines or computers for human labor. To see further evidence supportive of the ALM hypothesis Table VI lists the bottom 10 jobs by job growth. A comparison of the median job wages with the overall median suggests the decline in jobs has been largest for middling jobs in manufacturing occupations.

So far we have presented evidence for job polarization as an important phenomenon in the UK over the past 25 years and suggested that the pattern of employment changes is broadly consistent with the ALM view of the impact of technology on the demand for labor rather than the simple SBTC hypothesis. But, job polarization could be driven by factors other than technology. The next section provides a discussion of these issues.

Table IV
Top 10 Occupations by Job Growth

Occupation	*Median wage in 1979*	*Employment in 1979*	*Employment in 1999*	*% change in employment*
All	3.052	24 332 613	27 343 467	12.373
Care assistants & attendants	2.345	103 837	539 407	419.474
Software engineers	5.008	34 009	171 769	405.065
Management consultants & business analysts	4.745	18 811	81 803	334.868
Computer systems & data processing managers	5.065	43 239	178 701	313.286
Computer analysts & programmers	4.842	76 083	302 617	297.745
Educational assistants	2.272	45 040	173 763	285.793
Hospital ward assistants	2.572	7 460	26 986	261.705
Actors, entertainers, stage managers & producers	4.719	22 549	73 030	223.870
Treasurers & company financial managers	5.105	37 794	119 812	217.015
Financial institution and office managers	4.511	107 138	322 608	201.114

Notes: Employment data are taken from the LFS using 3-digit SOC90 codes. Wages are 1979 median hourly wages taken from the NES using 3-digit SOC90 codes.

Table V
Bottom 10 Occupations by Median Wage

Occupation	*Median wage in 1979*	*Employment in 1979*	*Employment in 1999*	*% change in employment*
All	3.052	24 332 613	27 343 467	12.373
Hairdresser & barbers	1.745	123 986	96 073	-22.513
Bar staff	1.832	119 455	188 319	57.647
Shelf fillers	1.938	49 699	97 144	95.462
Sales assistants	1.939	954 200	1 321 251	38.466
Retail cash desk & check-out operators	1.969	112 816	218 581	93.749
Petrol pump forecourt attendants	1.979	13 304	9 935	-25.321
Kitchen porters	2.003	178 758	143 092	-19.952
Waiters & waitresses	2.020	124 780	187 391	50.177
Cleaners	2.132	854 535	649 362	-24.009
Beauticians	2.145	24 536	28 946	17.972

Notes: Employment data are taken from the LFS using 3-digit SOC90 codes. Wages are 1979 median hourly wages taken from the NES using 3-digit SOC90 codes

Table VI
Bottom 10 Occupations by Job Growth

Occupation	*Median wage in 1979*	*Employment in 1979*	*Employment in 1999*	*% change in employment*
All	3.052	24 332 613	27 343 467	12.373
Boring & drilling machine setters & setter-operators	3.584	29 276	1 731	-94.086
Coal mine laborers	3.696	29 782	1 818	-93.892
Face trained coalmining workers, shotfirers & deputies	5.237	76 301	5 095	-93.322
Grinding machine setters & operators	3.557	56 426	8 164	-85.531
Laborers in foundries	3.219	14 801	2 505	-83.070
Laborers in engineering & allied trades	3.025	58 243	12 758	-78.095
Electrical, energy, boiler & related plant operatives & attendants	3.684	36 352	8 009	-77.968
Spinners, doublers & twisters (in textiles and tannery process operatives)	2.802	16 941	4 173	-75.363
Originators, compositors & print preparers (in printing and related trades)	3.404	48 878	12 162	-75.116
Rail signal operatives & crossing keepers	3.010	13 761	3 571	-74.045

Notes: Employment data are taken from the LFS using 3-digit SOC90 codes. Wages are 1979 median hourly wages taken from the NES using 3-digit SOC90 codes.

IV. ALTERNATIVE HYPOTHESES FOR JOB POLARIZATION

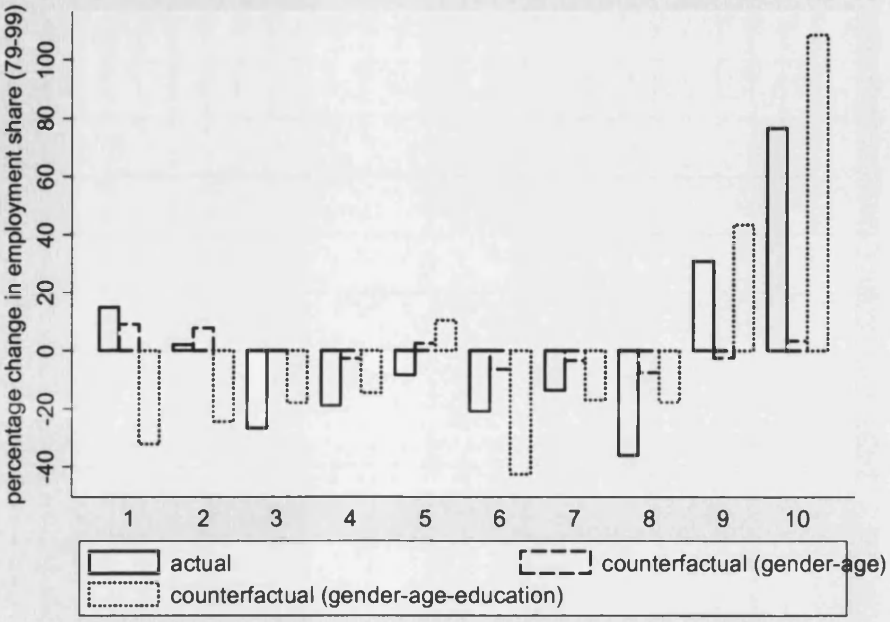
IV.A. Changes in labor supply

It is possible that changes in the structure of the labor force can explain some of the changes in the occupational structure of employment. The most important such changes are the increased feminization of the labor force and the increase in educational attainment though changes in the age structure and the proportion of immigrants could also conceivably be important. To assess the importance of these changes we did the following counter-factual exercise. First, we divided the labor force into cells (described in more detail below). Then keeping the initial occupational structure of employment within cells constant, we computed what the change in occupational employment would have been if the only change was the changing relative size of the cells in the overall labor force.

Figure IV shows the results for the changes divided by deciles. The first column in each decile shows the actual change in employment in each decile (this is the same as Figure I). The second column then shows the predicted change in employment in each decile when the labor force is divided into 2 gender and 12 age cells[9]. The predicted changes in the occupational structure are small compared to the actual. There is a small predicted rise in employment in the lousy jobs that is primarily caused by the increasing proportion of women in the labor force who are concentrated in the lowest-wage jobs. However this counter-factual takes no account of the substantial occupational up-grading of women over this period so is likely to over-state the true contribution of the feminization of employment to job polarization.

[9] We do not include immigrants as a separate category as the fraction of foreign-born in the UK labor force only rose from 7.3% in 1979 to 8.9% in 1999, making it a rather unimportant factor.

Figure IV
The Impact of Changes in the Composition of the Labour Force



Notes: Employment data are taken from the LFS using sing 3-digit SOC90 codes. Employment changes are taken between 1979 and 1999. Quality deciles are based on 3-digit SOC90 median wages in 1979 taken from the NES. The first bar gives actual employment changes as in Figure I. The second bar gives counterfactual employment changes keeping the 3-digit occupational composition within 24 gender-age cells constant over time. The third bar gives counterfactual employment changes keeping the 3-digit occupational composition within 96 gender-age-education cells constant over time.

The obvious omission from the above counter-factual is education: there has been

very substantial educational up-grading over this period (for

very substantial educational up-grading over this period (for example, the fraction of

increased from (

).25 in 1979 to 0.55 in 1999). The final column for each decile in

Figure IV show

s what happens if one divides the labor force into four educational lovely jobs it is unhelpful in explaining the growth in lousy jobs. The conclusion must be that changes in the structure of labor supply are unable to explain the broad pattern of job polarization.

IV.B. Changes in labor demand other than technology

Changes in the occupational structure of employment may also be caused by changes in the demand for different sorts of labor that are not caused by technology. Here we briefly discuss two of these: trade and the structure of product demand.

The role of international trade and out-sourcing has been a perennial 'alternative' hypothesis to technology as a potential explanation of changes in wage inequality. It undoubtedly has been important for some occupations (for example, the large decline in 'spinners, doublers and twisters' seen in Table VI is the result of a continuing shift of textiles to countries where labor is cheaper). And trade may be more important in the future e.g. with the out-sourcing of more skilled jobs. But the overall assessment of Freeman (2003) is that trade has had much smaller impacts on labor markets than commonly believed[10]. We are not going to investigate this in detail here but one would have to oppose this conclusion to argue that trade was the most important factor behind job polarization.

Perhaps potentially more important are changes in the structure of the demand for different products that then have consequences for the demand for different occupations. For example the rise in the number of care and hospital assistants seen in Table IV is partly the result of more old people and more old people being cared for outside the family. But it is important to realize that technology also plays a very

[10] This is consistent with many other studies. For example, Feenstra and Hanson [1998] estimate that expenditures on high-technology capital such as computers are about twice as important as outsourcing in explaining variation in relative wages of non-production workers in the US between 1979 and 1990. And, Borjas, Freeman and Katz [1997] find that immigration has had a larger impact on the skill composition than trade in the US between 1980 and 1990.

important role here as the increase in the demand for care has not been met by any great improvements in the productivity of caring because of the difficulty in applying technology to non-routine tasks[11]. And the dramatic decline in many jobs in manufacturing seen in Table VI is mostly the result of relatively inelastic consumer demand together with the rapid productivity growth in those sectors where products are produced in ways that have proved relatively easy to routinize.

In sum, none of the other hypotheses considered here seem to have the ability to explain the basic feature of job polarization though they are undoubtedly important for some specific occupations. In contrast, the ALM hypothesis does seem to have this broad explanatory power. But, before we uncritically accept the ALM hypothesis, we need to understand the evidence that is often quoted in support of SBTC. The next two sections consider two of these – the growth in non-manual employment and the rise in the educational attainment of the workforce.

V. UNDERSTANDING THE GROWTH IN NON-MANUAL EMPLOYMENT

A number of papers (e.g. Berman, Bound and Griliches [1994], Berman, Bound and Machin [1998], Machin and Van Reenen [1998]) have presented evidence that employment has shifted towards non-manual jobs and that this shift has been much more important within than between manufacturing industries. As non-manual jobs tend to be better-paid than manual jobs this is interpreted as evidence that technical change is biased towards more skilled workers. And the fact that most of the shifts are

[11] For example the Economist of March 13th 2004 quoted the inventor of the first industrial robot (Unimate, employed by General Motors in 1961), Joe Engelberger as saying that care of the elderly is the opportunity the robotics industry should be pursuing as "every highly industrialized nation has a paucity of help for vast, fast-growing ageing populations".

within industries suggests that this trend is related to technical change that has very pervasive effects on all sectors of the economy. As a statement about the average quality of jobs, this conclusion is undoubtedly right: our data also suggest that the 'average' job quality is increasing. But the simple binary distinction between manual and non-manual is simply not able to capture the increased polarization we have also argued is important[12].

If the shift-share analysis is done for broader occupation groups and for the whole economy, not just manufacturing, we get the results presented in Table VII. For each occupation Table VII reports a manual/non-manual indicator taken from the LFS (*M* or *NM* respectively). Occupations are ranked by their median wage. Then, for each of the two data sets, the first column reflects the total percentage point change in the share of each occupation group between 1979 and 1999. The second column measures the percentage point change due to changes within industries whereas the final column reports the change due to workers moving between industries.

The results are rather more nuanced than earlier studies would suggest and in line with the ALM hypothesis. There is a large increase in the employment shares of managerial and professional workers, an increase in lovely jobs that is mostly 'within'

[12] Indeed any binary distinction between low- and high-skill workers (whether in theoretical or empirical work) cannot have separate mean and variance effects.

Table VII
Shift-share Analysis of Employment Shares by Occupation

Occupation	*Wage*	*NES*			*LFS*		
		total	*within*	*between*	***Total***	*within*	*between*
Professional occupations *(NM)*	5.914	1.709	1.127	0.582	3.733	2.838	0.895
Managers and administrators *(NM)*	4.117	5.204	4.588	0.616	5.606	5.271	0.335
Associate professional and technical occupations *(NM)*	3.823	2.579	1.700	0.879	4.466	3.446	1.020
Craft and related occupations *(M)*	3.277	-8.158	-3.738	-4.420	-7.883	-3.461	-4.422
Plant and machine operatives *(M)*	3.055	-5.579	-1.809	-3.770	-5.195	-1.362	-3.833
Clerical and secretarial occupations *(NM)*	2.841	1.291	-1.879	3.171	-2.105	-5.388	3.283
Personal and protective service occupation *(NM/M)*	2.668	3.516	1.969	1.547	3.502	1.732	1.770
Other occupations *(M)*	2.558	-2.527	-2.775	0.248	-3.398	-3.564	0.166
Sales occupations *(NM)*	2.132	1.964	0.817	1.147	1.272	0.487	0.785

Notes: Employment changes are taken between 1979 and 1999 for the LFS and 1976-1995 for the NES. Reported wages are 1979 median hourly wages taken from the NES using 1-digit SOC90 occupations. The decomposition is done using 1-digit SIC80 industry codes.

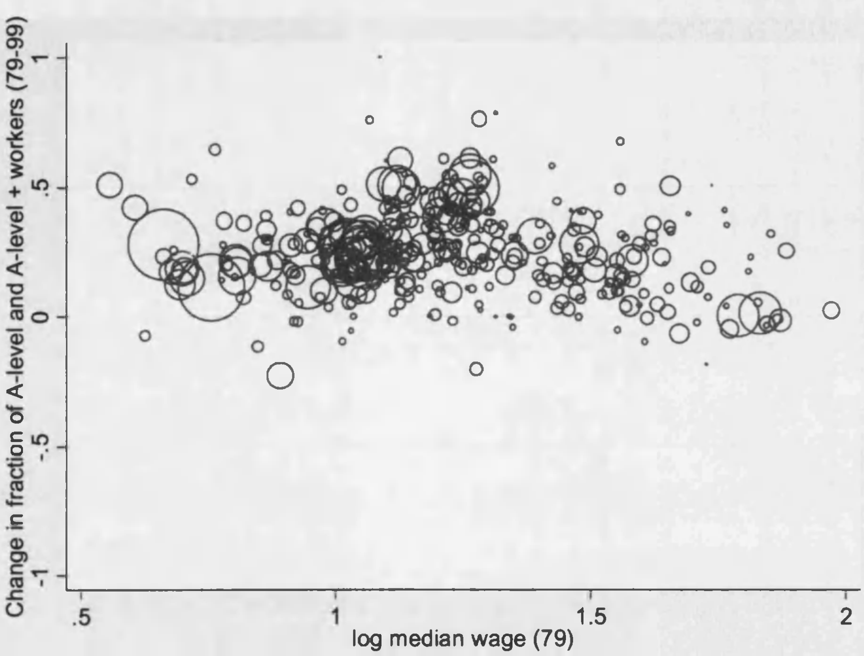
industries. Both craft workers and machine operatives have large negative within and between components reflecting both the impact of technical change and the shift towards services. Routine clerical occupations have large negative employment effects within industries together with a sizeable positive 'between' component reflecting the shift to services. The increase in the employment share of low-paid personal and protective services and sales occupations has a large within and between component reflecting the fact that technology has not managed to do these jobs and the shift toward services.

Therefore studies that use a simple manual/non-manual split (usually out of necessity rather than choice) and concentrate on manufacturing miss important features of the way the structure of employment is evolving. If one broadens one's view then one does see evidence for the ALM hypothesis.

VI. EDUCATION AND OCCUPATION

Another piece of widely cited evidence in favor of the SBTC hypothesis is that there has been a rapid increase in the level of educational attainment together with a rise in the returns to education. It is true that there is a lot of evidence that the average educational attainment of workers within jobs has changed. The evidence in Figure IV that the assumption of a fixed occupational structure within education groups predicts a fall in lousy jobs when there has been a rise implies a rise in the educational attainment of workers in low-wage occupations. A more direct way of seeing this is Figure V that shows the change in the fraction of workers that have education at 'A' level or above – at least 12 years of education – for each occupation. Almost all occupations show an increase, evidence of educational up-grading within occupations

Figure V
Change in Fraction of Workers by Education and Job Median Wage



Notes: Employment data are taken from the LFS using 3-digit SOC90 codes.

as the changes are above the horizontal axis.[13]

There are two interpretations of these findings. First, that what we have defined as a 'job' is not constant over time and the educational and/or skill requirements within jobs has risen possibly because of SBTC within jobs. Secondly, that as the educational attainment of the labor force has increased and middling jobs become relatively scarcer some educated workers have been forced to take lousier jobs than previously – this is the idea of the literature on over-qualification (see, for example, Sicherman [1991], Hartog [2000] and, for the UK, Green et al. [1999], Chevalier [2000] and Green and McIntosh [2002]) that typically finds that high proportions of people report that they are employed in jobs for which their educational qualifications are unnecessary. Employers may also respond by raising the minimum educational

[13] The rise in this proportion is smallest in some of the high-wage jobs but this is because the proportion of educated worker sin these jobs was already close to one in the 1970s leaving little scope for educational up-grading using 'A' levels as the cut-off.

standards to get certain jobs – what is known as credentialism. To distinguish these two hypotheses requires some information on changes in skill use within occupations. This is not so easy to find but we present two pieces of disparate information relevant to the question.

First, consider the data on the use of the five DOT measures used by Autor, Levy and Murnane [2003]. Table VIII presents data on the average level of skill use in 1977, the change from 1977 to 1991 and the decomposition of this change into a within-occupation and a between-occupation component. Panel A of Table VIII pools all occupations together and shows an overall increase in non-routine cognitive and interactive tasks together with a decrease in routine tasks (especially cognitive ones) and a smaller decrease in non-routine manual tasks. But, the decomposition suggests that, within occupations, there is only a rise in the non-routine interactive task and all other skills show declines, the decline being particularly large for the routine cognitive task. But from the point of view of educational up-grading it is what is happening in the lousy jobs that is perhaps of more interest. Panel B therefore does the same exercise for jobs in the bottom half of the wage distribution. Again one sees a big rise in the non-routine interactive task and large declines in routine tasks. But, most of the increase in skill requirements is between-occupation: within-occupation task requirements are generally falling. There is little evidence here that there is substantial SBTC within occupations (Spenner [1983] reaches similar conclusions).

Our second piece of evidence on changing skill requirements within occupations comes from the UK Social Change and Economic Life Initiative (SCELI) survey conducted in 1986 and the 2001 Skills Surveys[14]. Both of these surveys asked

[14] We are grateful to Francis Green for doing these computations for us.

Table VIII
DOT Task Shifts Within and Between Occupations

DOT Task Measure	*Mean 1977*	*Change 1977-1991*		
Panel A: Good and Bad Occupations				
		total	*within*	*Between*
Non-Routine Cognitive	3.755	0.084	-0.047	0.131
Non-Routine Interactive	2.417	0.504	0.137	0.367
Routine Cognitive	4.582	-0.854	-0.564	-0.290
Routine Manual	3.901	-0.146	-0.025	-0.121
Non-Routine Manual	1.198	-0.132	-0.094	-0.038
Panel B: Bad Occupations				
Non-Routine Cognitive	3.338	-0.027	-0.106	0.079
Non-Routine Interactive	2.169	0.367	0.019	0.348
Routine Cognitive	3.929	-1.116	-0.871	-0.245
Routine Manual	3.879	-0.224	-0.065	-0.159
Non-Routine Manual	0.847	-0.037	-0.032	-0.005

Notes: Task inputs are measured as in ALM [2003] and are between 0 and 10. For Panel A, the reported means are weighted using 463 3-digit COC occupations. Panel B uses 208 occupations with hourly earnings below overall average wages using 1984 CPS data. Changes between 1977 and 1991 are measured using 3-digit COC occupations and employment changes between 1984 and 1997.

workers about the educational qualifications necessary to get the job they do and were then also asked whether these qualifications were necessary to do the job. Only data at the 1-digit occupation level are comparable in the two datasets. Table IX presents some relevant information. The second column gives the change from 1986 to 2001 in the educational qualifications needed to get a job where qualifications are measured on a 5-point scale with 1 representing no qualifications and 5 a college degree. In all occupations there is a rise in the level of qualifications required, with a very large rise in sales occupations and elementary occupations. This could reflect greater skill requirements within occupations or a greater use of credentialism. There is evidence (shown in the third column) that more workers report in 2001 that the education required to get the job is not necessary to do the job but, in the absence of any information on the extent to which education is under-utilized, one cannot know whether this effect is large enough to outweigh the positive effect on skill levels of an increase in the level of education required.

These two pieces of disparate evidence are not entirely consistent. The DOT data do not suggest any significant skill up-grading within occupations, while the SCELI/SS data suggest an increase in the level of education required by employers although also an increasing proportion of workers reporting that this education is unnecessary to do the job. But, it does seem that the supply of skills may be increasing faster than the demand in the bottom half of the distribution because the extent of over-qualification does not seem to be falling over time and, according to some estimates (e.g. Felstead, Gallie and Green [2002]), is actually increasing.[15]

[15] It has always been something of a puzzle to reconcile these findings of widespread over-qualification with rising employment and relative wages of educated workers. Our finding of increased job polarization can explain why both phenomena may co-exist. The increased supply of skills that has been necessary to meet the increased number of lovely jobs poses a problem for the increased number of lousy jobs. Because there has been an increase in the mean but no increase in the variance of educational qualifications, those in lousy jobs are increasingly likely to have higher levels of education than necessary for doing the job.

Table IX
Changes in Skill Requirements within Jobs, 1986-2001

Occupation	*Change in education level required to get job, 1986-2001*	*Change in fraction reporting required education not necessary to do job, 1986-2001*
Managerial	0.25	0.014
Professional	0.12	0.021
Associate Professional	0.31	0.072
Clerical	0.10	0.046
Craft	0.25	0.043
Personal Services	0.50	0.110
Sales	0.54	0.093
Operatives	0.08	0.063
Elementary	0.24	0.076

Notes: Data come from 1986 SCELI data and 2001 Skills Survey. Education required is measured on a 5-point scale with 1 being no qualifications and 5 a college degree.

Consistent with this, Felstead, Gallie and Green [2002] report that there is excess demand for workers with no qualifications, an excess supply of people with low-level qualifications and a rising use of credentialism among the lowest-level occupations.

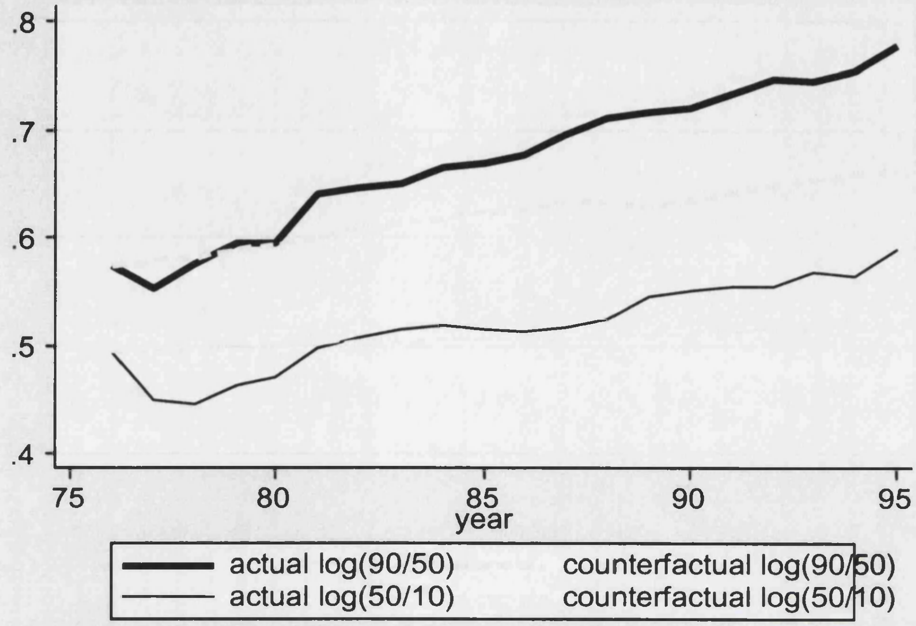
VII. JOB POLARIZATION AND THE RISE IN WAGE INEQUALITY

All the analysis so far has been about the quantity side of the labor market – what is happening to the employment of different types of workers. But, the polarization of employment could also be expected to have led to increased wage inequality. Of some interest is what fraction of the rise in wage inequality can be explained by this polarization of employment. This is the subject of this section.

In Figure VI we present the evolution of two measures of actual wage inequality over the period 1976-1995, the log(90/50) and the log(50/10) differentials, as well as a prediction of what would have happened if the only change in the wage distribution taking place is the change in the distribution of jobs in the economy. To this end we assign everyone in the base year (here, 1976) a weight that is equal to the total number of workers in a job in a given year divided by the job cell size in the initial period. We then compute counterfactual percentiles of the re-weighted wage distribution. As is well-known actual wage inequality rose very strongly in this period following a fall in 1977 (the result of the 'Social Contract' incomes policy then in place). The rise in inequality is somewhat larger at the top of the distribution than at the bottom. Since the counterfactual log median increases only very little, the rises in the counterfactual log(90/50) and log(50/10) reflect large polarization. In comparison with the actual changes, increased job polarization can explain 33% of the increase in the log(50/10) differential between 1976 and 1995 and 54% of the increase in the log(90/50) wage differential. It should also be noted that this process of polarization seems relatively smooth throughout the period: one cannot readily identify sub-periods in which all the change occurred. The remaining rise in wage inequality can be thought of as coming from one of two sources: differential changes in median wages across jobs and within-job wage inequality. For example, wage inequality will rise if median wages have risen faster in good jobs than bad jobs. The other potential source of increased wage inequality is an increase in within-job pay dispersion. To look for evidence of this Table X reports regression estimates of median wage growth onto the log of the initial median wage[16]. Since estimates will be biased downwards when using the initial wage on both sides of the regression equation, we use the NES

[16] We experimented with the inclusion of a quadratic term but this was never significant.

Figure VI
How Much of Actual Wage Dispersion Can Be Explained by Job Polarization?



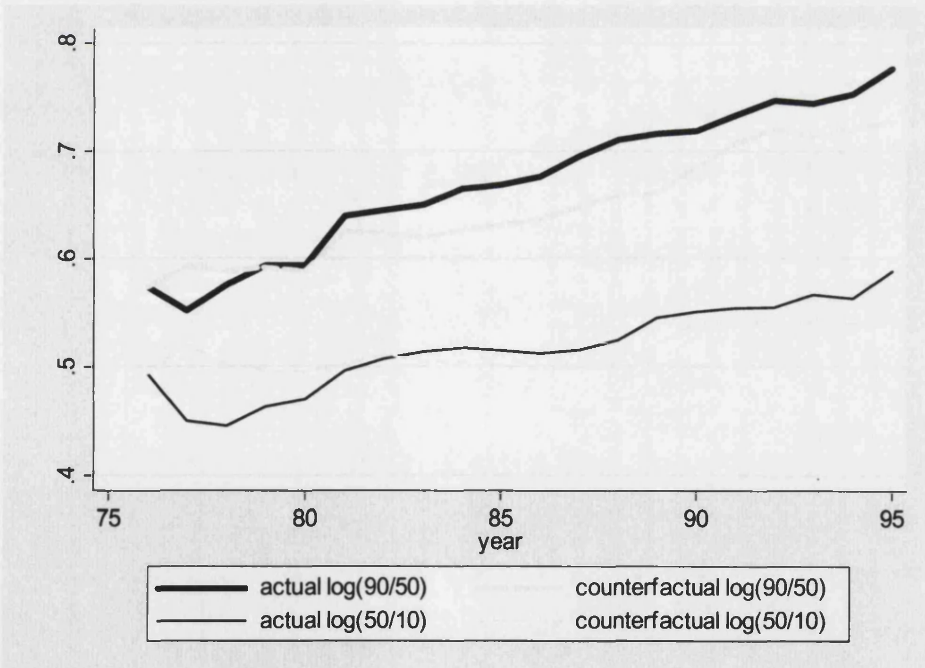
Notes: Data are taken from the NES. The figure uses 3-digit SOC90 codes as the definition of a job. The counterfactual keeps constant median wage and wage dispersion within occupations.

Table X
Relationship between Wage Growth and Initial Median Wage

Sample	Sample Period	Data	Relative to 1976	Relative to 1977
Men+Women	1976-95	NES (occ)	0.239 (0.029)	0.267 (0.029)

and run regressions using wages in 1977 (rather than 1976) as a covariate. All point estimates are positive and all are statistically significant. These results suggest allowing median wages to change over time while keeping the variance of pay within each job constant could close the actual-counterfactual gap further. The implications for wage inequality are presented in Figure VII. Here, we do the re-weighting described earlier and also adjust wages in every job cell by the change in log median wage in that cell. Now 51% of the increase in the log(50/10) differential between 1976 and 1995 and 79% of the increase in the log(90/50) wage differential can be explained.

Figure VII
The Impact of Job Polarization and Changing Relative Wages Across Jobs on Wage Inequality



Notes: Data are taken from the NES. The figure uses 3-digit SOC90 codes as the definition of a job. The counterfactual keeps constant wage dispersion within occupations but allows the actual median wage to vary in line with the data.

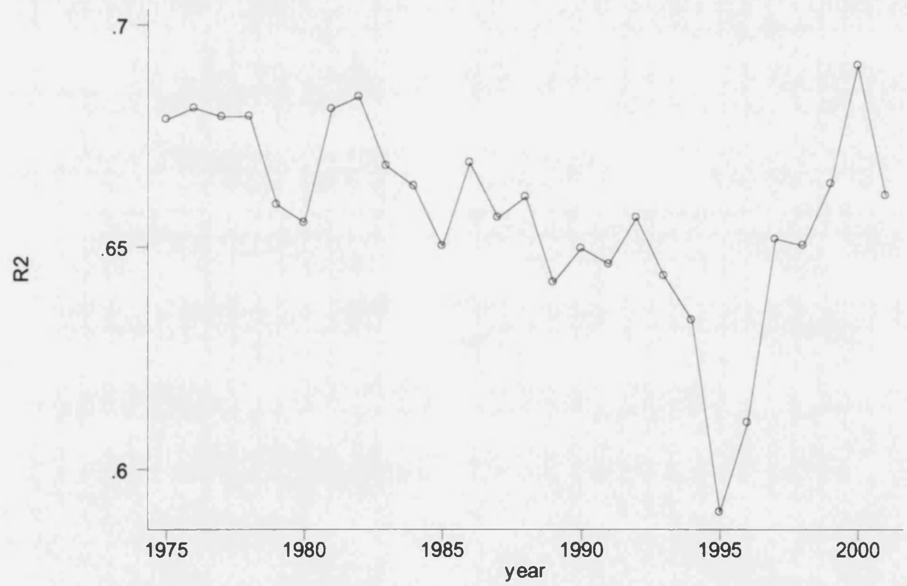
VII.A Within and between group wage inequality

One implication of this is that the rise in within-job wage inequality has a relatively minor part to play in explaining the overall rise in wage inequality. This is in contrast with some studies that try to explain wage inequality in terms of age and education that typically find that most of the rise in inequality is due to rising within-group wage inequality (see Levy and Murnane [1992] and Katz and Autor [1999] for a survey of the US literature and Machin [2003] for the UK). The studies are correct given the variables they use to try to explain the rise in wage inequality but the evidence here suggests that this conclusion is sensitive to how the groups are defined. Unfortunately, a small industry has been established based on the premise that wage inequality has risen very markedly among 'identical' workers and has been building theoretical explanations of this 'fact'.

One particularly simple way to understand this is to consider what is happening to the R^2 in earnings functions. Figure VIII graphs the R^2 from an earnings function estimated for each year on the NES in which the dependent variable is log hourly earnings, and the covariates include a complete set of dummies for age, industry and occupation, all interacted with gender. There are two things to note: first the R^2 is high – averaging almost two-thirds – compared to the one-third found in a standard specification using the US CPS. Secondly, there is no marked trend in the R^2 over time if one also includes the most recent years for which data are available (we believe this is also true for the US CPS, see for example Lemieux [2002]). The consequence is that the rise in the residual variance can explain only 1/3 of the total rise of the variance in log wages.

The conclusion that the importance of within-group wage inequality depends on the controls one includes in an earnings function seems also consistent with US studies that have more detailed controls than is usual in earnings functions. For example, Dunne et al. [2000] have controls for establishment fixed effects (which are obviously better than industry) and find they can explain much of the rise in wage inequality by widening between-plant wage gaps. It seems likely that much of these wage gaps between plants can be explained in terms of the characteristics (in gender, age, education and occupation) of the workers within them – for example Hellerstein at al. [1999] find that a fairly rudimentary set of controls (less than 30) can explain 40% of the variation in average wages across establishments.

Figure VIII
The Changing R^2 in the UK Earnings Function



Notes: Data are taken from the NES. The dependent variable is log hourly earnings and the covariates included are age, industry and occupation dummies, all interacted with gender. The demand for bad jobs has not resulted in a rise in wages at the bottom relative to the median. The rise in the number of bad jobs has coincided with a decline in their pay not just relative to the good jobs that are increasing in number but also relative to the middling jobs that are decreasing in number. If the labor market is competitive this does not seem consistent with a view in which technology causes a shift in the demand for different types of labor but the supply curve is stable and the observed changes in wages and employment are simply movements along this supply curve[17].

In a competitive labor market it is wages in different segments that determine the position on the supply curve, so to reconcile the observed increase in relative employment in the worst jobs together with a fall in their relative wages one would have to try to explain how a fall in wages at the bottom end of the labor market increases labor supply in that part of the labor market. While this might not be impossible (e.g. the labor supply curve could conceivably be backward-bending) it does not seem especially plausible. It is perhaps more plausible to think that the labor supply curve is not stable and the labor force has changed in a way that has increased the number of workers who typically do lousy jobs. But, the results in section four suggest that the changing composition of the workforce can only explain a small part of changing wage distribution.

It is possible that the relative skill requirements of middling and lousy jobs have been changing. For example Figure V suggests somewhat greater educational up-grading in middling than in lousy jobs. If this is the case then the average level of human capital may have risen in middling relative to lousy jobs and this can account for the relative wage movements.

[17] The discussion paper version of this chapter – Goos and Manning (2003) presents a simple three-skill competitive model of the labor market that can be used as a more formal justification for the discussion that follows. Juhn [1994] presents a model in which she claims that a fall in the demand for 'middling' jobs reduces wages more at the bottom but hers is really only a model with 2 types of skill but 'middling' people with some combination of both skills. In this example there is no well-defined sense of a fall in demand for middling jobs.

Another possible explanation for why wages have been falling in lousy jobs relative to those in 'middling' jobs is to think of the labor market as being non-competitive in some way. There are a number of ways in which this could be done.

For example, Acemoglu (2001) presents a model of a labor market with frictions in which an increase in the supply of skilled workers encourages employers to create more lovely and lousy jobs and fewer middling jobs. In this type of model 'supply creates its own demand' and there is no need to resort to demand shocks caused by technological change to explain job polarization. But it is a little bit hard to see how supply shocks of this type can explain the pattern of changes in occupational employment documented above – technology seems much more plausible as an explanation for these changes. But, the Acemoglu story may have some relevance for explaining what is happening within occupations when employers often have a decision about what level of skill to require of workers doing these jobs.

Another 'non-competitive' explanation is that institutions have changed in such a way as to lead to a fall in wages at the bottom end of the wage distribution. There is now a small literature in the US (DiNardo at al. [1996], Lee [1999], Teulings [2000]) that suggests that the evolution of unionization and the minimum wage can do a very good job in explaining what is happening to the bottom half of the wage distribution. The UK has also seen a marked decline in unionization, a decline in minimum wages (though they were never very strong) and the indexation of welfare benefits to prices not wages. Perhaps these changes can account for the rise in wage inequality in the bottom half of the distribution in the 1980s. We leave the further exploration of this to another paper.

VIII. CONCLUSIONS

There is little doubt that technology has a powerful impact on the labor market. But, the dominant current view about the nature of its impact, the hypothesis of skill-biased technical change is only a partial truth and cannot explain all of the important changes in the labor market (see Card and DiNardo [2002] for an additional list of puzzles and problems). Crudely, the SBTC hypothesis seems best able to explain what is happening in the top half of the wage distribution but not its bottom half. There, the more nuanced view about the impact of technology proposed by Autor, Levy and Murnane [2003] seems appropriate and it seems plausible that demand for 'middling' jobs has fallen. This chapter has provided UK evidence of increased job polarization that is consistent with the ALM hypothesis. It would be interesting to know whether similar phenomena can be observed in the Continental European countries that have not had the rises in wage inequality seen in the UK and the US.

CHAPTER II

THE IMPACT OF SHOP CLOSING HOURS ON LABOR AND PRODUCT MARKETS

INTRODUCTION

Over the past forty years, the majority of restrictions on shop opening hours have been repealed or declared unconstitutional in most US states, the UK, Canada and some European countries.[18] Often, proponents and opponents of liberalizing opening times have based their arguments on their expectations of employment effects from deregulation. Using that different US states deregulated their Sunday Closing Laws at different points in time and building on what we know about the structure of retail markets, it is therefore the aim of this chapter to shed some light on how deregulation most likely affects retail labor and product markets and therefore ultimately employment.

This chapter first examines the most commonly imposed restriction on US shop opening hours known as Sunday Closing Laws or Blue Laws.[19] The impact of deregulation on employment, sales and the number of shops is then analyzed in two ways. First, given that between 1977 and 1992 ten states deregulated at different points in time, this chapter uses the Census of Retail Trade data for years 1977, 1982, 1987 and 1992 to show that deregulation increases employment with 4.4 to 6.4 percent, revenue with 3.9 to 10.7 percent and the number of shops with 1 to 1.5 percent in deregulating industries. Second, given that no states deregulated after 1992, this chapter then uses the 1997 Census of Retail Trade to further test the hypothesis that employment, revenue and the number of shops in regulated industries are

[18] For example, see Section I for a discussion of the history of Sunday Closing Laws in the US, Maher [1995] for the UK, Skuterud [2004] for Canada and Kajalo [1997] for Europe.

[19] There is disagreement about the origin of the term "Blue Laws". Some claim it refers to the color of the paper upon which the first laws of New Haven were printed in 1665. New Haven ordered 500 hundred copies of its laws to be printed in England. These printed laws were returned on blue-colored paper. Others claim that the blue referred to by the term "blue laws" bears testimonial to the strictness with which the laws were observed by the Puritans. Just as a "true blue" dye never fades, so a person of fixed principles will not be easily swayed to depart from them. See Laband and Heinbuch [1987] for further discussion.

significantly smaller in states with Sunday Closing Laws. This is true looking at the fraction of total retail for regulated industries or comparing outcomes per consumer or per dollar of personal disposable income. Also, it is shown that deregulation is most likely to decrease employment, revenue and the number of shops in industries exempted from Sunday Closing Laws suggesting that deregulation can also have an important impact on the composition of employment across different industries. Finally, evidence suggests these quantity adjustments seem to happen without much of an impact on wages or prices.

This chapter finally presents a model building on standard assumptions about retail markets to explain the observed impact of deregulation on employment, sales and the number of shops in deregulating and non-deregulating industries. For deregulating industries, it is argued that longer shop opening hours will increase employment because labor partially is a quasi-fixed input factor that only varies with opening times (threshold labor effect). Also, in so far the observed increase in revenue due to increased product demand reflects an increase in the volume of sales, employment will further increase (sales effect). Finally, if the increase in revenue offsets the increase in labor costs, retailers will find it profitable to extend their opening hours in the short-run. In the long-run, the number of shops will therefore increase, further increasing employment (entry effect). However, to the extend that consumers substitute income towards deregulating industries, employment will fall in non-deregulating industries because of a decrease in total spending (sales effect) and a decrease in the number of shops (exit effect). In line with the empirical evidence, these are the channels through which it is argued that deregulation affects retail labor and product markets and therefore ultimately employment.

There is a small but growing literature related to the idea that product market regulation affects employment. Blanchard and Giavazzi [2002] present models in which monopolistic competition in the product market is combined with different bargaining regimes between employers and workers. In their baseline model, real wages increase and unemployment decreases following an exogenous increase in product market competition. However, the increase in real wages implies profits will fall and therefore the number of firms will decrease in the long-run. This in turn decreases product market competition such that the impact of deregulation on employment and real wages are partly self-defeating over time. But Blanchard and Giavazzi [2002] also show that deregulation of entry restrictions are more likely to be favorable since entry of firms leads to lower mark-ups and thus lower unemployment and higher real wages even in the long-run. In line with Blanchard and Giavazzi [2002], Krueger and Pischke [1997] argue that employment growth in many European countries may have been hampered by the presence of entry costs. To this end, Bertrand and Kramarz [2002] examine spatial variation in zoning laws (regulating the start-up of companies) for French retail industries to conclude that retail employment could have been more than 10 percent higher in the absence of such laws.

This chapter argues that deregulation of store opening hours in general and Sunday opening in particular can also have a lasting impact on the distribution of employment through its impact on retail markets. As far as the employment impact of Sunday opening is concerned, two recent empirical studies are noteworthy. First, Skuterud [2004] examines Sunday opening by shops in different Canadian provinces and first uses a difference-in-difference specification to find that deregulation increases employment in deregulating industries. Starting from a conditional labor demand specification, he then concludes this employment gain is driven by an estimated increase in threshold labor that is larger (in absolute value) than an estimated negative productivity effect. But this conclusion is problematic for three reasons. First, the positive threshold labor and negative productivity effects are imprecisely estimated since both have to be identified from the sales adjusted variation in employment. Second, the negative productivity effect reflects that Sunday opening smoothes consumption across days of the week and the concavity of the production function. But below we will point to existing microeconomic evidence that the relationship between the volume of shop sales and labor is unlikely to be very concave. If this is the case, a third puzzle then is why retailers would decide to extend their opening hours if the zero estimated sales effect is not attenuated due to poor data on sales.

Second, Burda and Weil [2001] do not restrict their analysis to the impact of Sunday opening on labor demand. They present a general equilibrium model including a common leisure externality and a business poaching externality for regulation to have real effects on employment, output, wages and prices. However, since their model really is one where supply generates its own demand, employment gains are directly and exclusively predicted from an increase in labor supply, ignoring the possible employment impact derived from the impact of deregulation on competition in retail product markets.

The remainder of the chapter is organized as follows. Section I documents the US history of Sunday Closing Laws. Section II describes the main data used and Section III shows that after deregulation, employment, revenue and the number of shops are most likely to increase in deregulating industries and possibly decrease in industries exempted from such laws. Also, we do not find support for an equally strong impact of deregulation on wages and output prices. Section V then starts from what we know about retail markets to present an integrated model of retail labor and product markets that predicts the observed impact of deregulation on deregulating and non-deregulating industries. The final section concludes.

I. SUNDAY CLOSING LAWS

Sunday Closing Laws are an ancient institution in American law. The first Sunday law passed on American soil was enacted by the Colony of Virginia in 1610. By the end of the 18th Century, all thirteen colonies had Sunday closing laws written in their statutes. During the heydays of Sunday Closing Laws at the end of the 19th Century, state regulation of Sunday commerce was so prevalent that 46 states restricted at least some businesses to open on Sunday.[20] In 1961 Sunday Closing Laws were ascribed the purpose of securing a common day of rest by the United States Supreme Court, making them binding in all thirty-three states in which statutes had not been repealed or declared unconstitutional by that time.[21]

After 1961, a further twenty-five states deregulated their Sunday Closing Laws.[22] Of these, Figure I shows the ten states that deregulated after 1979 (with dates when statutes were repealed or declared unconstitutional) as well as all eight states that still have Sunday Closing Laws today.[23] It is this variation that will be exploited in this chapter to identify the impact of shop closing hours on retail labor and product markets.

---

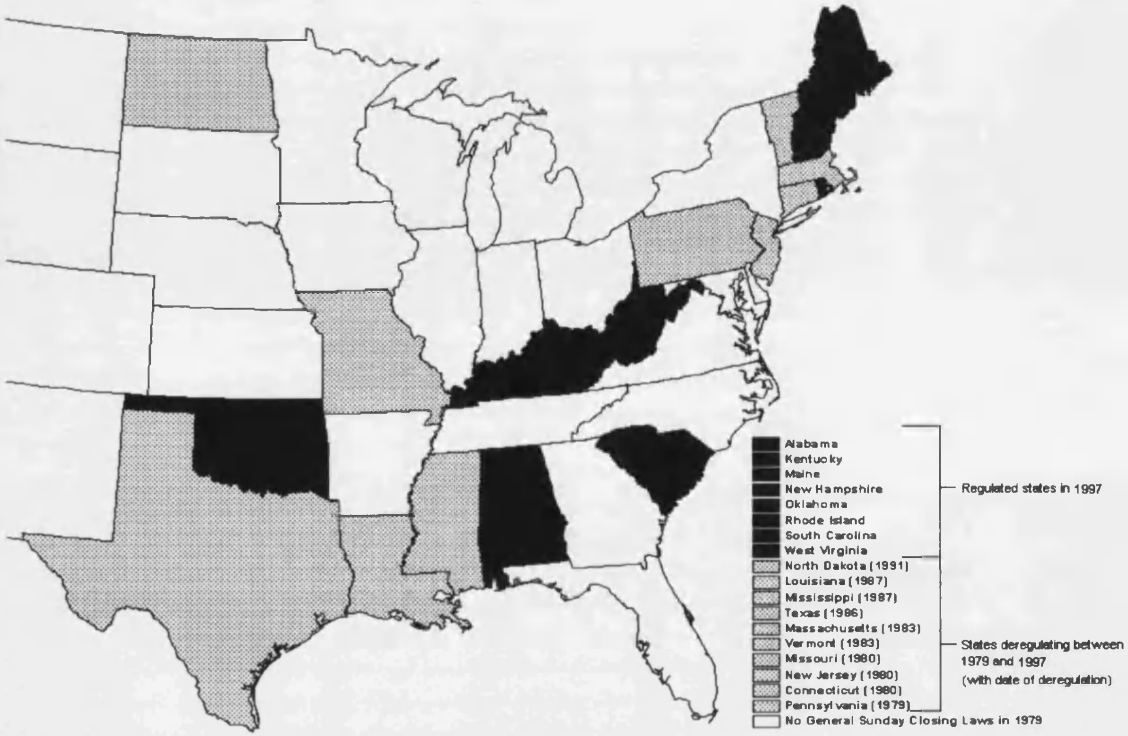
[20] Dilloff [1980] and Laband and Heinbuch [1987] provide more details on the early history of Sunday Closing Laws.

[21] See Theuman [2004] for further details of McGowan v Maryland, 366 U.S. 420 (1961) and its companion cases.

[22] Theuman [2004] further discusses state and federal cases decided since 1961 regarding the validity, construction and effect of Sunday Closing Laws.

[23] Appendices A and B contain more detailed legal references to the relevant (superseded) state statutes. Excluded are laws regulating automobile shops are shops selling alcohol on Sunday to avoid a patchwork of legislation. I would like to thank Neil Dilloff, David Laband and Duncan Alcroft for helping me find these references.

Figure I
The US History of Sunday Closing Laws, 1979-1997



II. DATA

The main data used in this study is the Economic Census of Retail Trade. The Census of Retail Trade is part of the Economic Census conducted every five years ending in 2 or 7. The strength of the Census of Retail Trade is that it aims to sample all retail activity in the US. From every survey, total weekly employment (number of paid employees in the week including March 12), real total annual revenue, the number of establishments and real total annual payroll are collected for a number of industries in retail. The procedures for data collection and dissemination can be found on the Census Bureau web-page.

The first main data set used in this chapter collects state level data for the years 1977, 1982, 1987 and 1992 from a variety of Census sources for eight SIC industries in retail. However, one difficulty is that only measures for every two consecutive years are comparable. This is due to changes in the sampling criteria and industry classification between 1977 and 1982 and changes in the industry classification between 1982 and 1987 and 1987 and 1992. To this end, the Census Bureau has made available two data sets for 1982 and 1987, one comparable with the previous sampling year and one comparable with the following sampling year.

The second main data set used in this study is the 1997 Census of Retail Trade. The advantage of this data is that it measures activity for more narrowly defined NAICS industries in retail. However, the more precise NAICS classification introduced in 1997 is no longer compatible with the SIC classifications used in earlier surveys. A second comparability problem between the 1997 Economic Census and previous waves arises because the 1997 Census of Retail Trade for the first time includes some new store types such as computer shops, office supply dealers, building material stores and other wholesalers generally open to the public. Nevertheless, given that deregulation did not happen after 1992, the 1997 Census of Retail Trade can still provide a consistent estimate of the long-run impact of deregulation. Moreover, because the 1997 Census of Retail Trade has information on more narrowly defined industries in retail, the impact of deregulation on shops only selling exempted products (food, prescription drugs or gasoline) can be more closely analyzed.

III. EMPIRICAL ANALYSIS

III.A Regulated and other industries in retail

Table I provides some information about the different types of shops in retail using the 1977-1992 Censuses of Retail Trade. The group of "*Regulated Industries*" consists of all industries prohibited to open on Sunday if a state has or would have Sunday Closing Laws. The remaining store retailers are listed in the group of "*Other Industries*". For this latter group it is less clear whether industries generally represent shops subject to or exempted from Sunday Closing Laws. For example, the group of food stores mainly consists of supermarkets and convenience stores. And even though supermarkets and convenience stores also sell food (which is an exempted product), there has been some controversy about their Sunday opening.[24] Similarly, the industry of drug and propriety stores generally represents beauty stores as well as pharmacies. Since beauty stores most often do not sell prescription drugs (another exempted product), they have mostly been prohibited to open on Sunday in contrast to pharmacies. Finally, the group of gasoline stations (another exempted product) generally represent exempted businesses in contrast to the industry of miscellaneous retailers even though this industry also includes the non-store retailers.

[24] See Theuman [2004] for a number of cases.

Table I
Percentages of Total Retail by Industry, 1977-1992

	Weekly employment	Annual Revenue	Number of Shops
		Regulated Industries	
Building materials and garden stores	6.405 (1.179)	8.107 (1.719)	7.600 (1.928)
General merchandise stores	20.32 (2.439)	17.80 (2.451)	4.047 (1.107)
Apparel and accessory stores	10.50 (1.564)	7.259 (1.382)	13.74 (1.764)
Furniture and home furnishing stores	6.291 (0.700)	6.382 (0.913)	10.23 (1.030)
		Other Industries	
Food stores	26.91 (2.319)	30.18 (2.248)	18.34 (2.545)
Drug and propriety stores	5.532 (0.776)	5.088 (0.961)	5.016 (0.921)
Gasoline service stations	7.490 (1.498)	11.34 (2.131)	12.83 (2.150)
Miscellaneous retailers	16.54 (2.750)	13.84 (3.776)	28.20 (3.971)

Notes: Data are taken from the Census of Retail Trade. Weekly Employment measures the number of paid employees in the week including March 12. Annual Revenue is in 1985 dollars. The industries listed are the most disaggregate information available from the data and are based on SIC industry codes. Mean percentages are calculated after pooling states and years and the numbers in brackets are standard errors.

The numbers in Table I are average percentages for each industry and the reported standard errors therefore reflect variation in the industry composition between states and years. For the regulated industries, general merchandise stores (mainly department stores) are the largest employers in contrast to furnishing and home furnishing stores which seem relatively small. For other industries, food stores are the biggest employer whereas gasoline stations are relatively small. All in all, these findings do not seem counter intuitive.

Table I gives mean percentages after pooling all states and years. To get an idea whether regulation has any impact, one would like to see whether these means differ significantly between states with and without Sunday Closing Laws at any point in time. This is done in Table II. Each row in Table II provides point estimates for β_1 using the following regression specification:

(1) $$\ln y_s = \beta_0 + \beta_1 I_s + \varepsilon_s$$

with y_s total weekly employment, annual revenue or the number of shops in regulated industries as a fraction of total retail in state s. The dummy variable I_s equals zero in states with Sunday Closing Laws and is therefore expected to be positive (negative) if deregulation has a positive (negative) impact. Table II shows that all estimates of β_1 are positive suggesting deregulation increases employment with about 2 percent, increases revenue with 3 to 5 percent and the number of shops with 1 to 5 percent. [25]

III.B Returns to deregulation for regulated industries

Even though the results in Table II do suggest some impact of deregulation, the reported coefficients will not reflect its causal impact on regulated industries if also other industries are indirectly affected by such laws. Moreover, equation (1) does not use that between 1977 and 1992, ten states deregulated at different points in time.

To use this variation in our analysis, an important question is how the Census sampling coincides with the timing of deregulation. The first column of Table III therefore lists the states that deregulated between 1977 and 1992 and the crosses

[25] Excluded are North Dakota and Missouri. North Dakota is excluded because it only deregulated in 1991. Missouri is excluded because deregulation in 1979 implied counties could opt-out of state regulation. This happened gradually with 34 counties choosing to opt-out between 1979 and 1985, a further 19 counties between 1986 and 1990 and 12 counties opted-out between 1991 and 1992. See Appendix B for more details.

Table II
Percentage Difference in Mean Fraction of Regulated Industries between States with and without Sunday Closing Laws, 1977-1992

	Weekly Employment	Annual Revenue	Number of Shops
1992	0.023	0.053	0.052
	(0.025)	(0.043)	(0.025)
1987	0.018	0.029	0.010
	(0.021)	(0.029)	(0.021)
1982	0.021	0.038	0.018
	(0.023)	(0.034)	(0.019)
1977	0.023	0.048	0.019
	(0.025)	(0.030)	(0.022)

Notes: Data are taken from the Census of Retail Trade. The numbers in brackets are standard errors. For the grouping of states into those with and without Sunday Closing Laws, see Appendices A and B.

reflect their effective date of deregulation relative to the Census sampling years. Depending on how long the impact of deregulation takes to complete, the group that deregulated by 1980 was most likely affected during the period 1977-1982. Similarly, the perceived impact period for the group that deregulated by 1983 is most likely to be 1982-1987 and for the group that deregulated by 1987 it is the period 1987-1992. Assuming a positive impact of deregulation, also note that the estimated returns to deregulation will be attenuated in so far the true and perceived impact periods do not overlap.

Table III
The Timing of Deregulation and Census Sampling, 1977-1992

	1977					1982					1987					1992
Deregulated by 1980																
Pennsylvania		X														
Connecticut			X													
New Jersey			X													
Deregulated by 1983																
Vermont						X										
Massachusetts						X										
Deregulated by 1987																
Mississippi										X						
Texas										X						
Louisiana										X						

Notes: References to exact dates of deregulation are found in Appendix B.

For each group of states, Table IV then compares outcomes in each deregulating industry with the same industry in non-deregulating states and other industries in every other state during the perceived impact period. This is done using the following specification:

(2) $\Delta \ln Y_{is} = \beta_0 + \beta_1 I_{is} + A_s + B_i + \varepsilon_{is}$

with $\Delta \ln Y_{is}$ the change in the log of employment, revenue or the number of shops in industry i and state s. The returns to deregulation are given by β_1 since I_{is} a dummy equal to 1 if industry i in state s deregulated in that period. Term A_s is a vector of state fixed effects to capture the sensitivity of retail activity to geographically dispersed macroeconomic shocks and cycles. [26] Finally, B_i is a vector of industry dummies to account for industry specific time shocks or trends common across states.

Columns [1], [3] and [5] suggest the returns to deregulation for deregulating industries are positive and point estimates range from 3.5 to 6 percent for employment, 4.5 and 10 percent for sales and 1 and 2 percent for the number of shops. Clustered standard errors indicate point estimates are marginally significant. To see whether state dummies mainly take out different shocks or cyclical variation between states, columns [2], [4] and [6] replace the state dummies with differences in the log of the state wide unemployment rate, personal disposable income and population. Also, because these controls seem to vary similarly within the five geographical divisions used by the Census Bureau, five region dummies are also added as additional controls to potentially exclude further bias. The point estimates and corrected standard errors are very similar to those obtained using the state dummies indeed suggesting that state specific time varying

[26] See Dzialo, Shank and Smith [1993] for a discussion of how different macroeconomic shocks and cycles have affected different regions in the US between 1977 and 1992.

Table IV
The Impact of Deregulation using All Industries, 1977-1992

	Weekly Employment		Annual Revenue		Number of Shops	
	[1]	[2]	[3]	[4]	[5]	[6]
			Period 1977-1982			
Dummy for deregulating industries	0.038	0.044	0.043	0.036	0.011	0.011
	(0.017)	(0.020)	(0.042)	(0.045)	(0.013)	(0.019)
State dummies	X		X		X	
State controls and region dummies		X		X		X
			Period 1982-1987			
Dummy for deregulating industries	0.060	0.054	0.097	0.085	0.021	0.012
	(0.042)	(0.038)	(0.027)	(0.028)	(0.020)	(0.018)
State dummies	X		X		X	
State controls and region dummies		X		X		X
			Period 1987-1992			
Dummy for deregulating industries	0.035	0.048	0.046	0.042	0.013	0.009
	(0.027)	(0.032)	(0.021)	(0.015)	(0.022)	(0.019)
State dummies	X		X		X	
State controls and region dummies		X		X		X

Notes: Data are taken from the Census of Retail Trade. The first specification includes industry and states dummies. The second specification includes industry dummies, growth in state wide unemployment, population and personal disposable income and 5 region dummies. Standard errors are clustered by whether the industry belongs to the group of deregulating industries or not interacted with state cells. The number of observations is between 389 and 400.

controls and region dummies are sufficient to capture state specific shocks and cycles even if state dummies can no longer be added to the difference-in-differences specification.

The estimates in Table IV do not necessarily reflect the true return to deregulation for deregulating industries if also other industries are indirectly affected by deregulation. To this end, consider the following specification using only the sample of regulated industries:

(3) $$\Delta \ln Y_{is} = \beta_0 + \beta_1 I_s + \beta_2 \Delta X_s + A_r + B_i + \varepsilon_{is}$$

with ΔX_s the change in the log of the unemployment rate, personal disposable income and population and with A_r a set of five region dummies. Note that relative to equation (2), equation (3) further provides a specification test in that all right-hand side coefficients are now specific to deregulating industries, just like the coefficient of interest β_1.

The point estimates in Table V suggest that deregulation increases employment with 4.5 to 6.5 percent, revenue with 4 to 10 percent and the number of shops with 1 to 1.5 percent in deregulating industries. Clustered standard errors reveal many of the estimated returns are statistically significant. Two further points are noteworthy. First, the estimated returns are relatively large. Given that our data suggest the average cyclical upswing increases revenue with about 20 to 30 percent (see for example Figure III below), an increase of sales by about 5 percent seems important. Second, the similarity of point estimates between Tables II, IV and V suggests that the estimated returns for other industries are largely zero. Note however this is not necessarily inconsistent with the idea that deregulation has a negative impact on more narrowly defined exempted industries as will be argued below.

Finally, an interesting question is whether all or just some deregulating industries gain from deregulation. To capture the possible non-linearity in (3), Table VI therefore uses the following specification for each deregulating industry in its perceived impact period:

(4) $$\Delta \ln Y_s = \beta_0 + \beta_1 I_s + \beta_2 \Delta X_s + A_r + \varepsilon_s$$

with $\Delta \ln Y_s$ the change in the log of annual revenue. Table VI shows that point estimates

Table V
The Impact of Deregulation using Regulated Industries, 1977-1992

	Change in Log (Dependent Variable)		
	Weekly Employment	Annual Revenue	Number of Shops
		Period 1977-1982	
Returns to deregulation	0.044	0.039	0.012
	(0.021)	(0.046)	(0.019)
		Period 1982-1987	
Returns to deregulation	0.064	0.107	0.015
	(0.038)	(0.028)	(0.018)
		Period 1987-1992	
Returns to deregulation	0.046	0.044	0.009
	(0.032)	(0.015)	(0.019)

Notes: Data are taken from the Census of Retail Trade. All regressions include changes in the log of the state wide unemployment rate, population and personal disposable income, 5 region dummies and industry dummies. Standard errors are clustered by state. The number of observations is between 197 and 200.

are generally positive though OLS standard errors are relatively large. The largest impact is estimated for the group of building materials and garden stores, furniture and home furnishings stores and clothes stores.

III.C Robustness checks

If deregulation is more likely to happen in states that have different persistent trend growth, the estimated returns to deregulation are biased. One way to see whether this is true is to estimate the returns outside the perceived impact period. To this end, Figure II plots the point estimates in Table V together with point estimates for other periods. For example, the first three bars in the top panel of Figure II draw the point

Table VI
The Impact of Deregulation on Annual Revenue in Each Regulated Industry, 1977-1992

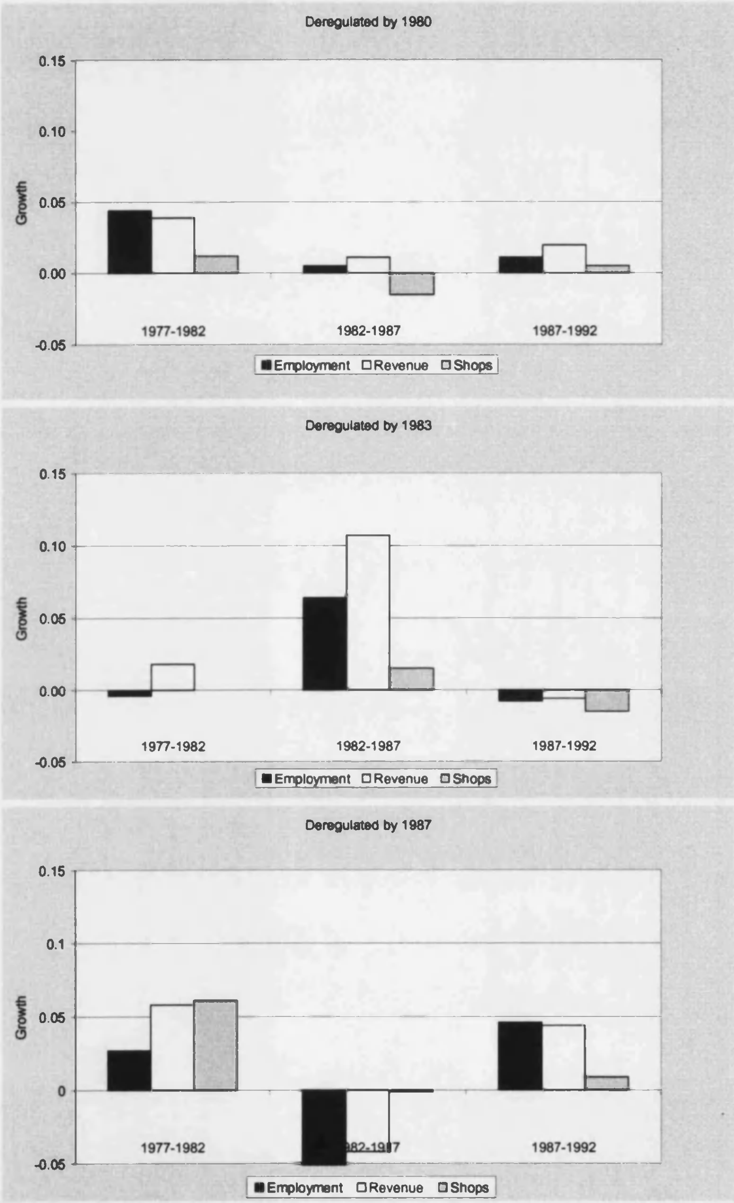
	Period		
	1977-1982	1982-1987	1987-1992
Building materials and garden stores	0.076 (0.094)	0.127 (0.114)	0.033 (0.088)
General merchandise stores	-0.001 (0.063)	0.086 (0.046)	0.127 (0.087)
Apparel and accessory stores	0.032 (0.041)	0.105 (0.068)	0.054 (0.058)
Furniture and home furnishing stores	0.037 (0.071)	0.106 (0.089)	0.038 (0.057)

Notes: Data are taken from the Census of Retail Trade. All regressions include changes in the log of the state wide unemployment rate, population and personal disposable income and 5 region dummies. The number of observations is between 48 and 50.

estimates given in the top panel of Table V. The next three bars estimate the returns to deregulation for states that deregulated by 1980 for the period 1982-1987 and so forth. It is clear from Figure II that the estimated returns in Table V do not seem largely affected by differing trends or measurement error in the timing of deregulation. Indeed, it can be shown that point estimates of trend-adjusted difference-in-differences for the group of deregulating industries are very similar to those presented in Table V.

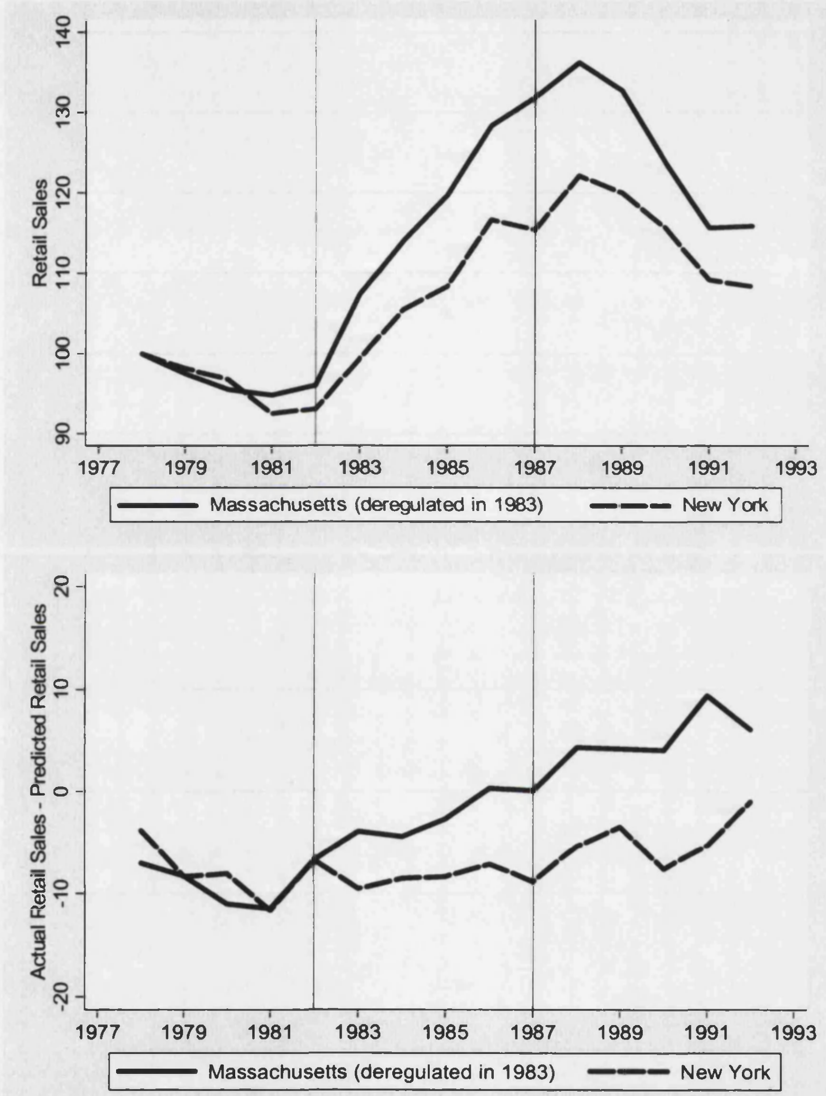
The point estimates presented earlier could also wrongly state the true returns to deregulation because of changes in data stratification by the Census Bureau. For example, whereas most of the point estimates in Table V are between 1 and 5 percent, the estimated returns for states that deregulated by 1982, Massachusetts and Vermont, seem relatively large. To see whether this reflects changes in Census procedures in those states,

Figure II
The Impact of Deregulation using Regulated Industries by Period, 1977-1992



Notes: Data are taken from the Census of Retail Trade. All regressions include changes in the log of the state wide unemployment rate, population and personal disposable income, 5 region dummies and industry dummies. Standard errors are clustered by state. The number of observations is between 195 and 200.

Figure III
Differences in Annual Retail Revenue between Massachusetts and New York, 1978-1992 (1978=100)



Notes: Data are taken from the annualized Monthly Retail Sales Survey. The series in the lower panel are constructed by taking the difference between actual (deflated and normalized to 1978=100 as in the top panel) and predicted revenue. Predicted revenue is expected revenue from changes in the log of state wide unemployment, population and personal disposable income.

Figure III uses an annualized measure of real retail revenue derived from the Monthly Retail Trade Survey between 1978 and 1992. Despite the fact that these historical series are only available for a few states, a useful comparison between Massachusetts (which deregulated in December 1982) and New York (which did not deregulate) can be made. The top panel of Figure III gives the raw differences between both states suggesting a difference-in-differences between 1982 and 1987 of about 10 percent, similar to the point estimate given in Table V. But one concern is the possibility that the difference-in-differences between 1982 and 1992 of about 3 percent is the better measure of the long-run impact of deregulation. However, the bottom panel of Figure III draws the series adjusted for changes in the unemployment rate, personal disposable income and population. The regression adjusted series suggests that deregulation did have a persistent and relatively large impact of 7 to 8 percent on retail revenue in Massachusetts. Note however that this does not imply the differences-in-differences presented in the second panel of Table V and the bottom panel of Figure III are necessarily unbiased. For example, deregulation in Massachusetts and Vermont did coincide with a strong cyclical upswing in consumer confidence in those states that could not entirely be controlled for by changes in the unemployment rate, personal disposable income or population.

Finally, despite controlling for state-industry fixed effects, state-time and industry-time specific shocks as well as persistent differences in trend growth, an important question remains about the randomness of deregulation. Ideally, deregulation is randomized and there are two good reasons to think why this might at least partially be the case here. First, Sunday Closing Laws have been declared unconstitutional by courts rather than directly repealed by state legislators in four out of ten deregulating states.[27] Deregulation will therefore be closer to a randomized experiment if the outcome of litigation procedures is less predictable. Second, the history of deregulation suggests part of the decision to deregulate is driven by a long-run West-to-East pattern across states not clearly correlated with any long-run patterns in product or labor market outcomes.

In sum, using that between 1979 and 1992 a number of states deregulated their Sunday Closing Laws at different points in time, employment, revenue and the number of shops are expected to increase in deregulating industries. This seems to be true using a number of specifications and different data sets. These insights will be further amplified in the next section using the 1997 Census of Retail Trade. Moreover, the next section also examines what is most likely to happen to employment, sales and the number of shops in exempted industries.

III.D Returns to deregulation for exempted industries

Given that deregulation did not happen after 1991, Table VII uses the 1997 Census of Retail Trade to derive a comparison similar to the analysis in Table II. The first row of Table VII gives mean percentages of total retail for all regulated industries in states with Sunday Closing Laws. The second row calculates mean percentages for the same industries in states without Sunday Closing Laws. The third row then gives the difference between the second and the first row, suggesting regulated industries are relatively smaller in states with Sunday Closing Laws in 1997. The final row of Table VII reports these differences as a fraction of row I. What these fractions suggest is that deregulation could increase employment, revenue and the number of shops with 6 to 7

[27] See Appendix B for further details.

Table VII
Mean Percentages of Total Retail for Regulated Industries in States with and without Sunday Closing Laws, 1997

	Weekly Employment	Annual Revenue	Number of Shops
I. States with Sunday Closing Laws	57.11 (1.145)	54.80 (1.363)	61.09 (0.798)
II. States without Sunday Closing Laws	60.71 (0.608)	58.67 (0.518)	65.59 (0.598)
II - I	3.596 (1.410)	3.868 (1.273)	4.496 (1.341)
(II -I)/ I	0.060 (0.023)	0.068 (0.022)	0.070 (0.020)

Notes: Data are taken from the 1997 Census of Retail Trade. The numbers in brackets are standard errors. For the grouping of states into those with and without Sunday Closing Laws, see Appendix A.

percent in regulated industries. Despite changes in the industry classification and sampling criteria in more recent Census data, these numbers are roughly comparable to those presented in Table II.

A further test is to see whether mean differences exist per consumer. The results are found in the top panel of Table VIII. The first row suggests that in deregulating industries, deregulation increases employment with 2.5 workers per thousand consumers, annual expenditure with $603 per person per year and the number of shops with 0.096 per thousand consumers. One can compare these estimated mean differences with mean differences for other industries given in the second row of Table VIII. For example, if retail is generally larger in regulated states as suggested by the mean differences for other industries, the difference-in-differences given in the third row of Table VIII suggest that deregulation increases employment with 3.4 jobs per thousand consumers, annual

Table VIII
Mean Differences between States with and without Sunday Closing Laws, 1997

	Weekly Employment	Annual Revenue ($1000)	Number of Shops
	Per 1000 inhabitants		
Regulated Industries	2.481	603.0	0.096
	(1.090)	(192.2)	(0.158)
Other Industries	-0.917	37.39	-0.204
	(0.202)	(20.79)	(0.021)
Difference-in-differences	3.399	565.6	0.301
	(0.914)	(100.4)	(0.099)
	Per $21.7m of personal disposable income		
Regulated Industries	-1.200	108.8	-0.223
	(1.822)	(235.9)	(0.228)
Other Industries	-3.576	-349.3	-0.410
	(1.323)	(133.8)	(0.141)
Difference-in-differences	2.376	458.1	0.187
	(2.252)	(271.2)	(0.268)

Notes: Data are taken from the 1997 Census of Retail Trade. The numbers are mean differences between states with and without Sunday Closing Laws. The numbers in brackets are standard errors. For the grouping of states into those with and without Sunday Closing Laws, see Appendix A.

expenditure with $565 per person per year and the number of shops with 0.3 per thousand inhabitants in deregulating industries.

Rather than comparing differences per consumer in each state, the bottom panel of Table VIII compares mean differences per dollar of personal disposable income. To allow for the numbers to be roughly comparable to those presented in the top panel, all means are multiplied with average personal disposable income per thousand inhabitants. The final row of Table VIII estimates that deregulation could increase employment with 2.4 per thousand inhabitants, annual expenditure with $458 per person per year and the number of shops with 0.19 per thousand inhabitants. Note that these differences are very similar to those presented in the top panel.

An interesting question is whether suppressed expenditure on regulated goods in states with Sunday Closing Laws affect all regulated industries equally. Table IX therefore gives mean differences for annual revenue similar to those presented in the first rows of the top and bottom panel of Table VIII for each regulated industry.[28] Table IX suggests these differences are all positive and roughly similar (except for department stores using the income normalization which could be explained by the relative low number of shops in those industries). If anything and in line with the findings in Table VI, the results in Table IX suggest consumers spend more mainly on building material and garden stores and furniture and home furnishings stores after deregulation.

Finally, the more detailed industry classification available from the 1997 Census of Retail Trade not only allows to examine how regulation affects activity in regulated industries but also shops exempted from Sunday Closing Laws. In particular, Table X breaks down drug and propriety stores into beauty shops and pharmacies since pharmacies together with gasoline service stations are most likely to generally represent shops exempted from Sunday Closing Laws in contrast to beauty shops. Similar to the comparisons in Table IX, Table X then compares average annual revenue between states with and without Sunday Closing Laws using both the population and income normalizations. Point estimates suggest that deregulation increases sales in regulated industries (cosmetics, beauty and perfume stores) and, if anything, decreases sales in

[28] In contrast to Tables I and VI, note that the group of miscellaneous retail stores has now been listed under "*Regulated Industries*". The reason for doing so is that the more aggregate industry classification used by the Census Bureau before 1997 does not allow excluding the non-store retailers from this group.

Table IX
Differences in Means for Annual Revenue (in thousands of $) between States with and without Sunday Closing Laws for Each Regulated Industry, 1997

	Per 1000 inhabitants	Per $21.7m of PDI
Building material and garden stores	156.4 (83.12)	44.34 (107.3)
General merchandise stores	107.7 (107.7)	-72.21 (135.1)
Apparel and accesory stores	95.34 (84.55)	24.76 (71.56)
Furniture and home furnishings stores	138.0 (48.18)	76.13 (41.50)
Miscellaneous retail stores	105.5 (11.63)	35.77 (11.48)

Notes: Data are taken from the 1997 Census of Retail Trade. The numbers are mean differences between states with and without Sunday Closing Laws. The numbers in brackets are standard errors. For the grouping of states into those with and without Sunday Closing Laws, see Appendix A.

Table X
Differences in Means for Annual Revenue (in thousands of $) between States with and without Sunday Closing Laws for Some Other Industries, 1997

	Per 1000 inhabitants	Per $21.7m of PDI
Drug and propriety stores	-7.647 (48.08)	-73.35 (40.29)
Cosmetics, beauty and perfume stores	17.17 (1.663)	8.402 (1.452)
Pharmacies and drug stores	-24.82 (45.20)	-81.75 (38.68)
Gasoline service stations	-53.41 (77.60)	-169.8 (106.2)

Notes: Data are taken from the 1997 Census of Retail Trade. The numbers are mean differences between states with and without Sunday Closing Laws. The numbers in brackets are standard errors. For the grouping of states into those with and without Sunday Closing Laws, see Appendix A.

industries exempted from such laws (pharmacies and drug stores and gasoline service stations).

III.E Deregulation, average weekly wages and prices

Table XI therefore uses the 1977-1992 Census data to examine the impact of deregulation on average weekly wages. This is made possible because the Census Bureau also provides a measure of annual payroll for industries in retail. Comparing the percentage changes in employment with the percentage changes in annual payroll, it is then possible to say something about the impact of deregulation on average weekly wages. The first column of Table XI reproduces the point estimates found in the first column of Table V. The second column in Table XI gives point estimates for annual payroll using a similar specification and correction of standard errors. The point estimates are very similar and if anything somewhat larger for the payroll data indicating a small but insignificant increase in average weekly wages.

Whether the increase in total revenue documented above is driven by an increase in output prices or the volume of sales (or both) ultimately depends on how retailers set prices given demand for their products and retailing costs. It might therefore come as no surprise that the theoretical literature on the price effect of liberalizing opening times is mixed and inconclusive.[29] For example, Burda and Weil [2001] predict that prices will increase after deregulation because of a fall in the productivity of finding a customer given all other shops are open longer. However, Clemenz [1990] more explicitly

[29] Despite a lack of theoretical and empirical research on the impact of longer opening times on employment, sales, the number of shops or wages, a small literature has emerged examining its impact on retail prices. Inderst and Irmen [2004] provide the most recent survey of this literature to conclude that it is divided about whether prices should increase or decrease after deregulation.

Table XI
The Impact of Deregulation on Average Weekly Wages using Regulated Industries, 1977-1992

	Change in Log (Dependent Variable)	
	Weekly Employment	Annual Payroll
	Period 1977-1982	
Returns to deregulation	0.044	0.055
	(0.021)	(0.033)
	Period 1982-1987	
Returns to deregulation	0.064	0.073
	(0.038)	(0.046)
	Period 1987-1992	
Returns to deregulation	0.046	0.061
	(0.032)	(0.032)

Notes: Data are taken from the Census of Retail Trade. All regressions include the change in the log of state wide unemployment, population and personal disposable income, 5 region dummies and industry dummies. Standard errors are clustered by state. The number of observations is between 198 and 200.

examines the role of search for equilibrium prices under imperfect information to conclude that deregulation could reduce prices if longer shopping hours facilitate price comparisons. And although these studies do differ in their conclusions, a common theme does emerge, namely that the price impact of deregulation is relatively small. Consequently, the model presented below first examines the impact of deregulation on employment, sales and the number of shops in regulated and exempted industries before also accounting in equilibrium for its possible impact on wages and prices.

IV. A MODEL TO ANALYZE THE IMPACT OF EXTENDING OPENING HOURS ON LABOR AND PRODUCT MARKETS

Throughout this section, it is assumed that each shop has some market power by offering a differentiated product. Building on these and other standard assumptions about retail markets, this section then provides a framework to explain the observed impact of deregulation by accounting for changes in consumer behavior, retail competition and ultimately labor demand. We will consider the case of Sunday opening, although the model equally applies to any extension of opening times. The model presented here is informal and a more technical exposition can be found in Appendix C.

IV.A The impact of extending opening hours on employment and sales in deregulating industries

Empirical evidence suggests that employment costs in retail vary with opening times and the volume of sales (Nooteboom [1982, 1983] and Thurik [1982]). The idea that part of labor costs only vary with opening times can be justified by noting that one must employ at least one worker at all times. Furthermore, there seems to be considerable empirical evidence in favor of constant marginal labor costs. Total labor costs per week therefore write as

(5) $$C = c_D D + c_X X$$

with c_D threshold labor costs per day, D the number of opening days a week, c_X constant marginal labor costs and X the volume of weekly sales. Weekly employment is then given by

(6) $N = n_D D + n_X X$

with n_D threshold labor per day and n_X inverse marginal labor productivity.

Equations (5) and (6) are related by that $c_D = wn_D$ and $c_X = wn_X$ with w the wage per unit of time. For example, assume that weekly sales are given by $X=1/n_X\,(N - n_D D)$ if N strictly exceeds the required amount of threshold labor and zero otherwise. Besides a standard production function, this relationship can also reflect a first order approximation to Nooteboom's [1983] "isomenes" (equal-waiting curves). In Nooteboom's model, employment depends positively on sales since firms want to keep their waiting time relative to service time at approximately n_X. This gives $N = N_D D + n_X X$ if $X > 1/n_X$ and zero otherwise. Integrating over wages then gives (5).

The impact of Sunday opening on weekly employment can then be analyzed through an increase of D in (6). If consumers only inter-temporally substitute income from others days of the week to Sunday, the volume of weekly sales does not increase and weekly employment only increases with the additional required threshold labor (threshold labor effect). However, in line with the empirical evidence shown above, total weekly sales in deregulating industries are expected to increase.[30] If this is the case, X will increase and therefore employment will further increase (sales effect).

IV.B The impact of extending opening hours on the number of shops in deregulating industries

[30] This would be the case if shopping at different days of the week are not perfect substitutes. For example, Grunhagen, Grove and Gentry [2002] present some evidence that consumers perceive Sunday shopping and shopping on other days of the week as distinct activities. Alternatively, an increase in total sales could be explained by that consumers were more rationed in their shopping time before deregulation. Jacobsen and Kooreman [2005] present a model along these lines and based on time-use data find that consumers increase their total shopping time following an extension of opening hours. It is not clear, however, whether their findings identify a model in which consumers are rationed ex ante or whether shopping on different days of the week are not perfect substitutes in consumption.

If the increase in total revenue outweighs the increase in labor costs, profits will increase and retailers will decide to open on Sunday.[31] In line with the empirical evidence shown above, the number of shops will therefore increase until all profits are exhausted. In sum, employment is likely to increase because of increased threshold labor, an increase in the volume of sales but also an increase in the number of shops. Assuming all shops are identical for simplicity, this is easily seen since total labor demand in the steady state is given by

$$(7) \qquad SN = n_D SD + n_X SX$$

with S the number of shops in deregulating industries. First, an increase in D increases threshold labor in each shop (threshold labor effect). Second, if an increase in D increases S, total threshold labor will increase (entry effect). Finally, if an increase in D increases S and X and therefore SX, the total volume of sales will increase resulting in a further increase in employment (sales effect).

IV.C The impact of extending shop opening hours on deregulating and non-deregulating industries

If total expenditure in regulated shops increases after deregulation, expenditure on goods or services in other industries will fall if total income is constant. If anything, deregulation is therefore expected to have a negative impact on total revenue, the number

[31] If threshold labor costs are too high, it is not immediately clear why retailers would decide to extend their opening hours. One possibility is that Sunday opening increases shop space utilization or that Sunday opening implies the optimal shop size becomes smaller because of a reduction in peak demand. If these cost savings outweigh the additional costs of Sunday opening, retailers will find it profitable to extend their opening hours. However, the predicted impact of deregulation on employment in deregulating industries remains as is described in this section.

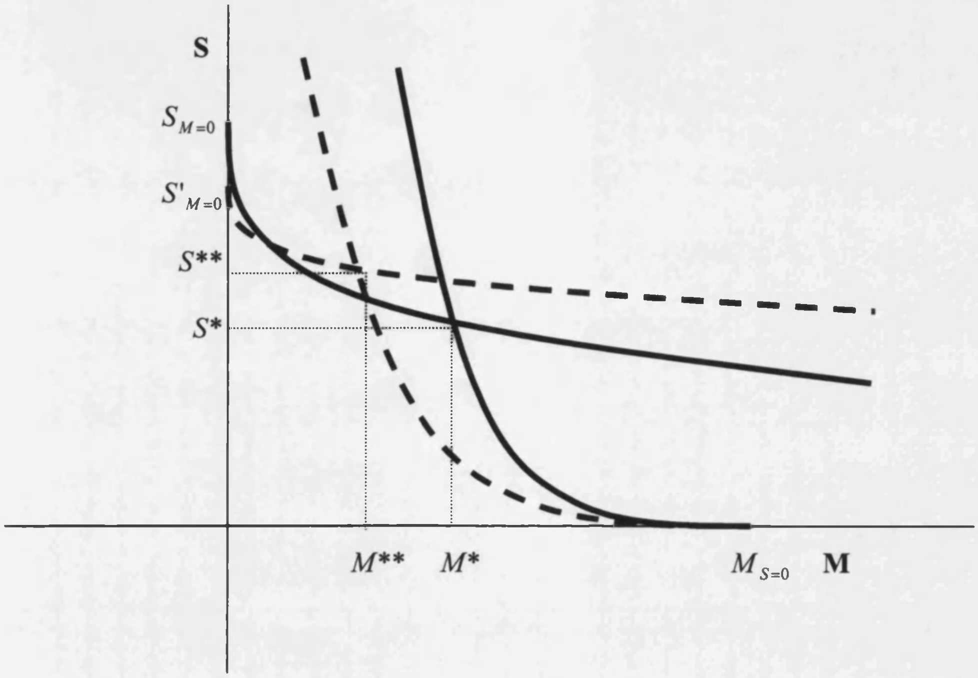
of shops and therefore employment in exempted industries as was already argued in the data.

To allow for this possibility, assume two types of shops exist: type *s* shops for which opening hours are being deregulated and type *m* shops for which opening hours are not being deregulated. Type *m* shops can either remain prohibited to trade on Sunday after deregulation or be exempted from Sunday Closing Laws before deregulation. Denote the number of type *s* shops as *S* and the number of type *m* shops as *M*. Also assume for simplicity all type *s* shops and all type *m* shops are identical but type *s* and type *m* shops can have different cost parameters.

Define the solid lines in Figure IV as the long-run zero-profit curves for type *s* shops (vertical axis) and type *m* shops (horizontal axis) before deregulation. First, the intercepts $S_{M=0}$ and $M_{S=0}$ are assumed to be finite and to depend positively on personal disposable income and negatively on threshold labor costs. Second, the zero-profit curves are assumed to be downward sloping for *S* and *M* strictly positive. To see this, consider the zero-profit curve for type *s* shops. If type *s* and type *m* goods are substitutes, an increase in *M* requires a decrease in the total fraction of income spent on type *s* goods. Consequently, higher *M* reduces profitability in type *s* industries which decreases *S*. Third, the intersection of the zero-profit curves determines the equilibrium number of shops S^* and M^* before deregulation. [32]

[32] An important question is whether the analysis as drawn in Figure IV can exist under standard assumptions about retailing costs, consumer preferences and retailer behavior. Appendix C therefore formally derives the analysis summarized in Figure IV using a Dixit-Stiglitz type model of monopolistic competition assuming labor costs are as in (5). Besides the standard Dixit-Stiglitz restrictions on the utility parameters, the appendix proofs that for a unique stable equilibrium to exist it is sufficient to assume that the substitutability of goods between shops of different type is less than the substitutability between goods of similar type. This assumption does not seem too restrictive.

Figure IV
The Impact of Deregulating Shop Closing Hours on the Number of Shops in Deregulating and Non-deregulating Industries



Assuming personal disposable income is constant, deregulation decreases $S_{M=0}$ to $S'_{M=0}$ because of increased total threshold labor costs. But deregulation also rotates upwards the zero-profit curve for type *s* shops around $S'_{M=0}$ and rotates inwards the zero-profit curve for type *m* shops around $M_{S=0}$. The zero-profit curve for type *s* shops rotates upwards and for type *m* shops rotates inwards because consumers spend a larger fraction of total income on type *s* goods after deregulation. Also note that the zero-profit curves rotate around their intercepts because the intercepts do not change if personal disposable income is constant. A new equilibrium is reached at (S^{**},M^{**}). Because S^{**} must lie on the zero-profit curve for type *s* shops that has shifted upwards, total expenditure on type *s* goods must increase. Moreover, if it is profitable for retailers to open on Sunday, the increase in total revenue outweighs the increase in threshold labor costs and $S^{**}>S^{*}$.

In line with the empirical evidence presented in the previous section, total employment in deregulating industries will increase because of an increase in threshold labor (threshold labor effect), total revenue (sales effect) and the number of shops (entry effect). However, the inward rotation of the zero-profit curve for type *m* shops reflects an unambiguous decrease in total expenditure on type *m* goods. In the short-run, some type *m* shops will make losses and eventually the number of type *m* shops unambiguously decreases. Total employment in non-deregulating industries is therefore expected to fall because of a decrease in total revenue (sales effect) and the number of shops (exit effect).

IV.D The impact of deregulation on wages and prices

If wages increase significantly (maybe because of legally imposed requirements for working Sundays), threshold and marginal labor costs will increase. It then is straightforward to show that the increase in total revenue, the number of shops and therefore employment in deregulating industries will be smaller. And consequently, the decrease in non-deregulating industries will be smaller too.

Finally, note that the analysis assumed that all type *s* shops set a unique price and all type *m* shops set a unique price before and after deregulation.[33] Consequently, the analysis above explicitly accounts for the possibility that deregulation affects aggregate prices since Figure IV allows for type *s* and type *m* shops to charge different prices in equilibrium. For example, assume labor productivity in type *s* shops is higher than in type

[33] In the model presented in Appendix C retailers perceive demand to be iso-elastic and charge a constant mark-up over marginal costs. Deregulation therefore has no impact on the price set by each retailer.

m shops such that type *s* shops charge a lower price. In this case, part of the increase in *S* and decrease in *M* is explained by consumers substituting income towards deregulating industries because type *s* workers are more productive. The more productive type *s* workers are, the bigger will be the gains (losses) for type *s* (type *m*) industries, the bigger will be the increase in average productivity and the bigger will the decrease in the aggregate price index. However, in so far the observed increase (decrease) in total revenue for deregulating (non-deregulating) shops reflects a change in prices, the expected impact of deregulation on employment in deregulating (non-deregulating) industries will be smaller.

V. CONCLUSIONS

This chapter has argued there are important deficiencies in our understanding of how shop closing hours affect consumer behavior, competition and therefore ultimately employment. Using Census of Retail Trade data, it was first shown that deregulation increases total employment, total revenue and the number of shops in deregulating industries. This is true for a number of specifications including a number of different control groups, different time periods and different data. However, estimates also suggest that deregulation decreases total employment, revenue and the number of shops in non-deregulating industries.

Building on standard assumptions about retail markets, this chapter then provided a framework to explain the observed impact of deregulation by accounting for changes in consumer behavior, retail competition and ultimately labor demand. Consistent with the empirical findings, it was shown how employment in deregulating industries increases because of an increase in threshold labor (a threshold labor effect), an increase the total volume of sales (sales effect) and an increase in the number of shops (entry effect). However, it was also argued that employment in exempted industries will fall because of a decrease in total expenditure in exempted industries (sales effect) as well as a decrease in the number of shops (exit effect).

In any future debate about the employment impact of extending opening hours, it seems therefore important to simultaneously account for consumer preferences, retailer costs and pricing behavior, the competitive nature of retail markets and wage policies in predicting its impact on retail employment in both deregulating and non-deregulating industries.

Appendix A
Current Sunday Closing Laws

Alabama	**Ala. Code 13A-12-1**: "The keeping of an open store on Sunday is prohibited."	
Kentucky	**Ky. Rev. Stat. Ann. 436.160**: "Any person who works on Sunday or employs another person on Sunday shall be fined not less than $2 and not more than 50$ for each violation."	**Ky. Rev. Stat. Ann. 436-165**: "Any legaslative body of any city or county may further permit or regulate retail sales on Sunday [...]."
Maine	**Me. Rev. Stat. Ann. Tit. 17:3204**: "Businesses cannot be open to the public on Sundays [...]."	
New Hampshire	**N.H. Rev. Stat. Ann. 332-D:2**: Opening of shops or selling any merchandise is prohibited on Sunday.	**N.H. Rev. Stat. Ann. 332-D:4**: "The governing body of any city or town may adopt bylaws and ordinances permitting and regulating retail businesses."
Oklahoma	**Okla. Stat. Tit. 21: 918**: "Secular labor, trades and all manner of public selling of any commodities are acts forbidden to be done on the first day of the week, the doing of which is Sabbath-breaking."	
Rhode Island	**R.I. Gen. Laws 5-23-1**: "Retail establishments licensed by the town council or any town may be permitted to open for business on Sundays between noon and 6 p.m and on holidays during normal working hours."	
South Carolina	**S.C. Code Ann. 53-1-5**: "It shall be unlawful for any person to sell at retail any goods on Sunday before 1.30 p.m on Sunday."	**S.C. Code Ann. 53-1-150 to 170**: "Counties also have the option of suspending certain Sunday closing laws."
West Virginia	**W. Va. Code 61-10-25**: "It shall be unlawful to engage in work, labor or business on Sunday."	**W. Va. Code 61-10-28**: "The county court of any county is hereby authorized to call a local option election for the purpose of determining the will of the voters as to whether the provisions of section 61-10-25 shall continue to have effect in said county."

Appendix B
The US history of Sunday Closing Laws, 1979-2004

	Sunday Closing Law	Date of Deregulation
North Dakota	**N.D. Cent. Code 12.1-30-01**: "It is a misdemeanor for any person between the hours of twelve midnight and twelve noon on Sunday to conduct a business or labor for profit [...]."	Repealed February 1991.
Louisiana	**La. Stat. Ann. 51-191**: "All stores, shops, saloons and all places of public business, [...], shall be closed at twelve o'clock on Saturday nights, and remain closed continuously for twenty-four hours, during which time no proprietor thereof shall give, trade, barter, exchange or sell any stock or any article of merchandise kept in his establishment."	Repealed December 1986.
Mississippi	**Ms. Stat. Ann. 97-23-67** generally prohibited sales on Sunday.	Repealed July 1986.
Texas	**Tx. Stat. Ann. 132-9001** prohibited the sale of goods on both the two consecutive days of Saturday and Sunday.	Repealed September 1986.
Massachussetts	**Ma. Gen. Laws Ann. 136.5**: "Sunday shall be a common day of rest. Whoever on Sunday keeps open his shop, [...], or does any labor, business or work, except works of necessity and charity, shall be punished [...]."	**Ma. Gen. Laws Ann. 136.50**: "It is no longer prohibited to keep open a store or shop and sell retail goods therein [...] provided [...] any store or shop shall not open for business on Sunday prior to the hour of noon." Effective December 1982.
Vermont	**Vt. Stat. Ann. 13-3351/52** related to the establishment of a common day of rest and the prohibition of business on such day.	**State v. Ludlow Supermarkets Inc. 141 Vt. 261, 448 A2d 791** struck down the entire state wide scheme. Effective May 1982.
Missouri	**Mo. Rev. Stat. 578.100**: "On Sunday, it is a misdemeanor to engage in in the business of selling clothing and wearing apparel and accessories; furniture; housewares; home business or office furnishings and appliances; hardware; tools; paints; building and lumber supply materials; jewelry; silverware; watches; clocks; luggage; musical instruments and recordings or toys."	**Mo. Rev. Stat. 578.110**: "Any counties may exempt itself from provisions 578.100 by vote of qualified voters at any election (for larger counties) or public hearing (for smaller counties)." Between November 1979 and June 1985, 34 counties deregulated, between April 1986 and November 1990, 19 counties deregulated and between April 1991 and April 1995, 22 counties deregulated.
New Jersey	**N.J. Stat. Ann. 2A-171-1** prohibited wordly employment or business on Sunday.	Repealed September 1979.
Connecticut	**Ct. Gen. Stat. Ann. 53-302a**: "No person, firm or corporation shall engage in work, labor or business, or employ others in work, labor or business on Sunday."	**Caldor's Inc. v. Bedding Barn Inc., 177 Conn. 304, 417 A.2d** struck down the entire state wide scheme. Effective April 1979.
Pennsylvania	**Pa. Stat. Ann. 18-7361**: "A person is guilty of a summary offense if he does or performs any worldly employment or business whatsoever on Sunday."	**Kroger Co. v. O'Hara Tp. 392 A2d 266 481 Pa. 101** struck down the entire state wide scheme. Effective October 1978.

Appendix C

This appendix presents a formal Dixit-Stiglitz type model of monopolistic competition to analyze the impact of deregulating shop closing hours. The informal model described in the text can be summarized using representative utility given by

$$\text{(C.1)} \quad U = \left(\left(\sum_{i=1}^{M} \left(\sum_{j=1}^{D_m} x_{ijm}{}^{\delta} \right)^{\frac{\gamma}{\delta}} \right)^{\frac{\rho}{\gamma}} + \left(\sum_{i=1}^{S} \left(\sum_{j=1}^{D_s} x_{ijs}{}^{\delta} \right)^{\frac{\gamma}{\delta}} \right)^{\frac{\rho}{\gamma}} \right)^{\frac{1}{\rho}}$$

with x_{ijm} consumption from shop i on day j given the opening hours of the shop were not deregulated (type m shop) and x_{ijs} consumption from shop i on day j given the opening hours of the shop are being deregulated (type s shop). The number of type m shops is M and the number of type s shops is S. The number of hours per week type m shops can trade is D_m and the number of hours per week type s shops can open is D_s. For the indifference curves to be convex and finite we need that $0 < \delta < 1$, $0 < \gamma < 1$, $0 < \rho < 1$. Also assume that $\rho < \gamma$ or that the substitutability between type m and type s shops is smaller than the substitutability between shops of similar type. Also note that consumer tastes for shopping during extended opening hours is captured by $1/\delta$.

Demand for retail goods

Denote P_m and P_s as the standard Dixit-Stiglitz price indices for each type m and type s shop and $\overline{P}_m$ and $\overline{P}_s$ the standard price indices for all type m and s goods respectively. If in equilibrium all type m shops set a unique weekly price p_m and all type s shops set a unique weekly price p_s, P_m and P_s simplify to

$$(C.2)\quad P_m = \left\{ \sum_{j=1}^{D_m} p_m^{\frac{\delta}{1-\delta}} \right\}^{\frac{\delta-1}{\delta}} = D_m^{\frac{\delta-1}{\delta}} p_m$$

$$(C.3)\quad P_s = \left\{ \sum_{j=1}^{D_s} p_s^{\frac{\delta}{1-\delta}} \right\}^{\frac{\delta-1}{\delta}} = D_s^{\frac{\delta-1}{\delta}} p_s .$$

Given (C.2) and (C.3), $\overline{P}_m$ and $\overline{P}_s$ write as

$$(C.4)\quad \overline{P}_m = \left\{ \sum_{i=1}^{M} P_m^{\frac{\gamma}{1-\gamma}} \right\}^{\frac{\gamma-1}{\gamma}} = M^{\frac{\gamma-1}{\gamma}} D_m^{\frac{\delta-1}{\delta}} p_m$$

$$(C.5)\quad \overline{P}_s = \left\{ \sum_{i=1}^{S} P_s^{\frac{\gamma}{1-\gamma}} \right\}^{\frac{\gamma-1}{\gamma}} = S^{\frac{\gamma-1}{\gamma}} D_s^{\frac{\delta-1}{\delta}} p_s .$$

Weekly demand at each type m and s shop is then given by

$$(C.6)\quad X_m = \left(\frac{P_m^{\frac{1}{\gamma-1}}}{\overline{P}_m^{\frac{\gamma}{\gamma-1}}} \right) \sigma_m Y = \sigma_m \frac{Y}{M}$$

$$(C.7)\quad X_s = \left(\frac{P_s^{\frac{1}{\gamma-1}}}{\overline{P}_s^{\frac{\gamma}{\gamma-1}}} \right) (1-\sigma_m) Y = (1-\sigma_m) \frac{Y}{S}$$

with σ_m the fraction of total income Y spent on all type m shops which in equilibrium is given by

$$(C.8)\quad \sigma_m = \frac{\overline{P}_m^{\frac{\rho}{\rho-1}}}{\overline{P}_m^{\frac{\rho}{\rho-1}} + \overline{P}_s^{\frac{\rho}{\rho-1}}} .$$

Supply of retail goods

Each shop sells a differentiated product. Shops have some monopoly power and enough shops exist such that each store perceives its demand only to depend on its own price. The first order conditions for maximizing profits then gives the familiar result that prices are a constant mark-up over marginal costs or $p_m\gamma = c_m$. Similarly, in equilibrium we must have that $p_s\gamma = c_s$. Assuming free entry of shops this gives the following zero-profit conditions:

$$\text{(C.9)}\quad \frac{1-\gamma}{\gamma}c_m X_m = F_m$$

$$\text{(C.10)}\quad \frac{1-\gamma}{\gamma}c_s X_s = F_s$$

with F threshold labor costs and other costs independent of sales.

Substituting (C.4) and (C.5) into (C.8) and (C.2), (C.4) and (C.8) into (C.6) and (C.6) into (C.9) and rearranging terms gives the following zero-profit condition for type m shops:

$$\text{(C.11)}\quad S = M\left(\frac{M_{S=0}-M}{M}\right)^{\frac{\gamma(\rho-1)}{\rho(\gamma-1)}}\left(\frac{D_m}{D_s}\right)^{\frac{\gamma(\delta-1)}{\delta(\gamma-1)}}$$

with

$$\text{(C.12)}\quad M_{S=0} \equiv \frac{1-\gamma}{\gamma}c_m\left(\frac{\gamma}{c_m}\right)^{\gamma}\frac{Y}{F_m}.$$

Similarly, the zero-profit condition for all type s shops can be written as

$$\text{(C.13)}\quad M = S\left(\frac{S_{M=0}-S}{S}\right)^{\frac{\gamma(\rho-1)}{\rho(\gamma-1)}}\left(\frac{D_s}{D_m}\right)^{\frac{\gamma(\delta-1)}{\delta(\gamma-1)}}$$

$$\text{(C.14)}\quad S_{M=0} \equiv \frac{1-\gamma}{\gamma}c_s\left(\frac{\gamma}{c_s}\right)^{\gamma}\frac{Y}{F_s}.$$

If $\gamma > \rho$, zero-profit conditions (C.11) and (C.13) have vertical asymptotes at $M = 0$ and $S = 0$ respectively. Inspection of the first order derivatives of S with respect to M in

(C.11) and M with respect to S in (C.13) learns that for all possible parameter values the first order derivative is strictly negative if $M < M_{S=0}$ in (C.11) and $S < S_{M=0}$ in (C.13) and zero if $M = M_{S=0}$ in (C.11) and $S = S_{M=0}$ in (C.13). For all possible parameter values the second order derivatives are strictly positive for all $M < M_{S=0}$ in (C.11) and all $S < S_{M=0}$ in (C.13). Figure IV illustrates the equilibrium (S^*, M^*). Also note that in (C.11) an increase in S of one requires a decrease in M less than one and in (C.13) a decrease in M of one requires an increase in S of less than one. This implies the equilibrium is stable.

The impact of deregulating shop closing hours

Using (C.11) for any given S, an increase in D_s (relative to D_m) requires a fall in M. Using (C.13), an increase in D_s requires an increase in S for any given M. The increase in S further decreases M in (C.11) and the decrease in M further increases S in (C.13) until a new stable equilibrium is reached. Assuming fixed daily costs of production are sufficiently small, the number of type s shops unambiguously increases and the number of type m shops unambiguously decreases following an increase in D_s. The dashed lines in Figure IV reflect the impact of deregulation on the number of type m and s shops in equilibrium.

Also note from (C.11) and (C.13) that for any given (S^*, M^*) the increase in S and the decrease in M following an increase in D_s are bigger if δ is smaller or consumers have a preference for shopping on Sunday. Similarly, the increase in S and the decrease in M

following an increase in D_s are bigger if ρ is bigger or the substitutability of goods between type *m* and type *s* shops is higher.

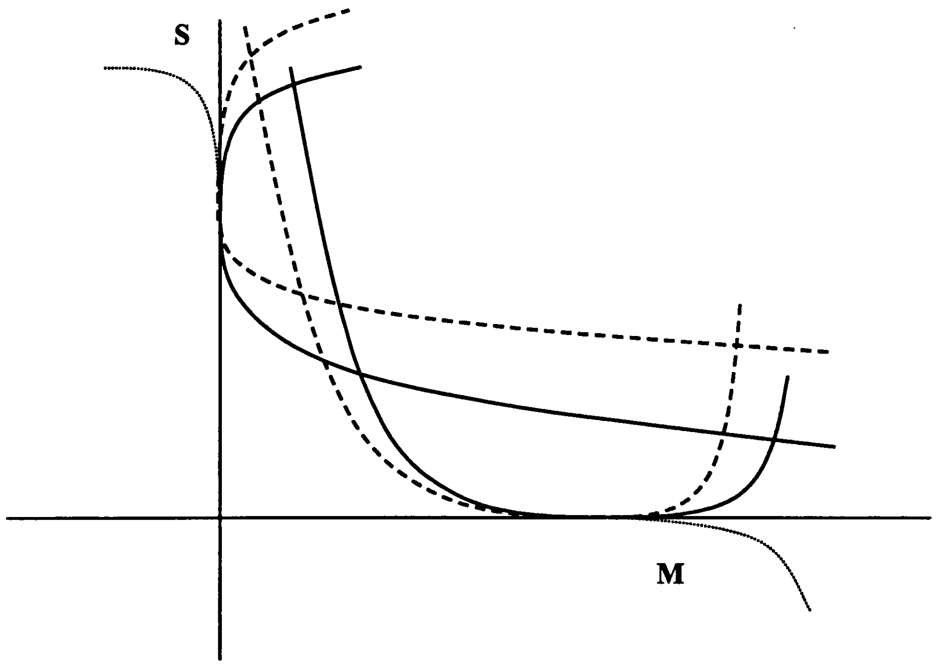
From (C.4), (C.5), (C.8), (C.11) and (C.13) it follows that an increase in D_s reduces the fraction of total income spent on type *m* goods, σ_m. The increase in S and the decrease in M further decrease σ_m as consumers will consume from every shop in equilibrium. The prediction therefore is that total sales in type *m* shops decrease and total sales in type *s* shops increase following an increase in D_s. Consequently employment in type *m* shops will decrease and employment in type *s* shops will increase.

It can also be shown that following an increase in D_s, the decrease (increase) of total sales in type *m* (*s*) shops will be larger if δ is smaller or if consumers have a preference for shopping on Sunday. Similarly, if type *m* and *s* goods are more substitutable in consumption, the decrease (increase) of total sales in type *m (s)* shops will be larger if ρ is bigger.

Multiple stable equilibria

The solid line in Figure C.I gives zero-profit conditions if $\gamma(\rho-1)/\rho(\gamma-1)$ is an even integer (and the dotted lines parts of the zero-profit conditions if $\gamma(\rho-1)/\rho(\gamma-1)$ is not an even integer) and shows multiple equilibria. The analysis remains qualitatively unchanged. For example, the comparative statics for an increase in D_s/D_m are given by the dashed lines. In all cases, S increases and M increases as before.

Figure C.I
Multiple stable equilibria



CHAPTER III

THE RECENT EXPANSION OF HIGHER EDUCATION IN BRITAIN, COLLEGE PREMIUMS AND WAGE INEQUALITY

INTRODUCTION

The continuous increase in the college-high school wage gap together with the relative increase in educated employment over the past twenty-five years has made many to believe that a secular increase in the relative demand for educated workers can go a substantial distance towards explaining college premiums (see Author, Katz and Kearney [2004] for the most recent overview of a very large literature). While this is probably true, little attention has been given to the importance of changes in the relative supply of college workers. One notable exception is Card and Lemieux [2001] (CL). More specifically for the UK, CL use the 1975-1996 General Household Survey (GHS) to argue that college premiums for cohorts born between 1955 and 1970 are higher due to a slowdown in the growth of educational attainment between 1973 and 1988.

This chapter uses the 1975-2003 GHS to analyze the impact of Britain's expansion in higher education between 1988 and 1994 documented by Walker and Zhu [2005]. If the CL hypothesis is correct, one would also expect to find evidence for it in falling college premiums for cohorts born between 1970 and 1976. To see whether this is true is the first aim of this chapter. The second aim of this chapter then is to analyze how important these inter-cohort differences in college attainment growth rates are in explaining changes over time in the average college premium and wage inequality.

The remainder of this chapter is organized as follows. Section I follows CL in presenting a simple supply and demand framework to explain variation in college premiums across age-year groups. The model presented shows that in a period of accelerating (decelerating) educational attainment, age group specific educational premiums are likely to twist so that inequality among younger workers compresses (expands) relative to the old if younger and older workers are not perfect substitutes.

Section II then examines the expansion of higher education in Britain between 1988 and 1994 in two ways. Pooling all years between 1975 and 2003, it first documents the slowdown in educational attainment growth rates for cohorts born between 1955 and 1970 and the consequent acceleration for cohorts born between 1970 and 1976. Second, regression residuals of relative supply measures regressed onto age and year fixed effects are plotted to show how these inter-cohort changes in educational attainment growth rates can explain part of the variation in the relative supply of college workers across age-year cells.

Section III then turns to estimates of college premiums to test the CL hypothesis in two ways. First, college-high school wage gaps by age-year cells are decomposed directly into age, year and cohort fixed effects. Using the increase in educational attainment between 1988 and 1994, this approach provides some evidence of lower relative earnings for the youngest cohorts. Second, substituting measures of relative supply of college workers by age-year cells for the cohort fixed effects provides some further evidence in support of the CL hypothesis. This is true accounting for the aging of cohorts born between 1955 and 1970 using the post-1996 sampling years or accounting for Britain's recent expansion in higher education.

Section IV then turns to the proximate question whether a steady trend in the relative demand for college workers is sufficient to explain the rise in the average college premium and therefore wage inequality after 1980. Accounting for a steady trend in the relative demand for and supply of college workers, Section IV shows that a significant part of the increase in the average relative wage of college workers and therefore a significant part of the rise in wage inequality can be explained by inter-cohort differences in college attainment growth rates. Relative to the secular increase in the relative demand for college workers, Britain's recent expansion of its higher education system is therefore expected to significantly decrease the college premium and therefore wage inequality. The final section concludes.

I. COLLEGE PREMIUMS AND THE RATE OF CHANGE IN INTER-COHORT EDUCATIONAL ATTAINMENT

This section follows CL in deriving an estimable structural equation relating relative wages for college workers for different age-year groups to changes in their relative supply. To see this, start by assuming aggregate output in period t takes the following CES form:

$$(1) \qquad Y_t = \left(\theta_{ht} H_t^{\rho} + \theta_{ct} C_t^{\rho}\right)^{1/\rho}$$

where θ_{ht} and θ_{ct} are technological efficiency parameters (assumed to be time specific) and where $-\infty < \rho \leq 1$ is a function of the elasticity of substitution ($\rho = 1 - 1/\sigma_E$) between high school (H_t) and higher education graduates (C_t) in production.

If younger and older workers with the same education are not perfect substitutes in production, H_t and C_t represent CES aggregates given by

$$(2) \qquad H_t = \left[\sum_j \alpha_j H_{jt}^{\eta}\right]^{1/\eta} \text{ and } C_t = \left[\sum_j \beta_j C_{jt}^{\eta}\right]^{1/\eta}$$

where α_j and β_j are age specific relative efficiency parameters and where $-\infty < \eta \leq 1$ is a function of the elasticity of substitution ($\eta = 1 - 1/\sigma_A$) between high school or college graduates of a different age. Note that (2) nests the less general case of perfect substitution between workers of different age, corresponding to the extreme case where $\eta = 1$ or σ_A is not finite and where the CES aggregates are just the sum of workers across age groups.

Efficient utilization of different skill groups then requires that the relative wages of college workers equal their relative marginal product within each age-year group. Writing the mean wage of high school and college workers of age j at time t as W_{jt}^h and W_{jt}^c respectively, one obtains the following estimable equation:

$$(3) \qquad \log(\frac{W_{jt}^c}{W_{jt}^h}) = -\log(\frac{\beta_j}{\alpha_j}) + \log(\frac{\theta_{ct}}{\theta_{ht}}) + \left[(\frac{1}{\sigma_A}) - (\frac{1}{\sigma_E})\right]\log(\frac{C_t}{H_t}) - (\frac{1}{\sigma_A})\log(\frac{C_{jt}}{H_{jt}}) + \varepsilon_{jt}.$$

The second term on the right-hand side of (3) reflects changes in the relative efficiency of college labor such as skill-biased technological change or other relative demand shocks. The third term accounts for changes in the aggregate relative supply of educated labor over time whereas the fourth term reflects the importance of age-year specific variation in the relative supply of college graduates (relative to changes common across age groups) and its coefficient thus measures the imperfect substitutability between workers of a different age. The final term reflects sampling error.

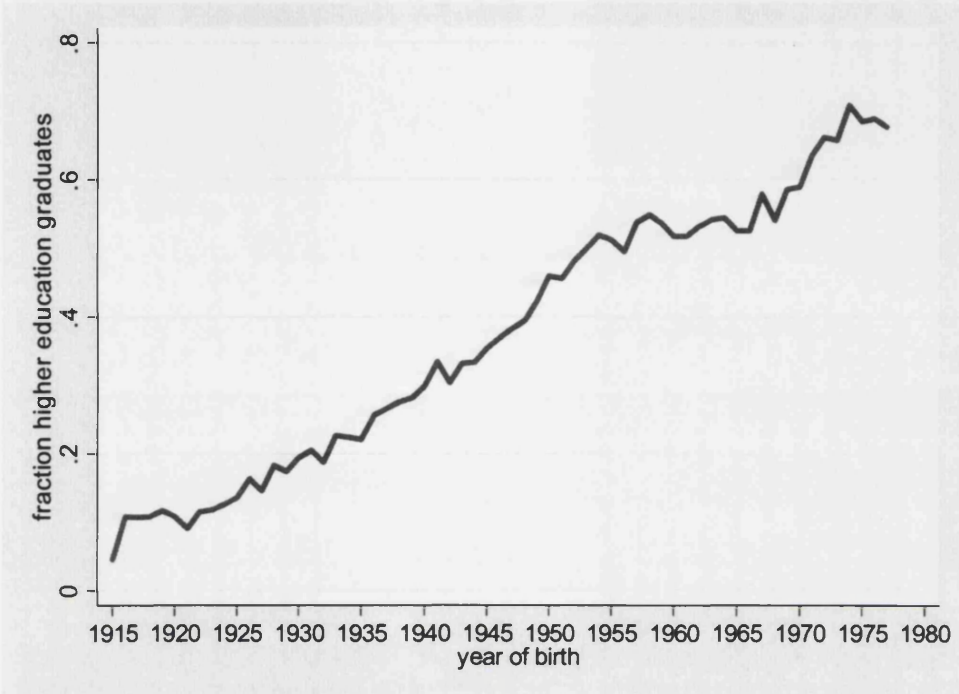
If college attainment rates are increasing at a constant rate, the relative supply of college workers by age-year groups would increase proportionately over time. If this would be the case, all the variation in college premiums would be captured by just a time trend (making abstraction of any relative age-earnings profile assumed constant over time as well as sampling variation). Equation (3) thus provides a test for the importance of any *acceleration* or *deceleration* in educational attainment if part of the variance in (C_{jt} / H_{jt}) used to identify σ_A is driven by differences in cohort attainment growth rates. More precisely, (3) shows that in a period of accelerating (decelerating) educational attainment, age group specific educational premiums are likely to twist so that inequality among younger workers compresses (expands) relative to the old if σ_A is finite. In explaining changes in college premiums, this model thus shows that it is not just the level of educational supply that matters but also its rate of change.

II. THE RECENT EXPANSION OF HIGHER EDUCATION IN BRITAIN

Figure I documents the fraction of higher education graduates by birth cohort pooling all GHS samples from 1975 to 2003. The group of college graduates consists of all workers with a college degree or a diploma from a professional institution below degree level but above GCE 'A' level standard. In contrast, the group of high school graduates consists of those whose highest qualification is any number of 'A'- or 'O'-levels, apprenticeships or workers with no qualifications. Relative supply measures are constructed by summing up usual weekly hours worked by all male workers. The Data Appendix provides more detailed information on how the relative supply of college graduates is measured consistently over time.

Figure I reflects the sharp and sudden changes in educational attainment growth rates in the UK for different birth cohorts. First, as already documented by CL using the 1975-1996 GHS, there was a slowdown in the inter-cohort trend of increasing

Figure I
College Attainment by Birth Cohort

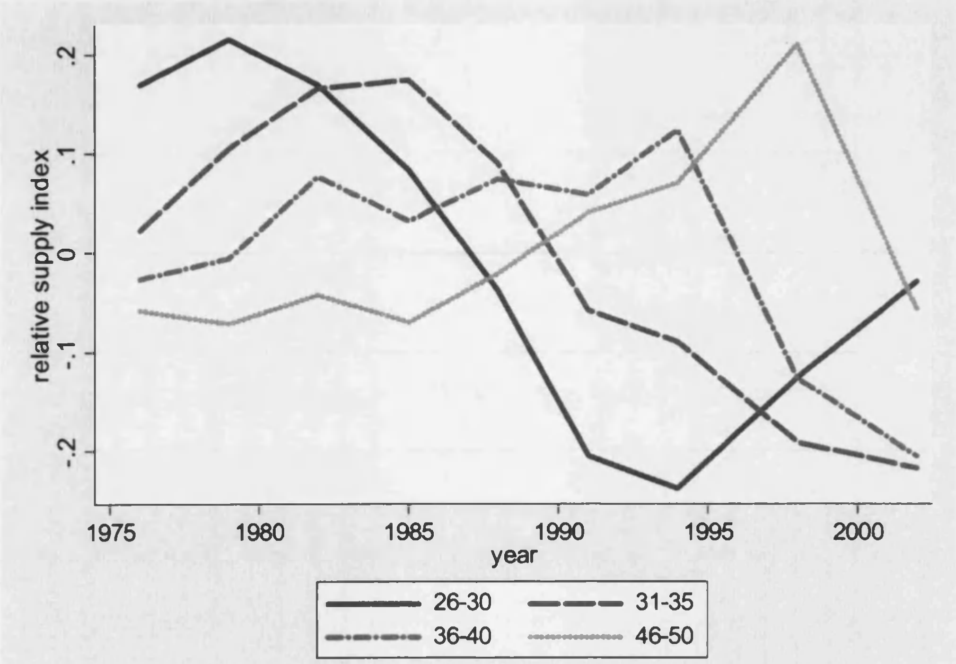


Notes: See the Data Appendix for more details about the construction of relative supply measures.

educational attainment starting with cohorts born just after 1955 and up to 1970. Second, Figure I also shows the recent expansion in Britain's higher education system between 1988 and 1994, corresponding to higher college attainment growth rates for cohorts born between 1970 and 1976. According to Walker and Zhu [2005], the recent expansion of higher education followed the removal of quotas on student numbers and the payment from central government for teaching each student, encouraging institutions to expand student numbers.

The inter-cohort differences in educational attainment growth rates shown in Figure I imply that the relative supply of college graduates might differ systematically by age. To see this more clearly, Figure II plots the residuals from a regression of log hours

Figure II
Age Group Specific Relative Supplies of College Graduates



Notes: The relative supply indices are the residuals from a regression of the log difference between hours worked by higher education graduates and high school graduates by age group and year group onto age group fixed effects and year group fixed effects. Residuals for age groups 51-55 and 56-60 are excluded from the graph. Year groups are defined as two year periods.

worked by education-age-year cells onto a dummy for college graduates, age group and year group fixed effects. Residual relative supply of workers aged 26-30 started to decrease in the early 1980's following the slowdown in college attainment growth rates for cohorts born just after 1955. The decrease in the relative supply of workers aged 26-

the variation shown in Figure I as the slowdown in educational attainment for cohorts born between 1955 and 1970 runs through the age bands up to the age of 46-50.

III. COHORT EFFECTS IN THE RETURNS TO COLLEGE

Table I tabulates college-high school wage gaps by age groups and year groups. The table entries are estimates of the difference in mean log weekly wages between men with a college degree versus those with any A-level or O-level qualification. Each year group contains a rolling age group and regressions for each age group within each year group include a linear age term and a dummy for which GHS sample the data are drawn from.[34] The Data Appendix contains more details about how the relative earnings measures have been constructed.

The entries in Table I provide a variety of information. First, comparisons down a column of the table show the change in the college premium for any given age group over time. Generally, relative wages for higher educated workers fell in the late 1970s and early 1980s before showing an increase from the 1980s onwards except for periods of relative stagnation in the early 1990s and early 2000s. Comparisons across the rows of Table I reveal the age profile of the college-high school wage gap at any point in time. As would be expected from the human capital literature (predicting that higher education graduates need to be on steeper earnings profiles), there seems to be evidence for a persistent concave relative age-earnings profile.

[34] By rolling age group is meant that, for example, for year group *(t-2)-(t+2)* workers aged 24 to 58 are used in *(t-2)*, workers aged 25 to 59 in *(t-1)*, workers aged 26 to 60 in *t*, workers aged 27 to 61 in *(t+1)* and workers aged 28 to 62 in *(t+2)*.

Table I
College-High School Wage Gaps by Age Groups and Year Groups

	Age groups						
Year groups	26-30	31-35	36-40	41-45	46-50	51-55	56-60
1975-1977	0.159	0.256	0.356	0.356	0.378	0.412	0.460
	(0.026)	(0.031)	(0.038)	(0.038)	(0.051)	(0.058)	(0.070)
1978-1982	0.110	0.240	0.291	0.367	0.360	0.361	0.426
	(0.018)	(0.020)	(0.025)	(0.029)	(0.035)	(0.044)	(0.057)
1983-1987	0.194	0.241	0.335	0.364	0.385	0.402	0.499
	(0.022)	(0.024)	(0.026)	(0.034)	(0.039)	(0.043)	(0.053)
1988-1992	0.274	0.375	0.388	0.325	0.465	0.439	0.369
	(0.027)	(0.028)	(0.029)	(0.030)	(0.039)	(0.047)	(0.070)
1993-1996	0.262	0.405	0.436	0.335	0.406	0.352	0.307
	(0.033)	(0.033)	(0.035)	(0.036)	(0.042)	(0.063)	(0.097)
1997-2000	0.325	0.485	0.568	0.483	0.499	0.472	0.315
	(0.040)	(0.042)	(0.053)	(0.052)	(0.060)	(0.071)	(0.103)
2001-2003	0.310	0.419	0.433	0.440	0.416	0.520	0.490
	(0.034)	(0.033)	(0.035)	(0.036)	(0.040)	(0.045)	(0.077)

Notes: The table entries are estimates of the difference in mean log weekly wages between men with a higher education degree versus those with A-level or O-level qualifications. Each year group contains a rolling age group and regressions for each age group within each year group include a linear age term and a dummy for which GHS sample the data are drawn from. See the Data Appendix for more details about the construction of higher education wage gaps by age groups and year groups.

However, given the evidence presented in Figures I and II, it is unlikely that all the variation in college premiums by age-year cells will be captured by age and year fixed effects only. For example, cohorts born between 1955 and 1959 enter the sampling frame in 1983-1987, twisting the relative age-earnings profile upwards. This seems to be true for all periods up to 1993-1996 when those born between 1965 and 1969 enter the sampling frame. A similar pattern can be detected for the 31-35 year olds between 1988 and 2000, for the 36-40 year olds after 1993 and for the 41-45 year olds after 1996.

Turning to the cohorts born between 1970 and 1976, the relative earnings of 26-30 year olds increased in 1997-2000. However, this increase seems small relative to an apparent positive year fixed effect. Also, relative earnings of 31-35 year olds dropped by more between 1997-2000 and 2001-2003 relative to other age groups suggesting lower relative earnings for cohorts born between 1970 and 1976 (though this seems to depend somewhat on the increase in the relative earnings for the oldest age groups).

A better test for the importance of inter-cohort differences in educational attainment growth rates would be to decompose the variance in the relative wage of college graduates by age-year cells into age group, year group and cohort fixed effects. Alternatively, the twisting of the series shown in Figure II can be used to identify the elasticity of substitution between different age groups in (3). The remainder of this section will take both approaches in turn.

III.A Cohort effects in the college premium

One way to look for the importance of changes in inter-cohort trends in educational attainment is to directly decompose the variation in relative earnings into age group, year group and cohort fixed effects. More formally, one can use the following regression equation:

(4) $\log(W_{jt}^{c} / W_{jt}^{h}) = A_j + B_t + D_{t-j} + e_{jt}$

where A_j and B_t capture age group and year group fixed effects respectively and where D_{t-j} is the product of a vector of year-of-birth dummies and their coefficients. The final term reflects sampling error.

Table II presents point estimates for year group and cohort coefficients using (4). The first two columns restate the results reported in CL. The first specification only uses cohorts born before 1950 and includes nothing but age and year fixed effects. The reported year effects show a decline in the college premium in the late 1970s and relative stability thereafter. The second column fits the data for all cohorts available up to 1996 but restricts cohort effects to be the same for those born before 1950 to allow for identification. It is clear from a comparison between the first and second column that the slowdown in educational attainment growth rates for cohorts born after 1950 goes some distance towards explaining variation in college-high school wage gaps by age-year cells. The third and fourth columns aim to replicate the CL findings using the more recent 1975-2003 GHS. The reported coefficients on the cohort dummies and their standard errors are very similar indeed.

Given the expansion of higher education in Britain between 1988 and 1994, the 1975-2003 GHS also allows to see whether cohorts born between 1970 and 1976 have lower returns to college. To this end, the final column of Table II includes data on all available cohorts. Remarkably, relative wages for the youngest cohorts are about fifty percent lower than for cohorts born a decade earlier. Also note that the coefficients for

Table II
Decompositions of College-High School Wage Differentials by Age and Year into Cohort, Age and Time Effects

	C-L		1975-1995		1975-2003
	oldest cohorts only	oldest cohorts same	oldest cohorts only	oldest cohorts same	oldest cohorts same
Year effects					
1975-1977	0.000	0.000	0.000	0.000	0.000
1978-1982	-0.086	-0.076	-0.026	-0.035	-0.034
	(0.021)	(0.018)	(0.019)	(0.017)	(0.018)
1983-1987	-0.057	-0.069	0.003	-0.021	-0.015
	(0.025)	(0.021)	(0.022)	(0.019)	(0.021)
1988-1992	-0.041	-0.037	-0.001	0.005	0.016
	(0.028)	(0.025)	(0.026)	(0.023)	(0.024)
1993-1996	-0.060	-0.039	-0.033	-0.021	-0.013
	(0.038)	(0.031)	(0.036)	(0.029)	(0.029)
1997-2000	-	-	-	-	0.044
					(0.033)
2001-2003	-	-	-	-	-0.021
					(0.044)
Cohort effects					
1950-1954	-	-0.009	-	0.006	-0.001
		(0.019)		(0.018)	(0.018)
1955-1959	-	0.075	-	0.089	0.074
		(0.025)		(0.024)	(0.024)
1960-1964	-	0.134	-	0.140	0.113
		(0.032)		(0.032)	(0.030)
1965-1969	-	0.162	-	0.146	0.160
		(0.046)		(0.047)	(0.037)
1970-1974	-	-	-	-	0.113
					(0.047)
1975-1979	-	-	-	-	0.103
					(0.073)
Degrees of freedom	14	20	14	20	30
R-squared	0.85	0.92	0.84	0.91	0.89

Notes: Standard errors are in parentheses. Models are fit by weighted least squares to the age group by year group wage gaps shown in Table I. Weights are the inverse sampling variances of the estimated wage gaps. All models include age group fixed effects.

cohorts born between 1955 and 1970 are similar to those in column four despite the fact that column five allows these cohorts to affect older age bands too through the inclusion of more recent GHS sampling years. If anything, this is evidence in support of the hypothesis that part of the variation in college-high school wage gaps by age-year groups can be explained by inter-cohort differences in educational attainment growth rates.

III.B Estimating the substitutability among cohorts

Equation (3) can be simplified to

$$(5) \qquad \log(W_{jt}^{c} / W_{jt}^{h}) = E_j - F_t - (1/\sigma_A)\log(C_{jt} / H_{jt}) + \mu_{jt}$$

where E_j is the product of a vector of age group dummies and their coefficients and where F_t captures skill-biased technological change, changes in aggregate relative supply or any other parallel shift over time of a relative age profile. The third term reflects the importance of age-year group specific variation in identifying a finite elasticity of substitution between workers of a different age. The final term reflects sampling error.

The first column of Table III replicates the point estimates found in CL reporting an elasticity of substitution of about 4 (1/0.233). The second column aims to reproduce this result using the 1975-2003 GHS and finds an almost identical estimate for σ_A. Also note that though the level of the estimated time effects (relative to the omitted year group 1975-1977) are somewhat larger in the current data set, the log point changes are very similar. The final column of Table III further includes sampling years 1996 to 2003. Just as in Section III.A, the use of more recent sampling years provides a twofold test of the CL hypothesis. First, it allows cohorts born between 1955 and 1970 to grow older and therefore to relatively increase the college premium for older age groups too. Second,

Table III
Estimated Models for the College-High School Wage Gap by Age and Year

	C-L	1975-1995	1975-2003
Age-group specific relative supply	-0.233 (0.058)	-0.240 (0.065)	-0.210 (0.050)
Year effects			
1975-1977	0.000	0.000	0.000
1978-1982	-0.032 (0.023)	0.068 (0.034)	0.056 (0.029)
1983-1987	0.060 (0.034)	0.143 (0.067)	0.162 (0.054)
1988-1992	0.149 (0.039)	0.203 (0.079)	0.231 (0.063)
1993-1996	0.199 (0.044)	0.266 (0.093)	0.285 (0.074)
1997-2000	-	-	0.356 (0.090)
2001-2003	-	-	0.384 (0.086)
Degrees of freedom	23	23	35
R-squared	0.86	0.87	0.87

Notes: Standard errors are in parentheses. Models are fit by weighted least squares to the age group by year group wage gaps shown in Table I. Weights are the inverse sampling variances of the estimated wage gaps. All models include age group fixed effects.

Britain's expansion in its higher education system between 1988 and 1994 possibly decreases college premiums for cohorts born between 1970 and 1976. Accounting for both, the final column of Table III finds an estimated partial elasticity of substitution between different age groups of about 5 (1/0.210) which is remarkably similar to estimates derived from the first and second column. The evidence in Table III therefore suggests that the CL hypothesis cannot be easily rejected.

In sum, it is intuitive to think that young college graduates are more suited to doing certain tasks relative to older college graduates. But what is remarkable is that differences in inter-cohort trends in the relative supply of educated labor seem to go a substantial distance towards explaining college premiums by age-year groups. This seems to be true whether looking for first-order evidence of cohort effects or whether using a more structural approach.

III.C Alternative specifications

One possible caveat using equation (5) is that comparisons are based on age and not experience cohorts. That is, high school graduates potentially have more experience than higher education graduates of the same age. Since relative earnings of higher education graduates are expected to rise over the life cycle according to the human capital literature (college graduates are on the steeper part of the age-earnings profile later in life), any change over time in the relative returns to experience correlated with changes in (C_{jt}/H_{jt}) will give biased estimates of σ_A. One way to assess the importance of this bias is to examine time variation in college premiums for experience groups rather than age groups.

Table IV pools workers with similar levels of labor market experience and repeats the analyses done in Tables II and III. Only those with 6 to 36 years of experience are used in the regressions using five-year experience groups. The assumption of the five-year bracket was made mainly for convenience to correspond to the five-year observation intervals used previously.

Table IV
Decompositions of College-High School Wage Differentials by Experience Groups

	1975-1995	1975-2003	1975-1995	1975-2003
Experience-group specific relative supply	-	-	-0.088	-0.068
			(0.083)	(0.047)
Year effects				
1975-1977	0.000	0.000	0.000	0.000
1978-1982	-0.032	-0.034	0.012	-0.005
	(0.019)	0.020	(0.052)	(0.047)
1983-1987	-0.016	-0.017	0.091	0.058
	(0.021)	0.022	(0.095)	(0.083)
1988-1992	0.001	0.005	0.152	0.011
	(0.024)	0.025	(0.113)	(0.097)
1993-1996	-0.031	-0.022	0.154	0.108
	(0.030)	(0.030)	(0.128)	(0.112)
1997-2000	-	0.028	-	0.172
		(0.033)		(0.131)
2001-2003	-	-0.043	-	0.232
		(0.045)		(0.140)
Cohort effects				
1950-1954	0.006	0.009	-	-
	0.019	(0.019)		
1955-1959	0.051	0.048	-	-
	0.026	(0.025)		
1960-1964	0.133	0.105	-	-
	0.036	(0.031)		
1965-1969	0.164	0.150	-	-
	0.055	(0.039)		
1970-1974	-	0.143	-	-
		(0.051)		
1975-1979	-	0.086	-	-
		(0.096)		
Degrees of freedom	46	66	23	35
R-squared	0.53	0.64	0.33	0.51

Notes: Standard errors are in parentheses. Models are fit by weighted least squares and weights are the inverse sampling variances of the estimated wage gaps. The wage gaps represent the wage difference between higher education and high school graduates with the same level of potential labor market experience. Only those with 6 to 36 years of experience are used in the regressions using five-year experience groups. The assumption of the five-year bracket was made to correspond to the five-year observation intervals.

The first two columns of Table IV decompose the variation in relative wages by experience-year cells into experience fixed effects (not reported), year fixed effects and cohort fixed effects while restricting the coefficient for all cohorts born before 1950 to be the same. Using years 1975 to 1996, the point estimates are relatively similar to those found in the fourth column of Table II. The second column of Table IV uses all available data and shows that the college premium for cohorts born after 1970 decreased in line with point estimates in the final column of Table II, though not significantly so before 1975.

Point estimates of the elasticity of substitution between different experience groups are reported in the last two columns of Table IV. Assuming the large standard errors are driven by measurement error in the dependent variable (potential rather than actual experienced is used in the analysis), the estimated elasticity of substitution between workers with different experience is finite and between 11 and 14.

The results in Table IV seem to require some caution for the evidence in support of the CL hypothesis found in Table III. However, point estimates in Table III are biased only if the relative returns to experience are unstable over time and have changed in a way consistent with changes in the relative supply by age groups. Also, point estimates of the elasticity of substitution derived from Table IV are likely to be upward biased since measurement error in true experience will bias the reported point estimates towards zero. Finally, though using experience groups rather than age groups in the analysis has the advantage of comparing workers with similar potential experience, one disadvantage is that the analysis in Table IV does not compare individuals who attended high school together and were subject to the same influences in their decision as to whether or not pursue further education. All in all, the evidence presented in Table IV does not seem sufficiently convincing in order to reject the CL hypothesis.

IV. COLLEGE ATTAINMENT GROWTH RATES, THE AVERAGE COLLEGE PREMIUM AND WAGE INEQUALITY

IV.A Changes in the average college premium and wage inequality

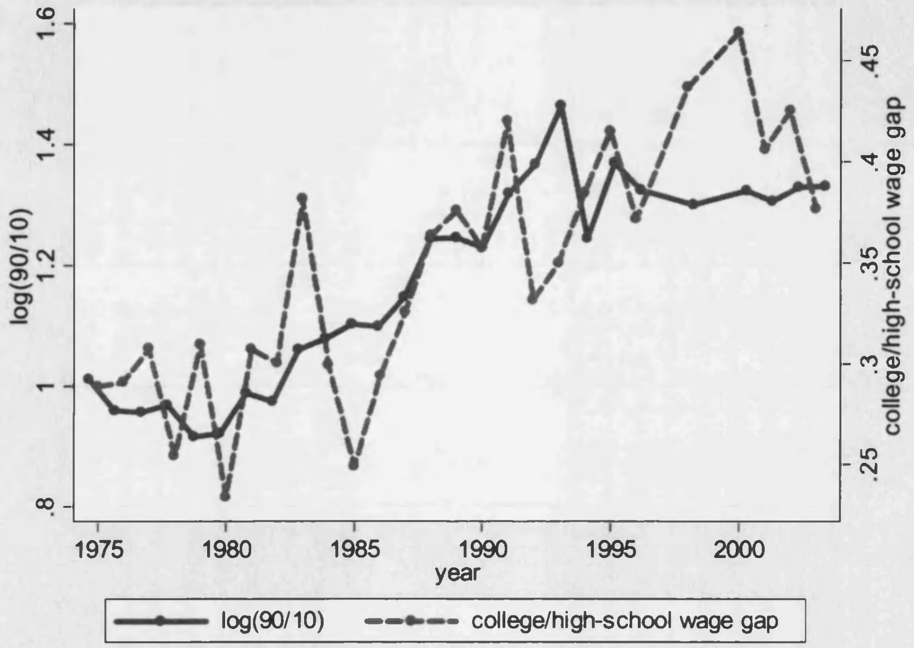
Figure III uses the 1975-2003 GHS to illustrate the well documented decrease in wage inequality during the late 1970s and its subsequent increase during the 1980s in the UK. Figure III also shows a similar pattern for the overall average college premium. This section will therefore examine the proximate question whether a significant part of the increase in the average college premium and therefore wage inequality during the 1980s can be attributed to the decreased growth of college attainment for cohorts born between 1955 and 1970.

Pooling observations into age groups and year groups as in the previous section, Figure IV plots the average college premium across age groups over time. That is, the solid line in Figure IV is given by

$$(6) \qquad \log(W_t^c / W_t^h) = \sum_j s_{jt} \log(W_{jt}^c / W_{jt}^h)$$

where s_{jt} is the fraction of all workers aged j at time t. In line with the estimated college premiums in Figure III, also this approach shows a sharp increase in the college premium after 1978-82.

Figure III
Wage Inequality and the Average College Premium



Notes: The college/high-school wage gap is the weighted mean across age groups of the estimated difference in mean log weekly wages between men with a higher education degree versus those with A-level or O-level qualifications for each two-year period. Each period contains a rolling age group and regressions for each age group within period include a linear age term and a dummy for which GHS sample year the data are drawn from.

IV.B Secular and non-secular changes in the average college premium and wage inequality

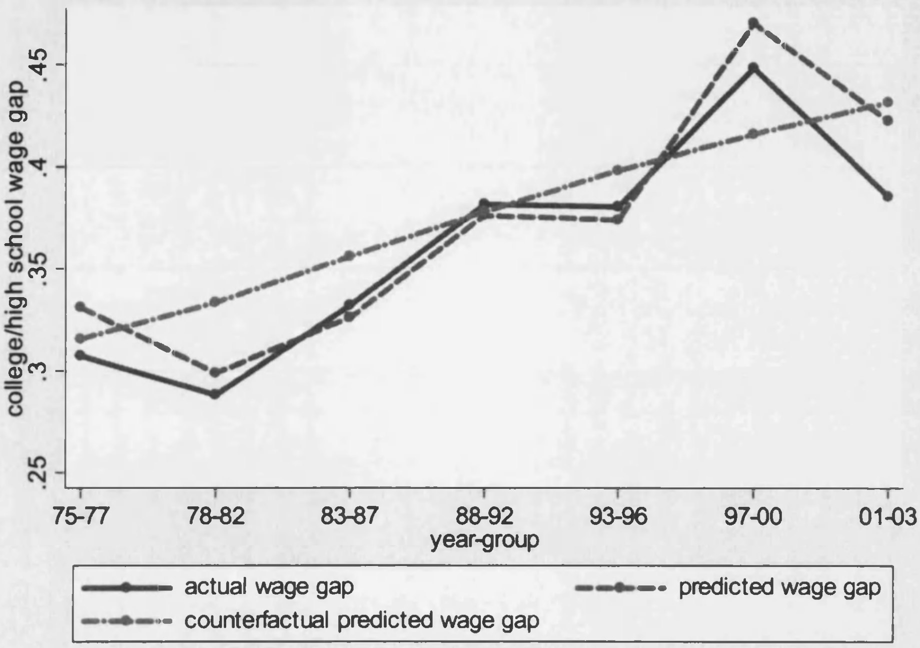
The dashed line in Figure IV consists of the predicted college premium in any given year using (5) and the full sample. More specifically, the plotted predicted wage gap for any year group t is given by:

$$(7) \qquad \hat{\log}(W^c_{jt}/W^h_{jt}) = \sum_j s_{jt}\, \hat{\log}(W^c_{jt}/W^h_{jt})$$

with

$$(8) \qquad \hat{\log}(W^c_{jt}/W^h_{jt}) = \hat{E}_j - \hat{F}_t - (1/\hat{\sigma}_A)\log(C_{jt}/H_{jt})$$

Figure IV
Actual, Predicted and Counterfactual Wage Gaps



Notes: The predicted wage gap is the weighted mean across age groups of the predicted difference in mean log weekly wages between men with a higher education degree versus those with A-level or O-level qualifications for each year group using equation (5). The counterfactual predicted wage gap uses GHS sampling years 1975 to 1990 to provide estimates of the predicted counterfactual impact of the secular increase in the relative demand for and supply of college workers between 1975 and 2003 and therefore does not account for the impact of inter-cohort differences in educational attainment growth rates.

where a hat reflects the use of coefficient estimates. As would be expected from the high R-squared found in the last column of Table III, predicted and actual college premiums move closely together, indicating the accuracy of the simple relative supply-demand model presented above.

However, changes in the predicted average college premium are not necessarily informative about the relative importance of a slowdown in educational attainment growth rates after 1978-82. More specifically, the fixed time effects in (8) also account for the impact of higher relative demand for college workers as well as an increase in the relative supply of college workers common across all age groups. A simple way to distinguish between secular relative demand or supply shifts and age group specific relative supply shocks is to construct a counterfactual series of the average college premium assuming there was no fall in educational attainment growth rates for cohorts born between 1955 and 1970.

Looking back at Figure I, it is clear that educational attainment rates grew at pretty much a constant rate for cohorts born before 1955. This implies that all (C_{jt} / H_{jt}) were increasing proportionately (say, at rate η) as more educated cohorts gradually entered older age brackets. If this is the case and if an increase in the relative demand for college workers is also best described by a linear time trend (say, with slope γ), equation (5) rewrites as:

$$(9) \qquad \log(W_{jt}^{c} / W_{jt}^{h}) = E_j - F_t - (1/\sigma_A)\log(C_{jt} / H_{jt}) + \mu_{jt}$$

where

$$(10) \qquad F_t = \log(C_{1975} / H_{1975}) + \gamma_{1975} + (\eta + \gamma)t$$

and

$$(11) \qquad \log(C_{jt} / H_{jt}) = \log(C_{j1975} / H_{j1975}) + \eta t .$$

Using only cohorts born before 1955 and corresponding estimated fixed time effects from (9), parameter estimates of all coefficients in (10) and (11) can be obtained. In practice, this implies restricting the GHS sample to years between 1975 and 1992. The decision to also include the late 1980s when estimating (10) and (11) (since cohorts born after 1955 start entering the sampling frame already in 1983-87) was made for several reasons. First, if cohorts born after 1955 do have a positive impact on the estimated time trends, the importance of inter-cohort differences in educational attainment growth rates will be understated. Second, it is sometimes argued that the 1980s were characterised by increased growth in the relative demand for college workers. Extending the time frame to include the late 1980s partially accounts for this possibility.

Equation (9) then shows that coefficient estimates from (10) and (11) together with the coefficient estimates used in (8) are sufficient to predict college premiums by age-year cells. Using (6) then gives the counterfactual predicted college premium for each year group between 1975-77 and 1988-92. Moreover, since predictions for (9) now only depend on the initial distribution of college attainment in 1975 and time *t*, the counterfactual college premium can easily be predicted for all year groups. This is the counterfactual series given by the dashed-dotted line in Figure IV.

Figure IV shows that between 1978-82 and 1997-00 the actual college premium increased with 16 log points from 0.28 to 0.44. Counterfactual wage gaps show that the college wage premium would have increased by 8 log points if only the relative demand and supply of college workers would have grown proportionately over time as they did before 1983-87. This suggests that the slowdown in educational attainment growth rates for cohorts born after 1955 could have increased the average college premium by as much as 8 log points or about half of its total increase. Similarly, Figure III showed a 40 log point increase in the log(90/10) wage differential from 0.92 in 1980 to 1.32 in 2000. Assuming that an 8 percentage point increase in the college premium leads to about a 16 log points increase in the log(90/10) wage differential, the fall in educational attainment growth rates for cohorts born after 1955 can explain as much as forty percent of the total increase in wage inequality.

The estimated impact of inter-cohort differences in educational attainment growth rates on the average college premium and wage inequality is derived from what is merely more than back-on-the-envelope computations. Their relevance should therefore be judged with some caution. Most importantly, counterfactual trends only account partially for the possibility of skill-biased technological change in the late 1980's (relative to previous periods) leading to an overstatement of the importance of age group specific differences in relative supply. However, there has been some confusion about whether or not the 1980s were the decade in which technological innovation increased worker productivity by more (see Card and DiNardo [2002] for a discussion). Moreover, there are also reasons to believe why the above numbers could be understating the importance of inter-cohort differences in educational attainment growth rates. First, the counterfactual series in Figure IV partially accounts for the impact of higher relative wages for workers born between 1955 and 1965 since the counterfactual time trends are estimated using sampling periods 1975-77 to 1988-1992 thereby including cohorts born after 1955 up to the age of 35. Second, because cohorts born after 1955 have not entered the oldest age brackets yet, the full impact of the slowdown in educational attainment growth rates on the increase in the actual college premium still is incomplete.

IV.C The recent expansion of Britain's higher education system, the average college premium and wage inequality

If inter-cohort differences in educational attainment growth rates could go a substantial distance towards explaining changes in the college premium and therefore wage inequality, predictions can be made about the impact of the recent expansion in

Britain's higher education system. Inspection of Figure I learns that the acceleration in educational attainment for cohorts born after 1970 is about as large as the deceleration for cohorts born between 1955 and 1970. Relative to the secular increase in the demand for and supply of college workers, the college premium is therefore expected to fall by about 8 percentage points and wage inequality by about 16 log points as the post 1970 birth cohorts will come of age.

V. CONCLUSIONS

There is little doubt that a simple model accounting for the "secular" increase in the relative demand for and supply of college workers can go a substantial distance towards explaining changes in the average college premium and wage inequality over time. However, in line with Card and Lemieux [2001], this chapter has argued that fluctuations in the rate of growth of the relative supply of college workers can also explain an important part of the variation in earnings dispersion. More specifically, the "episodic" events of a slowdown in educational attainment growth rates for cohorts born between 1955 and 1970 and the subsequent acceleration for cohorts born after 1970 seem important in understanding what has happened and will happen to the college premium and wage inequality over time.

This chapter first provided a twofold test of the Card and Lemieux [2001] (CL) hypothesis. Using the 1975-2003 GHS rather than the 1975-1996 GHS, it was shown that college premiums for cohorts born between 1955 and 1970 are higher, in line with the CL hypothesis since these cohorts are characterised by lower college attainment growth rates.

Moreover, the more recent data also accounted for the impact of Britain's expansion in higher education between 1988 and 1994, finding lower college premiums for those born between 1970 and 1976.

What many would see as the proximate question is whether a steady trend in the relative demand for college workers is sufficient to explain growing wage inequality after 1980. Using that for cohorts born before 1955 educational attainment growth was more or less constant, it was conjectured that only half of the increase in the average college wage premium could be explained by secular changes in the relative demand for and supply of college workers. Therefore, the fall in educational attainment growth rates for cohorts born between 1955 and 1970 could explain an important part of the rise in the average college premium and therefore wage inequality after 1980. Consequently, as cohorts born after 1970 will come of age, the college premium and wage inequality are expected to fall relative to the impact of a secular increase in the relative demand for college workers.

Data Appendix

A. Relative Supply Measures

U.K. workers are divided into five education groups for the purpose of constructing supply measures of higher education graduates relative to high school graduates using the 1975-2003 GHS. The group of higher education graduates consists of all workers with a higher degree (Census Level A); a first degree/university diploma or certificate/qualifications obtained from colleges of further education or from professional institutions of degree standard (Census Level B); HNC/HND/BEC/TEC Higher/City & Guilds Full Technological Certificate/university diploma or certificate/Qualifications obtained from colleges of further education or from professional institutions below degree level but above GCE 'A' level standard (Census Level C). The group of high school graduates consists of those whose highest qualification is any number of A-levels or O-levels with or without commercial qualifications; clerical and commercial qualifications without GCE 'O' level; GCE 'O' level in grades D or E; apprenticeships; no qualifications.

Relative supply measures are constructed by summing up usual weekly hours of work of all male workers (self-employed and wage and salary workers) by age and year. Because of the relative small size of the GHS samples, working hours are summed over age groups and year groups. For example, years 1978 to 1982 pool workers aged 24 to 58 in 1978, aged 25 to 59 in 1979, aged 26 to 60 in 1980, aged 27 to 61 in 1981 and workers aged 28 to 62 in 1982. Similar rolling age bands are used to construct other year groups and relative supply measures by age group within each year group.

B. Relative Wage Measures

Wage gaps in Table I are based on samples of weekly wages for men with a higher education degree and an A- or O-level degree. For years 1983 to 1987, reported wages are divided by pay period to construct average weekly wages.

The wage gaps are estimated in separate regression models for each age group/year group combination. These regressions all include a dummy for having a higher education degree, a linear age term and dummies for which GHS sample the observation was drawn from. A similar procedure is used to compute wage gaps by experience groups, except that the regression models for each experience group include a linear experience term instead of a linear age term. The inverse of the estimated variance of the coefficient on the dummy for having a higher education degree is used as weight in the models reported in the chapter.

CHAPTER IV

CYCLICALITY AND FIXED EFFECTS IN GROSS JOB FLOWS: A EUROPEAN CROSS COUNTRY ANALYSIS

INTRODUCTION

Over the past decade, a growing body of research has emerged focusing on the analysis of gross flows of jobs. Using different US data, Blanchard and Diamond (1990) and Davis, Haltiwanger and Schuh (1996) document the existence of continuous job creation and destruction, even within narrowly defined sectors, regions and plant types. Furthermore, they show that job creation is pro-cyclical whereas job destruction is counter-cyclical. Also, during the 1970s and 1980s, variation in job turnover (i.e. the sum of absolute job creation and job destruction) has been counter-cyclical and driven by the higher volatility of job destruction over the business cycle.

A number of models have been developed to explain the behavior of job flows. All these models start from the premise that workers or firms are subject to real idiosyncratic shocks that cause heterogeneity among economic agents to explain the continuity in employment flows. Moreover, the combination of micro shocks with reallocation frictions, like search or adjustment costs, provides a mechanism through which common demand or productivity shocks cause aggregate economic fluctuations. For example, Mortensen and Pissarides (1994) provide a model in which workers and firms are subject to idiosyncratic productivity shocks in a matching framework where the formation of worker-firm pairs is time consuming. Another example is Caballero and Hammour (1994), formalising the idea that business cycles (and in particular recessions) provide a cleansing mechanism for reducing organisational inefficiencies and resource misallocations.

Following the seminal work by Blanchard and Diamond (1990) and Davis, Haltiwanger and Schuh (1996), a host of new empirical studies has emerged.

Estimates of employment dynamics are included in Dunne, Roberts and Samuelson (1989) and Davis and Haltiwanger (1999) for the United States and Baldwin, Dunne and Haltiwanger (1998) for the United States and Canada; Konings (1995) and Blanchflower and Burgess (1996) for the United Kingdom; Leonard and Van Audenrode (1993) for Belgium; Boeri and Cramer (1992) for Germany; Broersma and Gautier (1997) for the Netherlands and Albaek and Sorensen (1995) for Denmark. Finally, the analysis of gross job flows has recently become increasingly important in understanding restructuring in developing and emerging economies.[35]

Although attempts have been made to compare gross job flows between countries, concerns have been raised about spurious time-series and cross-country variation in gross job flow measures due to the unavailability of comparable micro data.[36] For example, the unit of observation could be the firm or the plant, sector composition could be different or the period of observations may differ between the various studies.[37] One paper allowing for a consistent comparison of gross flows of jobs between two countries is Baldwin, Dunne and Haltiwanger (1998) using harmonised data for manufacturing industries in Canada and the US. They find that 1) job destruction is more cyclically volatile than job creation in both countries but more so in the US and 2) industries with high (low) gross job flows in Canada are characterized by high (low) gross job flows in the US. Accordingly, this result suggests that country specific differences are not important and that the process of job reallocation in both countries is best explained by an industry fixed effect.

[35] Examples are Konings, Lehmann and Schaffer (1996), Roberts and Tybouts (1998) and Haltiwanger and Vodopivic (2002).
[36] Examples are Davis, Haltiwanger and Schuh (1996) and OECD (1994,1996).
[37] Examples of how different sampling procedures can give rise to different conclusions are found in the OECD (1994) and Boeri (1996).

This chapter uses information on manufacturing establishments during the 1990s in four European countries: Belgium, France, Italy and the United Kingdom. The strength of the analysis comes from the fact that the data are comparable across countries: the sampling criteria, the time frame and the sector composition are uniformly defined. Furthermore, the data cover the majority of manufacturing employment in all four countries.[38] Finally, following the approach taken by Baldwin, Dunne and Haltiwanger (1998) this study is the first to provide measures of gross job flows for disaggregated industries within manufacturing for the listed European countries. The key issues addressed here are 1) whether time series of employment dynamics behave differently across European countries 2) whether persistent differences exist that are country specific and 3) whether persistent differences exist that are industry specific. The answers are yes, yes and no.

The chapter is organized as follows. The next section provides a general framework to analyse steady-state and out-of-steady state paths of job creation and job destruction. Section II describes the data and explains how job flow rates have been measured. Section III examines the cyclical pattern of gross job flows in all four countries. Section IV aims to explain the between-variation in gross job flows as a function of country and sector specific effects. Finally Section V concludes.

I. A FRAMEWORK TO ANALYSE CYCLICALITY AND FIXED EFFECTS IN GROSS JOB FLOWS

[38] In 1999, the number of firms for which employment flows can be calculated covers 87% of total manufacturing employment in Belgium, 63% in France, 54% in Italy and 62% in the UK. See Section II for further discussion.

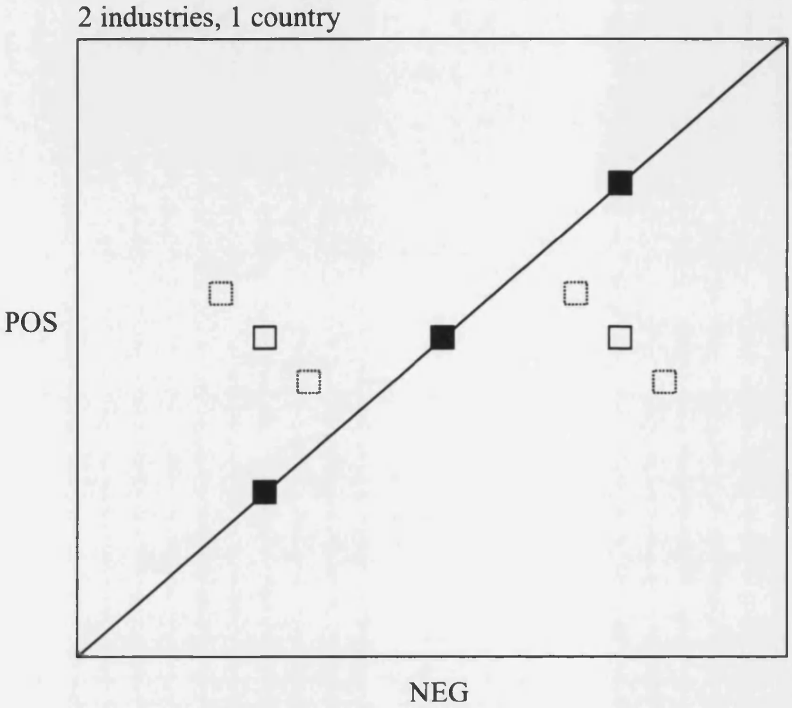
Given a sufficiently long sampling frame, steady-state implies that

$$(1) \qquad \left(\overline{POS_{ic}} - \overline{NEG_{ic}}\right) = 0 \qquad\qquad \forall i \in c$$

where *POS* is the job creation rate, *NEG* is the job destruction rate and where bars indicate sample averages across time. Subscript *ic* indicates industry *i* in country *c*. Equation (1) says that steady-state job creation and job destruction rates must be equal within each industry. However, different industries can have different job creation and destruction rates, even in the long-run. For example, Figure I depicts an industry with lower steady-state job creation and destruction (the bottom black square on the diagonal) and another industry with steady-state job creation and destruction closer to the top-right corner (the top black square on the diagonal). If this is the case, industry fixed effects capture time persistent differences in job creation or destruction rates between industries and are expected to go some distance towards explaining variation in gross job flows. Similarly, time persistent differences in job creation and destruction rates between countries (assuming two countries and a single industry in Figure I) could account for an important part of the total variation in job flows if countries systematically differ, even in the long-run.

If the sampling period is only long enough to average out the business cycle, out-of-steady-state dynamics could change the distribution of sector employment shares if $\left(\overline{POS_{ic}} - \overline{NEG_{ic}}\right) \neq 0$. Note that this could happen without a change in the steady-state unemployment rate. For example, Figure I shows the case where both industries have the same job creation rate but different job destruction rates (the solid white squares) and such that economy wide job flows remain the same as before or

Figure I
Job Creation and Job Destruction



steady-state

out-of-steady-state with no deviations from the economy-wide natural rate of unemployment

out-of steady-state with deviations from the economy-wide natural rate of unemployment

$\left(\overline{POS_c} - \overline{NEG_c}\right) = 0$ (the black square in the middle of the diagonal).

Finally, at any point in time the natural rate of unemployment will deviate from its steady-state level following temporary aggregate demand or productivity shocks. More precisely, based on predictions from Mortensen and Pissarides (1994), Pissarides (2000) and Caballero and Hammour (1994), aggregate demand or productivity shocks will move job creation and destruction rates in opposite directions over the business cycle and cause transitory dynamics in the economy wide unemployment rate. The behavior of job creation and destruction over the business cycle is illustrated in Figure I (the dotted white squares). An economy wide boom would be a relatively short period in which job creation is higher and job destruction is lower in each industry (relative to the solid white squares) whereas a slump would result in relatively higher job destruction and lower job creation in each industry. It is also intuitively clear that the volatility of job creation and destruction rates over the business cycle can depend on different reallocation frictions across countries.

II. DATA AND MEASUREMENT ISSUES

II.A Data

The data used are based on the reported unconsolidated company accounts of manufacturing establishments covering the years 1991-1999. The data are taken from a commercial data source compiled by Bureau Van Dijk, specialized in data collection and dissemination. The data source is referred to as the Amadeus database of establishment accounts. For France, Italy and the UK, to be included one of the following criteria has to be satisfied: operating revenue must be 1.5 million Euros or higher, total assets must be 3 million Euros or higher or the number of employees must be 15 or more. For Belgium the sampling criteria are somewhat less restrictive, given the respective numbers are 1 million, 2 million or 10 employees.

Only continuing establishments are retained in the analysis. However, this does not imply a balanced panel since a continuing establishment is defined as any establishment reporting strict positive employment in two consecutive years. The reason to exclude employment flows at the extensive margins of establishment employment is to avoid misclassification of establishment birth or death. Some papers have focussed on the impact of establishment entry and exit on employment flows, however not without difficulties.[39]

Table I presents the annual average amount of continuing establishments observed in each country-industry match as well as average establishment size. The distribution of establishments within each country is dispersed but seems similar across countries. Besides persistent differences in concentration and establishment size between industries, observed establishments seem to be bigger in the UK and Italy relative to Belgium and France. This is also seen from Figure II showing censored densities of average establishment size. The relative importance of small Belgian firms in the data is possibly due to the less restrictive sampling criteria. Nevertheless, Figure II shows another advantage of the Amadeus data set over most existing administrative sources: even though small establishments are likely to be underrepresented, they account for most of the observed establishment population in all four countries.

Table II compares sampled employment in continuing establishments with total paid manufacturing employment reported by the ILO and OECD. For each country, the first two columns of Table II give the ratio of observed total employment

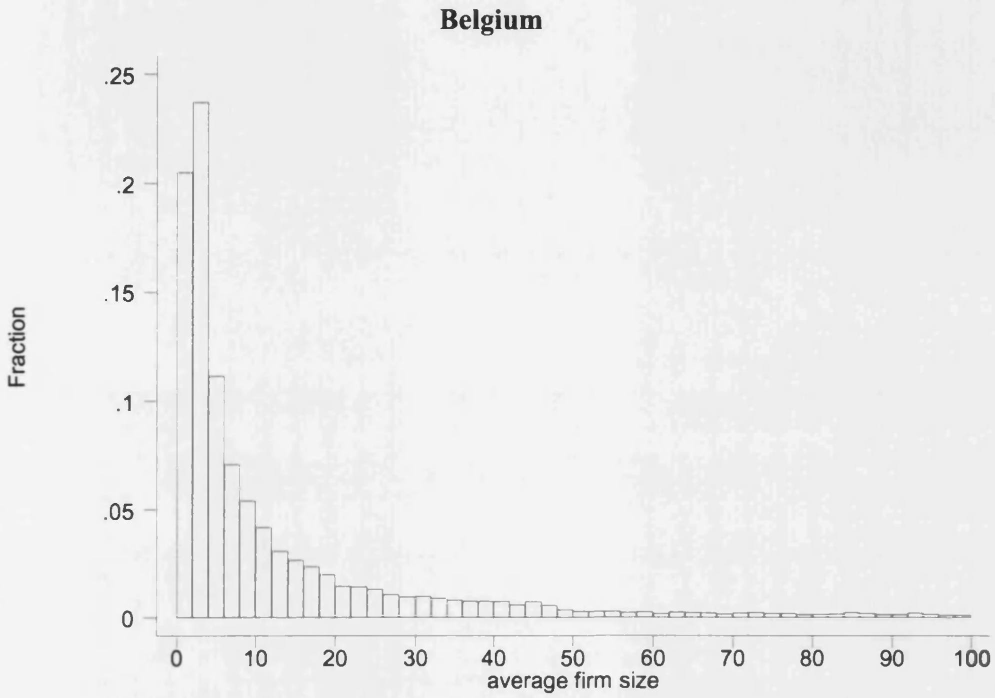
[39] The problems of including entry and exit have been analysed by OECD Employment Outlook (1994,1996) for a number of OECD countries and Boeri and Cramer (1992) for Germany.

Table I
Number of Observations and Average Firm Size by Industry and Country

	Belgium		*France*		*Italy*		*UK*	
	number of firms	*firm size*	*number of firms*	*firm size*	*number of firms*	*firm size*	*number of firms*	*firm size*
Food (15)	2125	31	3115	51	2110	63	915	401
Tobacco (16)	24	128	1	974	11	91	13	1613
Textiles (17)	836	43	686	54	2116	65	568	188
Apparel (18)	489	25	903	42	1021	65	297	242
Leather (19)	76	33	299	63	995	47	120	237
Wood (20)	690	15	1018	28	490	42	287	90
Paper (21)	229	59	433	106	562	72	448	199
Publishing (22)	1664	17	2631	29	792	75	1564	116
Petroleum (23)	16	275	39	510	104	75	41	866
Chemicals (24)	454	131	875	160	1200	112	825	320
Rubber (25)	477	44	956	94	1229	58	666	167
Other non-metals (26)	797	39	957	64	1283	66	333	269
Basic metals (27)	167	252	293	246	743	113	428	174
Fabricated metals (28)	2116	21	4138	37	2939	62	1987	144
Machinery (29)	774	49	1996	65	3042	79	966	234
Office machinery (30)	30	23	77	217	110	128	172	411
Elec. machinery (31)	313	75	629	107	1003	105	682	238
Communication (32)	96	182	467	143	338	174	332	266
Precision (33)	299	19	1084	48	472	73	450	323
Vehicles (34)	209	188	377	373	368	389	188	921
Other transport (35)	109	73	275	257	223	266	234	377
Furniture (36)	1065	19	1254	36	1434	48	1293	120
Recycling (37)	205	14	332	18	58	38	36	49
All sectors:	13263	40	23063	67	22645	79	12849	223

Notes: Industry numbers in brackets refer to 2-digit NACE Rev.1 classifications. The number of firms refers to the annual average number of firms reporting employment changes. Firm size refers to time averaged number of employees in these firms.

Figure II
Distribution of Continuing Firms According to Employment Size for Belgium and France (Censored at 100 Employees)



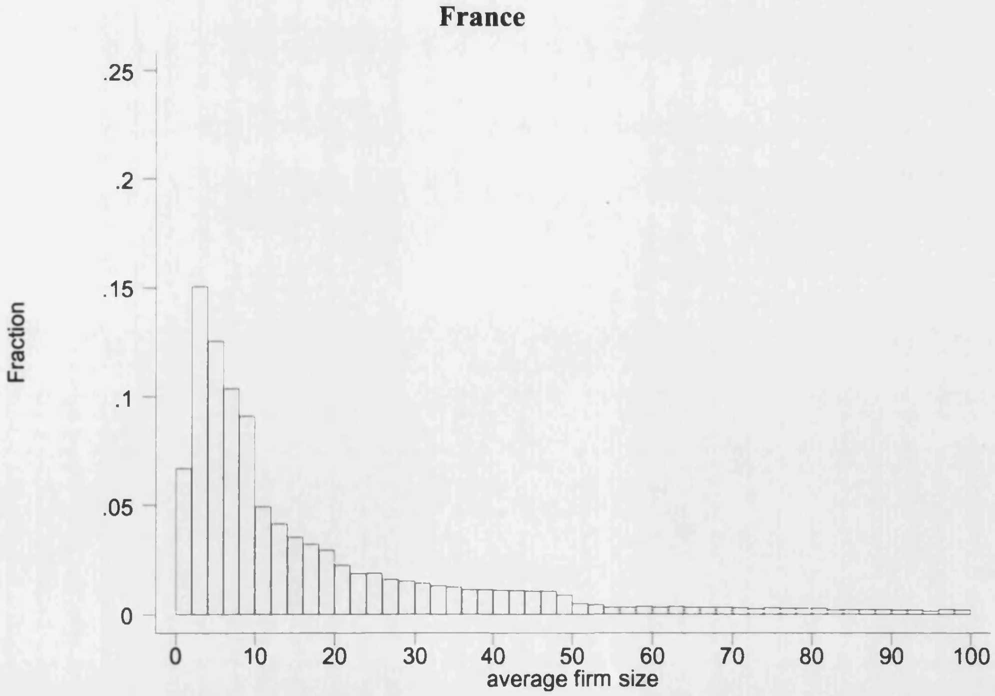
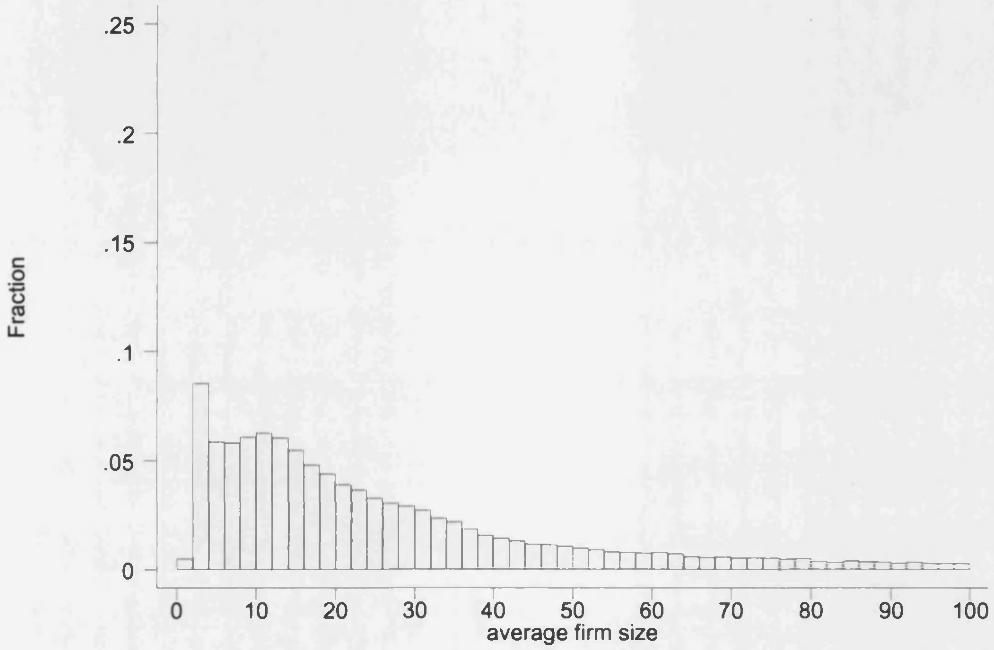


Figure II (cont.)
Distribution of Continuing Firms According to Employment Size for Italy and United Kingdom (Censored at 100 Employees)

Italy



United Kingdom

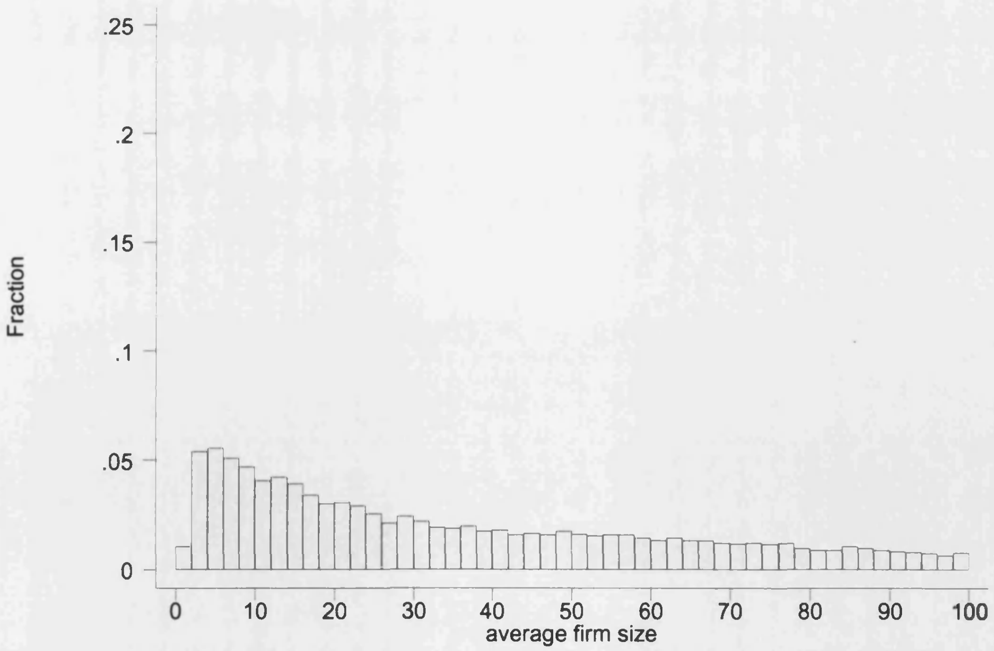


Table II
Coverage and Net Employment Growth in Manufacturing: Amadeus versus ILO and OECD

	Belgium					*France*				
	coverage		*net employment growth (in %)*			*coverage*		*net employment growth (in %)*		
Year	ILO	OECD	Amadeus	ILO	OECD	ILO	OECD	Amadeus	ILO	OECD
1991	-	0.59	-0.34	-	-1.64	-	-	-	-1.44	-1.45
1992	-	0.78	-1.95	-	-2.52	0.18	0.18	-0.59	-3.26	-3.25
1993	0.81	0.81	-3.23	-	-4.49	0.24	0.24	-3.58	-4.75	-4.76
1994	0.84	0.84	-2.30	-3.81	-3.93	0.24	0.24	-1.03	-2.87	-2.86
1995	0.85	0.85	-0.24	-0.69	-0.62	0.34	0.34	0.39	0.01	0.08
1996	0.84	0.84	-3.39	-1.75	-1.88	0.50	0.50	0.84	-1.06	-1.06
1997	0.87	0.87	-0.18	-2.11	-1.99	0.52	0.52	-0.29	-1.12	-1.12
1998	0.87	0.87	0.87	0.95	0.94	0.56	0.56	0.18	0.20	0.23
1999	0.87	0.87	-0.32	-0.91	-0.93	0.63	0.63	0.25	-0.31	0.17

Notes: Coverage is defined as the ratio of summed employment in continuing firms in Amadeus divided by ILO and OECD measures of paid employment in manufacturing. Net employment growth in Amadeus is defined as the average establishment level net employment growth weighted by the share of establishment employment in total employment. See equation (7) in the text for a formal definition. ILO and OECD net employment growth rates are defined as the change in the number of paid employees as a percentage of average manufacturing employment in the current and previous year.

Table II (cont.)
Coverage and Net Employment Growth in Manufacturing: Amadeus versus ILO and OECD

	Italy					*United Kingdom*				
	coverage		*net employment growth(in %)*			*coverage*		*net employment growth (in %)*		
Year	ILO	OECD	Amadeus	ILO	OECD	ILO	OECD	Amadeus	ILO	OECD
1991	-	-	-	-	-0.44	0.55	0.45	-6.76	-8.69	-4.93
1992	-	0.34	4.64	-	-0.74	0.68	0.56	-5.03	-5.18	-5.16
1993	0.31	0.31	1.79	-	-0.92	0.73	0.58	-3.66	-4.44	-1.71
1994	0.34	0.35	1.35	-0.51	-0.93	0.75	0.65	-1.57	0.43	-8.11
1995	0.41	0.42	2.77	-1.53	-0.13	0.76	0.67	0.48	2.46	1.32
1996	0.48	0.48	1.72	-0.40	0.63	0.76	0.65	-2.37	1.00	2.97
1997	0.58	0.58	-0.54	-0.45	-0.15	0.75	0.66	-3.23	1.40	-0.91
1998	0.56	0.56	0.75	1.96	1.32	0.67	0.60	-5.3	0.50	-0.13
1999	0.54	0.54	-1.76	0.07	1.21	0.62	0.54	-5.91	-3.59	-2.08

Notes: Coverage is defined as the ratio of summed employment in continuing firms in Amadeus divided by ILO and OECD measures of paid employment in manufacturing. Net employment growth in Amadeus is defined as the average establishment level net employment growth weighted by the share of establishment employment in total employment. See equation (7) in the text for a formal definition. ILO and OECD net employment growth rates are defined as the change in the number of paid employees as a percentage of average manufacturing employment in the current and previous year.

in Amadeus divided by the ILO and OECD estimates respectively. For Belgium, coverage ranges from 81% to 87%. In France, the sample captures between 18% and 63% of total manufacturing employment. The respective numbers for Italy are 31% and 54% and for the UK coverage is between 55% and 76%. For each country, the third, fourth and fifth column of Table II allow to compare net employment growth derived from the Amadeus with ILO and OECD information. Using the more complete OECD time series, coefficients of correlation and p-values are 0.79 (0.01) for Belgium, 0.91 (0.01) for France, -0.56 (11.30) for Italy and 0.72 (0.04) for the UK[40].

II.B Measuring gross job flows

Job creation, job destruction, net growth and job reallocation are measured as in Davis, Haltiwanger and Schuh (1996). The employment growth rate (g_{et}) of establishment *e* at time *t* is defined as the change in establishment employment (*n*) from *t-1* to *t*, divided by the average of establishment employment at time *t* and *t-1* or

$$(2) \qquad g_{et} \equiv 2\left[\frac{n_{et} - n_{et-1}}{n_{et} + n_{et-1}}\right]$$

This non-conventional growth rate measure is symmetric about zero and lies in the closed interval [-2,2]. For small values, g_{et} is approximately equal to the conventional growth rate.

Job creation for group *j* at time *t* is measured as the size-weighted sum of employment growth in all expanding firms in group *j* at time *t*. The job creation rate

[40] Using ILO net employment growth estimates for the UK and including all years gives a coefficient of correlation of 0.76 (0.02).

for group j at time t reflects job creation as a percentage of total employment in group j averaged over the current and previous period. So POS_{jt} is given by

$$(3) \qquad POS_{jt} \equiv \sum_{\substack{e \in j \\ g_{et} \geq 0}} g_{et} \left[\frac{N_{et}}{N_{jt}} \right]$$

where N_{jt} is the employment size of group j averaged over the current and the previous period. Together with the density of establishment size, the density of employment growth rates therefore determines the job creation rate.

Job destruction is defined as the sum of employment losses (in absolute value) in all contracting firms. Job destruction is converted to the job destruction rate by dividing through by a measure of total employment given by the average employment in the current and previous period. Algebraically, the job destruction rate for group j is defined as

$$(4) \qquad NEG_{jt} \equiv \sum_{\substack{e \in j \\ g_{et} < 0}} |g_{et}| \left[\frac{N_{et}}{N_{jt}} \right].$$

The job reallocation rate (*SUM*) is the sum of the job creation rate and the job destruction rate.

$$(5) \qquad SUM_{jt} \equiv POS_{jt} + NEG_{jt} = \sum_{e \in j} |g_{et}| \left[\frac{N_{et}}{N_{jt}} \right].$$

The net growth rate (*NET*) is defined as the difference between the job creation rate and the job destruction rate.

$$(6) \qquad NET_{jt} \equiv POS_{jt} - NEG_{jt} = \sum_{e \in j} g_{et} \left[\frac{N_{et}}{N_{jt}} \right].$$

III. CYCLICALITY IN GROSS JOB FLOWS

Figure I predicted job creation to behave pro-cyclically and job destruction to behave counter-cyclically if anything. But no predictions could be made about the cyclical pattern of job reallocation rates in general. Empirically, Blanchard and Diamond (1990) and Davis, Haltiwanger and Schuh (1996) document the higher volatility of US job destruction. Baldwin, Dunne and Haltiwanger (1998) repeat these findings using US and Canadian data. Konings (1995) shows the counter-cyclicality of job reallocation due to strong volatility of job destruction for the UK. However, whether the counter-cyclicality of job reallocation should be regarded as a stylised fact still remains unanswered. Boeri (1996) and Garibaldi (1998) document that job reallocation and net employment growth rates are uncorrelated in Canada, Denmark, Germany, Norway, Italy and Sweden whereas in France job reallocation and net growth rates are significantly positively correlated.

Different explanations have been given to the apparent empirical dichotomy in job reallocation rates between Anglo-Saxon and European countries. First, Garibaldi (1998) argues the difference could be explained by the existence of firing permissions in continental Europe. The key deviation from the dynamics in Mortensen and Pissarides (1994) is the introduction of an exogenous firing permission characterised by a Poisson arrival frequency. Job destruction is therefore not only costly, it is also time consuming and the Mortensen-Pissarides equilibrium no longer predicts overshooting of the job destruction rate if a negative macro-shock arrives. Second, Boeri (1996) suggests the counter-cyclicality of job reallocation in the US and UK could be a statistical artefact. Suppose job creation and destruction behave symmetrically over the business cycle for the economy as a whole but observed establishment size is censored at some lower bound through the sampling procedure. Also assume net employment growth to be bigger for smaller firms throughout the business cycle. Boeri (1996) then shows that by reducing the observed time-series variation of job creation rates, the correlation coefficients between job turnover and net employment growth rates are downward biased.

Estimates of aggregate gross job flows are given in Table III and Figure III. The time averages in Table III show that Belgian and French job creation and destruction rates are lower on average and are least dispersed over time. Considering the reported OECD and ILO net growth rates in Figure III, the strong process of job creation in the first half of the 90s in Italy and job destruction in the second half of the 90s in the UK could be explained by the weighting when calculating job flow rates as well as the sampling procedure.

Formally, pair wise Spearman correlations are given in columns (1)-(4) of Table IV. It follows from Table IV that job creation is strongly positively correlated with net growth in all countries. The Spearman correlations are significant at the 1% level for Belgium and France, at the 7% level for Italy and the 15% level for the UK. The job destruction rate is strongly negatively correlated with net employment growth for Belgium, Italy and the UK and a weakly negative correlation exists for France. Except for Italy, the rank correlation between the job creation and job destruction rate is negative. The job reallocation rate is most negatively correlated with the net growth rate in the UK. For Belgium, France and Italy, the Spearman correlations are small

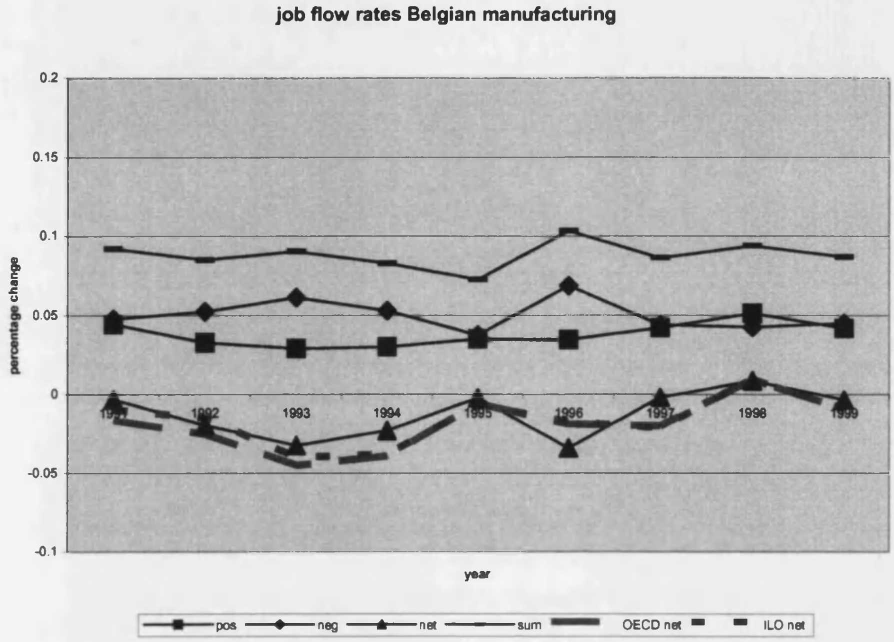
Table III
Job Flow Rates by Year for Belgian and French Manufacturing (in %)

	Belgium				***France***			
	job creation	*job destruction*	*net change*	*job reallocation*	*job creation*	*job destruction*	*net change*	*job reallocation*
Year								
1991	4.43	4.77	-0.34	9.20	-	-	-	-
1992	3.27	5.23	-1.95	8.50	4.34	4.93	-0.59	9.28
1993	2.90	6.13	-3.23	9.03	2.86	6.44	-3.58	9.30
1994	2.99	5.29	-2.30	8.28	4.28	5.31	-1.03	9.59
1995	3.51	3.75	-0.24	7.26	5.18	4.80	0.38	9.98
1996	3.47	6.86	-3.39	10.34	4.40	3.56	0.84	7.97
1997	4.23	4.41	-0.17	8.64	4.33	4.63	-0.29	8.96
1998	5.14	4.27	0.87	9.41	5.29	3.46	1.83	8.75
1999	4.19	4.51	-0.32	8.69	8.52	5.98	2.54	14.50
Mean	3.79	5.02	-1.23	8.82	4.90	4.89	0.01	9.79
Std. Dev.	0.75	0.98	1.52	0.84	1.53	0.98	1.77	1.86

Table III (cont.)
Job Flow Rates by Year for Italian and UK Manufacturing (in %)

	Italy				*United Kingdom*			
	job creation	*job destruction*	*net change*	*job reallocation*	*job creation*	*job destruction*	*net change*	*job reallocation*
Year								
1991	-	-	-	-	2.69	9.45	-6.76	12.15
1992	10.02	5.38	4.64	15.42	4.08	9.11	-5.03	13.20
1993	8.49	6.70	1.79	15.20	3.46	7.11	-3.65	10.57
1994	6.22	4.87	1.35	11.09	5.32	6.89	-1.57	12.21
1995	8.42	5.65	2.77	14.07	5.91	5.43	0.48	11.33
1996	10.47	8.76	1.72	19.23	5.03	7.39	-2.36	12.42
1997	7.08	7.62	-0.53	14.70	5.41	8.63	-3.23	14.04
1998	7.61	6.86	0.75	14.47	5.71	11.01	-5.30	16.72
1999	7.22	8.98	-1.76	16.20	5.26	11.18	-5.91	16.44
Mean	8.19	6.85	1.34	15.05	4.76	8.47	-3.70	13.23
Std. Dev.	1.47	1.52	1.95	2.27	1.04	1.82	2.18	2.02

Figure III
Aggregate Job Flow Rates for Belgium and France (continuing firms only)



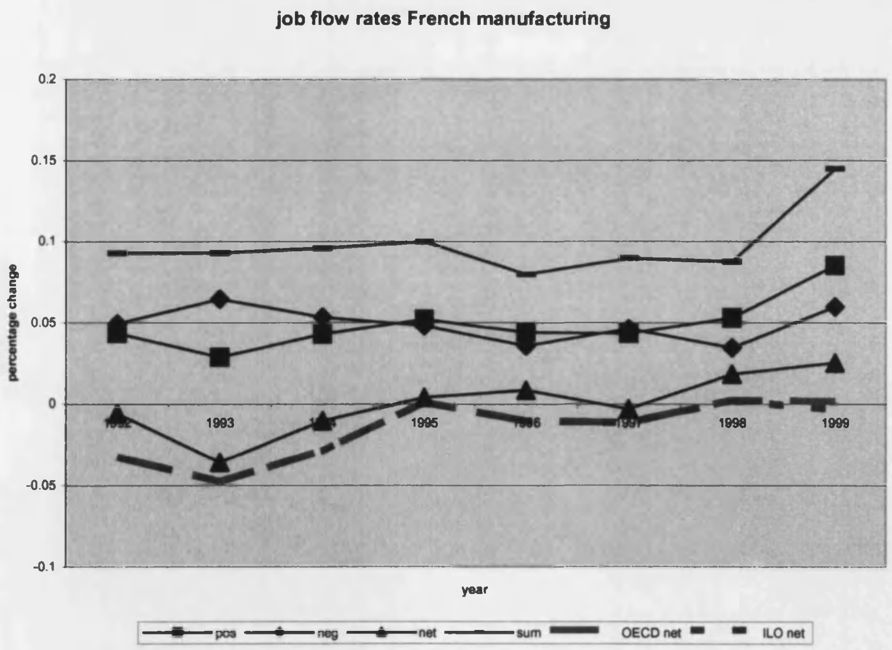
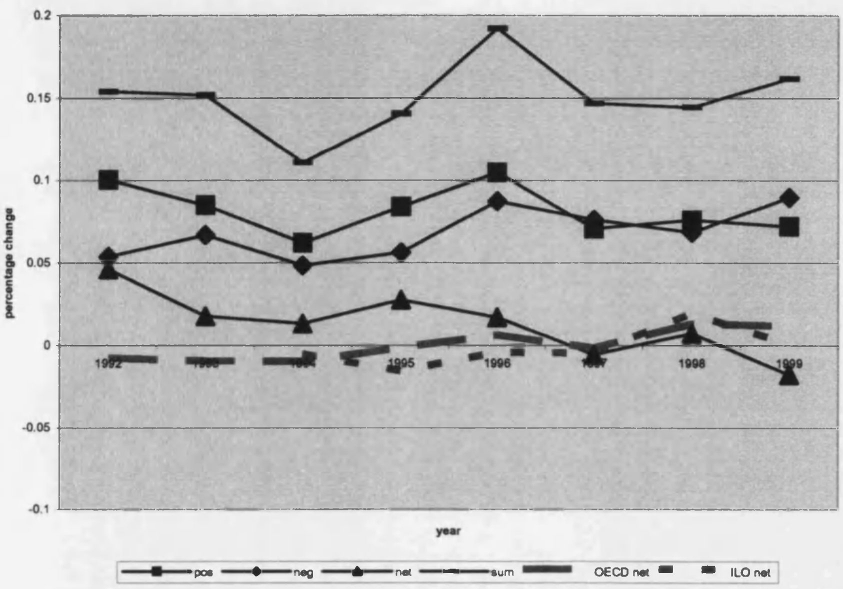


Figure III (cont.)

Aggregate Job Flow Rates by Country for Italy and UK (continuing firms only)

job flow rates Italian manufacturing



job flow rates UK manufacturing

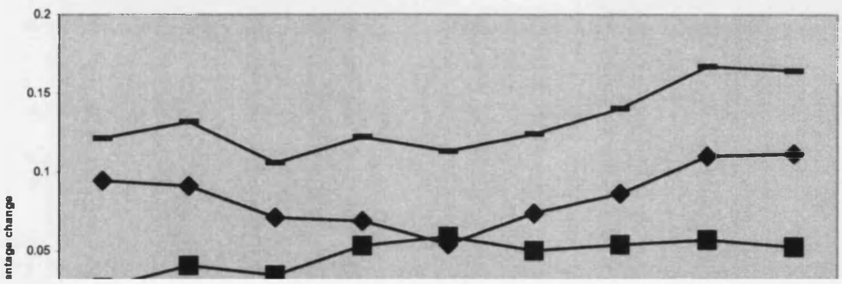


Table IV
Correlations Between Gross Job Flows and Net Employment Change

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Country	*(POS-NET)*	*(NEG-NET)*	*(POS-NEG)*	*(SUM-NET)*	$\frac{VAR(NEG)}{VAR(POS)}$	$\frac{VAR(POS)}{POS}$	$\frac{VAR(NEG)}{NEG}$
Belgium	0.78	-0.95	-0.68	-0.17	1.72	0.15	0.19
	(0.01)	(0.00)	(0.04)	(0.67)			
France	0.95	-0.48	-0.38	-0.05	0.41	0.55	0.22
	(0.00)	(0.23)	(0.35)	(0.91)			
Italy	0.67	-0.64	0.09	-0.05	1.08	0.26	0.34
	(0.07)	(0.08)	(0.82)	(0.91)			
UK	0.52	-0.90	-0.22	-0.38	3.08	0.25	0.44
	(0.15)	(0.00)	(0.57)	(0.31)			

Notes: Columns (1)-(4) report Spearman correlations with marginal significance levels in parentheses. The variance ratio's reported in column (5) are F-distributed if populations are (nearly) normal and independent. Under the null hypothesis that $VAR(NEG) \leq VAR(POS)$, the test statistics reported in column (5) have to be compared to the 5% critical values of 3.18 for Belgium and the UK and 3.44 for France and Italy. Columns (6) and (7) report coefficients of variation.

and highly insignificant, suggesting job destruction is as volatile as job creation. Column (5) restates the counter-cyclical path of the job reallocation rate in the UK and its a-cyclical dynamics in Continental European countries. Finally, columns (6) and (7) report the coefficients of variation for both the job creation and destruction rate. Expressed as a percentage of their mean, job destruction rates seem to be more volatile than job creation rates in the UK whereas less clear results hold for Belgium and Italy and the opposite seems to be true for France. Job reallocation rates are counter-cyclical in the UK due to stronger counter-cyclicality in job destruction rates whereas job reallocation rates are a-cyclical in Belgium and Italy and pro-cyclical in France.

Given high average establishment size in the UK, regression-to-the-mean bias could imply the results above. However, the reported coefficients in columns (6) and (7) of Table IV partially control for sample selection since the estimates reflect standardised measures of volatility. Moreover, for the regression fallacy to be at work, two conditions need to be satisfied: growth rates must be higher for smaller establishments and there must be some degree of sample selection. If samples are randomly drawn within each country, differences in population firm size distributions might still imply differences in the cyclical behavior of job creation and destruction rates if growth rates are not proportional. Given the identical sampling criteria for France, Italy and the UK, sample selection can only occur if reporting employment changes is more likely for bigger firms in the UK and smaller firms in France and Italy. However, there is no obvious reason why this hypothesis would be likely to hold. It is therefore more likely differences in establishment size densities explain the counter-cyclicality of job reallocation in the UK relative to mainland European countries.

Table V examines the counter-cyclicality of job turnover for each country-industry cell. The estimates reflect that the rank correlation between job reallocation and net employment growth can be negative or positive even within countries. For example, the correlation coefficient is positive and significant for France when pooling all industries. However, 9 out of 23 industries still report negative correlations. In Italy, 8 out of 23 industries report positive rank correlations whereas overall the estimate is negative and significant. Even in the UK where the pooled estimate is negative and highly statistically significant, 9 sectors report insignificant correlations. However, since no clear industry pattern can be found across countries, the cyclicality of employment flows seems to be partially country-industry specific.

So far it has been assumed all time variation in employment flows to emerge from temporary productivity or demand shocks. However, part of the variation in job flow rates can be due to time trends in flow series. Regressing job flow rates onto a constant and a linear time trend informs that job creation has increased gradually in the UK during the 1990s. The estimate (marginal significance level) is 0.31% (1.4%). In France, job creation also shows a significant upward trend of 0.48% (4.1%) but median and robust regressions both make the coefficient insignificant due to the influential year 1999. Using the job destruction rate as the dependent variable, robust regression yields an estimate of –0.35% (8.8%) for France whereas 0.44% (4.7%) is found for Italy using OLS. The job reallocation rate shows an upward trend in the UK of 0.58% (2.2%) annually. An upward trend in net growth rates is found for France

Table V

Coefficients of Correlation between Job Reallocation and Net Growth by Country-Industry Cells

Industry	***Belgium***	***France***	***Italy***	***UK***
Food (15)	-0.60 (0.09)	0.09 (0.82)	-0.67 (0.07)	0.07 (0.86)
Tobacco (16)	-0.77 (0.01)	-1.00 (0.00)	-0.25 (0.54)	-0.97 (0.00)
Textiles (17)	-0.45 (0.22)	-0.26 (0.53)	0.02 (0.95)	-0.67 (0.05)
Apparel (18)	-0.75 (0.02)	-0.59 (0.12)	-0.78 (0.02)	-0.28 (0.46)
Leather (19)	-0.50 (0.17)	-0.76 (0.03)	-0.57 (0.14)	-0.15 (0.70)
Wood (20)	-0.18 (0.64)	0.26 (0.53)	0.21 (0.61)	0.10 (0.80)
Paper (21)	-0.48 (0.19)	0.31 (0.45)	0.26 (0.53)	-0.28 (0.46)
Publishing (22)	0.30 (0.43)	0.17 (0.69)	-0.48 (0.23)	-0.08 (0.83)
Petroleum (23)	-0.50 (0.17)	-0.24 (0.57)	0.24 (0.57)	-0.42 (0.26)
Chemicals (24)	0.22 (0.57)	-0.52 (0.18)	0.55 (0.16)	-0.68 (0.04)
Rubber (25)	0.08 (0.83)	0.40 (0.32)	-0.09 (0.82)	-0.47 (0.20)
Other non-metals (26)	-0.23 (0.54)	0.07 (0.87)	-0.19 (0.65)	-0.62 (0.08)
Basic metals (27)	-0.43 (0.24)	-0.43 (0.29)	-0.64 (0.08)	-0.67 (0.05)
Fabricated metals (28)	0.15 (0.70)	0.14 (0.73)	-0.24 (0.57)	-0.52 (0.15)
Machinery (29)	-0.65 (0.06)	0.00 (1.00)	0.24 (0.57)	-0.82 (0.01)
Office machinery (30)	0.48 (0.19)	-0.36 (0.38)	-0.33 (0.42)	-0.07 (0.86)
Elec. machinery (31)	-0.53 (0.14)	0.05 (0.91)	0.26 (0.53)	-0.51 (0.15)
Communication (32)	-0.53 (0.14)	0.76 (0.03)	-0.02 (0.95)	-0.57 (0.11)
Precision (33)	-0.33 (0.38)	-0.26(0.53)	-0.14 (0.73)	-0.27 (0.49)
Vehicles (34)	-0.20 (0.60)	0.33 (0.42)	-0.36 (0.38)	-0.88 (0.00)
Other transport (35)	-0.47 (0.20)	-0.74 (0.04)	0.48 (0.23)	-0.15 (0.70)
Furniture (36)	-0.52 (0.15)	0.26 (0.53)	-0.45 (0.26)	-0.72 (0.03)
Recycling (37)	0.17 (0.67)	0.67 (0.07)	-0.09 (0.82)	0.53 (0.14)
All sectors	-0.09 (0.18)	0.19 (0.01)	-0.15 (0.04)	-0.29 (0.00)

Notes: the numbers reported are Spearman correlation coefficients (marginal significance level) between SUM and NET.

with an average annual increase of 0.61% (1.8%) and a negative trend of –0.67% (0.8%) is found for Italy due to observed high job creation in the first half of the sampling period. The observed upward trends in job creation in the UK and France and job destruction in Italy as well as the estimated downward trend of job destruction in France suggest structural changes in employment dynamics.

To summarise, this section observed pro-cyclical job creation and counter-cyclical job destruction rates in all four countries. Moreover, estimates show job destruction is more cyclically volatile in the UK, implying counter-cyclical job reallocation. Job reallocation rates are a-cyclical in Belgium and Italy and pro-cyclical in France. Symmetry of job creation and destruction can arise as a Mortensen-Pissarides outcome in which it is time consuming for firms to lay-off workers, as shown by Garibaldi (1998). However, besides country specific differences in job flow series, time variation in employment flows using country-industry cells learns that cyclical behavior differs between industries within each country. Moreover, the results suggest cyclicality is partially country-industry specific since no clear industry pattern can be found across countries. Finally, part of the observed variation in employment flows is captured by linear trends in job creation and destruction rates. Estimates for France reveal an upward trend in job creation and a downward trend in job destruction. Job destruction in Italy has gradually increased during the sampling period whereas for the UK a significant upward trend in job creation is found, reflecting persistent structural changes in employment dynamics.

IV. EMPIRICAL ANALYSIS OF COUNTRY, INDUSTRY AND YEAR FIXED EFFECTS IN GROSS JOB FLOWS

This section aims to decompose the variation in job flow rates into within-group and between-group components. Within-group variation reflects changes of job flow rates over time within a country-industry cell. Between-group variation only uses country, industry or country-industry cell specific differences that are persistent over time. To quantify alternative sources of variation in employment-flow data, estimating equations take the following form:

$$x_{ict} = \mu + \sum_{c=1}^{4} \alpha_c * country_c + \sum_{i=1}^{23} \beta_i * industry_i + \sum_{t=1}^{8} \gamma_t * year_t + \varepsilon_{ict} \qquad (7)$$

such that $\sum_{c=1}^{4} \alpha_c = 0$ and $\sum_{i=1}^{23} \beta_i = 0$ and $\sum_{t=1}^{8} \gamma_t = 0$.

where x_{ict} is the job creation, job destruction, job reallocation or net growth rate of industry i in country c at time t and μ is the grand mean. Vectors $country_c$, $industry_i$ and $year_t$ are column vectors of country, industry and time dummies respectively. Vectors α_c, β_i and γ_t are row vectors of country dummy, industry dummy and time dummy coefficients and ε_{ict} is an idiosyncratic shift parameter. The country and industry dummies capture part of the between-variation in the dependent variable. Country dummy coefficients capture country specific differences that are common across industries and over time or a country fixed effect. Similarly, industry dummy coefficients capture an industry fixed effect.

Table VI shows the time-averaged gross job flows for each country-industry cell. Differences in the estimates of Table VI between countries give rise to a country fixed effect whereas differences between industries common across countries reflect an industry fixed effect. In line with Figure I, Figure IV plots the time-averaged job creation and destruction rates from Table VI. It immediately follows from Figure IV that taking time averages controls for the business cycle but does not exclude transitory dynamics in job creation and destruction rates. Comparing countries, the majority of Belgian and British industries experienced higher job destruction on average. French industries are equally distributed around the diagonal whereas the majority of Italian industries had stronger job creation. Finally, time dummies capture part of the within-variation in the dependent variable or a time fixed effect.

IV.A Country fixed effects

A necessary condition for country specific differences to be important is a relatively large R-squared using (7) and setting β_i and γ_t equal to zero.[41] The R-squareds are given in Table VII and are 17.62% (job creation rate), 3.38% (job destruction rate), 10.94% (job reallocation rate and 2.27% (net growth rate). In

---

[41] The fact that the R-squared obtained after applying OLS to (7) while setting β_i and γ_t equal to 0 measures the between country variation common across industries is also seen through the following variance decomposition:

$$\frac{1}{NT}\sum_{i=1}^{I}\sum_{c=1}^{C}\sum_{t=1}^{T}(x_{ict}-\bar{x})^2=\frac{1}{NT}\sum_{i=1}^{I}\sum_{c=1}^{C}\sum_{t=1}^{T}(x_{ict}-\bar{x}_{ic})^2+\frac{1}{N}\sum_{i=1}^{I}\sum_{c=1}^{C}(\bar{x}_{ic}-\bar{x}_c)^2+\frac{1}{C}\sum_{c=1}^{C}(\bar{x}_c-\bar{x})^2$$

where bars indicate averages. The left hand side measures overall variation in x_{ict}. The first term on the right hand side captures within-group variation. The penultimate term on the right hand side measures the importance of between industry variance within countries whereas the last term gives weight to between country differences common across industries. A similar decomposition of the between-group variation can be derived in which the last term reflects the importance of an industry fixed effect. Also note that the above variance decomposition illustrates the orthogonality of country, industry and time fixed effects.

Table VI
Job Flow Rates by Industry for Belgian and French Manufacturing (in %)

	Belgium				*France*			
Industry	Job Creation	Job Destruction	Net Change	Job Reallocation	Job Creation	Job Destruction	Net Change	Job Reallocation
Food (15)	4.74	5.03	-0.29	9.77	5.73	4.38	1.35	10.11
Tobacco (16)	2.72	6.61	-3.89	9.32	0.00	4.77	-4.76	4.77
Textiles (17)	4.62	5.79	-1.17	10.41	4.05	5.78	-1.73	9.84
Apparel (18)	3.22	8.57	-5.35	11.79	4.75	5.66	-0.90	10.41
Leather (19)	2.06	7.14	-5.08	9.20	2.98	5.13	-2.14	8.11
Wood (20)	5.33	5.46	-0.14	10.79	6.03	3.83	2.19	9.86
Paper (21)	3.57	4.78	-1.21	8.36	5.14	4.34	0.79	9.48
Publishing (22)	4.89	4.84	0.06	9.73	5.24	4.44	0.80	9.69
Petroleum (23)	1.04	4.48	-3.43	5.52	2.17	7.50	-5.33	9.68
Chemicals (24)	2.94	3.21	-0.26	6.15	4.75	3.74	1.02	8.49
Rubber (25)	5.03	4.06	0.97	9.09	4.79	3.43	1.36	8.22
Other non-metals (26)	3.45	4.27	-0.82	7.73	3.25	4.66	-1.40	7.91
Basic metals (27)	1.46	4.80	-3.35	6.26	2.44	4.03	-1.60	6.47
Fabricated metals (28)	5.72	5.19	0.53	10.91	5.13	4.52	0.61	9.65
Machinery (29)	3.78	5.60	-1.82	9.37	4.07	4.49	-0.42	8.55
Office machinery (30)	8.02	5.86	2.16	13.87	4.25	8.14	-3.89	12.39
Elec. machinery (31)	3.14	4.55	-1.40	7.69	4.38	4.67	-0.29	9.05
Communication (32)	2.02	6.89	-4.88	8.91	7.47	4.55	2.92	12.02
Precision (33)	4.87	5.84	-0.97	10.71	5.31	7.94	-2.63	13.25
Vehicles (34)	3.14	4.75	-1.61	7.89	8.55	5.64	2.91	14.18
Other transport (35)	3.46	5.94	-2.47	9.40	1.91	7.39	-5.48	9.30
Furniture (36)	4.30	6.24	-1.95	10.54	5.11	4.73	0.38	9.84
Recycling (37)	6.96	6.28	0.68	13.24	7.01	3.87	3.14	10.87

Notes: Industry numbers in brackets refer to 2-digit NACE Rev.1 classifications.

Table VI (cont.)
Job Flow Rates by Industry for Italian and UK Manufacturing (in %)

	Italy				*United Kingdom*			
Industry	Job Creation	Job Destruction	Net Change	Job Reallocation	Job Creation	Job Destruction	Net Change	Job Reallocation
Food (15)	8.83	6.48	2.35	15.32	5.81	7.00	-1.19	12.81
Tobacco (16)	10.12	24.74	-14.62	34.87	0.33	5.50	-5.16	5.83
Textiles (17)	7.02	4.30	2.72	11.32	4.89	8.28	-3.39	13.17
Apparel (18)	6.93	10.61	-3.68	17.55	5.26	7.72	-2.46	12.98
Leather (19)	8.93	4.69	4.24	13.62	5.21	8.77	-3.56	13.98
Wood (20)	7.70	3.91	3.79	11.61	6.35	6.79	-0.45	13.14
Paper (21)	7.52	3.01	4.51	10.53	5.20	7.40	-2.19	12.60
Publishing (22)	7.44	12.45	-5.01	19.89	6.72	8.14	-1.42	14.87
Petroleum (23)	11.73	7.62	4.11	19.35	6.51	23.52	-17.01	30.03
Chemicals (24)	11.40	4.79	6.61	16.20	3.95	9.94	-5.99	13.89
Rubber (25)	9.36	3.71	5.65	13.07	6.43	6.36	0.07	12.80
Other non-metals (26)	7.45	5.16	2.30	12.61	4.46	10.34	-5.88	14.79
Basic metals (27)	8.45	8.09	0.36	16.53	3.53	8.91	-5.39	12.44
Fabricated metals (28)	8.22	9.52	-1.30	17.74	5.18	8.84	-3.66	14.02
Machinery (29)	9.30	6.13	3.16	15.43	4.69	8.47	-3.77	13.16
Office machinery (30)	7.11	14.53	-7.41	21.64	5.44	9.54	-4.10	14.99
Elec. machinery (31)	9.82	7.28	2.54	17.10	4.22	11.42	-7.20	15.64
Communication (32)	8.87	5.65	3.22	14.52	6.59	11.94	-5.35	18.53
Precision (33)	9.50	4.20	5.30	13.71	4.51	5.83	-1.32	10.34
Vehicles (34)	6.44	7.26	-0.83	13.70	3.31	8.26	-4.95	11.58
Other transport (35)	11.74	7.54	4.21	19.28	4.11	10.11	-5.99	14.22
Furniture (36)	7.27	6.67	0.61	13.94	6.23	6.76	-0.53	12.99
Recycling (37)	14.59	22.21	-7.62	36.80	7.84	6.88	0.95	14.72

Notes: Industry numbers in brackets refer to 2-digit NACE Rev.1 classifications.

Figure IV
Time Averaged Job Creation and Destruction Rates by Country (in %)

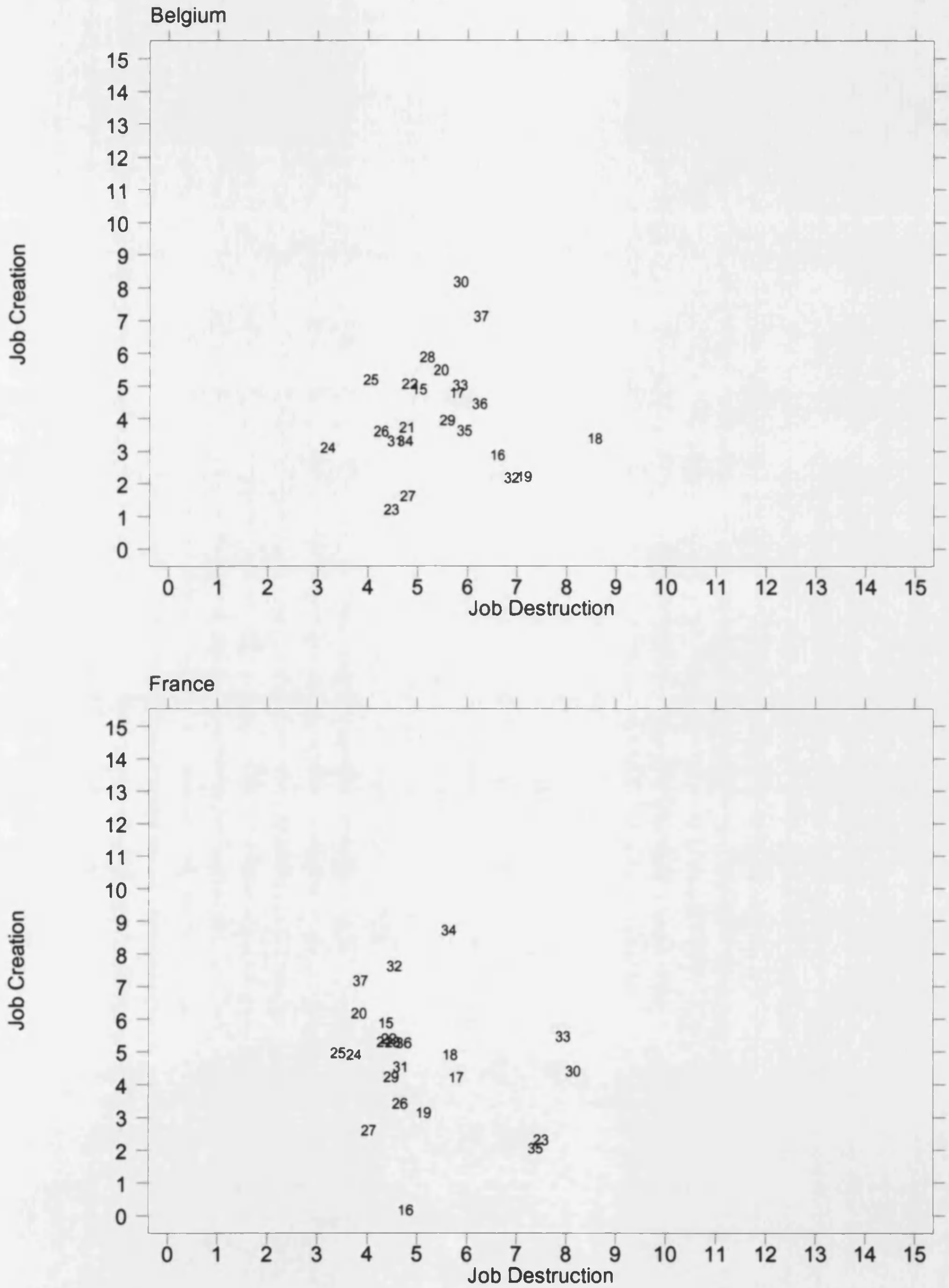
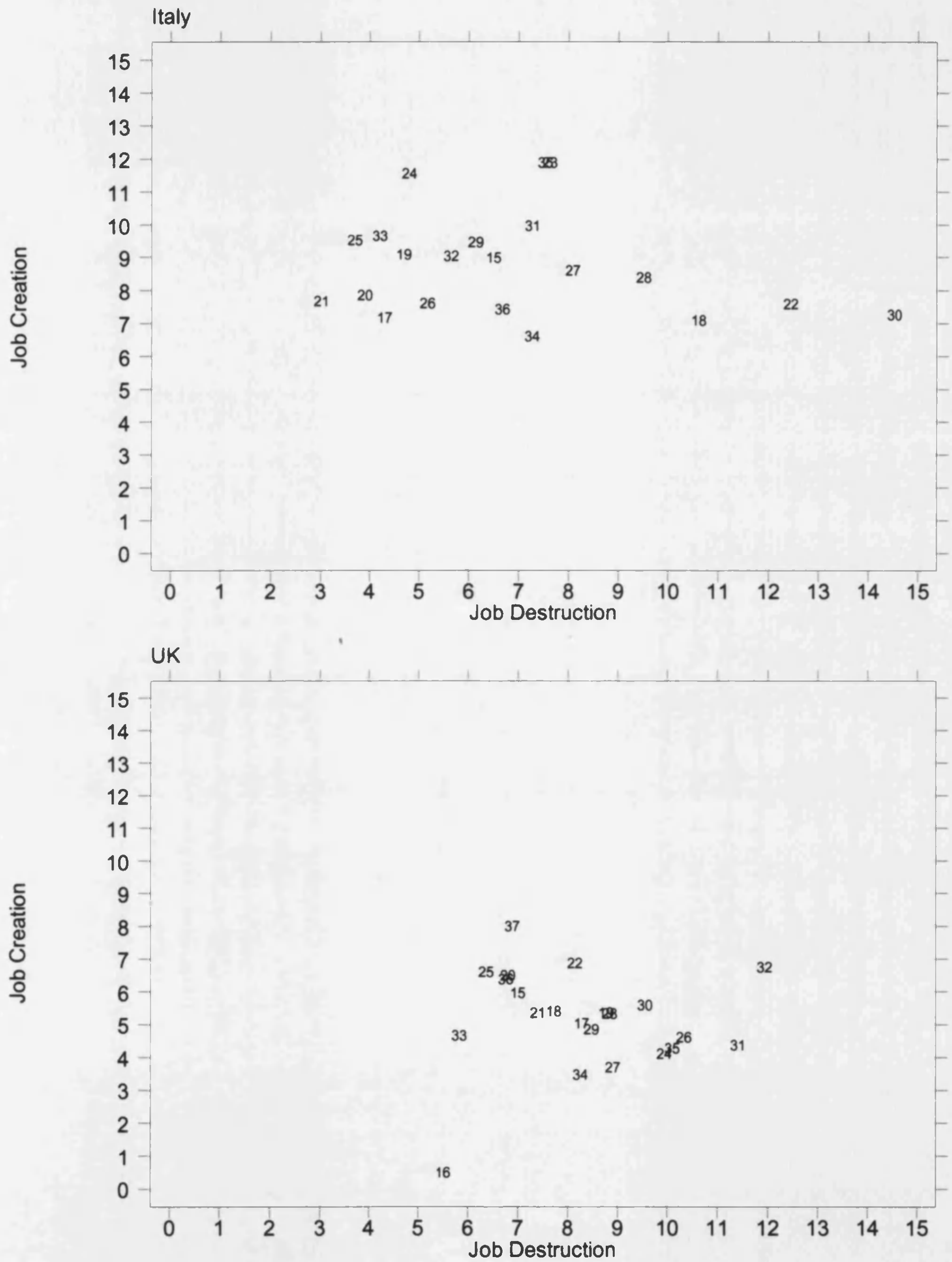


Figure IV (cont)
Time Averaged Job Creation and Job Destruction Rates by Country (in %)



Notes: Plotted numbers refer to 2-digit NACE Rev.1 classifications. See Table 2. Job creation and destruction rates for Italy and the UK are censored at 15%, dropping from the plot industries 16 and 37 for Italy and 23 for the UK.

Table VII
Fixed Effects in Gross Job Flows, 1992-1999

	Dependent variable			
	POS	*NEG*	*SUM*	*NET*
Controls				
Belgium	-0.016**	-0.015**	-0.032**	-0.002
France	-0.011**	-0.018**	-0.030**	0.008
Italy	0.033**	0.013*	0.046**	0.020**
UK	-0.005*	0.020**	0.015**	-0.026**
Food (15)	0.007	-0.012	-0.006	0.019
Tobacco (16)	-0.023**	0.032*	0.008	-0.055**
Textiles (17)	-0.004	-0.009	-0.013	0.005
Apparel (18)	-0.006	0.011	0.006	-0.017
Leather (19)	-0.008	-0.005	-0.013	-0.003
Wood (20)	0.008	-0.019	-0.011	0.027
Paper (21)	-0.003	-0.020	-0.023	0.018
Publishing (22)	0.005	0.004	0.009	0.001
Petroleum (23)	-0.003	0.040**	0.037*	-0.042
Chemicals (24)	0.001	-0.015	-0.014	0.016
Rubber (25)	0.008	-0.025*	-0.017	0.034*
Other non-metals (26)	-0.009	-0.008	-0.017	-0.001
Basic metals (27)	-0.017*	-0.005	-0.022	-0.011
Fabricated metals (28)	0.005	0.000	0.005	0.005
Machinery (29)	-0.002	-0.008	-0.009	0.006
Office machinery (30)	0.007	0.024*	0.031*	-0.017
Elec. machinery (31)	-0.003	0.000	-0.002	-0.003
Communication (32)	0.006	0.004	0.010	0.002
Precision (33)	0.004	-0.010	-0.006	0.015
Vehicles (34)	-0.003	-0.005	-0.008	0.002
Other transport (35)	-0.003	0.008	0.004	-0.011
Furniture (36)	0.001	-0.008	-0.007	0.010
Recycling (37)	0.034**	0.026*	0.061**	0.008
1992	-0.003	-0.010	-0.012	0.007
1993	-0.012**	-0.003	-0.016	-0.009
1994	-0.001	-0.013	-0.014	0.012
1995	0.002	-0.003	-0.001	0.006
1996	0.005	0.016	0.021*	-0.011
1997	-0.005	0.010	0.006	-0.015
1998	0.005	-0.005	0.001	0.010
1999	0.008*	0.008	0.017	-0.001
grand mean	0.056**	0.070**	0.126**	-0.013**
R-squared	0.25	0.09	0.17	0.08
R-squared $\beta_i=0$, $\gamma_t=0$	0.18	0.03	0.11	0.02
F-value $\beta_i=0$, $\gamma_t=0$	55.63	10.31	32.36	7.61
R-squared $\alpha_c=0$, $\gamma_t=0$	0.05	0.04	0.05	0.04
F-value $\alpha_c=0$, $\gamma_t=0$	1.96	1.32	1.50	1.41

*Notes: ** indicates significance at 1% level. * indicates significance at 5% level. Industry numbers refer to 2-digit NACE Rev.1 classification. Reported F-values are F-test statistics for the joint significance of included attributes. The number of observations is 782.*

particular, this result suggests about one fifth of the variance in job creation is captured by persistent country specific differences.

Turning to the significance of point estimates, the existence of country specific differences requires the significance of the country dummy coefficients in (7). Table VII shows that point estimates for the job creation rate are all statistically significant and the estimated grand mean is 5.56%. The predicted time-averaged job creation rates are 3.96% (Belgium), 4.46% (France), 8.86% (Italy) and 5.06% (United Kingdom). Point estimates for the job destruction rate are all highly statistically significant. The predicted grand mean for the job destruction rate is 7% and the predicted time-averaged job destruction rates are 5.5% (Belgium), 5.2% (France), 8.3% (Italy) and 9% (United Kingdom). The predicted job reallocation rates are 9.5% (Belgium), 9.6% (France), 17.2% (Italy) and 14.1% (United Kingdom). Net growth rates deviate significantly from the estimated grand mean (–1.3%) in Italy (2%) and the UK (-2.6%). Finally, Table VII also reports F-values being F-test statistics for the joint significance of attributes included in the regressions. Country dummies are jointly statistically significant at the 1% level for all measures of job flows.

The estimates therefore reflect the presence of a country fixed effect. But this effect is partially driven by above average job creation in Italy and job destruction in the UK. To assess the importance of these outliers, the periods 1991-1995 for Italy and 1996-1999 for the UK can be excluded from the regression analysis. For the job creation rate, discarding observations in the first half of the sampling period for Italy and in the second half for the UK does not change point estimates. For the job destruction rate, the coefficient for Italy is 0.032 and becomes statistically significant at the 1% level whereas the coefficient for the UK becomes insignificant. Using the job reallocation rate as the dependent variable leaves the coefficient for the UK insignificant. Finally, the country dummy coefficients are all individually statistically insignificant when net growth rates are used as the dependent variable. Therefore, similarities in net employment growth rates mask country specific differences in job creation and destruction. Relative to the UK, Italy has experienced higher and Belgium and France have experienced lower job creation and destruction on average.

Explaining country specific variation in employment dynamics requires attributes that differ between countries and are correlated with job creation and destruction rates such as differences in labor market regulation. The usual suspects are income taxes, unemployment benefits and employment protection such as firing taxes. Empirically however, these policy variables are likely to be correlated. For example, Boeri et al. (2001) show unemployment benefits and firing taxes are negatively correlated. Besides omitted variable bias, some covariates may suffer from simultaneity bias as well. For example, Ichino et al. (2001) argue employment protection legislation can be influenced by aggregate labor market conditions like unemployment. Consequently, this chapter does not attempt to detect causal relationships between policy variables and employment flows but suggests further research on the importance of labor market institutions in Continental European countries would be welcome.

IV.B Industry fixed effects

Industry dummies capture industry specific variation that is common across countries. Considering (7) and setting α_c and γ_t equal to 0, the R-squared measures the importance of between industry variations common across countries. The R-squareds are 5.09% (job creation rate), 4.21% (job destruction rate), 4.57% (job reallocation rate) and 4.24% (net growth rate). These numbers already question the existence of a dominant industry fixed effect in employment flows.

Alternatively, the significance of the industry dummy coefficients in (7) informs about the presence of an industry fixed effect. Point estimates are given in Table VII. OLS estimates for the job creation rate are not statistically significant except for tobacco, basic metals and recycling industries with only the latter showing higher than average job creation across countries. Point estimates for the job destruction rate are not statistically significant except for 5 out of 23 industries. Particular is higher job destruction in tobacco, petroleum and recycling industries. Using the job reallocation rate as the dependent variable an industry fixed effect is observed for petroleum and recycling industries, driven by a strong process of job destruction and creation respectively. The last column of Table VII reads that on average some industries grew and others contracted but differences are not statistically significant. The reported F-values in Table VII are smaller and reflect the joint significance of industry dummies only for the job creation rate as the dependent variable. Controlling for possible sampling error yields similar results. These results do not allow to conclude that persistent industry specific differences are important.

The absence of an industry fixed effect does not support earlier findings by Baldwin, Dunne and Haltiwanger (1998) who use comparable data for US and

Canadian manufacturing covering the period 1973-1993. They conclude that common technologies as well as other common elements dominate the long-run structural relationship between industries. Accordingly, they argue, it is difficult to distinguish between both countries in terms of the industrial structure of gross job-flow rates. However, the analysis here learns that this result cannot be generalised to all industrialised economies.

IV.C Time fixed effects and interaction effects

A time fixed effect captures part of the within-group variation in employment flows. The insignificance of year dummy coefficients in Table VII reflects that productivity or demand shocks are not common across countries and industries or do not affect employment dynamics similarly in different country-industry cells.

The modelling of interaction effects between time and country or industry dummies enables to capture time-variation within country-industry cells that is country or industry specific. Estimating equations take the following form:

$$(8) \qquad x_{ict} = \mu + \sum_{c=1}^{3} \alpha_c * country_c + \sum_{t=1}^{7} \gamma_t * year_t + \sum_{ct=1}^{21} \kappa_{ct} * country_c Xyear_t + \varepsilon_{ict}$$

or

$$(9) \qquad x_{ict} = \mu + \sum_{i=1}^{22} \beta_i * industry_i + \sum_{t=1}^{7} \gamma_t * year_t + \sum_{it=1}^{154} \lambda_{it} * industry_i Xyear_t + \varepsilon_{ict}$$

where $country_c Xyear_t$ and $industry_i Xyear_t$ are column vectors of dummy variables, one for each country-year or industry-year couple respectively.

Applying OLS to (8) while dropping Belgium and 1992, the joint significance of the interaction coefficients involving each country informs about a country-year

Table VIII
Interactions of Country and Time Effects in Gross Job Flows

	Dependent variable			
	POS	*NEG*	*SUM*	*NET*
country-year specific effect for				
France	0.844	0.880	0.845	0.897
Italy	0.032	0.116	0.111	0.078
UK	0.513	0.272	0.520	0.188
Adjusted R-squared of estimating equation including				
country dummies	0.173	0.030	0.106	0.019
country and time dummies and interaction effects	0.190	0.044	0.127	0.028

Notes: The numbers are marginal significance levels of tests for the joint significance of all interactions containing the dummy in the first row of the table.

specific effect. The numbers in the top panel of Table VIII are marginal significance levels of such tests. The reported p-values indicate that country-year specific variation in employment flows is statistically insignificant. Relatively low p-values for Italy can be explained by above average job creation in 1996 and below average job creation in 1997 and above average job destruction in the period 1995-1997. Therefore, aggregate shocks that are country specific do not seem to fit the variation in employment flows very well. Moreover, Table VIII illustrates point estimates are relatively small in absolute value. This result follows from the similarity between adjusted R-squareds using (7) and (8) while setting β_t and γ_t equal to zero: fixed time effects and interaction effects only add little to the explained variation in the dependent variable.

Table IX
Interactions of Industry and Time Effects in Gross Job Flows

	Dependent variable			
	POS	*NEG*	*SUM*	*NET*
Industry-year specific effect for				
Tobacco (16)	0.871	0.038	0.173	0.050
Textiles (17)	0.982	0.999	0.999	0.999
Apparel (18)	0.999	0.996	0.999	0.994
Leather (19)	0.982	0.997	0.997	0.993
Wood (20)	0.943	0.999	0.997	0.997
Paper (21)	0.998	0.999	0.999	0.999
Publishing (22)	0.968	0.944	0.971	0.924
Petroleum (23)	0.321	0.095	0.387	0.036
Chemicals (24)	0.934	0.999	0.998	0.995
Rubber (25)	0.999	0.999	0.998	0.999
Other non-metals (26)	0.999	0.994	0.996	0.998
Basic metals (27)	0.999	0.992	0.996	0.996
Fabricated metals (28)	0.999	0.999	0.999	0.999
Machinery (29)	0.999	0.998	0.998	0.999
Office machinery (30)	0.745	0.829	0.784	0.836
Elec. machinery (31)	0.994	0.999	0.999	0.997
Communication (32)	0.992	0.996	0.989	0.998
Precision (33)	0.922	0.992	0.972	0.992
Vehicles (34)	0.332	0.923	0.622	0.943
Other transport (35)	0.576	0.925	0.957	0.731
Furniture (36)	0.999	0.999	0.999	0.998
Recycling (37)	0.846	0.003	0.032	0.006
Adjusted R-squared of estimating equation including				
industry dummies	0.022	0.013	0.016	0.013
industry and time dummies and interaction effects	-0.060	-0.015	-0.044	-0.007

Notes: The numbers are marginal significance levels of tests for the joint significance of all interactions containing the dummy in the first row of the table. Industry numbers refer to 2-digit NACE Rev.1 classification.

Equation (9) allows for the presence of industry-year specific effects. Marginal significance levels of interactions effects by industry are given in Table IX. The relative low p-value for the tobacco industry can be explained by larger job destruction in 1996 across countries. Also, significant above average job destruction in recycling in 1995 is reflected in the presence of an industry-year specific effect. All in all, evidence for a prevalent industry-year specific effect is not very strong. Comparison of the R-squareds excluding and including time dummies and interaction effects from the estimating equation learns that including time dummies and interaction terms decreases the model's adjusted R-squared: fixed time effects and interaction effects add nothing to the explained variation in the dependent variable.

To conclude, estimates do not reveal the presence of a country-year or industry-year specific effect in employment dynamics. This implies the cyclicality of employment flows is neither country nor industry specific. Therefore, besides an overall country fixed effect, country-industry specific or "residual" variation in employment flows best explains job creation and destruction.

V. CONCLUSIONS

One paper allowing for a consistent comparison of gross flows of jobs between countries is Baldwin, Dunne and Haltiwanger (1998) using harmonised data for manufacturing industries in Canada and the US. They find that job destruction is more cyclically volatile than job creation in both countries but more so in the US and that industries with high (low) gross job flows in Canada are characterized by high (low) gross job flows in the US. Accordingly, this result suggests that country specific differences are not important and that the process of job reallocation in both countries is best explained by an industry fixed effect.

This chapter uses information on manufacturing establishments during the 1990s in four European countries: Belgium, France, Italy and the UK. The strength of the analysis comes from the fact that the data are comparable across countries and manufacturing industries: the sampling criteria, the time frame and the sector composition are uniformly defined. Furthermore, the data cover the majority of manufacturing employment in all four countries. The results suggest 1) job destruction is more cyclically volatile only in the UK and 2) a country fixed effect dominates the between-variation in employment flows.

As shown theoretically by Garibaldi (1998), symmetric behavior of job creation and destruction over the business cycle can arise as an outcome in which it is time consuming for firms to lay-off workers due to firing frictions. Besides the potential impact of labor market regulation on the cyclicality of employment flows, different labor market institutions in Continental European countries could also explain an important part of the country specific differences in employment dynamics.

GENERAL CONCLUSIONS

Every day, the continuous mutations of our labor markets affect the income and job prospects of many workers. In this respect, the past thirty years have been no exception. Wage inequality has increased considerably and an ever higher educated workforce is increasingly employed in both low-paid and high-paid service jobs. These are the changes that this thesis aimed to explain.

First, it would be natural to assume that the increase in wage inequality together with the relative increase in high-paid service jobs could be explained by an increase in the relative demand for skilled labor or skill-biased technological change (SBTC). But the SBTC hypothesis is only a partial truth since it seems best able to explain what is happening in the top half of the wage distribution but is uninformative about the increasing employment shares of workers in low-wage service jobs. There, the more nuanced view about the impact of technology recently proposed by Autor, Levy and Murnane [2003] (ALM) seems more appropriate and it seems plausible that the relative demand for routine middling jobs has fallen. This thesis has provided UK evidence of increased job polarization between 1975 and 1999 that is consistent with the ALM hypothesis. Moreover, it has argued that an important part of the increase in wage inequality both at the top and the bottom of the wage distribution can be explained by job polarization. However, the finding that during the 1980s the wages in the lowest paid jobs are falling relative to those in the middling jobs presents something of a puzzle for the ALM hypothesis. But more recent studies (Autor, Katz and Kearney [2004] and Goos and Manning [2005]) have argued that the ALM hypothesis seems consistent with the deceleration of growth in lower tail inequality since the late 1990s. More precisely, it is argued that earnings as well as employment of workers in low-paid jobs (relative to the middling jobs) increased during the late 1990s. Since it seems crucial for the ALM hypothesis to see what has happened to the relative wages of workers in jobs intense in non-routine manual tasks, I therefore think one idea worth pursuing would be to look deeper into the measurement of task premiums and their changes over time.

Crudely, the continuing investment by the UK government in making higher education ever more accessible seems at odds with the documented increase in the relative demand for workers in low-wage jobs. Not only are college workers more likely to be paid a high school wage due to job polarization, the continuous expansion of higher education also reduces the college premium through an increase in the relative supply of college workers. More precisely, this thesis has argued that fluctuations in the rate of growth of the relative supply of college workers can explain an important part of the changes in earnings dispersion. To this end, it was shown that the fall in educational attainment growth rates for cohorts born between 1955 and 1970 could explain an important part of the rise in the average college premium and therefore wage inequality after 1980. Consequently, as cohorts born after 1970 will come of age, the college premium and wage inequality are expected to fall relative to the impact of any increase in the relative demand for low-paid and high-paid service jobs following the recent expansion of Britain's higher education system.

The relative importance of low-wage service jobs is also expected to increase in Continental European countries due to increased product market competition. This could happen indirectly through the increasing trend towards outsourcing of mainly routine middling jobs but also directly through product market deregulation. For example, following the deregulation of shop closing hours in the US and the UK, policy makers in most Continental European countries are under increasing pressure to allow shops to open on Sunday. This thesis has argued that one important effect of such deregulation would be an increase in retail employment in deregulating industries but only if consumers have a sufficiently strong "taste" for Sunday shopping. Whether this is the case probably differs between countries. But even in those countries where consumers do not value their common leisure greatly, this thesis has shown that equally important in any debate about the employment impact of extending opening hours are retailing costs, pricing behavior, the competitive nature of retail markets and wage policies. Moreover, it was argued that the optimal policy maker would also account for the predicted fall in product demand and therefore retail employment in non-deregulating industries or countries.

Finally, this thesis has shown that also more persuasive regulation seems to affect employment in Continental European countries. In contrast to Anglo-Saxon countries where job destruction is more cyclically volatile than job creation and gross job flows are best captured by industry fixed effects, it was shown that job destruction is as cyclically volatile as job creation and that a country fixed effect dominates the between-country variation in gross job flows. For example, this can arise as an outcome in which it is time consuming for firms to lay-off workers due to firing frictions.

BIBLIOGRAPHY

Acemoglu, Daron [2001], "Good Jobs versus Bad Jobs", Journal of Labor Economics, XIX (2001), 1-21.

Acemoglu, Daron [1999], "Changes in Unemployment and Wage Inequality: An Alternative Theory and Some Evidence", American Economic Review, 89 (1999), 1259-1278.

Albaek, K. and Sorensen, B.E. [1995], "Worker Flows and Job Flows in Danish Manufacturing, 1980-1991", Economic Journal, 108, 1750-71.

Autor, David H, Larry Katz and Melissa Kearney [2004], "Trends in US Wage Inequality: Re-Assessing the Revisionists", mimeo MIT, August 2004.

Autor, David H, Frank Levy and Richard J Murnane [2003], "The Skill Content of Recent Technological Change: An Empirical Exploration," Quarterly Journal of Economics, CXVIII (2003), 1279-1333.

Baldwin J., Dunne, T. and Haltiwanger, J. [1998], "A Comparison of Job Creation and Job Destruction in Canada and the United States", Review of Economics and Statistics, LXXX: 3, 347-356.

Baumol, William J. [1967], "Macroeconomics of Unbalanced Growth: the Anatomy of Urban Crisis," American Economic Review, LVII (1967), 415-426.

Berman, Eli, John Bound and Stephen Machin [1998], "Implications of Skill-Biased Technological Change: International Evidence," Quarterly Journal of Economics, CXII (1998), 1245-79.

Berman, Eli, John Bound and Zvi Griliches [1994], "Changes in the Demand for Skilled Labor within U.S. Manufacturing: Evidence from the Annual Survey of Manufactures," Quarterly Journal of Economics, CIX (1994), 367-97.

Bertrand, M. and F. Kramarz [2002], "Does Entry Regulation Hinder Job Creation?: Evidence from the French Retail Industry", Quarterly Journal of Economics, Vol. 117 no. 4, pp. 1369-413.

Blanchard, O. and F. Giavazzi [2002], "Macroeconomic Effects of Regulation and Deregulation in Goods and Labor Markets", MIT Dept. of Economics Working Paper No. 01-02.

Blanchard. O. and Diamond, P. [1992], "The Flow Approach to Labor Markets", American Economic Review, 82:2, 354-359.

Blanchard, O. and Diamond. P. [1990], "The Cyclical Behavior of the Gross Flows of US Workers", Brookings Papers on Economic Activity, 2: 1990, 58-155.

Blanchard, O. and Diamond, P. [1989], "The Beveridge Curve", Brookings Papers on Economic Activity, 1: 1989, 1-62.

Blanchflower, D.G. and Burgess, S.M. [1996], "Job Creation and Job Destruction in Great Britain in the 1980s", Industrial and Labor Relations Review, 50, 17-38.

Bluestone, Barry and Bennet Harrison [1988], "The Growth of Low-Wage Employment: 1963-1986," American Economic Review, LXXVIII (1988), 124-128.

Boeri, T., Garibaldi, P. and Macis, M. [2001], "Adaptability of Labor Markets: a Tentative Definition and a Synthetic Indicator", mimeo Fondazione Rodolfo Debenedetti.

Boeri, T. [1996], "Is Job Turnover Countercyclical", Journal of Labor Economics, 4, 603-25.

Boeri, T. and Cramer, U. [1992], "Employment Growth, Incumbants and Entrants: Evidence from Germany", International Journal of Industrial Organisation, 10, 545-565.

Borjas, George J., Richard B. Freeman and Lawrence F. Katz [1996], "Searching for the Effect of Immigration on the Labor Market", American Economic Review, Vol. 86, No. 2, May 1996, 246-251.

Broersma, L. and den Butter, F.A.G. [1997], "Job Flows in Dutch Manufacturing, 1979-1993: Empirical Evidence and Theoretical Implications", De Economist, 145:1, 47-64.

Burda, M. and P. Weil [2001], "Blue Laws", mimeo Humboldt-University of Berlin.

Burtless, Gary [1990], "A Future of Lousy Jobs?: The Changing Structure of US Wages", Brookings Institution Press.

Card, David and John DiNardo [2002], "Skill-Biased Technological Change and Rising Wage Inequality: Some Problems and Puzzles," Journal of Labor Economics, XX (2002), 733-783.

Card, David and John DiNardo [2002], "Skill-Biased Technological Change and Rising Wage Inequality: Some Problems and Puzzles", Journal of Labor Economics, 20-4, 2002.

Card, David and Thomas Lemieux [2001], "Can Falling Supply Explain the Rising Return to College for Younger Men? A Cohort-Based Analysis", Quarterly Journal of Economics, May 2001, 705-746.

Card, David and Thomas Lemieux [1996], "Wage Dispersion, Returns to Skill, and Black-White Wage Differentials," Journal of Econometrics, LXXIV (1996), 319-61.

Chevalier, Arnaud [2000], "Graduate Over-education in the UK," Centre for the Economics of Education Discussion Paper No. 7, 2000, LSE.

Clemenz, G. [1990], "Non-Sequential Consumer Search and the Consequences of a Deregulation of Trading Hours", European Economic Review, Vol. 34, pp. 1323-37.

Costrell, Robert M. [1990], "Methodology in the "Job Quality" Debate," Industrial Relations, XXIX (1990), 94-110.

Diloff, N. J. [1980], "Never on a Sunday: The Blue Laws Controvery", The Maryland Law Review, Vol. 39 no. 4, pp. 679-714.

Davis, S.J., Haltiwanger, J. [1999], "Gross Job Flows", in: Ashenfelter, O. & Card, D, editors, Handbook of Labor Economics, Vol. 3, Amsterdam, North-Holland, 2714-2803.

Davis, J.S., Haltiwanger J.C., Schuh, S. [1996], "Job Creation and Destruction", MIT Press.

Davis, S.J., Haltiwanger, J. [1992], "Gross Job Creation, Gross Job Destruction and Employment Reallocation", Quarterly Journal of Economics, 107:3, 819-63.

DiNardo, John., Nicole Fortin and Thomas Lemieux [1996], "Labor Market Institutions and the distribution of wages, 1973-1992," Econometrica, LXIV (1996), 1001-1044.

Dixit, A. K. and J. E. Stiglitz [1977], "Monopolistic Competition and Optimum Product Diversity", The American Economic Review, Vol. 67 no. 3, pp. 297-308.

Dunne, Timothy, Lucia Foster, John Haltiwanger and Kenneth Troske [2000], "Wage and Productivity Dispersion in US Manufacturing: The Role of Computer Investment," NBER Working Paper 7465, 2000.

Dunne, T., Roberts, M.J. and Samuelson, L. [1989], "Plant Turnover and Gross Employment Flows in the Manufacturing Sector", Journal of Labor Economics, 7, 48-71.

Dzialo, M.C., Shank, S.E. and D.C. Smith [1993], "Atlantic and Pacific Coasts' Labor Markets Hit Hard in Early 1990's", Monthly Labor Review, February 1993, pp. 32-39.

Farber, Henry S. [1997], "Job Creation in the United States: Good Jobs or Bad?", Princeton Industrial Relations Section Working Paper, No. 385, July 1997.

Feenstra, Robert C. and Gordon H. Hanson [1999], "The Impact of Outsourcing and High-Technology Capital on Wages: Estimates for the United States, 1979-1990", Quarterly Journal of Economics, CXIV (1999), 907-940.

Felstead, Alan, Duncan Gallie and Francis Green [2002], "Work Skills in Britain, 1986-2001," Department for Education and Skills, 2002.

Richard B. Freeman [2003], "Tade Wars: The Exaggerated Impact of Trade in Economic Debate", NBER Working Paper 10000, September 2003.

Garibaldi, P. [1998], "Job Flow Dynamics and Firing Restrictions", European Economic Review, 42, 245-275.

Garibaldi, P., Konings, J., Pissarides, C. [1997], "Gross Job Reallocation and Labor Market Policy", in: Snower, D.J. & de la Dehesa G., editors, Unemployment Policy: Government Options for the Labor Market, New York: Cambridge University Press.

Gittleman, Maury B. and David R. Howell [1995], "Changes in the Structure and Quality of Jobs in the United States: Effects by Race and Gender, 1973-1990," Industrial and Labor Relations Review, XLVIII (1995), 420-440.

Goos, Maarten and Alan Manning [2005] "Can A More Nuanced View of Skill-Biased Technological Change Explain the Recent Changes in Wage Inequality?", Reflets et Perspectives, XLIII, 2005/2.

Goos, Maarten and Alan Manning [2003] "Lousy and Lovely Jobs: the Rising Polarization of Work in Britain", LSE CEP Discussion Paper No. 604, Dec 2003.

Gradus, R H. J..M. [1996], "The Economic Effects of Extending Shop Opening Hours", Journal of Economics, vol. 64 no.3, pp. 247-263.

Green, Francis and Steven McIntosh [2002], "Is there a Genuine Underutilisation of Skills Amongs the Over-Qualified," LSE CEP Working Paper, 2002.

Green, Francis, Steven McIntosh and Anna Vignoles [1999], "Overeducation and Skills – Clarifying the Concepts," LSE CEP DP No. 435, 1999.

Grunhagen, Marko, Stephen J. Grove and James W. Gentry [2001], "The Dynamics of Store Hour Changes and Consumption Behavior: Results of a Longitudinal Study of Consumer Attitudes Towards Saturday Shopping in Germany", Clemson University Centre for International Trade Working Paper 0010913.

Haltiwanger, J. and M. Vodopivec [2002], "Gross Worker and Job Flows in a Transition Economy: An Analysis of Estonia", Labor Economics, 5, 601-630.

Hartog, J. [2000], "On returns to Education: Wandering Among the Hills of ORU Land,"in Heijke, Hans and Muysken, Joan, eds. Education and training in a knowledge-based economy. London: Macmillan, 2000.

Hellerstein, Judith K, David Neumark and Kenneth R. Troske [1999], "Wages, Productivity and Worker Characteristics: Evidence from Plant-Level Production Functions and Wage Equations", Journal of Labor Economics, VII (1999), 409-446.

Howell, David R. and Edward N. Wolff [1991], "Trends in the Growth and Distribution of Skills in the U.S. Workplace, 1960-1985," Industrial and Labor Relations Review, XLIV(1991), 486-502.

Ichino, A., Polo, M. and Rettore, E. [2001], "Are Judges Biased by Labor Market Conditions?", mimeo European University Institute.

Ilg, Randy E. and StevenE. Haugen [2000], "Earnings and Employment Trends in the 1990s," Monthly Labor Review, March 2000, 21-33.

Ilg, Randy E. [1996], "The nature of employment growth, 1989-1995," Monthly Labor Review, CXIX (1996), 29-36.

Inderst, R. and A. Irmen [2004], "Shopping Hours and Price Competition", forthcoming in European Economic Review.

Jacobsen, J.P. and P. Kooreman [2005], "Timing Constraints and the Allocation of Time: The Effects of Changing Shopping Hours Regulation in the Netherlands", European Economic Review, Vol. 49, pp. 9-27.

Juhn, Chinhui [1999], "Wage Inequality and Demand for Skill: Evidence from Five Decades," Industrial and Labor Relations Review, LII (1999), 424-443.

Juhn, Chinhui [1994], "Wage Inequality and Industrial Change: Evidence from Five Decades," NBER Working Paper No. 4684, 1994.

Juhn, Chinhui, Murphy Kevin M. and Brooks Pierce [1993], "Wage Inequality and the Rise in Returns to Skill," Journal of Policital Economy, CI (1993), 410-442.

Kajalo, S. [1997], "Sunday Trading, Consumer Culture, and Shopping – Will Europe Sacrifice Sunday to Recreational Shopping?", mimeo Helsinki School of Economics, pp.18.

Katz, Lawrence F. and David H. Autor [1999], "Changes in the Wage Structure and Earnings Inequality," in Ashenfelter, Orley and David Card (eds) Handbook of Labor Economics. Volume 3A. Amsterdam: North-Holland, 1999, 1463-1555.

Katz, Lawrence F. and David H. Autor [1999], "Changes in the Wage Structure and Earnings Inequality," in Ashenfelter, Orley and David Card (eds) Handbook of Labor Economics. Volume 3A. Amsterdam: North-Holland, 1999, 1463-1555.

Katz, Larry and and Kevin M. Murphy [1992], "Changes in Relative Wages, 1963-87: Supply and Demand Factors", Quarterly Journal of Economics, 107 (February 1992), 35-78.

Konings, J., Lehmann, H. and Schaffer, M. [1996], "Job Creation and Job Destruction in a Transition Economy: Ownership, Firm Size and Gross Job Flows in Polish Manufacturing", 1988-1991, Labor Economics, 3, 299-317.

Konings, J. [1995], "Job Creation and Job Destruction in the UK Manufacturing Sector", Oxford Bulletin of Economics and Statistics, 57, 5-24.

Kosters, Marvin H. and Murray N. Ross [1988], "A Shrinking Middle Class?," Public Interest, 1988, 3-27.

Krueger, A.B. and J.S. Pischke [1997], "Observations and Conjectures on the U.S. Employment Miracle", NBER Working Paper 6146.

Laband, D.N. and D.H. Heinbuch [1987], "Blue Laws: History, Economics and Politics of Sunday-Closing Laws".

Lee, David S. [1999], "Wage Inequality in the United States During the 1980s: Rising Dispersion or Falling Minimum Wage?," Quarterly Journal of Economics, CXIV (1999), 941-1023.

Lemieux, Thomas [2002], "Decomposing Wage Distributions: a Unified Approach" Canadian Journal of Economics, 35 (4), November 2002, 646-88.

Levy, Frank and Richard J. Murnane [1992], "U.S. Earnings Levels and Earnings Inequality: A Review of Recent Trends and Proposed Explanation", Journal of Economic Literature, 30:3, September 1992, 1333-1381.

Machin, Stephen [2002], "Labor Market Inequality and Changes in the Relative Demand for Skills", mimeograph LSE, 2002.

Machin, Stephen and John van Reenen [1998], "Technology and Changes in Skill Structure: Evidence from Seven OECD Countries," Quarterly-Journal-of-Economics, CXIII (1998), 1215-44.

Maher, Imelda [1995], "The New Sunday: Reregulating Sunday Trading", Modern Law Review , 58: 72-86.

Meisenheimer, Joseph R. II [1998], "The services industry in the 'good' versus 'bad' jobs debate," Monthly Labor Review, February 1998, 22-47.

Mortensen, D. and Pissarides, C. [1994], "Job Creation and Destruction in the Theory of Unemployment", Review of Economic Studies, 61, 397-416.

Mortimer, J.T. [2003], "Working and Growing Up in America", Harvard University Press, pp. 304.

Murphy, Kevin M and Finis Welch [1993] "Occupational Change and the Demand for Skill, 1940-1990", American Economic Review, LXXXIII (1993), 122-36.

Nooteboom [1983], "Trading Hours and Economies of Scale in Retailing", European Small Business Journal, 1 (2).

Nooteboom [1982], "A New Theory of Retailing Costs", European Economic Review, 17, pp.163-186.

OECD [2001], "The Characteristics and Quality of Service Sector Jobs," OECD Employment Outlook, 2001, 89-128.

OECD [1996], "Employment Adjustment, Workers and Unemployment", OECD Employment Outlook, Chapter 5, 161-182.

OECD [1994], "Job Gains and Job Losses in Firms", OECD Employment Outlook, Chapter 3, 103-135.

Pissarides, C. [2000], "Equilibrium Unemployment Theory", MIT Press.

Pissarides, C. [1990], "Equilibrium Unemployment Theory", MIT Pres.

Sicherman, Nachum [1991], "Overeducation in the Labor Market", Journal of Labor Economics, IX (1991), 101-122.

Simon, Herbert A. [1960], "The Corporation: Will it Be Managed by Machines?", in M.L. Anshen and G.L. Bach, eds, Management and Corporations 1985, New York, NY: McGraw-Hill, 1960.

Michael Skuterud [2004], "The Impact of Sunday Shopping on Employment and Hours of Work in the Retail Industry: Evidence from Canada", forthcoming European Economic Review.

Spenner, Kenneth I. [1983], "Deciphering Prometheus: temporal change in the skill level of work," American Sociological Review, XLVIII (1983), 824-37.

Spitz, Alexandra [2003], "IT Capital, Job Content and Educational Attainment," Centre for European Economic Research Discussion Paper No. 03-04, 2003.

Teulings, Coen, N. [2000], "Aggregation Bias in Elasticities of Substitution and the Minimum Wage Paradox," International Economic Review, XLI (2000), 359-98.

Theuman, J. E. [2004], "Validity, Construction, and Effect of "Sunday Closing" or "Blue" Laws – Modern Status", American Law Reports, ALR4th

Thurik [1982], "Labor Productivity in French Supermarkets", Report 8210/S of Erasmus University, Rotterdam.

Wright, Erik Olin and Rachel Dwyer [2003], "The Patterns of Job Expansions in the United States: a comparison of the 1960s and 1990s," Socio-Economic Review, 2003, 1: 289-325.

Walker, Ian and Yu Zhu [2005], "The College Wage Premium, Overeducation, and the Expansion of Higher Education in the UK", IZA DP No. 1627, pp.29