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EVALUATION

Labour market effects of working time reductions and demographic changes

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Abstract

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This thesis consists of three self-contained essays.

Essay I studies the impact on actual hours worked of a 5 % working time reduction for one class of shift workers in Sweden using individual level panel data from firms' payroll records during the second quarter each year. The main result is that actual hours only decreased by approximately 35 % of the reduction in standard hours. Quantile regression results show that the effect was relatively homogeneous over the distribution of hours worked. Much larger effects are derived by studying the effects of the individuals' scheduled hours. This indicates that a low rate of actual implementation may explain the results and suggests that using variation in self-reported, rather than contractual, standard hours may have biased the results of previous studies.

Essay II extends a model of equilibrium unemployment showing that a *general* working time reduction will reduce equilibrium unemployment unless the firms have fixed costs for workers. A counteracting effect exists if firms have substantial fixed costs. A testable implication is that a *partial* working time reductions preferred by the workers should reduce hourly wages unless firms have substantial fixed costs. A 5 % working time reduction for shift workers in Sweden is studied using register-based panel data. The results show that hourly wages increased as a result of the working time reduction. Such an increase in wage demands is consistent with fixed costs and would tend to increase equilibrium unemployment if working hours were reduced for all workers.

Essay III studies the effects of changes in the age structure on aggregate labour market performance using a panel of Swedish local labour markets. The methodology of Shimer (2001) is used for studying the effects of youth cohort size and is extended to include the full age distribution. The results show that young workers benefit from belonging to a large cohort. This is in line with previous results for the US. Furthermore, it is shown that most of the positive effect for young workers is due to an inward shift in the Beveridge-curve. In contrast to the US experience, older workers in Sweden do not benefit from large youth cohorts. Further results show that large numbers of 50 to 60 year old workers have an adverse effect on the labour market. This is consistent with negative externalities from well-matched individuals.

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[1] So many in fact that the Department Jester once insinuated that I was collecting advisors as a hobby.

[2] I have even been known to free-ride to non-existent places such as imaginary dog-shelters.

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Introduction[*]

This thesis consists of three self-contained essays. The first two essays discuss the effects of working time reductions. *Essay I* is an empirical investigation of a working time reduction's effects on actual hours worked. It is based on individual level micro data. *Essay II* discusses the effects on equilibrium unemployment and presents estimates of wage effects using the same data set as *Essay I*. The third essay differs from the previous two. It deals with the issue of how changes in the population age structure affects local labour markets and uses aggregate data in the empirical analysis.

There are some common denominators between the first two papers and the third paper; most notably they all deal with labour market effects of changes in the nature of labour supply (hours of work and the age composition) and they all use standard panel data methods in the empirical analysis. However, since the differences clearly dominate the similarities, I will not attempt to create an artificial common framework for the papers in this introduction. Rather, the following three sections discusses the three papers separately.

How do working time reductions affect actual working time?

The first essay in this thesis asks a simple question: is it necessarily true that a centrally determined working time reduction reduces the actual working time? While the answer might seem obvious, it is not clear from a theoretical perspective what the effect on actual hours should be. A standard result in the literature (see, e.g., Calmfors and Hoel, 1988) is that firms may want to increase the total amount of hours worked by using more overtime if standard hours are reduced. While this result crucially hinges on some quite restrictive assumptions,[1] it still shows that the answer to the question is less than obvious.

A further complication arises due to the Swedish institutional environment. The bargaining institutions allow for quite large flexibility regarding the implementation of central agreements and labour market laws. One indication that this formal flexibility has real implications is that a large part of the wage increases in the 1980's was due to "wage drift" (see Hibbs and Locking, 1996).

[*] Special thanks to Laura Larsson for valuable comments on this introduction.

[1] These assumptions include that firms can choose overtime, that the overtime premium is constant and that the firms would use some overtime even in the absence of the working time reduction

The question asked in *Essay I* is whether there exists a corresponding "working time drift" that would reduce the effectiveness of working time regulations as a policy tool.

It is possible to find evidence in the international literature of working time reductions where there was a lag in the implementation (Hunt, 1999) or where the implementation was only partial (Crepon and Kramarz, 2000). However, previous empirical studies have still lent nearly unanimous support for the notion that actual hours follow standard hours closely (see, e.g., Hunt, 1999 and Friesen, 2002 as well as Jacobsson and Ohlsson, 2000).

Essay I studies the impact of a 5 % working time reduction for one class of shift workers in Swedish manufacturing. The reduction was decided upon in a series of central agreements between the Swedish Employers Confederation (SAF) and the Swedish Trade Union Confederation (LO). It was implemented in several steps between 1983 and 1988. To identify the effect of the working time reduction the change in hours worked between before and after the reduction is studied. By using the corresponding change for the workers not covered by the reduction it is possible to purge the results from other time effects.

A unique register based data set is used. It was collected from firms' payroll records on actual hours worked during the second quarter each year. This differs from most previous studies that have been based on survey data reported by the individual workers themselves. A further contribution of the essay is to use quantile regression techniques to study the impact of the working time reduction on the distribution (rather than just the mean) of actual hours worked.

The main result is, perhaps surprisingly, that the impact of the working time reduction was much smaller than intended. Actual hours were only reduced by between 30 and 40 % of the reduction in standard hours. Quantile regression results show that the effect was quite homogeneous.[2] This means that the percentage reductions in actual hours were approximately the same for those working relatively many hours as for those working relatively few.

One possible reason for why the working time reduction had such a small effect is that the reduction was not fully implemented at the local level. In an attempt to study this hypothesis closer I use data on individuals' scheduled hours as a measure the implementation. These data are voluntarily reported by the firms and thus less reliable than the data used otherwise in the essay. With this caveat in mind, the results suggest that one reason for the small average ef-

[2] At least in the central part of the distribution

fect is in fact that it was not implemented for all workers. The results also suggest that a similar study on German data (Hunt, 1999) may have overestimated the effect of working time reductions on actual hours by using a method that fails to account for the rate of implementation.

Would a general working time reduction lead to a sustained reduction of unemployment?

The second essay of the thesis continues where the first essay ended. It studies the effects of working time reductions on unemployment. Reduced working hours may be motivated in several different ways but one of the most popular and (perhaps misleadingly) intuitive motivations is as a means for increased employment. This idea is usually referred to as "work sharing".

Economists have discussed the concept of work sharing in length since the 1980's when it was a popular policy tool in continental Europe. Most of the discussion has been concerned with how the hiring decisions of individual firms would be affected by reduced working hours. This is somewhat surprising, at least if we are concerned with the long run effects on unemployment (i.e., with effects on "equilibrium unemployment"). We generally think of equilibrium unemployment as ultimately being determined by how the employed workers (or the unions) trade off wages to the risk of becoming unemployed. The focus of the analysis should thus be on more "structural" factors that can affect this trade-off.

Essay II attempts to identify mechanisms through which a working time reduction may affect equilibrium unemployment. A simple model (based on Houpis, 1993) is set up. The analysis shows that a working time reduction would tend to lower the equilibrium unemployment rate by making work more favourable relative to unemployment since the workers would have to give up less leisure when working.[3] There will, however, be an offsetting effect *if* the firms have large fixed costs for their workers. Thus, the long run effect on unemployment can, from a theoretical perspective, be positive as well as negative.

The sign of the long run effect on unemployment depends on what happens to the workers wage demands. This is used as an argument to set up a test based on the wage effects of a working time reduction that only affects a small number of workers (a "partial" working time reduction). The argument is that a

[3] This effect does *not* require that workers individually (or collectively) would prefer the reduction.

partial working time reduction should lead to falling wages if work sharing is possible through wage restraint.

Formally, it is shown that *hourly* wages should fall for workers affected by a partial working time reduction unless the firms have substantial fixed costs.[4] Falling hourly wages is therefore consistent with a model that unambiguously predicts that a general working time reduction would lead to permanently lower unemployment. *Rising* hourly wages is, on the other hand, a sign of increased wage demands (possibly due to fixed costs) that will tend to raise the equilibrium unemployment rate if working hours are reduced for all workers.

Essay II studies the wage effects of the working time reduction (using the same micro level data) that was studied in *Essay I*. The results show that hourly wages rose sharply due the working time reduction. This is evidence of increased, rather than decreased, wage demands. Further results, using the data on scheduled hours, indicate that the workers that received a larger reduction in actual hours also experienced a larger wage increase. This suggests that the positive wage effect is not driven by the small average impact of the working time reduction found in *Essay I*.

The conclusion is that the partial working time reduction lead to increased wage demands possibly due to fixed costs of firms. Such wage demands would tend to raise the equilibrium unemployment rate if working hours were reduced for all workers.

How does the age structure affect the unemployment rate?

The third essay of this thesis studies how the local labour markets in Sweden are affected by changes in the population age structure. The focus is on the role played by young individuals, but other aspects of the age structure are also studied.

A recent paper (Shimer, 2001) studies how the size of youth cohorts affects the age-specific unemployment rates at the state level in the United States. The results are highly surprising; the more young workers, the lower the unemployment rates (and the higher the participation rates) *for all age groups*. These effects are particularly strong for *older* workers.

The theoretical explanation for the results is based on the hypothesis that old and well-established workers are content with their positions and thus reject

[4] This argument requires that the workers prefer the reduction at the individual level.

most job offers. In a search-theoretical framework this can make it less profitable for firms to open vacancies in labour markets with many older workers. Essentially this argument means that the matching between jobs and workers is more efficient when a large fraction of the workers are newcomers in the labour market.

Essay III uses an empirical strategy that is highly influenced by Shimer (2001) to study the effects of youth cohort size using Swedish local labour market data. The share of young workers is predicted by using population data for previous (rather than current) years since it is quite possible that young workers move to regions with good labour market prospects.

It is shown that the youth share is highly correlated with the sizes of other age groups. Most notably there is a positive correlation between the share of young workers and the share of individuals just over 20 years older, i.e. the most likely age group of their parents. Due to these correlations, we may accidentally attribute effects of other age groups to the youth share if they are not accounted for in the empirical model. Thus, the essay also studies the effects of all age groups simultaneously.

The results show, just as the results for the United States did, that young workers appear to benefit from belonging to a large youth cohort, both in terms of lower unemployment and higher employment. It is also shown that labour market performance is worse for all age groups when there are many individuals aged 50 to 60 at the labour market. Further results based on a search-theoretical framework show that there was both an inward shift in the "Beveridge-curve" and a favourable move along the curve (for young workers). All of these results support Shimer (2001) in that the labour market works better the younger the workers are and that the effect is due to improved matching between jobs and workers.

However, quite in contrast to the results for the United States, *Essay III* also shows that large youth cohorts had a *negative* effect on the labour market performance of the oldest workers. This is not only in contrast with the empirical results in Shimer (2001), but also contradicts the theoretical foundation in that paper that requires that *all* workers benefit from an increase in the share of young workers.

The essay does not present a theoretical explanation for the discrepancies between the United States and Swedish estimates. One observation, however, that may indicate where to find an explanation is that the incidence of long term unemployment among older workers in Sweden is much higher than among younger workers. This suggests that the search theoretical framework might give a less accurate description of the labour market for this group. However, for an understanding of the policy implication of the results, for Sweden as well as the United States, it is necessary to find a comprehensive theory that can explain both sets of results. Thus, more research is clearly needed before any of the results can be used for policy purposes.

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Essay I

Do working time reductions reduce actual working time?

Evidence from Swedish register data*

1 Introduction

While a large and increasing number of workers state that they would prefer to work fewer hours (OECD, 1998 and Torp and Barth, 2000), others are involuntarily unemployed. Thus, working time reductions as a policy against unemployment, or *work sharing*, does carry an intuitive appeal. Other motives for working time reductions, such as positive health effects, increased labour supply or increased welfare in general, fuel the public working time debate even in years of low unemployment rates. Consequently, many OECD countries have implemented working time reductions during the last 20 years. Hunt (1998) reports that Sweden is the only out of 19 surveyed countries where working hours in manufacturing remained unchanged during 1984-95.

It is often assumed in the public discussion that the actual number of hours worked by individuals can be directly affected by public policy. Naturally, this is an important assumption for the effectiveness of working time reductions as

* Helpful comments were given by Dominique Anxo, Mahmood Arai, Erling Barth, Mikael Carlsson, Anders Forslund, Christian Nilsson, Henry Ohlsson as well as seminar participants at Göteborg University, Uppsala University, SOFI and the 2001 EEA and EALE conferences. Thanks also to SAF and Ari Hietasalo for supplying the data and to Bertil Edin for help with some institutional background.

a policy instrument, regardless of the motivation for the policy. The issue of how changes in standard working hours affect actual hours is particularly interesting in the Swedish case since the labour market institutions are quite flexible in allowing for agreements at lower level of bargaining that contradict central agreements and labour market laws. Evidence of the relationship between standard and actual hours in Sweden is, however, scarce, despite of the large interest in working time issues.[1] Empirical work on Swedish data includes Jacobson and Ohlsson (2000) that study aggregate time series data and find an effect from legislated working time on actual hours. Pencavel and Holmlund (1988) study industry level relationships between labour demand, hours and wages. Very little empirical work has been done on micro level data even outside of Sweden, for a review see Hunt (1998). Recent examples are Friesen (2002) that studies the impact on hours and wages of cross-sectional differences in Canadian working time laws and Hunt (1999) that studies the effects of an industry level working time reduction on hours, wages, and employment in Germany. Hunt finds that hours were reduced by almost the predicted amount, that hourly wages rose to compensate for the loss in earnings and that employment fell.

This paper studies the impact of a 5 % working time reduction for one class of shift workers ("2-shift workers") in the Swedish manufacturing and mining industries using an individual level register based panel data set. The data set is collected from firms' payroll records and contains information on each employee's total number of hours worked during the second quarter each year. This is the first study of the effects of a working time reduction on actual hours that uses register data. The measure of hours used is actual hours worked during one quarter each year. The paper is thus not limited to studying the effect on hours worked in a specific (or "usual") week.

The working time reduction that is studied was contracted upon in central agreements. This should ensure that the effects resemble those of a legislated working time reduction, except for possible general equilibrium effects that cannot be studied since the reduction only covers a limited number of workers. However, the very fact that the reduction only affected some of the workers (the 2-shift workers) within each industry (and firm) makes the reduction par-

[1] The interest in working time issues in Sweden is highlighted by the ongoing working time committee scheduled to present suggestions of working time reforms in June 2002 and the recent completion of the second report in two years on working time issues from the National Institute for Economic Research (Konjunkturinstitutet, 2002).

ticularly interesting to study. The reason is that the other workers can be used to control for time effects. Furthermore, it is possible to control for local industry and year-specific factors through the use of narrowly defined dummies since the data set is quite large and contains information on all workers in firms covered by the central agreements.

The results show that actual working hours were reduced by only about 35 % of the reduction in standard hours. The estimated effect is robust to a number of specifications with different identification strategies, different control variables and regardless of whether the effect is measured relative to daytime workers or other shift workers not covered by the reduction. Quantile regressions are used to study the impact on the distribution of hours and the results show that the small effect was roughly homogeneous over the distribution, with somewhat smaller effects for the uppermost quantiles. Further evidence suggests that one reason for the small average effect may be that only some workers experienced a reduction of the locally determined scheduled hours. *Essay II* of this thesis studies the wage effect of the same reduction and shows that hourly wages rose as a result of the reduction. The wage increase was largest for the workers that experienced the largest reduction in actual hours.

The remainder of this paper is structured as follows: *Section 2* describes the institutional setting and the working time reduction *Section 3* describes the data and the empirical strategies. *Section 4* presents evidence of the average effect on actual hours worked and *Section 5* gives some evidence of heterogeneous effects. *Section 6* concludes.

2 Background

2.1 Institutional setting

The Swedish labour market institutions are described as “negotiated flexibility” by Anxo and O’Reilly (2000) since they allow for quite large possibilities to sign local level agreements that deviates from central agreements and most labour market laws. Swedish working time regulations impose a maximum of 40 working hours per week and 200 hours of overtime per year and allow for a minimum of 5 weeks vacations. However, these regulations are only restrictions as long as the bargaining parties do not agree otherwise. The Swedish working time act, as well as most other Swedish labour market laws, can be modified partly or entirely in favour of either of the labour market parties (with few exceptions, see Bylund and Viklund, 1992) in agreements between the parties.[2]

Until the middle of the 1980's Sweden had a three-tiered bargaining system with central agreements for blue-collar workers struck at the national level, followed by bargaining at the industry and plant levels. The central agreements were struck between the Swedish Employers' Confederation (SAF)[3] and the Swedish Trade Union Confederation (LO).[4] These organisations are confederations of industry-wide unions and employer organisations. While noting that central agreements no longer are an important part of the labour market institutions it should be emphasised that most labour market laws still have essentially the same status as the central agreements had previously.

Formally, the central agreements were not binding unless realised at the industry level, but the organisations agreed to work for the implementation of the central agreements.[5] Negotiations at the industry (and plant) level often take place under "no-agreement" clauses stating that the agreement from the higher level of bargaining (or equivalently, the laws) should take effect if no other agreement can be reached.

Finally, bargaining takes place at the plant level under "peace obligation" meaning that strikes and lockouts are banned. However, as pointed out by Nilsson (1993), conflicts that occasionally do arise may be quite costly to the firms. The plant-level agreements cover all workers at the plant, regardless of whether or not the workers are union members.

The amount of freedom at the plant level to make agreements that contradicts the industry level contracts varies between industries. An example of a flexible contract is the engineering industry contract ("Verkstadsavtalet"), which is the largest industry contract and covers almost half of the data used in this paper. The first section in the paragraph on working time issues states:

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[2] This possibility to renegotiate most labour market laws has been somewhat circumvented by the implementation of EU regulations. However these changes took place after the period that is to be studied in this paper and most of the restrictions set up by the EU regulations are not binding in practice, see SOU (1995).
[3] The name was changed to "The Swedish Confederation of Enterprise" in 2001.
[4] The institutional description in the remainder of this section is based on Nilsson (1993) unless otherwise stated.
[5] Indeed, reading the industry level contracts shows that the working time reduction that is studied in this paper was fully implemented at the industry level.

> *This paragraph replaces the working time act in all aspects.*
> Verkstadsavtalet 1989-90, page 17.

However, many paragraphs in the contract (e.g. a 40 hours workweek for daytime workers) follow the law closely. This contract also admits a large amount of freedom to the local parties to agree upon working time issues, which is illustrated by the following quote:

> *Differences in amounts and timing of the hours-of-work during different parts of the year shall be possible. Through an increased use of different working time schedules, e.g. with variations in amounts and timing of the hours-of-work, improved possibilities will be achieved to fit the schedules in accordance with the interests of the plants as well as the workers*.
> Verkstadsavtalet 1989-90, page 18.

The relationship between central agreements and actual outcomes is unclear *a priori* due to the multi-layered bargaining structure and the presence of "no-agreement" clauses for the local level bargaining parties. The importance of central agreements for actual hours has not previously been studied empirically and the only Swedish study of the impact of legislated working time reductions is on macro data (Jacobson and Ohlsson, 2000). However, studies on the importance of central agreements on wages show that approximately half of the wage increases in the mining and manufacturing industries were due to "wage drift" at the plant level.[6] The empirical part of this paper investigates whether or not there is a "working-time drift" corresponding to the wage drift.

A previous study on German data by Hunt (1999) arrives at the conclusion that actual hours follow standard hours closely. However, her paper studies the effect of self-reported standard hours that differs from the contractual standard hours. This difference between contractual standard hours and self-reported standard hours can be viewed as an indication of "working-time drift" if the individuals report the length of the standard workweek as agreed upon locally. To focus on the effects of the relevant policy tool, i.e. central agreements or laws, this paper studies the direct effect of central agreements on actual hours worked in Sweden.

[6] See Hibbs and Locking (1996) and Nilsson (1993).

2.2 The working time reduction

This section describes the working time reduction that is studied in the empirical section. The reduction was targeted at one form of shift workers ("2-shift" workers) in the Swedish manufacturing and mining industries. There are four major shift form categories for blue-collar workers in Sweden: daytime, 2-shift, discontinuous 3-shift and continuous 3-shift.[7] 2-shift workers work Mondays to Fridays and alternate between morning shifts (e.g. 5:30 a.m. – 2:00 p.m. with a 30 minutes break) and afternoon shifts (e.g. 2:00 p.m. – 10:30 p.m. with a 30 minutes break).[8] Discontinuous 3-shift workers work Mondays to Fridays on schedules that ensure 24 hours production during the workweek. Continuous 3-shift workers have schedules that allow for continuous production 24 hours per day, 7 days per week, with the exception of a few specific holidays.

Figure 1 shows the development over time of standard working hours, as determined in central agreements, for the different shift forms. Between 1983 and 1988 there was a gradual reduction in standard working time for 2-shift workers from 40 to 38 hours per week. The reduction was the result of a series of central agreements between the Swedish Employers Confederation (SAF) and the Swedish Trade Union Confederation (LO).[9] These agreements were implemented at the industry level either as a reduction of working hours on a weekly basis or with time off in lieu.[10] This paper studies how the actual working time for 2-shift workers changed during this time compared to the actual working time of other blue-collar workers.

As in any study of a legislated or negotiated policy reform a natural question to ask is whether the standard hours' reduction may have been endogenous to a change in actual hours that would have taken place anyhow. This risk is minimised by studying a reduction that is determined at the highest possible bargaining level. Also, the paper uses other workers within the same industry to control for spurious industry specific time effects. And, given the small effect found in the empirical part of the paper, endogeneity is not likely to be a real problem.

[7] Anxo and Sterner (1995) provide a description of the use of shift work in Sweden from 1968 to 1990.

[8] The examples are from the default working schemes in the engineering industry contract (Verkstadsavtalet) 1989-90, which is by far the largest contract in Swedish manufacturing.

[9] LO (1988).

[10] Based on readings of industry level contracts.

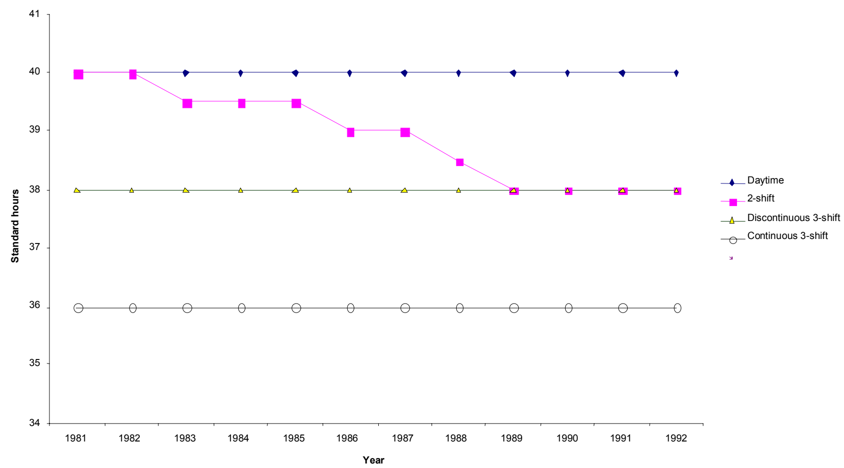

Figure 1. Standard hours by shift form 1981-1992

3 Data and empirical strategy

3.1 Data

Individual level panel data that were collected from the payroll records of private sector firms by the Swedish Employers Confederation (SAF) are used. Data cover earnings and working hours for the *second quarter* each year on all privately employed blue-collar workers covered by the central agreements in Sweden. The paper uses data from 1981 to 1992. The motivation for this time frame is a labour market conflict in the second quarter of 1980 and a change in the data collection procedure in 1993. Most 2-shift workers are employed in the mining and manufacturing sectors and data from other sectors are not used due to reasons described in *Section 3.3.2*. The data set has not been widely used for microeconomic research in the past but it is the base of Statistics Sweden's aggregate data on working hours and wages.[11] It should be noted that the measure of actual hours used in this paper refers to paid hours; unpaid hours are not re-

[11] One example of a study that uses micro data from the same database is Petersen et al (1996).

corded, but this is not likely to be a problem since only blue-collar workers are studied.

The working time data are decomposed into straight-time hours, piece-rate hours and overtime hours. The dependent variable in the empirical analysis is "actual hours", defined as the total amount of remunerated working time and thus including straight-time hours and piece-rate hours as well as overtime hours. The motivation for this choice of dependent variable is that straight-time hours (or piece-rate hours) may be influenced by the amount of overtime worked since overtime hours may be compensated with time-off in lieu. The problem can be illustrated by the following hypothetical example: Suppose that a worker does work some constant amount of overtime each quarter. Before the working time reduction she is compensated with time-off, but after the reduction she receives financial compensation instead (either due to her own or the firm's choosing).[12] Thus, the impact of the standard hours reduction would be attenuated, even though the amounts of overtime worked remained unchanged. This example illustrates that it is important to study actual hours, rather than overtime, if we are interested in how a reduction in standard hours affects the total number of hours actually worked.

The earnings data are reported in several parts such as total earnings, overtime premium and shift compensation. The data set further contains information on industry level contract, municipality and size of the firm as well as the workplace. The firms cannot be identified but individuals can be followed over time. Individual characteristics are not recorded except for age and gender. Standard hours from central agreements are assigned to the observations according to their shift form (see *Figure 1*).

Table 1 shows the variables used in the paper. The size variables are categorical, taking 9 different values for the size of the firm and 10 values for the size of the workplace. A workplace is defined as workers covered by the same agreement within the same firm.

[12] As an example, the engineering contract stipulates that the firm has the right to determine the amount of overtime an individual is working while the workers have the right to choose between financial compensation and time off. Who makes these choices in practice is however an open question, note e.g. that the firm can influence the workers choices by determining which individual that actually will work overtime.

Table 1. The used variables and their sources.

Source	Variables			
	Working time	Wages	Firms	Individuals
Swedish Employers Confederation (SAF)	Overtime hours	Total earnings	Industry contract	Fixed effect indicator
	Actual hours (= straight-time hours + piece-rate hours + overtime)	Shift compensation	Size of firm (9 dummies)	Age
	Scheduled weekly hours		Size of workplace (10 dummies)	Gender
	Shift form		Municipality (289)	
Central agreements	Standard hours by shift form			

Only workers aged 25-55 during the full sample period (i.e. workers aged 25-44 in 1981) are studied to minimise potential problems with differences in age-effects and retirement patterns between shift forms. The first part of *Table 2* shows descriptive statistics for the raw data set. The columns show statistics for workers during 1981-82 and 1989-92. The reason is that the empirical analysis focuses on these two time periods. Only very obvious outliers have been removed from the sample used for the tree first columns of the table.[13] It can be deduced from the table that the average actual overtime premium was between 58 % and 69 % of the hourly wage and that between 41 % and 66 % of workers work some overtime. Total overtime use was between 1.7 % and 3.2 % of actual hours worked.

[13] The main restrictions are the exclusion of observations with zero or more than 900 actual working hours during the quarter. Observations with nominal hourly earnings below 20 SEK and above 100 SEK in 1981 are dropped. These numbers are increased by 7.5 % (the estimated time trend in the sample) each year. The number of observations dropped by this procedure is very small.

Table 2. Descriptive statistics for daytime and 2-shift workers in manufacturing

	Raw data			Regression data		
	Day and 2-shift 1981-82	Day 1989-92	2-shift 1989-92	Day and 2-shift 1981-82	Day 1989-92	2-shift 1989-92
Number of Observations	359,459	454,991	92,963	142,930	198,962	42,484
Number of individuals	201,847	159,187	37,429	81,371	69,511	16,838
Fraction male workers	0.78	0.74	0.74	0.85	0.86	0.83
Age	34.2 (5.71)	43.5 (5.74)	43.1 (5.67)	34.8 (5.7)	43.8 (5.7)	43.6 (5.7)
Standard hours (per week)	40	40	38	40	40	38
Quarterly actual hours (including overtime)	382.9 (122.6)	399.8 (128.6)	391.9 (123.8)	434.2 (59.3)	444.8 (63.0)	433.3 (63.0)
Fraction of actual hours due to overtime (OT)	0.017 (0.036)	0.026 (0.043)	0.032 (0.044)	0.017 (0.031)	0.025 (0.037)	0.033 (0.039)
Fraction of obs. with OT > 0	0.41	0.53	0.66	0.46	0.60	0.73
Fraction of actual hours due to OT if OT > 0	0.042 (0.046)	0.048 (0.048)	0.049 (0.046)	0.036 (0.036)	0.042 (0.039)	0.045 (0.040)
Straight-time hourly wage (SEK)	37.0 (4.5)	71.6 (11.6)	72.5 (11.3)	37.6 (4.0)	73.2 (10.2)	73.7 (10.0)
Hourly wage including overtime premium (SEK)	37.4 (4.4)	72.7 (11.5)	74.1 (11.2)	38.0 (4.0)	74.3 (10.1)	75.4 (9.9)
Hourly earnings (SEK)	38.3 (5.0)	73.5 (11.9)	81.9 (13.6)	38.9 (4.6)	74.9 (10.4)	83.3 (12.3)
Quarterly earnings (SEK)	14,701 (5,127)	29,358 (10,360)	32,002 (10,941)	16,878 (3,019)	33,272 (6,314)	35,965 (6,799)
OT-premium (share of earnings)	0.010 (0.025)	0.015 (0.027)	0.020 (0.030)	0.010 (0.021)	0.014 (0.023)	0.020 (0.026)
Hourly OT-premium if OT > 0 (% of straight-time wage)	0.59 (0.72)	0.58 (0.42)	0.69 (0.51)	0.59 (0.57)	0.58 (0.38)	0.69 (0.58)
Shift compensation share (SCS)	0.020 (0.046)	0.010 (0.034)	0.091 (0.056)	0.020 (0.044)	0.007 (0.027)	0.091 (0.052)
Fraction of observations with reported scheduled hours	0.53	0.51	0.61	0.56	0.55	0.64
Fraction of observations with scheduled hours >30 if reported	0.97	0.96	0.98	0.99	0.99	1.00
Scheduled hours if >30	39.9 (1.02)	39.8 (1.81)	39.0 (1.35)	39.9 (0.82)	39.9 (2.18)	39.1 (1.21)

Note: Day and 2-shift workers can only be separated after 1988. Standard deviations are in parentheses. *Depending on year and shift form.

Given that the data cover hours during a quarter each year it is possible to study the full impact on hours of work. All workers do not work full time; furthermore, people take sick leaves and are absent from work for other reasons such as parental leave. A large portion (about 50 % of the sample) of the workers also works overtime. This gives a distribution of quarterly actual hours that exhibits substantial variation. *Figure 2* describes the distribution of actual hours worked during the second quarters of 1989.[14] Overtime use and absenteeism may change over time for reason that are unrelated to the working time reductions. Thus, the identification strategy is to use daytime workers (and in some cases 3-shift workers) to control for such time effects.

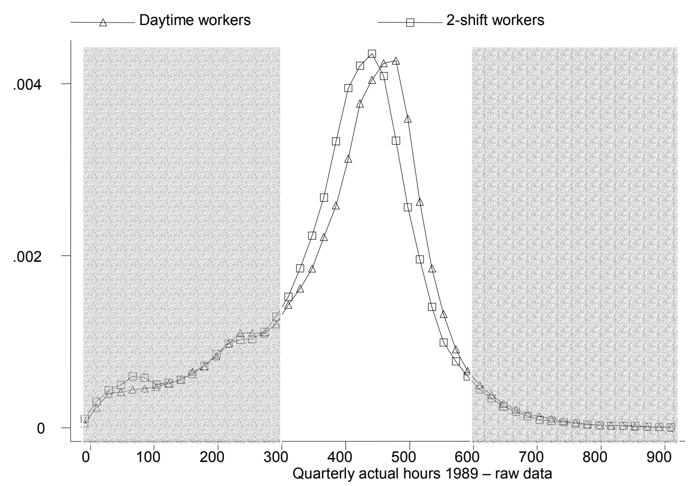

Figure 2. Hours worked by day and 2-shift workers in the 2nd quarter of 1989. The shaded areas are not used in the regressions.

[14] The reason for not pooling the observations from all years in the graph is that the mean working time differs between years thus generating a multi-peaked distribution that is difficult to interpret.

As explained in *Section 2* it is conceivable that there exist local agreements that are contradicting the central agreements. The focus of this paper is, however, on the direct effects of the central agreements on actual hours worked since central agreements (or laws) are the available policy tools. Nevertheless, *scheduled weekly hours* are used for studying the implementation of the central agreements. This variable contains the number of hours that a firm reported that the individual should work during a normal workweek. It was not mandatory for firms to report scheduled hours, the response rate was between 51 % and 61 % (see *Table 2* above). The fact that the information was collected as a survey with such a modest response rate should be kept in mind when interpreting the results based on this information in *Section 5.2*.

It is not possible to separate the differences in actual hours that are due to variations in weekly hours from variations in the number of days worked. Thus, there is a cost in precision since the lowest part of the distribution comes from sources that are unrelated to the working time reduction such as short-term contracts (e.g. seasonal work), labour market churning or labour market conflicts. Changes in the number of workers that work very few days during the quarter, have a very large impact on the estimated mean effect. Thus, only workers with more than 300 actual hours (seven and a half 40 hour weeks *or* five hours per working day for 12 weeks) during the quarter are included in the sample. However, as can be seen in *Section 5.1* there is little to suggest that the effect of the working time reduction should be larger for workers in the lower end of the distribution. Comparing those results with results from quantile regressions on the full distribution (displayed in the appendix) suggest that the median effect was quite insensitive to this restriction. This also shows that the effect may have been smaller (and in the extreme case, of opposite sign) in the lowermost part of the (full) distribution.

Some further restrictions have been applied on the data set used in the regressions. Observations with more than 600 hours are removed to reduce the influence of outliers, but the results are not sensitive to this restriction. For individuals with multiple observations in one year only the observation with the highest number of hours is used. Dropping these individuals or adding the actual hours to the observation with the highest number of hours did not change the results. Observations in industries that employed less than ten 2-shift workers after the reduction as well as industries with less than 100 observations in total are dropped to reduce the number of industry dummies. Individuals observed only before or after the reduction are dropped. *Table 3* shows the number of observations dropped at each stage in this procedure. Only the observations from before (1981-82) and after (1989-92) the reduction have been included in the table since the regressions only use observations for these years for reasons described in *Section 3.2*. The table shows that the restriction that all workers should be observed both before and after the reduction lead to a substantial reduction in the sample size. This restriction is not necessary but these observations would not contribute to the identification since the effect of the reduction is identified from the change in actual hours between the two time periods.

Table 3. The number of observations remaining after applying restrictions on the sample

	1989-92		All used years (1981-82 and 1989-92)	
Restriction	Day	2-shift	Day and 2-shift	All (including 3-shift)
Manufacturing & mining	454,991	92,963	885,982	977,876
Largest number of hours/year and individual	435,896	89,246	875,236	964,641
More than 300 hours worked in the 2nd quarter	353,207	73,461	707,436	783,455
Large agreements	338,001	73,386	661,063	732,155
Individual observed before and after the reduction	198,962 [69,511]	42,484 [16,838]	384,381 [81,383]	437,921 [92,046]
Reported scheduled hours >30	75,274 [27,703]	15,665 [6,522]	143,630 [32,327]	164,390 [36,631]

Note: The two bottom sets describes the number of observations in the data sets used in the paper, the number of individuals are in brackets. The data sets that only contain observations with reported scheduled hours have been constructed by first dropping observations without (or with 30 or less) reported scheduled hours and than applying the other restrictions.

The second part of *Table 2* above shows descriptive statistics for the main data set that is used in this paper. The differences between the raw data and the applied data set are small. The main differences are that the fraction of male workers and the average of actual hours are increased by the imposed restrictions. *Figure 3* plots the distribution of actual hours in 1989 for daytime and two shift workers in the applied data set. The shape of the distribution as well as the difference between shift forms is very similar to the raw data (see *Figure 2*) which should be reassuring.

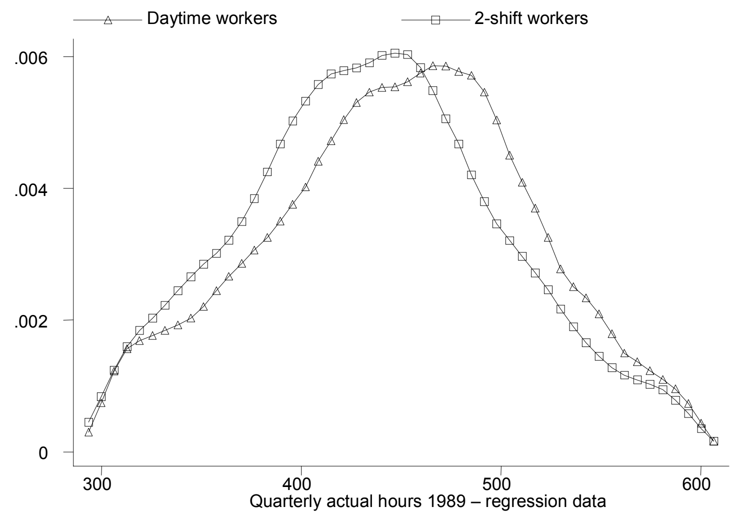

Figure 3. Hours worked by day and 2-shift workers in the 2nd quarter of 1989 for the sample used is the regressions.

3.2 Empirical set-up

Daytime workers and 2-shift workers were not separated in the data set before 1988. This is a problem since standard hours are assigned to workers based on their shift form. The solution is to limit the data-use to observations from the years before (1981-82) and after (1989-92) the gradual reduction. This is convenient since standard hours were the same (40 hours per week) for both daytime and 2-shift workers in 1981 and 1982 and the shift form is known for the other used years.[15]

[15] There are additional problems with the intermediate years such as variations in the timing of the industry level implementations that are difficult to link to the data and agreements that take effect during the second quarter.

This approach gives a clear-cut differences-in-differences experiment where the years before the reduction are compared to the years after, with other blue-collar workers as a control group. A further advantage of the approach is that the relatively long period between the years before and after the reduction should reduce the influence of potential transitionary dynamics. Note that a crucial assumption for this identification strategy is that daytime workers can be used to control for all changes over time that would have taken place in the absence of the working time reduction. As a test of this identifying assumption, 3-shift workers are used as an alternative control group in *Section 4.2*.

Consider a model for how actual hours for 2-shift workers changed relative to actual hours of daytime workers as a result of the standard hours reduction. Let α_i indicate an individual fixed effect and *Age* and a third order age-polynomial. The parameter β_1 measures the initial (time invariant) difference in working time between 2-shift workers and daytime workers, β_2 measures time effects interacted with sector dummies (the number of sector dummies differs between specifications, see *Section 4.1*). The parameter of interest is γ that denotes the actual hours' elasticity with respect to standard hours (h^s). Disregarding the error term, we may thus write the log of actual hours (h^a) as:

$$\ln h_{it}^{a} = \gamma \ln h_{it}^{s} + \beta_1 D_{it}^{2-shift} + D_{t}^{Year} D_{it}^{Sector} \beta_2 + Age_{it} \beta_3 + \alpha_i . \qquad (1)$$

The effect of the working time reduction as captured by γ is the change in actual hours for 2-shift workers relative to daytime workers within the same sector. The logarithmic specification is chosen with the quarterly data in mind, it should be the accurate specification if the impact of the working time reduction is proportional to the number of weeks worked. A linear specification would however yield nearly identical results.

3.3 Initial differences in actual hours

The fact that the data set does not contain information on whether workers are daytime or 2-shift workers before the working time reduction does not cause any problems for the standard hours variable (i.e. the variable of interest), as explained in *Section 3.2*. However, it does cause problems for measuring the initial difference in actual hours between the shift forms as captured by β_1 in equation (1). The estimates of the effect of the reform will be biased if the model restricts β_1 to zero and there were differences in actual hours before the reduction that are not captured by the other covariates.

3.3.1 Identification without estimating initial differences in actual hours

The panel structure of the data offers one solution to this problem. Individuals can be followed over time, which allows for the use of individual fixed effects. These fixed effects remove all the initial differences for workers that had the same shift form before and after the working time reduction.

The within transformation that removes the individual specific effects highlights under which conditions we can estimate the model without knowing the shift form of workers before the working time reduction. Subtracting individual means (denoted by bars) and using $\widetilde{X}$ to denote the deviation from individual means of the time-sector effects and the age-polynomial we may rewrite equation (1) as:

$$\ln h_{it}^{a} - \overline{\ln h_{i}^{a}} = \gamma(\ln h_{it}^{s} - \overline{\ln h_{i}^{s}}) + \beta_1 (D_{it}^{2-shift} - \overline{D_{i}^{2-shift}}) + \widetilde{X}_{it}\beta \,. \qquad (2)$$

We can thus identify the effect without knowing the shift form before the working time reduction if one of the following two assumptions is valid:

A1: $\beta_1 = 0$
A2: $D_{it}^{2-shift} = \overline{D_{i}^{2-shift}} \quad \forall i,t$.

Assumption A1 states that there are no differences in working time between shift forms that are independent of standard hours. Assumption A2 states that those who were 2-shift workers after the reduction were 2-shift workers before the reduction as well. The permanent difference is, under A2, removed as a part of the individual fixed effects and the change in actual working time due to the working time reduction for these workers is captured by γ . Under assumption A1 or A2 the within-transformation yields:

$$\ln h_{it}^{a} - \overline{\ln h_{i}^{a}} = \gamma(\ln h_{it}^{s} - \overline{\ln h_{i}^{s}}) + \widetilde{X}_{it}\beta \qquad (3)$$

which can be estimated without knowledge of the shift form before the reduction.

Estimation of (3) is equivalent to estimation of equation (1) under the restriction $\beta_1 = 0$, and we may estimate:

$$\ln h_{it}^{a} = \gamma \ln h_{it}^{s} + D_{t}^{Year} D_{it}^{Sector} \beta_2 + Age_{it} \beta_3 + \alpha_i \,. \qquad (4)$$

Thus, as long as very few workers switched shift forms during the intermediate years of the reduction we may estimate the model *as if* there was no difference in actual hours before the reduction in standard hours. This is possible even if there were real differences before the reduction.

3.3.2 Identification by using a proxy for initial differences in actual hours

The identification discussed above is, however, problematic if there were substantial differences in hours between shift forms before the reduction *and* many workers did change their shift forms during the intermediate years. The applied solution is to construct a proxy for the shift form of the worker. This proxy is constructed using the fraction of shift compensation to total earnings. Henceforth this fraction is referred to as the *shift compensation share* (SCS). This variable, unfortunately, also includes the premium given to daytime workers that perform work outside the normal working hours (e.g. the engineering industry contract for 1989-90 stipulates that a premium should be paid for work performed after 4:30 p.m.). This will generate measurement errors in the proxy. The accuracy of the proxy can be evaluated for the years 1989-92 when the true definition is available.

The incidence of 2-shift work is much larger in manufacturing and mining than in other sectors. In other sectors it is more common that daytime workers have shift compensation without formally being 2-shift workers. Thus, this study focuses on manufacturing and mining to minimise the problems with measurement errors in the proxy for initial differences.

The proxy is constructed by choosing a cut-off level where all workers with a SCS over the cut-off are classified as 2-shift workers. The chosen cut-off level is at 7 % since this generates a (local) maximum of the fraction of actual 2-shift workers among the workers that are classified as such by the proxy. *Table 4* shows the precision of the proxy during 1989-92, the years during which the procedure can be evaluated. The results in the table show that the accuracy of the proxy is quite high, the proxy is accurate for over 80 % of the workers that are recorded as 2-shift workers.

Table 4. Accuracy of the proxy

		Proxy		
		Day	2-shift	**All**
	Day	79.8 %	2.8 %	**82.6 %**
True	2-shift	5.5 %	11.9 %	**17.4 %**
	All	**85.3 %**	**14.7 %**	**100 %**
Accuracy of the proxy*		93.6 %	80.8 %	91.7 %

Note: Results from the 1989-92 data set used to define manufacturing and mining workers as 2-shift workers if they have a shift compensation share (SCS) of more than 7 %. "True" definitions refer to the original definitions in the data set. * The "accuracy" numbers are calculated as the number of correctly classified workers divided by the total number of workers classified in the category by the proxy, i.e., for Daytime workers 79.8/85.3 for 2-shift workers 11.9/14.7 and for All workers (79.8+11.9)/100.

The expected attenuation bias (due to measurement errors) of the estimate of the initial difference between daytime and 2-shift workers ($\hat{\beta}$) in the absence of other covariates is $\frac{\hat{\beta}}{\beta^{TRUE}} = 1-(\nu+\eta)$.[16] The parameters ν and η denotes the fractions of workers erroneously classified as 2-shift workers and daytime workers. By using the numbers in *Table 4* we get $\nu = 0.192$ and $\eta = 0.064$. Thus, one would expect the estimates of the initial difference using the true definition to be 1.3 times the estimate based on the proxy. In principle it is possible to correct for the bias this generates on the estimated effect of standard hours (γ) by using the procedure in Aigner (1973) but the double fixed effects (individuals and sector-years) model makes the implementation difficult.

While noting that the estimates of the initial difference between shift forms will be biased to zero it should be noted that the proxy is quite good. Furthermore, the results presented later in the paper (*Section 4*) show that the estimates of the initial difference in actual hours are quite small. Since the models include fixed effects one may suspect that the measurement-error problems for β_1

[16] See Aigner (1973).

are increased. However, while the fixed effects may amplify the attenuation of the estimate of initial differences, they are also reducing the *impact* of these errors on the variable of interest. This is due to the individuals that do not change their shift form as explained in the previous subsection. Using the proxy to study the persistence of the shift forms shows that 81 % of the workers had the same shift form before and after the working time reduction. Note also that this probably is a slight underestimate of the true persistence due to the measurement errors in the proxy.

Importantly, it is shown in *Section 4* that the estimates of the effect of the reduction are unaffected if the proxy is included or excluded. This shows that the differences in actual hours before the reduction conditional on the individual fixed effects were small, suggesting that the measurement error problems have a relatively minor impact on the variable of interest.

To sum up this section, *Table 5* describes the three main variables studied in the paper, standard hours, scheduled weekly hours (for the implementation) and proxy for initial differences between shift forms.

Table 5. Main variables of interest

Variable	Effect	Source	Comments
Standard hours	The direct effect of the agreement	Shift form code and central agreements	Unproblematic for the used years (1981-82 and 1989-92).
2-shift proxy	Initial differences between shift forms	Shift compensation share (SCS)	Measurement errors due to false positives and negatives, (evaluated for 1989-92 in *Table 4*).
Scheduled hours	The effect of implementation	Scheduled hours as reported	Non-compulsory question: Approximately 55% response rate.

4 The average effect

The purpose of this section is to investigate how total actual (paid) hours changed for the average 2-shift worker when standard hours were reduced The effect is unknown *a priori* due to the flexible bargaining institutions described in *Section 2*.

4.1 Effects relative to daytime workers

The models that are estimated in this section are motivated in more detail in the previous section. As explained in that section we may estimate a model without controlling for the shift form before the reduction in scheduled hours as long as very few workers changed their shift forms. Restating equation (4) with an added error term we get

$$\ln h_{it}^{a} = \gamma \ln h_{it}^{s} + D_{t}^{Year} D_{it}^{Sector} \beta_2 + Age_{it} \beta_3 + \alpha_i + \varepsilon_{it} . \qquad (5)$$

Results from the estimation of this model are presented in *Table 6*. The definition of *Sector* in this table is based on union contract. This should ensure that differences in contractual overtime compensation are controlled for since the effect is measured relative to other workers within the same union contract. The result indicates that actual hours were reduced by only 40 % of the reduction in standard hours. The estimate is different from both zero and one at the one-percent level of statistical significance.[17]

To verify that the relatively low estimate is not the effect of large initial differences and large turnover between shift forms we may use the proxy described in *Section 3.3.2* and estimate equation (1) directly. By adding an error term to equation (1) we get

$$\ln h_{it}^{a} = \gamma \ln h_{it}^{s} + \beta_1 D_{it}^{2-shift} + D_{t}^{Year} D_{it}^{Sector} \beta_2 + Age_{it} \beta_3 + \alpha_i + \varepsilon_{it} . \qquad (6)$$

The estimates of the initial difference (β_1) as well as the effect of the standard hours reduction (γ) based on equation (6) are presented in the second column of *Table 6*. The results show that the initial difference in working time

[17] Estimates (not reported) on samples where observations with less than 300 actual hours were included in the regressions yield somewhat smaller effects.

was relatively small (in the order of 1 %). Importantly, the results show that the estimates of the impact of the working time reduction are largely unaffected by the inclusion of this proxy (it is reduced to 34 %). An additional model based on equation (6) but without the individual fixed effects is estimated for robustness. The changes in results (presented in the third column of *Table 6*) are as expected (see *Section 3.3.2*). Both the estimated initial difference and the standard hours' elasticity are slightly larger in absolute values, but the differences in estimates are not of economic significance.

Table 6. Elasticities of actual hours with respect to standard hours

Control group	Estimated parameter	Individual fixed effects		No individual fixed effects
		Not controlling for initial differences	Controlling for initial differences	Controlling for initial differences
Daytime workers	Standard hours (γ)	0.397 (0.021)	0.343 (0.022)	0.418 (0.019)
	Initial difference (β_1)	--	-0.012 (0.001)	-0.016 (0.001)
Number of observations		384,381	384,381	384,381
Number of individual fixed effects		81,383	81,383	--
Number of time-sector effects		320	320	320
Degrees of freedom		302,673	302,672	384,055

Note: The dependent variable is the log of actual hours worked during the second quarter each year. Sample period is 1981-82 and 1989-92. The standard hours estimates are elasticities. The initial difference estimates measure the (constant) effect of being a 2-shift worker at time *t* and are based on the proxy described in *Section 3.3.2.* All regressions include 320 year-contract interaction dummies and an age cube. Huber-White standard errors are in parentheses. The first column is based on equation (5), the second on equation (6) and the third on equation (6) but without the individual fixed effects.

The advantage of studying a partial working time reduction that affects some workers within the same contract is that one may use other workers experiencing quite similar conditions as a control group. A potential worry may however be that daytime workers within firms that employ many 2-shift workers are demanding compensation in terms of reduced hours as the result of the reduction for 2-shift workers. This would imply that the estimated effect of this working time reduction is smaller than the effect would be if all workers were covered by the reduction (essentially this is a question of a contaminated control group).

We can check for this potential problem by varying the definition of *Sector* in equation (6). We should see smaller effects the finer the definition of a sector if daytime workers within firms with 2-shift workers had their actual hours reduced as a result of the standard hours reduction for 2-shift workers. *Table 7* shows estimates of equation (6) with three definitions of a *Sector*: With only one sector that includes all workers (the first column). With one sector dummy for each union contract (the second column, a replication from *Table 6*). With one sector dummy for each unique combination of union contract, municipality, size of firm *and* size of workplace (the third column). The estimates are stable when the definition of *Sector* is changed, and importantly, there are no systematic differences along the lines described above. This suggests that contamination, i.e. that daytime workers within firms or contracts with many 2-shift workers were affected by the reduction, should not be a major concern.

Table 7. Elasticities of actual hours with respect to standard hours

Control group	Estimated parameter	Year effects	Contract and year interactions	Contract, size of firm, size of workplace, municipality and year interactions
Daytime workers (equation 6)	Standard hours (γ)	0.284	0.343	0.302
		(0.021)	(0.022)	(0.024)
	Initial difference (β_1)	-0.013	-0.012	-0.014
		(0.001)	(0.001)	(0.001)
Number of observations		384,381	384,381	384,381
Number of individual fixed effects		81,383	81,383	81,383
Number of time-sector effects		6	320	25,253
Degrees of freedom		302,986	302,672	277,739

Note: The dependent variable is the log of actual hours worked during the second quarter each year. Sample period is 1981-82 and 1989-92. The standard hours estimates are elasticities. The initial difference estimates measure the (constant) effect of being a 2-shift worker at time *t* and are based on the proxy described in *Section 3.3.2.* All regressions include individual specific fixed effects as well as an age cube. The time effects are year and sector interactions, where the definition of the sector varies between columns. The first column has raw year effects, the second year-contract interactions (a replication from *Table 6*) and the third column year contract, municipality, size-of-firm and size-of-workplace interactions. Huber-White standard errors are in parentheses.

4.2 Effects relative to other shift workers

The results presented so far crucially hinges on the assumption that daytime workers are a valid control group. This is necessary for the estimates to be robust to spurious changes between years in hours worked, e.g. due to the business cycle effects or changes in the sick-leave propensity. An additional concern may be that the ageing of the population taking place during the implementation of the working time reduction affects shift workers and daytime workers differently. This section therefore uses 3-shift workers as an alternative control group to test the sensitivity of the results.

All workers are included in the estimation even though the effects are measured relative to 3-shift workers.[18] The effect of the working time reduction is measured relative to 3-shift workers by including year effects (by sector) that are separated between daytime workers and (all) shift workers. This is accomplished by interaction of the dummy:

$$D_{it}^{s} \equiv D_{it}^{2-shift} + D_{it}^{Disc.3-shift} + D_{it}^{Cont.3-shift} ,$$

that equals one for all shift workers and zero for daytime workers with the year effects:

$$\ln h_{it}^{a} = \gamma \ln h_{it}^{s} + D_{it}^{s}\left\{\beta_1 D_{it}^{2-shift} + \lambda D_{it}^{Disc.3-shift} + D_{t}^{Year} D_{it}^{Sector}\beta_2\right\} + Age_{it}\beta_3 + \alpha_i + \varepsilon_{it} . \quad (7)$$

Table 8. Elasticities of actual hours with respect to standard hours, comparing to 3-shift workers.

Estimated parameter	Year effects	Contract and year interactions	Contract, size of firm, size of workplace, municipality and year interactions
Standard hours (γ)	0.349 (0.022)	0.328 (0.024)	0.303 (0.027)
Number of observations	437,921	437,921	437,921
Number of individual fixed effects	92,046	92,046	92,046
Number of time-sector effects	12	627	31,260
Degrees of freedom	345,855	345,240	314,607

Note: The dependent variable is the log of actual hours worked during the second quarter each year. Sample period is 1981-82 and 1989-92. The standard hours estimates are elasticities. All regressions are based on equation (7) and include individual specific fixed effects, dummies that control for permanent differences between each shift form and an age cube. The time effects are year and sector interactions (interacted with a dummy for day or shift work), where the definition of the sector varies between columns. The first column has raw year effects, the second year-contract interactions and the third column year contract, municipality, size-of-firm and size-of-workplace interactions. Huber-White standard errors are in parentheses.

[18] The reason is that this increases the number of 3-shift workers that can be included in the estimation since we may use 3-shift workers that are observed as daytime workers before (or after) the reduction and as 3-shift workers after (before) the reduction. Including the daytime workers help to identify the individual fixed effects of 3-shift workers that are observed as daytime workers during some years.

Table 8 displays standard hours estimates based on equation (7). The estimates are quite close to the estimates relative to daytime workers. This is true regardless of how a sector is defined. One minor difference to the previous subsection is that the estimates are larger the wider the definition of a sector. This could potentially imply that the 3-shift workers are a contaminated control group since 2-shift workers experienced a larger reduction relative to the average 3-shift worker than relative to 3-shift workers within a more narrowly defined unit. The differences in estimates are however reasonably small and statistically insignificant, suggesting that this problem even if it exists should not be of great importance.

5 Heterogeneous effects

The average effect of the working time reduction on actual hours is estimated to be quite small. The elasticity of actual hours with respect to standard hours is in the interval 0.3 to 0.4. This differs substantially from the estimates from the German working time reductions studied in Hunt (1999) that range from 0.85 to 1. Two different aspects of the implementation are studied in this section to give further insights as to why the reduction had such a small mean impact. The first part uses quantile regression techniques to study how the effect varies over the distribution of actual hours and the second part uses data on scheduled weekly hours as a measure of the implementation.

5.1 Distribution effects

A standard result in the theoretical work sharing literature is that actual hours may increase if standard hours are reduced through increased overtime in firms that already used overtime before the reduction (e.g. due to large fixed costs).[19] This somewhat counterintuitive result is based on the fact that a shorter standard workweek increases the wage cost per worker if overtime were used before the reduction since an overtime premium has to be paid for a larger fraction of the total working time. This increases the marginal cost of a worker at a given number of actual hours worked. The marginal cost of hours is, however,

[19] See e.g. Calmfors and Hoel (1988). The argument crucially hinges on an assumption of a constant hourly overtime premium.

unchanged (it equals the hourly wage plus the overtime premium). Thus firms will choose to substitute from workers to hours. This mechanism requires that firms would have used overtime in the absence of a working time reduction.

The theoretical result predicts that actual hours should decrease for workers in the lower end of the distribution and increase in the upper end, which makes the impact of the working time reduction on the *distribution* of hours an interesting topic. The effects on the distribution of actual hours are studied with quantile regression techniques.[20] The model used for this purpose differs somewhat from the ones used in the earlier sections. The reason is that fixed effects cannot be combined with quantile regressions. Since the model is estimated without individual fixed effects it may be viewed as estimating the response from the firms side (as is the theory sketched above) rather than the individuals side and the age polynomial is thus not included in the regressions either. The estimated (differences-in-differences) model is:

$$\ln h_{it}^{a} = c + \gamma \ln h_{it}^{s} + \beta_1 D_{it}^{2-shift} + D_t^{Year} \beta_2 + \varepsilon_{it} \,. \tag{8}$$

For a test of the robustness of these results, the model is estimated in deviations from individual means as well. However, it is important to stress that this is not equivalent to including a dummy for each individual; rather the estimates should be interpreted as the effects on the distribution of deviations from individual means.

Quantile effects are estimated for the 20th, 40th, 50th (median), 60th and 80th percentile. The estimates (presented in *Table 9*) should be interpreted as the effect of the working time reduction on the working time of the 20th percentile worker etc. The results do not give any support for the notion that the effect should be of opposite sign for workers in the upper end of the distribution, neither in the basic nor in the individual-mean differenced data. On the contrary, the effect seems to have been roughly homogeneous over the quantiles and the effect is not larger than 50 % in any part of the distribution. Even though both data sets do give some evidence of a slightly smaller effect for the 80th percentile than for the median, which is consistent with the prediction that the upper end should be less affected by the reduction, it cannot explain the small mean effect. This suggests that, if the small effect is due to an increase in overtime, it

[20] A previous study on Canadian cross sectional data, Friesen (2002), studied the effect on the hours-distribution using a hazard function framework.

has to be an increase in overtime for most workers, not only the workers at the upper end of the distribution.

Table 9. Quantile elasticities of actual hours with respect to standard hours

Individual means subtracted?	Estimated parameter	Percentiles				
		20th	40th	50th (Median)	60th	80th
No	Standard hours (γ)	0.303 (0.033)	0.423 (0.024)	0.445 (0.018)	0.471 (0.021)	0.396 (0.025)
		[0.000]	[0.317]	--	[0.152]	[0.067]
	Initial difference (β_1)	-0.021 (0.002)	-0.019 (0.001)	-0.018 (0.001)	-0.016 (0.001)	-0.008 (0.001)
		[0.058]	[0.269]	--	[0.024]	[0.000]
Yes	Standard hours (γ)	0.349 (0.030)	0.351 (0.022)	0.360 (0.019)	0.360 (0.020)	0.278 (0.026)
		[0.696]	[0.526]	--	[0.937]	[0.001]
	Initial difference (β_1)	-0.013 (0.002)	-0.014 (0.001)	-0.013 (0.001)	-0.012 (0.001)	-0.012 (0.001)
		[0.860]	[0.116]	--	[0.548]	[0.490]

Note: All estimates are based on equation (8) and include year dummies. Dependent variable is the log of actual hours. Simultaneously bootstrapped standard errors (100 repetitions) are in parentheses and p-values from F-tests of equality with the median effects are in brackets.

The Appendix shows results based on the full sample, without the restriction to only use observations with more than 300 and less than 600 actual hours during the quarter. These results show that the median effect is in the same order as when the restrictions are applied. Furthermore, it is shown that the effect on the lower part of the distribution is negative unless the individual means have been subtracted, and that the effect on the uppermost part of the distribution is small when the individual means have been subtracted. However, none of these results indicate that the small average effect was due to an increase in overtime in the upper end of the distribution.

5.2 The local implementation

One reason for the discrepancy between the results presented in *Section 4* the results in Hunt (1999) may be that this paper studies the direct effect of a central agreement on actual hours whereas Hunt studied the effect of self-reported standard hours on actual hours. By using reported scheduled weekly hours (see *Section 3.1*), instrumented by standard hours, as the explanatory variable we can get an idea of how the estimates in this paper relates to Hunt (1999). The interpretation of the IV estimates is the effect on actual hours for the workers that changed their weekly scheduled hours due to the change in standard hours.

It was shown in *Table 2* that the response rate for scheduled hours was quite low, between 51 % and 61 % in the raw data. This may be of some concern and the lower panel of *Table 10* thus shows estimates of the mean effect (based on equations (5), (6) and (7)) using the sample with reported scheduled hours. The estimates are somewhat smaller than the effect when using the full sample. Even though the estimates are qualitatively similar, this should warrant some caution when interpreting the results below.

The models estimated in this section are identical to the models that were estimated in *Section 4*. All regressions include year effects interacted with contract dummies and an age cube. Denoting scheduled weekly hours by $h_{it}^{scheduled}$ and suppressing the fact that it is instrumented by standard hours we may write the model without controlling for initial differences as:

$$\ln h_{it}^{a} = \phi \ln h_{it}^{scheduled} + D_{t}^{Year} D_{it}^{Sector} \beta_2 + Age_{it} \beta_3 + \alpha_i + \varepsilon_i , \qquad (9)$$

and the model that includes the proxy to control for initial differences as:

$$\ln h_{it}^{a} = \phi \ln h_{it}^{scheduled} + \beta_1 D_{it}^{2-shift} + D_{t}^{Year} D_{it}^{Sector} \beta_2 + Age_{it} \beta_3 + \alpha_i + \varepsilon_i \quad (10)$$

Results are presented in the upper panel of *Table 10*. The estimates of the effect of the reduction are 0.88 and 0.95. This is reasonably close to (and not significantly different from) one and thus in the range of the results in Hunt (1999). *Table 10* also shows estimates of the elasticity of actual hours with respect to scheduled hours with 3-shift workers as the control group:

$$\begin{aligned} \ln h_{it}^{a} = \phi \ln h_{it}^{scheduled} + D_{it}^{s} \left\{ \beta_1 D_{it}^{2-shift} + \lambda D_{it}^{Disc.3-shift} + D_{t}^{Year} D_{it}^{Sector} \beta_2 \right\} \\ + Age_{it} \beta_3 + \alpha_i + \varepsilon_{it} \end{aligned} \qquad (11)$$

These estimates are somewhat smaller than the estimates relative to daytime workers (0.81) but still not statistically different from one.

Table 10. Elasticities of actual hours with respect to scheduled and standard hours for workers with reported scheduled hours

Equations	Parameter	Relative to daytime workers: Not controlling for initial differences (eq. 9 and 5)	Relative to daytime workers: Controlling for initial differences (eq. 10 and 6)	Relative to 3-shift workers: Equations (11) and (7)
(9), (10) and (11)	Scheduled hours (ϕ) Instrument: Standard hours	0.953 (0.093)	0.883 (0.103)	0.807 (0.115)
	Initial difference (β_1)	--	-0.006 (0.002)	--
(5), (6) and (7)	Standard hours (γ)	0.343 (0.033)	0.294 (0.034)	0.271 (0.039)
	Initial difference (β_1)	--	-0.012 (0.002)	--
Number of observations		143,630	143,630	164,390
Number of individual fixed effects		32,327	32,327	36,631
Number of time-sector effects		231	231	456
Degrees of freedom		111,067	111,066	127,295

Note: The dependent variable is the log of actual hours worked during the second quarter each year. Sample period is 1981-82 and 1989-92. The standard hours and scheduled hours estimates are elasticities. All regressions include individual specific fixed effects and an age cube as well as year and union contract interaction dummies. The sample is restricted to individuals with reported scheduled hours that exceed 30 hours per week. All regressions include individual specific fixed effects and an age cube. The time effects are year and contract interaction dummies. The last column have separate time effects for day and all shift workers and dummies that control for permanent differences between each shift form (see equations 7 and 11). Huber-White standard errors are in parentheses.

The results suggests that the workers who did see a change in their scheduled weekly working hours also saw a change in their actual hours without much substitution to overtime. This indicates that one of the reasons why the average effect differs so radically from Hunts' (1999) results for Germany may be that the focus of this paper is on the direct effect on actual hours from standard hours as defined in central agreements. Hunt on the other hand studied the effect of standard hours through self-reported hours which may correspond more closely to the measure of scheduled hours used here. The results also suggest that one reason for the small average effect may be the fact that the scheduled workweek remained unchanged for parts of the workers. Qualitatively, this is in line with the French experience from the 1982 reduction; Crépon and Kramarz (2000) report that 20 % of workers did not reduce their hours from 40 to 39 hours. The proportion of non-compliers seems much higher for Sweden, which may be explained by the high degree of flexibility in the local level implementation of agreements in Sweden described in *Section 2*.

6 Conclusions

Working time reductions are currently discussed in Sweden as well as in other European countries such as France where the weekly working time recently was reduced to 35 hours. The motives for working time reductions in the public discussion varies over time and between countries; sometimes the policy is viewed as an instrument to reduce unemployment but it is also occasionally argued for as a means to increase labour supply or to generate positive welfare effects in general. A crucial assumption for the effectiveness of working time reductions as a policy tool, regardless of the objective, is that changes in standard hours has a real effect on actual hours worked. The relationship between standard and actual hours is particularly interesting in the Swedish case since the bargaining institutions are quite flexible in allowing for local solutions that differ from those stipulated in central agreements or laws.

This paper has studied the impact on actual hours worked of a partial working time reduction in the Swedish manufacturing industry. The reduction that covered one class of shift workers is particularly interesting since other workers within the same industries and firms are not covered by the reduction. Thus, it is possible to study how the workers that were covered by the reduction changed their hours worked relative to other workers employed under very similar conditions. The identification can be made even stronger by comparing to both daytime workers and other classes of shift workers that has an even more unconventional timing of their working hours than the covered workers. Individual level panel data from firms' payroll records is used to study the impact on actual hours, measured as total hours worked during the second quarter each year.

The results show that the impact of the reduction was much smaller than intended. Actual hours were reduced by only about 35 % of the reduction in standard hours. This result is robust to a number of specifications: with and without individual fixed effects, with different sets of covariates, and when measured relative to daytime workers or other shift workers.

Further evidence from quantile regressions shows that the effect was roughly homogeneous over the distribution of hours, possibly with a somewhat smaller effect for the highest percentiles. Thus, the small mean effect is not explained by the prediction from standard work sharing theory that reduced standard hours may *increase* the actual working time of the upper percentile workers.

To reconcile the results with a previous study on German data that found much larger effects (Hunt, 1999), a model that accounts for the local implementation is examined. This is done by using information on weekly scheduled hours voluntarily reported by the firms for slightly more than half of the workers. Since this information comes from a non-compulsory question with a modest response rate, it is possible that it is less accurate than the information from the payroll records. With this caveat in mind, the effect of locally determined scheduled hours, instrumented by standard hours, is estimated and it is shown that the effect is close to what was stipulated in the central agreements. This shows that one reason for the small mean effect may be the fact that scheduled hours remained unchanged for many workers. The fact that local scheduled hours may differ from centrally determined standard hours further suggests that it is important to study the direct effect of standard hours on actual hours to get an unbiased estimate of the impact of a working time reduction.

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Appendix: Quantile regressions on the complete distribution

Table A1. Quantile elasticities of actual hours with respect to standard hours: the complete distribution of working hours.

Individual means sub-tracted?	Estimated parameter	Percentiles				
		20th	40th	50th (Median)	60th	80th
No	Standard hours (γ)	-0.633 (0.060)	0.143 (0.030)	0.271 (0.023)	0.351 (0.020)	0.364 (0.024)
		[0.000]	[0.000]	--	[0.000]	[0.001]
	Initial difference (β_1)	-0.032 (0.003)	-0.032 (0.001)	-0.027 (0.001)	-0.023 (0.001)	-0.013 (0.001)
		[0.000]	[0.000]	--	[0.000]	[0.000]
Yes	Standard hours (γ)	0.204 (0.052)	0.311 (0.035)	0.311 (0.030)	0.267 (0.029)	0.088 (0.043)
		[0.009]	[0.988]	--	[0.003]	[0.000]
	Initial difference (β_1)	-0.002 (0.003)	-0.007 (0.002)	-0.007 (0.001)	-0.006 (0.002)	-0.003 (0.002)
		[0.055]	[0.620]	--	[0.461]	[0.022]

Note: All estimates are based on equation (8) and include year dummies. The sample is constructed analogously to the sample used in main text except for the restriction on hours worked during the second quarter. The number of observations is 503,111. Dependent variable is the log of actual hours. Simultaneously bootstrapped standard errors (100 repetitions) are in parentheses and p-values from F-tests of equality with the median effects are in brackets.

Essay II

Working hours, wages, and equilibrium unemployment [*]

1 Introduction

Unemployment in the European Union is considered too high by most observers. At the same time, an increasing number of workers wish to work fewer hours (OECD 1998). This has lead unions and policymakers in several European countries to push for working time reductions. Partly, this has been motivated by "work sharing", i.e., as a policy for reducing unemployment. An implicit assumption behind this proposal is often that a fixed number of working hours is demanded in the economy, and that more workers thus could find employment if hours per worker were reduced. Theoretical and empirical work by economists have lent little support to this idea. The main theoretical objection has been that total demand for labour services would fall due to substitution from labour to capital and reduced production (a recent survey is Kapteyn et al, 2000).

The work sharing proposal, as well as many of the objections made against it, is focused on an analysis of firms' labour demand, rather than on a model of equilibrium unemployment. This paper explicitly limits the analysis to effects on equilibrium unemployment from working time reductions. It is argued that a

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credible analysis of equilibrium effects has to be conducted in a model that is consistent with the stylised fact that the equilibrium unemployment rate is unaffected by technological change. This removes any short run effects from the model.

Using this argument to pin down the functional forms, a model is set up to derive conditions under which equilibrium unemployment is a function of working hours. The model shows that a general working time reduction would tend to lower the wage pressure and equilibrium unemployment since the cost of forgone leisure when working is lower the shorter the workweek is. This is true even if the working time is reduced below what is preferred by the individual workers. A counteracting effect is derived if firms have fixed costs for their employees. A shorter workweek increases the importance of fixed costs. This reduces the wage sensitivity of employment, causing increased wage demands and a rising equilibrium unemployment rate. Thus, the net effect of a working time reduction on equilibrium unemployment is ambiguous.

The paper argues that studying the wage response to a *partial* working time reduction will give important evidence of whether or not equilibrium unemployment would fall in response to a general working time reduction. It is shown that hourly wages of covered workers should fall when there is a reduction in working hours for a small number of workers unless there are fixed costs. Falling hourly wages in the response to a partial working time reduction is therefore consistent with work sharing as a feasible long run policy. Rising hourly wages, on the other hand, is evidence of increased wage demands (possibly due to fixed costs) that would tend to increase equilibrium unemployment if working time were reduced for all workers.

The empirical part of the paper studies a 5 % reduction in standard working hours, as defined in central agreements, for one class of shift workers in the Swedish manufacturing and mining industries. The results show that hourly wages were increased as a result of the working time reduction. The rise in hourly wages was sufficient to leave monthly earnings constant relative to other workers. *Essay I* in this thesis showed that the actual working time was reduced by only about 35 % of the reduction in standard hours. The evidence suggested that one reason for the small effect is that only some workers had their actual hours reduced. A study of differences in wage responses reveal that the rise in relative wages was most pronounced for the workers who had their actual hours reduced the most.

The conclusion of the paper is that the partial working time reduction lead to an increase in wage pressure, possibly due to fixed costs of employment. Such an increase in wage pressure would tend to raise equilibrium unemployment if a general working time reduction was implemented.

1.1 Previous studies

The major part of the theoretical literature on working hours' regulations was published in the 1980's when work sharing was frequently discussed as a labour market policy in continental Europe. Examples of models studying the effect of a working time reduction on labour demand are Hart (1987) and papers by Calmfors and Hoel (1988 and 1989) that deal with shift work and overtime. Booth and Ravallion (1993) studies the importance of fixed costs for the partial equilibrium effects on labour demand of a working time reduction. For a survey of bargaining models for working time, see Earl and Pencavel (1990). Theoretical studies of the effect of working time reductions on equilibrium unemployment are less common; some exceptions are Houpis (1993), Marimon and Zilibotti (2000) and Rocheteau (2002).

Empirical work on Swedish data includes Pencavel and Holmlund (1988) that study the industry level relationships between labour demand, hours and wages. Jacobson and Ohlsson (2000) study aggregate time series data and find an effect from legislated working time on actual hours, but also that employment and working hours are unrelated. Very little empirical work has been done on micro level data; for an international review see Hunt (1998). Examples are Crépon and Kramarz (2000) that study the effects on employment of the French 1982 working time reduction and Hunt (1999) that studies the effects of an industry level working time reduction on hours, wages, and employment in Germany. Hunt finds that hours were reduced by almost the predicted amount, that hourly wages rose to compensate for the loss in earnings and that employment fell.

1.2 Outline

The paper is structured as follows: *Section 2* discusses predictions from equilibrium theory for the effects of a working time reduction on unemployment. *Section 3* shows what we may learn about these effects from partial working time reductions. *Section 4* describes the working time reduction and the data used in the empirical section of this paper. *Section 5* presents empirical evidence. *Section 6* concludes.

2 Equilibrium theory

2.1 Lessons from the (absence of) effects of economic growth

The simple work sharing argument rests on the assumption that the total demand for working hours is fixed. The demand for labour services may, however, change with hours worked for several reasons, such as substitution of inputs from labour to capital, changes in total output, and skill match problems. A large part of the literature on work sharing discusses the importance of these effects in an attempt to determine whether labour demand would increase or decrease if the working time is reduced.

How labour demand is affected by working time reductions is important for employment effects in a single firm introducing a shorter workweek. It is also of interest for the welfare implications of a general working time reduction. This paper does, however, aim at evaluating work sharing as a long run policy. The focus should thus be on mechanisms that could affect equilibrium unemployment, which excludes labour demand effects.

A decomposition of the overall effect of a working time reduction on equilibrium unemployment (e_{uh}) into two parts makes this point clearer. The two parts are a direct effect at a given *productivity per worker* $\left(e_{uh}\big|_{F_N}\right)$ and an effect through the productivity per worker $\left(e_{uF_N} \cdot e_{F_N h}\right)$:

$$e_{uh} = e_{uh}\big|_{F_N} + e_{uF_N} \cdot e_{F_N h} . \qquad (1)$$

This decomposition allows us to use one of the most convincing stylised facts of labour economics, namely that equilibrium unemployment is independent of total factor productivity (that increases with economic growth).[1] This fact implies that equilibrium unemployment also should be independent of the

[1] This fact is obvious from time series of unemployment (that has no trend, see e.g. Layard et al, 1991) and total factor productivity (that grows exponentially, see e.g. Barro and Sala-i-Martin, 1995).

productivity per worker (i.e. that $e_{uF_N} = 0$).[2] Thus, the overall effect of a working time reduction has to equal the effect at a given productivity per worker:

$$e_{uh} = e_{uh}\big|_{F_N} . \tag{2}$$

This does *not* necessarily imply that equilibrium unemployment must be independent of working hours as suggested by Layard et al (1991). However, it *does* imply that all remaining effects must work through the wage setting process if we are willing to accept that equilibrium unemployment and wages are determined by a long run wage setting relationship and an aggregate labour demand curve.

As is shown in detail later on in this section, there are credible mechanisms through which a working time reduction may affect equilibrium unemployment even though condition (2) holds. These mechanisms work through the wage setting process. This fact is important, as it is the foundation of the empirical test performed later on in the paper.[3] The idea of the test is that, if equilibrium unemployment is independent of aggregate labour demand, it could only be affected by changes in wage pressure. And, studying the wage responses to a partial working time reduction will give evidence of such effects.

The model presented below gives a formal theoretical foundation for this intuition. The structure of the model follows the equilibrium model of Houpis (1993). The contributions are twofold. First, the choice of functional forms generates an unemployment rate that is independent of technical change. Thus, all effects through the production per employee are removed, and equation (2) holds. Second, the model allows for equilibrium effects of firms' fixed costs of employment. It is shown that the net effect on unemployment from a working time reduction depends on the relative importance of counteracting effects through the firms' fixed costs and the workers' value of leisure. The sign of the net effect is in general indeterminate.

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[2] There is a close correspondence between a working time reduction and economic growth. Both shift the level of labour demand, and since the shifts in labour demand induced by economic growth does not affect equilibrium unemployment, the same should be true for the shift induced by a working time reduction.

[3] It should be stressed that the model presented below only assumes that the unemployment rate is independent of total factor productivity (implying that equation (2) is satisfied). The fact that the remaining mechanisms work through the wage setting process is derived from the model.

2.2 Wage setting

Throughout the analysis, it is assumed that workers do not perform overtime work and that working hours is a policy variable. This is done strictly for convenience; all the results would carry through if overtime work was allowed as long as the amount of overtime is determined independently of wages, see Nordström Skans (2001) for details.

Actual wages (w) in each firm are assumed to be determined by a *firm level* monopoly union i (with fixed membership M), but a "right-to-manage" model with bargaining over wages would not change any of the results.[4] The wage is set in a trade-off between the benefits of higher wages and the risk of reduced employment (N). The unions objective is to maximise the weighted average of the *utility* for the employed, $v(wh, T-h)$ where T is total time endowment, and the outside option for workers loosing their jobs, V^u.[5] Employment is a function of wages, $N^*(w)$, determined by the labour demand of the firm. Thus, union i solves the problem:

$$\begin{aligned} &\max_{w_i} \Omega = \frac{N_i}{M_i} v(w_i h, T-h) + \frac{(M_i - N_i)}{M_i} V^u \\ &s.t.\, N_{\mathrm{i}} = N_i^*(w_i) \end{aligned} . \qquad (3)$$

The first order condition is:

$$\varepsilon_{Nw_i} = w_i \frac{\partial v(w_i h, T-h)}{\partial w_i} \left[v(w_i h, T-h) - V^u \right]^{-1}, \qquad (4)$$

where $\varepsilon_{Nw_i} \equiv -\frac{dN_i^*}{dw_i}\frac{w_i}{N_i^*}$ is the wage elasticity of employment for firm i.

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[4] In fact, it is also possible to replicate the conclusions from this model using a version of the standard matching model in Pissarides (2000).

[5] Occasionally, it is assumed in similar models that unions only care about wages with the motivation that the wage-bill is what the members actually observe. Note, however, that the model still would require that unions care about what happens to the earnings of the workers that get displaced, which in itself is unrelated to the observed wage-bill. Thus, if unions care about the financial losses of the displaced workers, it is not clear why they should disregard the benefits in terms of increased leisure.

The outside option V^u is the *expected utility* for workers loosing their jobs. It is assumed to be exogenous for each union during wage setting but it is endogenous at the national level in the derivation of equilibrium unemployment. In accordance with standard assumptions it is assumed that the probability for displaced workers to become rehired in another firm is equal to one minus the unemployment rate (u). Thus, the expected utility for workers loosing their jobs is the weighted average of the utility of an employed worker, $v(wh, T-h)$, where w is the market wage (not under the influence of the individual unions) and the utility of an unemployed worker, $v(B,T)$, where B is the unemployment insurance:[6]

$$V^u = u v(B,T) + (1-u)v(wh, T-h)\,. \tag{5}$$

Rewriting (4) under the assumption of equation (5) and solving for unemployment gives:

$$u = \frac{1}{\varepsilon_{Nw}}\left[\frac{\partial v(wh,T-h)}{\partial w}\frac{w}{v(wh,T-h)}\right]\cdot\left[1-\frac{v(B,T)}{v(wh,T-h)}\right]^{-1} \tag{6}$$

So far this model resembles the model in Houpis (1993). In what follows, the model is made more precise by the incorporation of the assumptions necessary for removing all effects through the production per employee. This requires that (6) is independent of the wage level which is true under two conditions. First, that unemployment insurance is indexed to the average wage level, B=bwh. Second, that utility is isoelastic in earnings,

$$v(wh,T-h) = \frac{(wh)^{\sigma}}{\sigma}\phi(T-h)\,, \tag{7}$$

where $\phi(T–h)$ can be any function but it is reasonable to assume that more leisure is better (ϕ'>0) and that we have an interior solution to an individuals optimal labour supply decision (ϕ''<0).

Rewriting (6) under these assumptions and solving for unemployment gives:

[6] By assuming that all firms and unions are identical we may drop the index i.

$$u = \frac{1}{\varepsilon_{Nw}} \frac{\sigma}{1 - \widetilde{b}(h)} \tag{8}$$

where $\widetilde{b}(h) \equiv b^{\sigma} \dfrac{\phi(T)}{\phi(T-h)}$ is an "utility adjusted" replacement ratio, i.e. the replacement ratio adjusted to take account of risk aversion and the relative leisure cost of working. From (8) we see that we may have two possible mechanisms for effects on equilibrium unemployment, one through the wage elasticity of employment and one through changes in the utility adjusted replacement ratio.

2.3 The wage elasticity of employment

Since equilibrium unemployment is independent of production per employee, the same has to be true for the wage elasticity of employment. This does, however, not imply that the wage elasticity necessarily is independent of working time. Below it is shown that the elasticity is affected by working time reductions if there are fixed costs of employment. [7]

Denote the state of technology by A and assume that the production function is Cobb-Douglas, $A\left(Nh^{\beta}\right)^{\alpha}$, with decreasing marginal productivity of labour $(\alpha < 1)$. Marginal productivity may be decreasing more rapidly if hours are increased than if employment is increased, implying $\beta < 1$.[8] Costs are equal to wage costs (*whN*) plus, possibly, a fixed cost per employee (*AcN*). The fixed cost must be proportional to the technology parameter (A) to ensure that the unemployment rate is independent of technological progress, this is important for reasons described in *Section 2.1*.[9] Thus, the problem for firm i is:

[7] It is straightforward to show that the elasticity is affected by working time reductions if $d\left(\dfrac{C''_{Nw}}{F''_{NN} - C''_{NN}} \dfrac{w}{N}\right) \Big/ dh \neq 0$ in optimum where $\pi = F(N,h)\text{-}C(N,h,w)$ is the maximand of the firm; F is the production function, C is total costs and the subscripts denote derivatives. The fixed cost argument is used here since it has a clear economic interpretation, but other similar arguments could possibly be made.

[8] The rate of decline in the marginal productivity of hours is not important for the qualitative results of the analysis. For a more elaborated analysis of the importance of hourly productivity in a partial equilibrium monopoly union model, see Booth and Ravallion (1993).

[9] Holmlund (2000) shows that "vacancy costs" (that could be viewed as a fixed cost) should be indexed to the state of technology, but not to hours.

$$\max_{N_i} \pi = A\left(N_i h^{\beta}\right)^{\alpha} - w_i h N_i - AcN_i \,.$$

The first order condition for a maximum is:

$$N_i^* = \left[\left(w_i h + Ac\right)^{-1} h^{\alpha\beta} A\alpha\right]^{\frac{1}{1-\alpha}} \,. \tag{9}$$

Differentiate to get the wage elasticity of employment:

$$\varepsilon_{Nw_i} \equiv -\frac{dN_i^*}{dw_i}\frac{w_i}{N_i^*} = \frac{1}{1-\alpha}\frac{w_i h}{w_i h + Ac} \,. \tag{10}$$

Normalise the number of firms and the size of the labour force to 1, implying $N = (1-u)$. Use these normalisations to transform the firm's first order condition (9) into a function of the unemployment rate and use (10) to get

$$\varepsilon_{Nw} = \frac{1}{1-\alpha}\left[1 - c \cdot \frac{(1-u)^{1-\alpha}}{\alpha h^{\alpha\beta}}\right]. \tag{11}$$

Thus, the wage elasticity of employment is independent of the state of technology, but not independent of working hours if there are fixed costs. Furthermore, it is positively related to both the unemployment rate and to working hours.

2.4 Equilibrium work sharing

2.4.1 Case 1: No fixed costs

We know from the firm's profit maximisation problem and equation (11) that the wage elasticity of employment is a constant in the case with no fixed costs (i.e. when $c = 0$). Differentiating (8) under this assumption, and noting that $\tilde{b}(h) < 1$ (or all workers would prefer to be unemployed), we see that unemployment is increasing in working hours:

$$\varepsilon_{uh}\Big|_{c=0} = \left(h\frac{\phi'(T-h)}{\phi(T-h)}\right)\frac{\tilde{b}(h)}{1-\tilde{b}(h)} > 0 \,. \tag{12}$$

As a special case one may consider a situation where workers (or more precisely, unions) do not derive any utility from leisure (thus $\phi(T\text{-}h)=1$), resulting in $\varepsilon_{uh}\big|_{c=0} = 0$. This is the situation discussed in Layard et al (1991), and the conclusion was that a working time reduction leaves the unemployment rate unaffected. But, equation (12) shows that a working time reduction lowers the equilibrium unemployment rate as long as workers derive utility from leisure (and the wage elasticity of employment is constant, more on this below). This is true for the most general utility function that is consistent with the stylised fact that unemployment is independent of the state of technology. [10]

The intuition is that a shorter workweek reduces earnings for both employed and unemployed workers. This change in the average earnings level in the economy cannot affect equilibrium unemployment due to the independence of the productivity per worker (causing independence of the earnings level). The effect on unemployment from a working time reduction is due to the fact that the shorter the workweek is, the lower is the cost of forgone leisure when working and thus the utility adjusted replacement ratio $\tilde{b}(h)$. This will lead to lower wage pressure and consequently to lower equilibrium unemployment.

2.4.2 Case 2: Fixed costs

In the analysis above, it was assumed that the wage elasticity of employment is independent of working hours. This assumption is violated if firms have fixed costs for their employees. The importance of fixed costs increases if the working time is reduced, resulting in a lower employment elasticity and, consequently, higher wage pressure which will tend to raise the equilibrium unemployment rate. The net effect on unemployment is found by differentiating (8), using (11):

$$\varepsilon_{uh} = (1-u)\frac{\varepsilon_{uh}\big|_{c=0} - \alpha\beta\dfrac{Ac}{wh}}{(1-u) + u(1-\alpha)\dfrac{Ac}{wh}} \tag{13}$$

[10] Note also that the parameter for the productivity of hours (β) does not enter equation (12).

The sign of the net effect on unemployment is indeterminate. The larger the proportion of fixed cost and the less decreasing the marginal product of hours are, the more likely it is that equilibrium unemployment increases if hours are reduced.

This simple model has shown that a working time reduction can affect unemployment even if unemployment is independent of the state of technology. Furthermore, we see that the sign of the effect on unemployment in general is indeterminate. It depends on whether wage pressure is increased (i.e. the fixed cost effect dominates) or decreased (i.e. the effect through the utility adjusted replacement ratio dominates).

3 What we may learn from partial reductions and hourly wages

General working time reductions are rare, and it is not trivial to identify the effects of macroeconomic reforms due to the difficulty of separating the relevant effects from other time effects. Partial working time reductions, on the other hand, has the advantage of only affecting few individuals. This generates natural control groups that can be used in empirical work. However, is important to note that the effects of partial and general working time reductions may well differ. The purpose of this section is to show hat we may learn from the effects of a partial working time reduction about the effects of a general working time reduction on equilibrium unemployment.

The idea is to study the wage responses to a partial working time reduction since the formal model presented in *Section 2* showed that a general working time reduction may affect equilibrium unemployment through changes in wage pressure. More precisely, it is shown below that *hourly* wages should fall due to a partial working time reduction in a model without fixed costs, i.e., if the conditions for a general working time reduction to unambiguously reduce unemployment are satisfied.

Formally, restate the wage equation (4) for union *i* with the explicit utility function (7) required for the unemployment rate to be independent of the level of technology to get:

$$\frac{(w_i h)^\sigma}{\sigma}\phi(T-h) = \frac{\varepsilon_{Nw_i}}{\varepsilon_{Nw_i}-\sigma}V^u . \tag{14}$$

A partial working time reduction will by definition only affect a limited number of workers and it is reasonable to assume that the outside option (V^u) is independent of the reduction. Differentiating (14) under this assumption, using (10) and solving for the elasticity of hourly wages with respect to working hours:

$$\varepsilon_{wh_i} = \frac{h\frac{\phi'(T-h_i)}{\phi(T-h_i)} - (\sigma + Q_i)}{\sigma + Q_i} \tag{15}$$

where $Q_i \equiv \frac{\sigma}{\varepsilon_{Nw_i}-\sigma}\frac{Ac}{w_i h_i + Ac} > 0 \text{ if } c > 0$.

The model in *Section 2* showed that work sharing was a feasible policy unless firms have fixed costs. In that case we know that $Q = 0$, and the wage elasticity in equation (15) is determined solely by the workers individual preferences. If individual workers desire a shorter workweek (15) will be positive and we should observe *hourly* wages falling as the result of a partial working time reduction.[11]

However, there is a counteracting effect if the firms have fixed costs for their employees (i.e., if c>0, which implies Q>0). This effect is interesting since it is completely analogous to the general equilibrium effect derived in equation (13). A working time reduction increases the importance of fixed costs, reducing the wage elasticity of employment and therefore cause increased wage demands. These wage demands will increase actual wages as long the working time reduction only affects few workers, but transform into increased unemployment if the working time reduction is general.

[11] This case was derived in Houpis (1993). It is restated here using the most general utility function consistent with an unemployment rate that is independent of the state of technology giving the first order condition for individual optimum as $\varepsilon_{vh} = \sigma - h\frac{\phi'(T-h)}{\phi(T-h)} = 0$. The second order condition ensures that $\varepsilon_{vh} < 0$ if the workweek is longer than optimal.

Thus, the model predicts that we only will observe rising hourly wages as a result of a working time reduction if there is an increase in wage pressure.[12] This argument is based on an assumption that workers prefer a shorter workweek. Evidence shows that workers in general do prefer to work fewer hours; survey data from 1998 suggest that Swedish full time workers on average preferred to work 6.8 hours less per week (Torp and Barth 2001).[13] This implies that, if we find that hourly wages increased as a response to a partial working time reduction, it can be interpreted as a rise in wage pressure (possibly due to fixed costs) that would tend to increase unemployment if the reduction was general.

4 A partial working time reduction

This section describes the working time reduction that is studied in the empirical section. The reduction was targeted at one form of shift workers ("2-shift" workers) in the Swedish manufacturing and mining industries.[14] There are 4 major shift form categories for blue-collar workers in Sweden: daytime, 2-shift, discontinuous 3-shift and continuous 3-shift.[15] 2-shift workers work Mondays to Fridays and alternate between morning shifts (e.g. 5:30 a.m. – 2:00 p.m. with a 30 minutes break) and afternoon shifts (e.g. 2:00 p.m. – 10:30 p.m. with a 30 minutes break).[16] Discontinuous 3-shift workers work Mondays to Fridays on schedules that ensure 24 hours production during the workweek. Continuous 3-shift workers have schedules that allow for continuous production 24 hours per day, 7 days per week, with the exception of a few specific holidays.

Figure 1 shows the development over time of standard working hours, as determined in central agreements, for the different shift forms. Between 1983 and 1988 there was a gradual reduction in standard working time for 2-shift

[12] It can be noted that these results are independent of whether hourly productivity is affected by a working time reduction (i.e. of the value of β).
[13] A "revealed preference" argument implying that workers do prefer shorter hours is that working time reductions generally are proposed by unions rather than by firms.
[14] For a description of Swedish working-time related institutions, see *Essay I* of this thesis.
[15] Anxo and Sterner (1995) provide a description of the use of shift work in Sweden from 1968 to 1990.
[16] The examples are from the default working schemes in the Engineering contract (Verkstadsavtalet) 1989-90, which covers more workers than any other contract in Swedish manufacturing.

workers from 40 to 38 hours per week. The reduction was the result of a series of central agreements between the Swedish Employers Confederation (SAF) and the Swedish Trade Union Confederation (LO).[17] These agreements were implemented at the industry level either as a reduction of working hours on a weekly basis or with time off in lieu.[18]

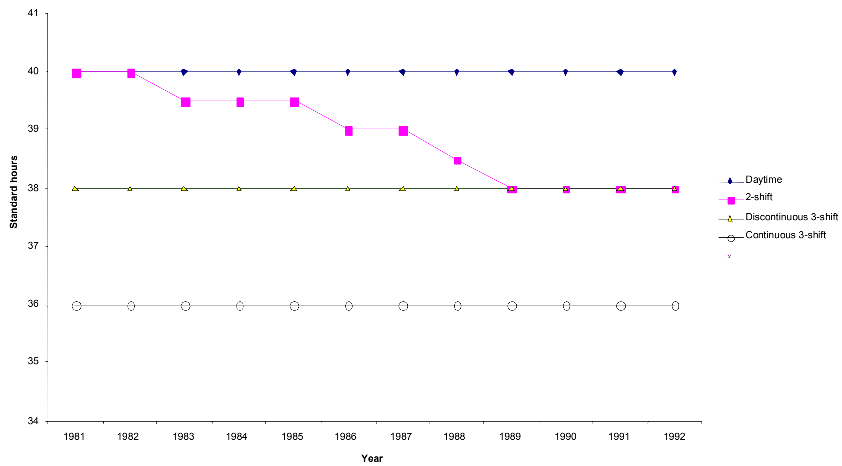

Figure 1. Standard hours by shift form 1981-1992

The central agreements do not explicitly state how the wage rates should respond to the reduction but they suggest that monthly wages remain constant relative to the monthly wages of other workers. However, two important facts make the response of actual wages interesting. The first is that a large portion (approximately half) of actual wage increases were unrelated to the central agreements, i.e. they were due to "wage drift", at this time (see Hibbs & Locking, 1996 and Nilsson, 1993). The second reason is that 1982 was the last year of centralised wage setting in Sweden (Hibbs & Locking, 1996). This fortunate timing of events means that we are studying actual wage responses during a period where it is reasonable to assume that the only central agreements of inter-

[17] LO (1988).
[18] Based on readings of industry level contracts.

est for the wage difference between 2-shift workers and other workers were the working time agreements.

The empirical section of paper studies how the actual hourly wages for 2-shift workers changed relative to the wages of other workers during this time. One deviation from the working time reductions discussed in the theory section is that workers within the same firm are compared to each other whereas the theoretical model assumed that the workers receiving the reduction worked in separate companies. However, this is unproblematic as long as the wage elasticities of employment for the two groups do not interact (a formal proof is available on request).

As in all studies of policy reforms it is natural to ask whether the reduction may be endogenous. This risk is minimised by studying a working time reduction that is determined at the highest possible bargaining level. This is quite unusual for partial working time reductions and should be an advantage relative to, e.g., Hunt (1999) that studied the effects of industry-level working time reductions in Germany. In contrast, this study uses workers within the same industry as a control group. Furthermore, the most likely form of endogeneity is that 2-shift workers may have had a strong preference for working time reductions. It is straightforward (calculations are available from the author) to show that wages should be reduced even more in the absence of fixed costs if 2-shift workers derive more disutility from working hours than daytime workers do. This means that the empirical results would be biased in favour of finding a negative effect on wage-pressure. Given the results in *Section 5*, this type of endogeneity would mean that the “true” effect is even stronger than estimated, and thus further support the conclusions.

4.1 Data

The study uses individual level panel data collected from private sector firms by the Swedish Employers Confederation (SAF). Since the data set is collected from the payroll records of the firms, it should be accurate except for black market (tax evasive) work. Data cover earnings and working hours for the *second quarter* each year on all privately employed blue-collar workers covered by the central agreements in Sweden. The paper uses data from 1981 to 1992. The motivation for this time frame is a labour market conflict in the second quarter of 1980 and a change in the data collection procedure in 1993. Most 2-shift workers are employed in the mining and manufacturing industries. Data from other industries are not used due to reasons described in *Appendix B*. The data have not been widely used for microeconomic research in the past but they are the base of Statistics Sweden's aggregate data set on working hours and wages.[19]

The working time data and the wage data are decomposed into several parts, such as straight-time wages and hours, overtime premium and hours, shift compensation, etc. The data set further contains indicators for industry level contract and municipality as well as size of the firm and the workplace. The firms can not be identified but individuals can be followed over time. Individual characteristics are not recorded except for age and gender. Standard hours from central agreements are assigned to the observations according to their shift form.

Only workers aged 25-55 during the full sample period (i.e. workers aged 25-44 in 1981) are studied to minimise potential problems with differences in age-effects and retirement patterns between shift forms. See *Appendix A* for further details about the data set, descriptive statistics and applied restrictions.

4.2 Empirical set-up

Daytime workers and 2-shift workers were not separated in the data set before 1988. This is a problem since standard hours are assigned to workers based on their shift form. Only observations from the years before (1981-82) and after (1989-92) the gradual reduction is used for this reason. This is convenient since standard hours were the same (40 hours per week) for both daytime and 2-shift workers in 1981 and 1982.[20]

This approach gives a clear-cut differences-in-differences experiment where the years before the reduction are compared to the years after, with other workers as a control group. One advantage of the approach is that the relatively long period between the years before and after the reduction should reduce the influence of transitionary dynamics (e.g. due to nominal wage rigidities) on the results.

Consider a model for how average hourly earnings for 2-shift workers changed relative to hourly earnings for daytime workers as a result of the reduction. Let α_i indicate an individual fixed effect and *Age* is a third order age-

[19] One example of a microeconometric study on this data set is Petersen et al (1996).

[20] There are additional problems with the intermediate years such as variations in the timing of the industry level implementations that are difficult to link to the data and agreements that take effect during the second quarter.

polynomial. The parameter β_1 measures the initial (and by assumption time invariant) difference in hourly wages between 2-shift workers and daytime workers, β_2 measures time effects interacted with sector dummies (the number of sector dummies differs between specifications). The parameter of interest is γ that denotes the wage elasticity with respect to standard hours (h^s). Disregarding the error term, we may thus write the log of hourly earnings (*w*) as:

$$\ln w_{it} = \gamma \ln h_{it}^s + \beta_1 D_{it}^{2-shift} + D_t^{Year} D_{it}^{Sector} \beta_2 + Age_{it}\beta_3 + \alpha_i . \qquad (16)$$

The wage effect of the working time reduction as captured by γ is the change in wages for 2-shift workers relative to daytime workers within the same sector.

The model is presented as a wage equation that includes standard hours as a regressor but the only variation in standard hours that is used to identify this effect is the reduction for 2-shift workers. The only other variation (during the sample period) in standard hours that could be exploited is the permanent difference between 3-shift workers and other workers (see *Figure 1*). However, dummies are used for removing these differences in the regressions that include 3-shift workers (see *Appendix C*) since this paper is focusing on the effects of changes in standard hours.

The fact that the data set does not contain information on whether workers are daytime or 2-shift workers before the working time reduction does not cause any problems for the standard hours variable (i.e. the variable of interest), as explained in *section 4.2*. However, it does cause problems for measuring the initial difference in wages between shift forms as captured by β_1 in equation (16). The estimates of the effect of the working time reduction will be biased if the model restricts β_1 to zero and there were systematic wage differences between shift forms before the reduction that are not captured by the other covariates.

The panel structure of the data offers one solution to this problem. Individuals can be followed over time, which allows for the inclusion of individual fixed effects. These fixed effects removes all the initial differences for workers that did not change shift forms during the years of the reduction. The fixed effects model can be estimated as if there were no initial wage differences unless many workers changed shift forms during the sample period. Thus would give the model:

$$\ln w_{it} = \gamma \ln h_{it}^{s} + D_{t}^{Year} D_{it}^{Sector} \beta_2 + Age_{it} \beta_3 + \alpha_i \,. \tag{17}$$

Estimation of equation (17) gives consistent estimates of the effects of a change in standard hours even if there were real differences before the reduction as long as workers did not change shift form (see *Appendix B* for a further discussion).

The identification discussed above is, however, problematic if there were substantial wage differences between shift-forms before the reduction *and* many workers did change their shift forms during the intermediate years. The applied solution to this potential problem is to use a proxy for the shift forms of the workers and estimate equation (16) directly. The proxy is constructed by classifying as 2-shift workers all (daytime and 2-shift) workers with a fraction of shift compensation to total earnings that exceeds 7 %. This procedure is evaluated using data from after 1988 where the true definitions are available, showing that 80 % of the workers classified as 2-shift workers by the proxy actually were 2-shift workers. More details of the procedure can be found in *Appendix B*.

4.3 Actual hours

Essay I of this thesis studied the effects of the working time reduction on actual hours. It was shown that the impact was far smaller than expected from the central agreements. The estimated elasticity of actual hours with respect to standard hours was between 0.3 and 0.4. Furthermore, the evidence suggested that the small effect on actual hours was due to a low actual implementation rate. That leaves us with two interesting questions regarding hourly wages. Did average hourly wages for 2-shift workers rise or fall? And, did the 2-shift workers whose actual hours were reduced experience a fall in hourly wages relative to the 2-shift workers whose actual hours remained constant?

The actual reduction in working hours is not necessarily exogenous to the wage increase. This makes an interpretation in terms of wage pressure of the results from the comparison *between* 2-shift workers less convincing. However, these results are used for evaluating whether the small impact of the reduction on actual hours affects the estimates of the wage effect for the *average* 2-shift worker.

5 Evidence

5.1 The average wage effect

As described in *Section 4.1* it is possible to identify the effect without using a proxy for the initial wage difference if very few workers changed shift form during the years of the reduction. Individual fixed effects removes all the initial differences for workers that did not change their shift forms during the years of the reduction. The first model that is estimated in this section is based on equation (17), restated here for convenience with an error term:

$$\ln w_{it} = \gamma \ln h_{it}^{s} + D_{t}^{Year} D_{it}^{Sector} \beta_2 + Age_{it} \beta_3 + \alpha_i + \varepsilon_{it} \qquad (18)$$

The baseline model includes one sector dummy (D_{it}^{Sector}) for each union contract, implying that the model allows for each contract to have a unique year effect.

Results from estimating this model are presented in the first column of *Table 1* below. The dependent variable is the straight-time hourly wage. The estimated effect is negative (-0.32) which implies that hourly wages rose sharply due to the working time reduction. This is a rejection of the model without fixed costs that showed that a general working time reduction would reduce equilibrium unemployment. The reason is that that model *also* predicted *reduced* hourly wages in the response to a partial working time reduction such as the one studied here. Instead, the results in *Table 1* show that there was an increase in wage pressure (consistent with fixed costs) that would tend to increase the unemployment rate if working hours were reduced for all workers.

To estimate the effect while relaxing the assumptions that no workers changed their shift form or that there were no initial wage differences we use the proxy (discussed in *Appendix B*) for whether the worker is a 2-shift worker or not before the reduction. Estimating equation (16) while using the proxy for $D_{it}^{2-shift}$ we get both the initial difference in working time (β_1) and the effect of the change in standard hours (γ). Adding an error term to equation (16) we get:

$$\ln w_{it} = \gamma \ln h_{it}^{s} + \beta_1 D_{it}^{2-shift} + D_{t}^{Year} D_{it}^{Sector} \beta_2 + Age_{it} \beta_3 + \alpha_i + \varepsilon_{it} \qquad (19)$$

The second column of *Table 1* shows estimates based on equation (19). The estimate of initial wage differences is close to zero (-0.005) and the estimated effect of standard hours is negative (-0.34) and very similar to the specification where the proxy for initial wage differences was excluded. The fact that the estimate of initial wage differences is negative even though the 2-shift workers have an unconventional timing of their workdays may be surprising. However, this is driven by the fact that the used wage measure excludes the overtime premium and the compensation for working unusual hours.[21] It should be noted that the reason for defining the dependent variable this way is that it is the definition that gives the weakest results. Including overtime premium and shift compensation into the wage measure would strengthen the results even further regarding the effects of standard hours, see *Appendix C*.

It is important to note that the inclusion of the proxy does not affect the standard hours' estimate (γ). The fact that there is such a small effect on the variable of interest from including the proxy suggests that the measurement error in the proxy also be of little importance for the standard hours' estimate.[22] Furthermore, it should be noted that any potential bias should cause underestimation of the standard hours' effect since the initial difference appears to be negative.

A model based on equation (19) but without individual fixed effects is estimated to investigate whether there are any indication that the fixed effects exacerbate the measurement error problems. The estimated effect of standard hours is even stronger (-0.45) than in the fixed effects model, and the estimate of the initial difference is still close to zero (-0.002). This further suggests that measurement errors in this covariate do not drive the main result that the working time reduction led to an increase in wage pressure.

It can be noted that the absolute value of the wage-elasticity with respect to standard hours (0.34) is close to the elasticity of actual hours with respect to standard hours (estimated to be between 0.3 and 0.4, see *Essay I*). This implies that monthly earnings remained largely unchanged for 2-shift workers relative to other workers.

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[21] Interestingly, these results show that the base wage for shift-workers is lower than for other workers suggesting that the actual wage premium for shift workers is lower than what is implied from the shift compensation share.

[22] Although we know from *Appendix B* that that the proxy does contain some noise we also know that it contains more signal than noise and that the signal does not seem to matter (for what we care about). Thus, we should not have to worry too much about the noise.

Appendix C presents results from variations on the estimated model to verify that the results are robust. Equation (19) is estimated with different definitions of a *Sector*. The results are robust to changing the model from a very crude version where all workers are assumed to work within the same sector (i.e. there are only raw year dummies) to a version with a *Sector*-dummy for each *combination* of industry, municipality, size of firm and size of workplace. The model is also estimated with alternative wage measures for the dependent variable. It is shown that the alternative wage measures generate even stronger results. Furthermore, a model that compares the wages of 2-shift workers with the wages of other shift workers instead of daytime workers is estimated. The results are robust to this change of control group, regardless of how a *Sector* is defined and regardless of the used wage measure.

Table 1. Elasticities of hourly wages with respect to standard hours

Control group	Estimated parameter	Individual fixed effects		No individual fixed effects
		Not controlling for initial differences	Controlling for initial differences	Controlling for initial differences
Daytime workers	Standard hours (γ)	-0.315 (0.011)	-0.338 (0.011)	-0.453 (0.013)
	Initial difference (β_1)	--	-0.005 (0.001)	-0.002 (0.001)
Number of observations		467,768	467,768	467,768
Number of individual fixed effects		94,395	94,395	--
Number of time-sector effects		330	330	330
Degrees of freedom		373,038	373,037	467,432

Note: The dependent variable is the log of straight-time hourly wages during the second quarter each year. Sample period is 1981-82 and 1989-92. The standard hours estimates are elasticities. The initial difference estimates measure the (constant) effect of being a 2-shift worker at time *t* and are based on the proxy described in *Appendix B*. All regressions include 330 year-contract interaction dummies and an age cube. Huber-White standard errors are in parentheses. The first column is based on equation (18), the second on equation (19) and the third on equation (19) but without the individual fixed effects.

5.2 Differences in effects between 2-shift workers

Essay I of this thesis showed that the working time reductions impact on actual hours was surprisingly small and that this could be the result of a low rate of implementation at the local level. The 2-shift workers that experienced reduced (locally determined) scheduled hours also appeared to experience a larger reduction in actual hours. This section studies whether the increase in relative wages for the average 2-shift worker was due to increased wages for those whose scheduled hours remained unchanged. The empirical specification uses scheduled weekly hours as an explanatory variable along with standard hours (that captures the effect on the average 2-shift worker):

$$\begin{aligned} \ln w_{it} &= \gamma \ln h_{it}^{s} + \lambda \ln h_{it}^{scheduled} + \beta_1 D_{it}^{2-shift} \\ &\quad + D_t^{Year} D_{it}^{Sector} \beta_2 + Age_{it} \beta_3 + \alpha_i + \varepsilon_{it} \end{aligned} \tag{20}$$

The data collection procedure did not require that firms reported scheduled hours, reducing the response rate to between 51 % and 61 % in the raw data (see *Table A2, Appendix A*), and we can only use observations with reported scheduled weekly hours for the estimation of (20). The first two columns in *Table 2* below show that the effect of the standard hours reduction on hourly earnings when estimated on observations with reported scheduled hours (-0.20) is somewhat smaller than the effect estimated on all observations (-0.34) displayed in *Table 1*. This discrepancy implies that the wage effect of the working time reduction differed somewhat between workers with and without reported scheduled hours. Nevertheless, this section uses workers with reported scheduled hours to study differences between 2-shift workers in the effects of the working time reduction on hourly earnings, but some caution is warranted when interpreting the results.

Two different samples are used, one that includes all workers with more than 30 scheduled hours per week, and one where only workers with scheduled hours equal to standard hours, or equal to 40 are included. The first sample allows for wage differences depending on scheduled hours for all workers. The second sample isolates the wage effect for 2-shift workers that had their scheduled hours reduced to *exactly* 38 compared to the 2-shift workers whose hours remained at 40.

Table 2. Differences in the wage responses between 2-shift workers

Estimated parameter	Equation (19)		Equation (20)	
	Base sample	Restricted sample	Base sample	Restricted sample
Scheduled hours (λ)	--	--	-0.163 (0.012)	-0.295 (0.036)
Standard hours (γ)	-0.203 (0.020)	-0.198 (0.021)	-0.149 (0.019)	-0.090 (0.024)
Initial difference (β_1)	-0.004 (0.001)	-0.008 (0.001)	-0.005 (0.001)	-0.008 (0.001)
Number of observations	166,481	157,315	166,481	157,315
Number of individuals	36,062	34,301	36,062	34,301
Number of time-sector effects	242	240	242	240
Degrees of freedom	130,171	122,768	130,170	122,767

Note: The dependent variable is the log of straight-time hourly wages during the second quarter each year. Sample period is 1981-82 and 1989-92. Only workers with reported scheduled hours are included in the regressions. Restricted sample only includes workers with scheduled hours equal to standard hours or 40 hours. The standard hours and scheduled hours estimates are elasticities. The initial difference estimates measure the (constant) effect of being a 2-shift worker at time *t* and are based on the proxy described in *Appendix B*. All regressions include individual fixed effects, year-contract interaction dummies and an age cube. Huber-White standard errors are in parentheses. The first two columns are based on equation (19), the third and fourth on equation (20).

The last two columns of *Table 2* show that the elasticity with respect to scheduled hours (given standard hours) is negative (–0.16 and –0.30), for both samples. The elasticity of standard hours (given scheduled hours) is also negative (–0.15 and –0.09) but much smaller than previously. This suggests that hourly wages rose for all 2-shift workers, but that the effect was strongest for the workers that saw a decrease in scheduled hours and thus also a larger reduction in actual hours. This result should be interpreted with caution since differences in the implementation between observations may well be endogenous to the wage effect. The result does however not contradict the finding in the previous sections that a working time reduction increases, rather than decreases, wage pressure. Importantly, this also suggests that the rise in hourly wages for the average 2-shift worker is not a result of the small impact on actual hours since the workers who experienced the largest increase in wages were the workers that received the largest reduction in scheduled hours.

6 Conclusions

The aim of this paper has been to study the equilibrium effects of a general working time reduction. This has been done by extending a theory of work sharing to be consistent with the stylised fact that equilibrium unemployment is independent of the level of technology and hence of production per employee. It is shown that equilibrium work sharing is a feasible policy, regardless of whether the workers prefer the reduction or not, in the absence of fixed costs. The reason is that a shorter workweek makes it relatively less costly in terms of forgone leisure to work. The relative cost of working in terms of financial remuneration has to be unaffected by the length of the workweek for unemployment to be independent of the level of technology. Firms' fixed costs have, on the other hand, a counteracting effect in equilibrium. A working time reduction reduces the share of firms' fixed costs that are affected by wage demands and this reduces the wage sensitivity of labour demand. This will induce workers to increase their wage demands which, in equilibrium, will tend to increase the unemployment rate.

It was also shown that hourly wages should fall if the working time was reduced for a small group of workers preferring such a reduction, and firms did not have substantial fixed costs. Thus, it is possible to test for the conditions required for a general working time reduction to unambiguously reduce unemployment, by studying the wage response to a partial working time reduction. This is done by comparing the wages of workers covered by a working time reduction with the wages of other workers to see whether or not there is an increase in workers wage demands when the workweek is shortened.

The empirical part of the paper performs such a test by studying the wage-response to a 5 % working time reduction for one class of shift workers during the 1980's in Sweden. Register based data on wages and hours for blue-collar workers in Swedish manufacturing are used to study the impact of the working time reduction. One advantage of studying this particular working time reduction is that it is possible to compare the affected workers with other workers within the same sector and region. A further advantage is that comparisons can be made with daytime workers as well as with other shift workers, thereby controlling for the possibility that there was an increase in wages for shift workers in general.

The results show that hourly wages increased as a result of the working time reduction. The increase was sufficient to leave monthly earnings constant (at least). This is consistent with previous results from Germany (Hunt, 1999). The estimates are robust to changes in the estimated model, from a simple model with only year dummies to a model with a dummy for each combination of year, union contract, municipality, size of firm and size of workplace. The results are also robust to changing the control group from daytime workers to other shift workers and to different definitions of the wage variable.

One unexpected feature of the reduction that is studied is that the impact of the standard hours reduction on actual hours worked was relatively small (the elasticity was between 0.3 and 0.4, see *Essay I*). To check whether or not this has affected the estimated wage responses, an additional model is estimated. The wage impact for workers who, on average, experienced a larger reduction in actual hours are compared to the wage impact on other workers covered by the agreed-upon reduction. The results show that the wage increase was greatest for the workers that had the larger reduction in actual hours. Though this set of results may well be due to endogeneity it indicates that the reason for the estimated wage increase is *not* that some workers received financial compensation instead of an actual working time reduction.

The conclusion is that the partial working time reduction lead to a substantial increase in the wage demands of the affected workers, consistent with fixed costs being an important mechanism in transmitting a working time reduction to the wage setting process. Since an increase in the wage demands of all workers would tend to increase equilibrium unemployment, the results indicate that a general working time reduction may lead to an increase in equilibrium unemployment.

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Appendix A: The data set

Table A1 show the variables used in the paper. The size variables are categorical, taking 9 different values for the size of the firm and 10 values for the size of the workplace. A workplace is defined as workers covered by the same contract within the same firm.

Table A1. Variables in the data set

Source	Variables			
	Working time	Wages	Firms	Individuals
SAF	Actual hours (including overtime)	Straight-time wages	Industry contract	Fixed effect indicator
	Scheduled weekly hours	Overtime premium	Size of firm	Age
	Shift form	Shift compensation	Size of work place	Gender
		Total earnings	Municipality	
Central agreements	Standard hours by shift form			

The first part of *Table A2* shows descriptive statistics for the raw data set. The columns show statistics for workers during 1981-82 and 1989-92. The reason is that the empirical analysis focuses on these two time periods. Only very obvious outliers have been removed from the sample used for the tree first columns of the table.[23] It can be deduced from the table that the average actual overtime premium is between 58 % and 69 % of the hourly wage and that between 41 % and 66 % of workers work some overtime. Total overtime use was between 1.7 % and 3.2 % of actual hours worked. It should also be noted that between 51 % and 61 % of the observations had their scheduled hours reported.

[23] The main restrictions are the exclusion of observations with zero or more than 900 actual working hours during the quarter. Observations with nominal hourly earnings below 20 SEK and above 100 SEK in 1981 are dropped. These numbers are increased by 7.5 % (the estimated time trend in the sample) each year. The number of observations dropped by this procedure is very small.

Table A2. Descriptive statistics for daytime and 2-shift workers in manufacturing

	Raw data			Regression data		
	Day and 2-shift	Day	2-shift	Day and 2-shift	Day	2-shift
	1981-82	1989-92	1989-92	1981-82	1989-92	1989-92
Number of Observations	359,459	454,991	92,963	173,324	242,841	51,603
Number of individuals	201,847	159,187	37,429	94,395	80,993	19,733
Fraction male workers	0.78	0.74	0.74	0.80	0.80	0.78
Age	34.2 (5.71)	43.5 (5.74)	43.1 (5.67)	34.8 (5.67)	43.8 (5.78)	43.5 (5.68)
Standard hours (per week)	40	40	38	40	40	38
Quarterly actual hours (including overtime)	382.9 (122.6)	399.8 (128.6)	391.9 (123.8)	408.4 (87.4)	413.7 (96.7)	406.2 (92.1)
Fraction of actual hours due to overtime (OT)	0.017 (0.036)	0.026 (0.043)	0.032 (0.044)	0.016 (0.030)	0.023 (0.036)	0.030 (0.039)
Fraction of obs. with OT > 0	0.41	0.53	0.66	0.43	0.56	0.68
Fraction of actual hours due to OT if OT > 0	0.042 (0.046)	0.048 (0.048)	0.049 (0.046)	0.036 (0.037)	0.042 (0.040)	0.045 (0.040)
Straight-time hourly wage (SEK)	37.0 (4.5)	71.6 (11.6)	72.5 (11.3)	37.4 (4.2)	73.1 (10.8)	73.8 (10.6)
Hourly wage including overtime premium (SEK)	37.4 (4.4)	72.7 (11.5)	74.1 (11.2)	37.8 (4.1)	74.1 (10.7)	75.3 (10.5)
Hourly earnings (SEK)	38.3 (5.0)	73.5 (11.9)	81.9 (13.6)	38.8 (4.7)	74.7 (11.0)	83.1 (12.8)
Quarterly earnings (SEK)	14,701 (5,127)	29,358 (10,360)	32,002 (10,941)	15,841 (3,922)	30,883 (8,305)	33,658 (8,736)
OT-premium (share of earnings)	0.010 (0.025)	0.015 (0.027)	0.020 (0.030)	0.009 (0.021)	0.014 (0.023)	0.019 (0.026)
Hourly OT-premium if OT > 0 (% of straight-time wage)	0.59 (0.72)	0.58 (0.42)	0.69 (0.51)	0.59 (0.56)	0.58 (0.37)	0.69 (0.57)
Shift compensation share (SCS)	0.020 (0.046)	0.010 (0.034)	0.091 (0.056)	0.022 (0.046)	0.008 (0.029)	0.091 (0.053)
Fraction of observations with reported scheduled hours	0.53	0.51	0.61	0.56	0.55	0.64
Fraction of observations with scheduled hours >30 if reported	0.97	0.96	0.98	0.97	0.97	0.99
Scheduled hours if >30	39.9 (1.02)	39.8 (1.81)	39.0 (1.35)	39.9 (0.91)	39.8 (2.14)	39.1 (1.27)

Note: Day and 2-shift workers can only be separated after 1988. Standard deviations are in parentheses. *Depending on year and shift form.

A few restrictions have been applied on the data set used in the regressions. For individuals with multiple observations in one year only the observation with the highest number of hours is used. Dropping these individuals or adding the actual hours to the observation with the highest number of hours did not change the results. Observations with less than 120 hours or more than 600 hours worked during the quarter are removed to reduce the influence of outliers, but the results are not sensitive to this restriction. Workers in industries that employed less than ten 2-shift workers after the reduction as well as industries with less than 100 observations in total are dropped to reduce the number of industry dummies. Individuals observed only before or after the reduction are also dropped from the sample.

Table A3. The number of observations remaining after applying restrictions on the sample

Restriction	1989-92		All used years (1981-82 and 1989-92)	
	Day	2-shift	Day and 2-shift	All (including 3-shift)
Manufacturing & mining	454,991	92,963	907,413	977,876
Largest number of hours/year and individual	435,899	89,243	875,236	964,641
More than 120 hours worked in the 2nd quarter	418,513	85,891	839,751	927,571
Large agreements	399,615	85,817	783,754	865,878
Individual observed before and after the reduction	242,841 [80,993]	51,603 [19,733]	467,768 [94,395]	532,195 [106,331]
Reported scheduled hours >30	86,604 [30,813]	18,867 [7,556]	166,481 [36,062]	--

Note: The two bottom sets describes the number of observations in the data sets used in the paper, the number of individuals are in brackets. The data sets that only contain observations with reported scheduled hours have been constructed by first dropping observations without (or with 30 or less) reported scheduled hours and than applying the other restrictions.

Table A3 shows the number of observations dropped at each stage in this procedure. Only the observations from before (1981-82) and after (1989-92) the reduction have been included in the table since the regressions only use observations for these years for reasons described in *Section 4.2*. The table shows that the restriction that all workers should be observed both before and after the reduction leads to a substantial reduction in the sample size. This restriction is not necessary but these observations would not contribute to the identification since the effect of the reduction is identified from the change in actual hours between the two time periods.

The second part of *Table A2* shows descriptive statistics for the data set that is used in the main part of this paper. The differences between the raw data set and the data set used that is used in the regressions are small. The main differences are that the fraction of male workers and the average of actual hours are increased by the imposed restrictions.

Appendix B: Initial wage differences

B.1 Identification without estimating initial wage differences

The within transformation that removes the individual specific effects highlights under which conditions we may estimate the model without knowing the shift forms of the workers before the working time reduction. Subtracting individual means (denoted by bars) and using $\widetilde{X}$ to denote the deviation from means of the time-sector effects and the age-polynomial we may rewrite equation (16) as:

$$\ln w_{it} - \overline{\ln w_i} = \gamma(\ln h_{it}^s - \overline{\ln h_i^s}) + \beta_1 (D_{it}^{2-shift} - \overline{D_i^{2-shift}}) + \widetilde{X}_{it}\beta \,. \qquad \text{(B1)}$$

Without knowing the shift form before the working time reduction we can thus identify the relevant effect if one of the following two assumptions is valid:

$$\text{A1:} \quad \beta_1 = 0$$
$$\text{A2:} \quad D_{it}^{2-shift} = \overline{D_i^{2-shift}} \quad \forall i, t \,.$$

Assumption A1 states that there are no differences in hourly wages between shift forms that are independent of standard hours. Assumption A2 states that those who were 2-shift workers after the reduction were 2-shift workers before the reduction as well. The permanent difference is, under A2, removed as a part of the individual fixed effects and the change in working time for these workers due to the working time reduction is captured by γ. Under assumption A1 or A2 the within transformation yields:

$$\ln w_{it} - \overline{\ln w_i} = \gamma(\ln h_{it}^s - \overline{\ln h_i^s}) + \widetilde{X}_{it}\beta \,, \qquad \text{(B2)}$$

which can be estimated without knowledge of the shift form before the reduction. Note that (B2) is equivalent to the within transformation of equation (16) under the restriction $\beta_1 = 0$:

$$\ln w_{it} = \gamma \ln h_{it}^s + D_t^{Year} D_{it}^{Sector} \beta_2 + Age_{it}\beta_3 + \alpha_i \,. \qquad \text{(B3)}$$

Thus, as long as very few workers switched shift forms during the intermediate years of the reduction we may estimate the model *as if* there was no difference in actual wages before the reduction in standard hours. This is possible even if there were real differences before the reduction.

B.2 Identification by using a proxy for initial wage differences

The identification discussed above is, however, problematic if there were substantial wage differences between shift-forms before the working time reduction and few workers had the same shift for before and after the reduction. The solution to this potential problem is to construct a proxy for the shift form of the worker. This proxy is constructed using the fraction of shift compensation to total earnings. Henceforth, this fraction is referred to as the *shift compensation share* (SCS). This variable, unfortunately, also includes the premium given to daytime workers that perform work outside the normal working hours (e.g. the engineering industry contract stipulates for the period 1989-90 that a premium should be paid for work performed after 4:30 p.m.). This will generate measurement errors in the proxy. The accuracy of the proxy can be evaluated for the years 1989-92 when the true definition is available.

The incidence of 2-shift work is much larger in manufacturing and mining than in other sectors. In the other sectors it is more common that daytime workers have shift compensation without formally being 2-shift workers. Thus, this study focuses on manufacturing and mining to minimise the problems with measurement errors in the proxy for initial differences.

A 7 % cut-off level of the SCS (all workers with a SCS over 7 % are classified as 2-shift workers) delivers a (local) minimum of the fraction of daytime workers erroneously classified as 2-shift workers. *Table B1* displays the precision of the proxy during 1989-92; the years during which the procedure can be evaluated.

The expected attenuation bias (due to measurement errors) of the estimate of the initial difference between daytime and 2-shift workers ($\hat{\beta}$) in the absence of other covariates is $\frac{\hat{\beta}}{\beta^{TRUE}} = 1-(\nu+\eta)$.[24] The parameters ν and η denotes the fractions of workers erroneously classified as 2-shift workers and daytime workers. By using the numbers in *Table 3* we get $\nu = 0.205$ and

[24] See Aigner (1973).

$\eta = 0.071$. Thus, one would expect the estimates of the initial difference using the true definition to be 1.4 times the estimate based on the proxy. In principle it is possible to correct for this biased covariate (see Aigner 1973) but the double fixed effects (individuals and sector-years) model makes the implementation difficult.

Table B1. Accuracy of the proxy.

		Proxy		
		Day	2-shift	**All**
True	Day	79.5%	2.9%	**82.5%**
	2-shift	6.1%	11.4%	**17.5%**
	All	**85.6%**	**14.4%**	**100.0%**
Accuracy of the proxy*		92.9%	79.5%	90.9%

Note: Results from the 1989-92 data set used to define manufacturing and mining workers as 2-shift workers if they have a shift compensation share (SCS) of more than 7 %. "True" definitions refer to the original definitions in the data set. *The "accuracy" numbers are calculated as the number of correctly classified workers divided by the total number of workers classified in the category by the proxy, i.e., for Daytime workers 79.5/85.6 for 2-shift workers 11.4/14.4 and for All workers (79.5+11.4)/100.

While noting that the estimates of the initial difference between shift forms will be biased to zero it should be noted that the proxy is quite good. Furthermore, the results presented in the paper (*Section 5*) show that the estimates of the initial difference in wages are quite small. Since the models include fixed effects one may suspect that the measurement-error problems for β_1 are increased. However, while the fixed effects may amplify the attenuation of the estimate of initial differences, they are also reducing the *impact* of these errors on the variable of interest. This is due to the individuals that do not change their shift form as explained in the previous subsection. Using the proxy to study the persistence of the shift forms shows that 81 % of the workers had the same shift form before and after the working time reduction. Note also that this probably is a slight underestimate of the true persistence due to the measurement errors in the proxy.

Importantly, it is shown in *Section 5* that the estimates of the effect of the reduction are unaffected if the proxy is included or excluded. This shows that the differences in wages before the reduction conditional on the individual fixed effects were small, suggesting that the measurement error problems have a relatively minor impact on the variable of interest.

Appendix C: Robustness

C.1 Alternative specifications and wage measures

When identifying the effect of the change in standard hours on 2-shift workers, daytime workers are used to control for time effects that are allowed to differ between industries. The underlying assumption for this identification is that wages of other workers were unaffected by the change in standard hours for 2-shift workers. It is, however, conceivable that other workers in firms that employ 2-shift workers demanded a compensation for the improvements for the 2-shift workers. This would imply that actual wages were rising less for 2-shift workers relative to other workers than if the other workers had been truly unaffected and thus lead to attenuation of the estimated wage pressure effect.

To verify that this is not the case, *Table C1* shows estimates based on equation (19) with different sector definitions. The first column only controls for raw year effects (i.e. there are no sector dummies). The second column is a replication from *Table 1* in the paper with a sector dummy for each contract. The third column has a unique sector dummy for each *combination* of industry level contract, municipality, size of the firm (categorised by nine dummies) and size of the work place (categorised by 10 dummies). We should see systematic differences between these models if other workers were affected indirectly. The estimates for standard hours should be closer to zero the more controls are included since wages for 2-shift workers should have risen more relative to the average daytime worker than relative to daytime workers in the same workplace. The estimates are, however, surprisingly stable (ranging from –0.31 to –0.34), suggesting that the daytime workers were unaffected by the 2-shift workers' working time reduction. Thus, the estimates of the effect of the working time reduction on wage pressure seem valid.

Two theoretically well-defined wage measures can be studied to identify the wage pressure effect of a working time reduction: hourly earnings (total earnings divided by total hours) and the straight-time hourly wage. This paper has followed Hunt (1999) in focusing on the effect on straight-time wages. The main reason for this somewhat arbitrary choice is that it is the measure that gives the weakest results; as is shown below all conclusions are strengthened if the wage measure is changed so as to include other types of compensation.

Table C1. Elasticities of hourly wages and earnings with respect to standard hours

Estimated parameter	Straight time wages			Hourly wages, including overtime premium	Total hourly earnings
	Year effects	Contract and year interactions	Contract, size, municipality and year interactions	Contract and year interac-tions	Contract and year interactions
Standard hours (γ)	-0.309	-0.338	-0.337	-0.409	-0.586
	(0.011)	(0.011)	(0.012)	(0.011)	(0.011)
Initial difference (β_1)	-0.004	-0.005	-0.002	-0.003	0.094
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Number of observations	467768	467768	467768	467768	467768
Number of individuals	94395	94395	94395	94395	94395
Number of time-sector effects	6	330	26358	330	330
Degrees of freedom	373361	373037	347009	373037	373037

Note: The dependent variable is the log of hourly wages or earnings during the second quarter each year. Sample period is 1981-82 and 1989-92. All estimates are based on equation (19). The standard hours estimates are elasticities. The initial difference estimates measure the (constant) effect of being a 2-shift worker at time *t* and are based on the proxy described in *Appendix B*. All regressions include individual fixed effects, year-contract interaction dummies and an age cube. Huber-White standard errors are in parentheses.

To verify the robustness of the results, *Table C1* show estimates based on equation (19) where the overtime premium is included in the wage measure. This standard hours' estimate is somewhat larger (-0.41) than the results presented previously, indicating that the working time reduction resulted in an increase in the fraction of hours that were compensated with an overtime premium. Estimates on total hourly earnings where shift compensation also is included are presented in the final column of *Table C1*. The effect of the working time reduction is now estimated to be even larger in magnitude (-0.59). However, the estimate of the initial difference in hourly earnings, as captured by the

2-shift proxy, indicates that the total earnings of 2-shift workers were 10 % higher than for daytime workers. This should give a negative bias in the standard hours' estimate since the 2-shift proxy does not fully capture the initial difference in wages, which may explain the large estimated effect of the standard hours reduction. Furthermore, the fact that the 2-shift proxy is constructed from an earnings-category that is included in this dependent variable may be problematic suggesting that the estimate should be interpreted with caution.

C.2 Effects relative to other shift workers

The results presented so far crucially hinges on the assumption that daytime workers are a valid control group. This is necessary to control for the wage changes that occur over time independently of the working time reduction. As a test the sensitivity of the results, this section uses 3-shift workers as an alternative control group in.

All workers are included in the estimation even though the effects are measured relative to 3-shift workers.[25] The effect of the working time reduction is measured relative to 3-shift workers by including year effects (by sector) that are separated between daytime workers and (all) shift workers. This is accomplished by interaction of the dummy

$$D_{it}^{s} \equiv D_{it}^{2-shift} + D_{it}^{Disc.3-shift} + D_{it}^{Cont.3-shift},$$

that equals one for all shift workers and zero for daytime workers with the year effects:

$$\begin{aligned} \ln w_{it} = {} & \gamma \ln h_{it}^{s} + D_{it}^{s}\left\{\beta_1 D_{it}^{2-shift} + \lambda D_{it}^{Disc.3-shift} + D_{t}^{Year} D_{it}^{Sector} \beta_2\right\} \\ & + Age_{it}\beta_3 + \alpha_i + \varepsilon_{it} \end{aligned} \tag{C1}$$

[25] The reason is that this increases the number of 3-shift workers that can be included in the estimation since we may use 3-shift workers that are observed as daytime workers before (or after) the reduction and as 3-shift workers after (before) the reduction. Including the daytime workers help to identify the individual fixed effects of 3-shift workers that are observed as daytime workers during some years.

Table C2. Elasticities of hourly wages with respect to standard hours, comparing to 3-shift workers.

Estimated parameter	Straight time hourly wage			Hourly wages, including overtime premium	Total hourly earnings
	Year effects	Industry and year interactions	Industry, size, municipality and year interactions	Industry and year interactions	Industry and year interactions
Standard hours (γ)	-0.264 (0.012)	-0.299 (0.013)	-0.298 (0.013)	-0.334 (0.012)	-0.568 (0.013)
Number of observations	532195	532195	532195	532195	532195
Number of individuals	106331	106331	106331	106331	106331
Number of time-sector effects	12	649	32868	649	649
Degrees of freedom	425844	425207	392988	425207	425207

Note: The dependent variable is the log of hourly wages or earnings during the second quarter each year. Sample period is 1981-82 and 1989-92. All regressions are based on equation (C1) and include individual specific fixed effects, a dummy for each shift form, year-sector interaction dummies (interacted with a dummy for day or shift work) and an age cube. The standard hours estimates are elasticities. Huber-White standard errors are in parentheses.

Results from regressions based the specification of equation (C1) are shown in *Table C2*. The estimates of the effects on straight-time wages are somewhat smaller than, but quantitatively similar to, the effects relative to daytime workers, ranging from –0.26 to –0.30 depending on the definition of the *Sector* dummies. The table also shows estimates on the alternative measures of wages, estimates that are almost identical to those relative to daytime workers. Thus, the impression from the comparison with daytime workers that there was a substantial increase in hourly wages due to the working time reduction is supported, regardless of the choice of covariates and definition of the dependent variable.

Essay III

Age effects in Swedish local labour markets[*]

1 Introduction

The topic of this paper is the labour market effects of changes in the age composition of the working-aged population. Macro and labour economists have been discussing the relationship between the age structure and labour markets for at least 30 years. The general idea has been that more young people should result in a higher unemployment rate since the youth unemployment rate is higher than the average unemployment rate.[1]

Studies of indirect effects have previously focused on identifying "cohort crowding" effects, i.e. the hypothesis that young workers perform worse on the labour market if they belong to large cohorts.[2] These studies, which mainly used time series data and older cohorts as control groups, generally found negative cohort size effects for young workers.

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[1] Perry (1970) is the seminal paper; two more recent examples are Gordon (1982) and Shimer (1998).

[2] See Bloom et al (1987) for a review and Korenman & Neumark (2000) for a recent study.

The methodology used in the "cohort crowding" literature assumes that cohort size only affects the members of that particular cohort. In a recent paper, Shimer (2001) challenges this idea. Studying state level data from the United States, Shimer finds that an increase in the share of young workers in the economy *reduces* the unemployment rate and increases the labour force participation rate. Such beneficial effects from large youth cohorts are observed for *all* age-specific unemployment and participation rates. The effects are particularly strong for *older* workers, which reconciles the results with the cohort crowding literature that used older workers as a control group.

The empirical results are important for several reasons. Regions from which young workers migrate will lose their ability to attract firms if the firms prefer with a younger labour force. Hence, emigration of young workers would worsen the labour market conditions of all remaining workers in the original region. Thus, there are strong implications for policies that affect regional mobility, if these findings are robust. But, naturally, it is necessary to know the underlying mechanisms to fully understand the policy implications.[3]

Standard labour market models such as the matching model (Pissarides, 2000) can not explain these empirical findings. A standard matching model predicts an increase in the unemployment rate when the youth share is increased since young people enter the labour market unmatched and it takes time to find a job. In an attempt to find a consistent explanation for the empirical results, Shimer (2001) develops a matching model ("the Fluid Labour Market hypothesis") with match-specific productivity, on-the-job search and increasing returns to scale in the matching process. He shows that the tendency for young workers to be poorly matched can reduce the expected search costs for firms and, thereby, increase the number of firms (jobs) per worker in equilibrium so that unemployment goes down for all workers.

This paper contributes to the literature by giving additional empirical evidence on the labour market effects of changes in the age structure. The empirical approach used in Shimer (2001) is applied to Swedish local labour market data to study how unemployment and participation rates are affected by the age structure in a different institutional setting. In addition to being of interest in its own right, this should shed some further light on possible explanations for the US experience.

[3] One such issue is whether or not the effects of immigration resemble those of young workers entering the labour market.

To further investigate the relationship between the age structure and the labour market, the paper estimates the effects of other changes in the age structure. This allows us to free the results from an arbitrary restriction on the ages at which a worker should be classified as a young worker. Furthermore it allows us to study the effects of the share of older people on the economy, an issue of growing importance considering the ageing population in many OECD-countries.

The estimates of the effects of large youth cohorts show that young workers benefit from belonging to a large cohort, at least in terms of lower unemployment. This is in line with the results in Shimer (2001) and contradicts the cohort-crowding hypothesis. There are little or no effects on prime aged labour market performance. Quite in contrast to the US experience, however, the Swedish results indicate that large youth cohorts adversely affect the oldest workers.

The models that allow the full age distribution to affect the labour market show that the youth share effects are robust to this alteration. Furthermore, a large share of workers aged 50-60 has a negative impact on labour market performance of most age groups, both in terms of higher unemployment and lower employment.

It is also shown that more of the positive employment effect on young workers from large youth cohorts is manifested in manufacturing and mining than in construction and services. This indicates that local product demand is not the mechanism at work. Furthermore, estimates of youth share effects on tightness are positive, but most of the effect on youth unemployment rates appears to come from a shift in the Beveridge curve. This is consistent with an explanation of based on increased matching efficiency in the youth labour market.

The paper is structured as follows. *Section 2* discusses the data, *Section 3* presents evidence of age-effects on unemployment, labour force participation and employment. *Section 4* gives further evidence by deriving partial effects, and *Section 5* gives some concluding remarks.

2 Data

The data have been collected from various sources. Population data come from Statistics Sweden’s population register (RTB) that contains information on age and the place of residence for all individuals living in Sweden on December 31st each year. These data are available for all years since 1968.

The data on employment come from Statistics Sweden's RAMS register that documents the employment in November of all individuals in the population register. The data are available for each municipality from 1985.

Unemployment and vacancy data come from the National Labour Market Board (AMS). The data contain information on the number of registered vacancies and the number of individuals registered as openly unemployed at an unemployment office. The numbers of unemployed by municipality and age group are measured at the end of November each year to match the employment data as close as possible. The number of unemployed workers has been grouped into the following age categories: 16-19, 20-24, 25-54 and 55-64.[4]

It should be noted that this paper only considers the openly unemployed workers as being unemployed and that the share of workers enrolled in labour market programs in Sweden is quite large.[5] The program participants are treated identically to individuals enrolled in regular education for the purpose of this paper, i.e. they are considered as being out of the labour force.[6] Unfortunately, it is not possible to test the sensitivity of the results in this dimension since age-specific data on the number of program participants at the municipal level are unavailable before 1991.

All the data have been collected at the municipal level. However, some of the municipalities are rather arbitrary administrative divisions of greater labour market regions. Thus, the data have been aggregated up to match Statistics Sweden's definitions of local labour markets (LLM:s). The algorithm that generated the LLM:s uses data on commuting habits to aggregate municipalities with frequent cross-border commuting into one LLM. Thus, using the LLM as the unit of observation should reduce problems with spatial correlation due to commuting.

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[4] The data for the period 1985-90 come from AMS-archives and were grouped this way. The data from 1991 onwards have been constructed from AMS "event database" HÄNDEL The unemployment figures are based on the number of individuals in "applicant-categories" 11-14 and differ somewhat from AMS official unemployment series. However, this seems to be the most consistent way to construct the series.

[5] In fact, Calmfors et al (2002) show that expenditures on active labour market policy as a fraction of GDP was higher in Sweden than in any other country during 1986-95.

[6] From a search theoretical perspective this is probably a good approximation since available evidence shows that the job-search intensity of program participants is much lower than that of the openly unemployed (Calmfors et al, 2002).

Table 1. Descriptive statistics for 109 LLM:s (averages over 1985-99).

Variable	Age group	Mean	Std	Min	Median	Max
Unemployment rate	16-24	0.126	0.032	0.032	0.125	0.214
	16-19	0.090	0.022	0.022	0.090	0.156
	20-24	0.136	0.036	0.037	0.134	0.232
	25-64	0.064	0.020	0.024	0.060	0.143
	25-54	0.061	0.019	0.024	0.058	0.136
	55-64	0.076	0.026	0.024	0.071	0.177
	All (16-64)	0.071	0.020	0.025	0.068	0.147
Labour force participation rate	16-24	0.554	0.035	0.426	0.552	0.694
	16-19	0.304	0.037	0.205	0.300	0.496
	20-24	0.764	0.043	0.645	0.768	0.861
	25-64	0.838	0.025	0.708	0.841	0.901
	25-54	0.890	0.019	0.780	0.893	0.926
	55-64	0.660	0.054	0.454	0.671	0.794
	All (16-64)	0.787	0.023	0.657	0.788	0.857
Employment to population rate	16-24	0.490	0.042	0.341	0.486	0.672
	16-19	0.283	0.038	0.179	0.279	0.486
	20-24	0.666	0.049	0.500	0.662	0.825
	25-64	0.786	0.037	0.614	0.789	0.880
	25-54	0.836	0.029	0.675	0.838	0.903
	55-64	0.612	0.063	0.402	0.624	0.776
	All (16-64)	0.732	0.034	0.565	0.735	0.836
Youth share	16-24/16-64	0.183	0.018	0.133	0.185	0.237
Population	All (16-64)	49 926	125 626	1 930	16 700	1 130 458

Note: The statistics are for the variation between LLM averages over 1985-99.

Statistics Sweden has updated the LLM definitions every five years since 1988. The definition used in this paper is from 1993, the year closest to the middle of the sample period. Thus, the original 284 municipalities are aggregated into 109 LLM:s.[7] Descriptive statistics for the LLM:s are presented in *Table 1*.

[7] The municipality of Nyköping was split in 1992 and parts of the old municipality were included in another LLM according to the 1993 definition. They must however be included in the Nyköping LLM in the analysis in order to get the time series consistent.

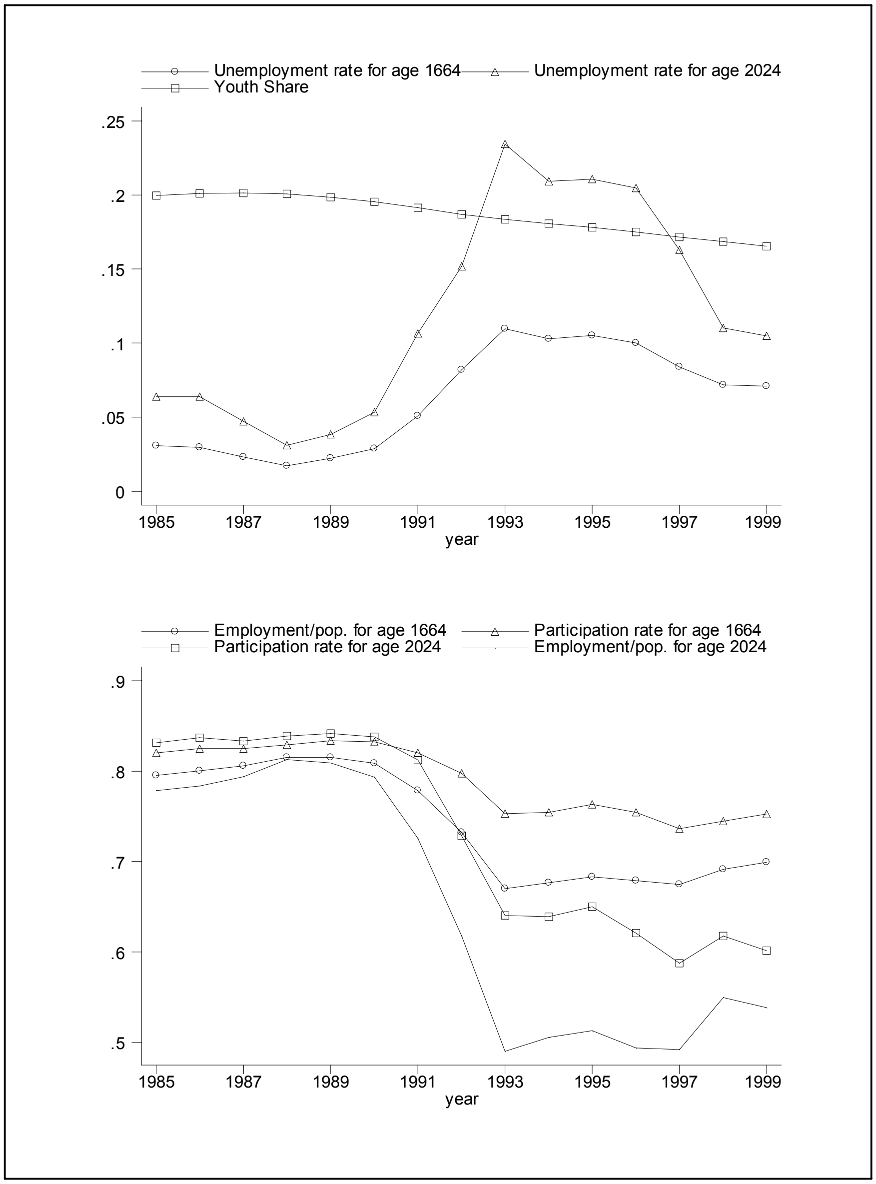

Figure 1. Averages over local labour markets of the youth share and the unemployment, employment and participation rates of all workers and workers aged 20-24.

Figure 1 shows the national averages over time for some of the data used in the paper. Two distinct features can be seen from these graphs: there was a negative trend in the share of young workers, and there was a severe worsening of labour market conditions during the first years of the 1990's. Time dummies are used in the empirical specification to avoid identifying effects from this aggregate pattern.

3 Age structure and unemployment

The starting point of this section is to study how the share of young working aged individuals affects the labour market. This is accomplished by applying an empirical approach similar to Shimer (2001) on Swedish data. The youth share (*YS*) is defined as

$$YS_{it} \equiv \left(\frac{\text{population aged 16 - 24}}{\text{population aged 16 - 64}} \right)_{it}, \tag{1}$$

where i indexes the local labour market and t the year.

Migration could be a potential problem for this study, particularly since some of the local labour markets are quite small (see *Table 1*). It is possible that young workers in Sweden are more mobile than older workers are.[8] To the extent that the mobility is motivated by labour market conditions, we may have problems with reversed causality where low unemployment rates may generate high youth shares. The solution is to use age structure of the 16 years younger population, lagged 16 years, as an instrument to avoid problems of endogenous youth shares.[9] The instrument is equal to the youth share such as it *would have been*, had there been no migration (or deaths) among the relevant cohorts during the last 16 years. Thus, for the youth share in LLM i in year t the instrument is constructed according to the following:

[8] Indeed this is indicated e.g. by Storrie and Nättorp (1997).

[9] Korenman and Neumark (2000) and Shimer (2001) have used lagged birth rates as instrumental variables. Since Swedish municipality-level birth rates only are available from 1968, they can not be used as instruments in this study.

$$\text{Instrument for } YS_{it} \equiv \left(\frac{\text{population aged 0 - 8}}{\text{population aged 0 - 48}} \right)_{i,t-16} \qquad (2)$$

This instrument predicts the future youth share well as is evident from *Table 2* below which shows estimates from first stage regressions and *Figure 2* that plots the youth share against its instrument.

Table 2. Validity of the instruments.

	No fixed effects	Including area and year fixed effects
Estimate	1.025	0.612
(Standard error)	(0.013)	(0.016)
[t-value]	[78.6]	[38.4]
R^2	0.79	0.94

Note: Dependent variable is the youth share, estimates are for the instrument defined in equation (2). Sample is a panel of 109 local labour markets during 1985-99.

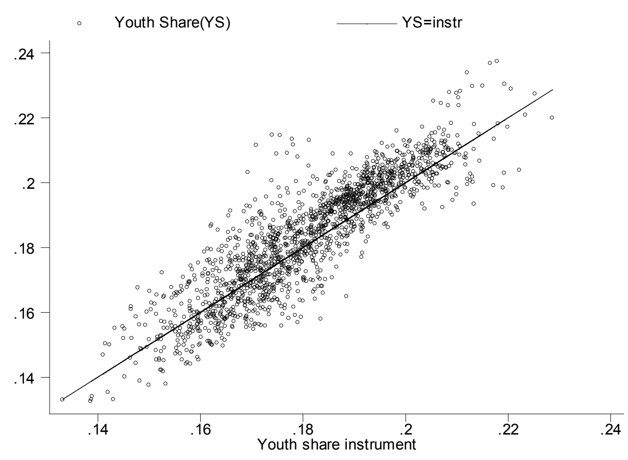

Figure 2. The youth share and the instrument (see equation 2).

3.1 The effects of youth cohort size

The estimates in this section are based on a double fixed-effects (area and year) specification similar to Shimer's (2001). Denoting the unemployment rate for age group *k* by UR^k and the youth share by *YS* yields the model:

$$UR_{it}^{k} = \alpha_{i}^{k} + \beta_{t}^{k} + \gamma^{k} YS_{it} + \varepsilon_{it}^{k} \qquad (3)$$

This model is estimated using the instrument defined in equation (2) with several different dependent variables such as the unemployment rate, the participation rate and the employment to population rate of different age groups (16-19, 20-24, 25-54, 55-64 and 16-64).[10]

Shimer (2001) estimated models where the youth share as well as the dependent variables entered in logarithms. However, this is slightly problematic since the estimates may change if we chose to estimate the effects of the share of older workers instead (the logs of these shares are not perfectly correlated even thought the actual shares are). For the estimates of the youth share effect this should not be a major concern, but the model is not well suited for an analysis where more age groups are allowed to affect the labour market as in *Section 3.3*. The reason is that we know *by definition* that the sum of changes in the population shares *must* equal zero, but this is not true for the logarithms of the shares. Thus, the estimates can be sensitive to the choice of reference group in a logarithmic specification.[11] The base-line model used in this paper is therefore linear.[12]

Estimates of youth share effects on age-specific unemployment, employment to population and participation rates are found in *Table 3*. The estimates have Newey-West corrected standard errors since estimation of equation (3) generates first order autocorrelated residuals (e.g. 0.55 for the average unemployment rate and 0.41 for the unemployment rate of 20-24 year olds). There are no signs of higher order autocorrelation.

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[10] Denoting the number of unemployed by U, employed by E and the population by Pop we get the unemployment rate UR = U/(U+E), the participation rate PR = (U+E)/Pop and the employment to population rate ER = E/Pop.

[11] In principle it is possible to estimate effects of all age groups without a reference group if the shares enter in logarithms, resulting in estimates that cannot be interpreted since the shares by definition always sum to one.

[12] An additional advantage of the linear model is that the autocorrelation problem discussed below is much worse in the logarithmic model.

Table 3. Estimates of the youth share effect.

Age group of dependent variable	Dependent variable		
	Unemployment rate	Labour force participation rate	Employment to population rate
16-19	-0.805** (0.231)	1.320** (0.244)	1.452** (0.234)
20-24	-0.637** (0.217)	1.606** (0.266)	1.927** (0.287)
25-54	0.182* (0.088)	0.310** (0.077)	0.172 (0.107)
55-64	0.495** (0.168)	-0.763** (0.227)	-0.882** (0.183)
All (16-64)	0.167 (0.097)	0.129 (0.088)	0.065 (0.097)

Note: Estimates are for the effects of the youth share defined as the share of 16-64 year old individuals that are 16-24 years old. Regressions are based on IV models (instrument: see equation 2) with fixed area (109 LLM:s) and year effects, see equation (3). Sample period is 1985-99 and sample size is 1635. First order Newey-West corrected standard errors are in parentheses. *Statistical significance at 5 % level. **Statistical significance at 1 % level.

The estimates show that young workers benefit from belonging to a large cohort. Large youth cohorts give lower youth unemployment rates as well as higher participation and employment rates. This is quite in contrast to the "cohort-crowding" hypothesis.

The effects on youth unemployment rates are of quite large magnitudes. An estimate of -1 predicts a 1.8 percentage points increase in the dependent variable if the youth share is increased with one standard deviation. Thus, the estimate for the effect on the unemployment rate of 20 to 24 year olds (-0.64) implies a decrease of the unemployment rate of roughly 1.2 percentage points if the youth share is increased by one standard deviation.

The evidence from the youth cohort size on prime aged workers labour market outcome is incoherent. The estimates point to an increase in the unemployment rate as well as the participation rate. The resulting effect on employment is insignificant and positive.

The oldest workers seem to be adversely affected by large youth cohorts in terms of higher unemployment as well as lower labour force participation and employment.

The effect on the local average unemployment rate, which includes a compositional effect (since younger workers have higher unemployment rates than other workers, see *Table 1*) has a positive sign but is insignificantly different from zero. The effects on participation and employment rates are also positive in sign, but insignificant.

While comparing the results to a null-hypothesis of no effects at all from changes in the age structure is quite natural, it is also possible to compare the results to a null hypothesis of *only compositional effects*. The compositional effects can by calculated by assuming that all age-specific rates are constant. Thus, using the numbers in *Table 1* we get derivatives with respect to the youth share that should equal 0.020 for the average unemployment rate, –0.285 for the average participation rate and –0.295 for the average employment rate if the age-specific unemployment, participation and employment rates are constant.[13] Studying *Table 3* we see that null-hypothesises of only compositional effects of the youth share on average employment and participation rates are rejected. The null hypothesis of only compositional effects on the average *unemployment* rate can, however, not be rejected.

The appendix shows estimates of youth share effects from a variety of different models. The results show that the estimates are robust to many different treatments of the autocorrelation problem, such as including a lagged dependent variable, using an AR (1) correction or aggregating up the data to 5-year averages. It is also shown that the results are robust to a logarithmic specification and to the use of area trends instead of year dummies. Further results also show that the estimated effects are very stable over time. The only caveat is that the estimates do not appear to be very robust to estimation in differences, this is particularly true for the employment rate results for 16-19 year olds.

3.2 Comparing the results to Shimer (2001)

Overall, the estimates presented above confirm the results in Shimer (2001) regarding the effects on young workers of belonging to a large cohort. The youth unemployment rates are decreased and we also see a significant increase in labour force participation and employment. The evidence for prime aged workers on the other hand is mixed and the estimated effects on older workers differ substantially from those estimated in Shimer (2001). The main difference is

[13] The derivatives are calculated according to the following: For the participation rate $dPR/dYS= PR_{16\text{-}24}- PR_{25\text{-}64}$. And for the employment rate $dER/dYS= ER_{16\text{-}24}- ER_{25\text{-}64}$. For the unemployment rate $dUR/dYS= UR_{16\text{-}24}{*}\ PR_{16\text{-}24}/PR_{16\text{-}64}- UR_{25\text{-}64}{*}\ PR_{25\text{-}64}/PR_{16\text{-}64}$.

that older workers appear to be adversely affected by large youth cohorts in Sweden – whereas they benefit in the US.

This section replicates the model from Shimer (2001) as closely as possible to ensure that the difference in results is not driven by differences in specifications. The specification of Shimer (2001) has the youth share, the instrument and the dependent variable entering in logarithms. Furthermore, it uses an FGLS AR (1) correction to deal with the autocorrelation problem. Thus, denoting the estimated autocorrelation parameter by $\hat{\rho}$, the estimated model can be written as: [14]

$$\ln UR_{it}^{k} - \hat{\rho} \ln UR_{it-1}^{k} = \alpha_{i}^{k} + \beta_{t}^{k} + \gamma^{k} \left(\ln YS_{it} - \hat{\rho} \ln YS_{it-1} \right) + \varepsilon_{it}^{k} \qquad (4)$$

Results are presented in *Table 4*.[15] The table reproduces some results from Shimer (2001) for comparison. Estimates for males only are used for the age groups where only gender-separated results were reported. The only difference between the estimated models is that the Swedish model uses the log of the lagged population structure as the instrument whereas the model of Shimer (2001) uses the log of lagged birth rates. The table clearly shows that the estimated effects in the two countries are similar for young workers whereas they do differ for older workers. The difference in results is largest for the oldest age group.

[14] The FGLS procedure used for the estimation is Cochrane-Orcutt (Green, 1997, p. 748-49).

[15] Some small LLM:s do not have unemployed people in all age groups in all years, resulting in missing values when the unemployment rate is in logarithms (22 cases for 16-19 year olds, 2 cases for 20-24 year olds and 2 cases for 55-64 year olds). These missing values have been imputed to equal the minimum observed value of the unemployment rate in that age group (e.g. 0.0024 for 16-19 and 0.0029 for 20-24 year olds) to avoid problems of an endogenously unbalanced panel.

Table 4. Estimates of the youth share effect: logarithmic AR(1) specifications.

Age group of dependent variable	ln(Unemployment rate) Sweden	ln(Unemployment rate) *USA (Shimer, 2001)*	ln(Participation rate) Sweden	ln(Participation rate) *USA (Shimer, 2001)*
16-19	-2.912** (0.707)	*-1.012** *(0.512)*	0.136 (0.245)	*0.565*** *(0.145)*
20-24	-1.549** (0.539)	*-2.180*** *(0.419)*	0.325** (0.092)	*0.197*** *(0.044)*
25-54	-0.673 (0.437)	*-2.346*** *(0.356)*	0.040* (0.020)	*0.068*** *(0.024)*
55-64	0.193 (0.486)	*-3.994*** *(0.725)*	0.001 (0.058)	*0.179** *(0.075)*
All (16-64)	-0.269 (0.409)	*-1.807*** *(0.307)*	-0.022 (0.027)	*0.102*** *(0.035)*
Observations	1526	*784-882*	1526	*784-882*

Note: Estimates are for the effects of the *log of* the youth share defined as the share of 16-64 year old individuals that are 16-24 years old. Regressions are based on IV models with fixed area (109 LLM:s or 51 States) and year effects, see equation (4), instruments are the logarithm of equation 2 for Sweden and the logarithm of average birth rates lagged 16-24 years for the US. The models are AR(1) corrected, see equation (4). Sample period for Sweden is 1985-99. Estimates for the US from Shimer (2001) are for males only (except for the 25-54 year olds), based on a state level panel, sample period is 1978-96 with some missing values. Standard errors are in parentheses. *Statistical significance at 5 % level. **Statistical significance at 1 % level.

3.3 The effects of older cohorts

The results in *Section 3.1* and *3.2* showed that Swedish and US data generate similar estimates of youth share effects on the labour market outcomes of young workers. Meanwhile, estimates of youth share effects on the outcomes of older workers differed substantially.

A possible explanation for the differences in estimates between Sweden and the US is that the correlations between the youth share and other demographic changes might differ between the two countries. This could be illustrated by the following hypothetical example: Suppose that a large share of young workers also is associated with a large share of 35 to 45 year old workers. Assume further that this age group has a lower propensity to be unemployed than other workers in the age interval 25 to 54 do. This would imply that compositional changes *within this age group* that are correlated with the youth share will generate a negative bias on the youth shares estimates for the age group 25 to 54.

The ideal situation for identifying the effects of changes in the youth cohort size is when the youth share is uncorrelated with changes in the age structure *within* the two respective groups (young and old workers). This is not necessarily the case, and we may get misleading results if the labour market is affected by demographic changes within the two groups as well.

Figure 3 shows the correlations between the relative size (population share) of each one-year age group and the instrument for the youth share. The population share (for age j=16,17,..,64) is defined as:

$$S_{it}^{j} \equiv \left(\frac{\text{population aged } j}{\text{population aged 16 - 64}} \right)_{i,t} \tag{5}$$

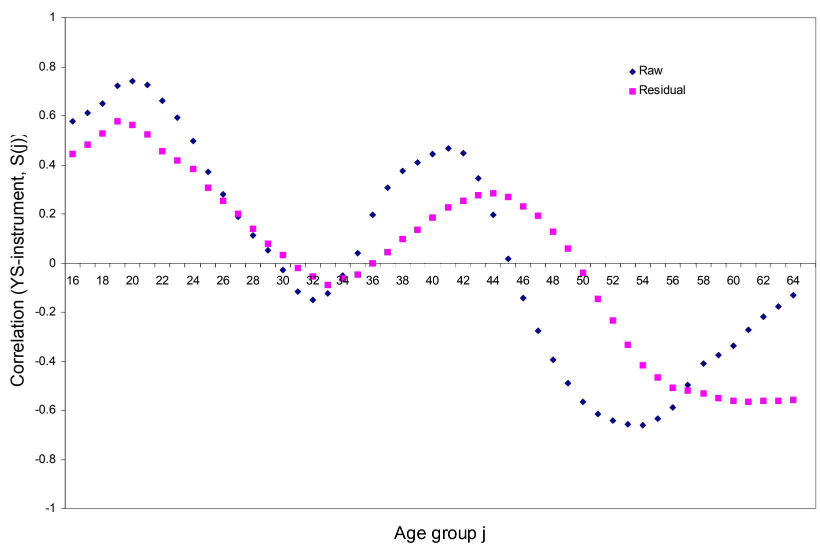

Figure 3. The correlations between the relative size of each age group and the instrument for the youth share (see equation, 2). Correlations are for the raw data and for residuals from regressions on area and year fixed effects.

The figure shows that the instrument for the youth share is positively correlated not only with the share of young workers, but also with the shares of 35 to 45 year-olds.[16] This is true both for the raw correlations and for the residuals after removing the fixed effects. The positive correlation is perhaps not surpris-

[16] Previous versions of this paper included a similar figure for the correlations between the actual youth share and the one-year population shares. That figure was close to identical to the one presented here.

ing since these are the most likely age groups of the young workers' parents. Interesting to note is the strong cyclical pattern in the raw data where there seem to be peaks with 20 year intervals.

The correlation structure is important since we expose ourselves to the risk of mixing youth share effects with effects of the population shares of older age groups if those are unaccounted for in the empirical model. Thus, the remainder of this section studies the effects of the entire age distribution on the labour market to assess the robustness of the results presented earlier.

Studying the outcomes of group *k*, and using the population shares S^{j} (j=16,…64) as explanatory variables, we have the model:

$$UR_{it}^{k} = \alpha_{i}^{k} + \beta_{t}^{k} + \sum_{j=16}^{64} \gamma_{j}^{k} S_{it}^{j} + \varepsilon_{it}^{k} \tag{6}$$

A normalisation is required since the population shares always sum to one. One convenient reference point is to restrict the sum of the estimates to zero:

$$\sum_{j=16}^{64} \gamma_{j}^{k} = 0\,. \tag{7}$$

In practice it is difficult to estimate all the 49 population share parameters separately due to their inherent colinearity. There are two different solutions to this problem in the literature, use wider age groups or restrict the estimates to follow a polynomial functional form (see Fair & Dominguez, 1991 and Higgins, 1998). The second strategy is followed here due to the availability of high quality data on the size of each one-year age group. However, the strategies yield very similar results. It is assumed that the pattern of the population share parameters can be approximated by a fourth order polynomial functional form in age.[17] This gives a set of 49 linear restrictions on the original parameters according to

$$\gamma_{j} = a + b\cdot j + c\cdot j^{2} + d\cdot j^{3} + e\cdot j^{4}\ ,\ \ j = (16,....,64)\,. \tag{8}$$

[17] The choice of a fourth order restriction is based on the observation that many of the estimates show signs of a third order functional form with one min and one max. Allowing for one additional parameter should ensure that this pattern is not generated by the imposed restriction.

Equation (6) is estimated after the data has been transformed according to the normalisation (7) and the set of linear restrictions (8). The transformations are trivial since all restrictions are linear, see Fair and Dominguez, (1991) for details. This leaves the parameters, *b* to *e*, to be estimated.[18] After estimation it is possible to recover the original parameters (γ_{16}-γ_{64}) with standard errors from equation (8).[19]

The issue of endogenous migration that may change the population structure is still a potential problem. To avoid this problem, instruments that correspond to the youth share instrument defined in equation (2) are used, i.e. a 16 years lagged measure of 16 years younger population:

$$\text{Instrument for } S_{it}^{j} \equiv \left(\frac{\text{population aged } j-16}{\text{population aged 0 - 48}} \right)_{i,t-16} \tag{9}$$

All estimates in this section are based on IV models with fixed area and year effects and Newey-West corrected standard errors, but using an AR(1) correction instead would yield very similar results. Estimates are displayed graphically in *Figure 4*. On the horizontal axis are the age groups (j = 16,..,64) and the vertical axis displays the estimates (the γ_j:s) of the corresponding population share effect. The estimates should be interpreted with the normalisation of equation (7) in mind, i.e. that they always sum to zero. Thus, a significant positive employment rate estimate for age group j (i.e. $\gamma_j > 0$) implies a positive effect on employment if the share of j years old workers (S^j) is increased and all other shares are reduced correspondingly.

The panels of *Figure 4* show that the effects on the two outcome variables, the unemployment rate and the employment to population rate, are mirror images. The age groups that have a negative effect on unemployment also have a positive effect on employment in most cases. The estimates also show that the estimated effects of young workers presented in *Section 3.1* remain largely unaffected by the inclusion of other age groups in the empirical analysis.

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[18] The parameter *a* is derived using (7).

[19] The standard errors are calculated directly from equation (8) after estimation of the parameters *a* to *e* using the covariance matrix of these estimates; this is possible since the j:s of the polynomial restriction are nonstochastic.

A large share of workers in the age groups 50 to 60 has an adverse effect on the outcomes of most workers, both in terms of higher unemployment and lower employment. The only exception is the employment rate of the oldest age group (aged 55-64) that increases with the number of 55 year old workers. This is probably a compositional effect since the labour force participation of this group is declining sharply with age.

It is worth noting that there are very small effects of workers in the age groups closest to retirement. This is perhaps surprising; at least if we are willing to view the population shares of these age groups as a proxy for the outflow from the labour market.

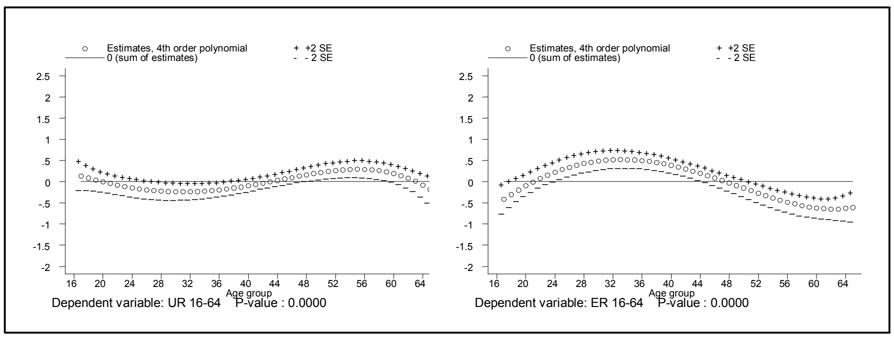

Figure 4a. Estimates of population share effects of the age groups 16, 17,..,64 on average unemployment (UR) and employment (ER) rates. All estimates are based on IV models (instrument: 16 years lagged population shares, see eq. 9). Estimates are restricted to sum to zero and follow a fourth order polynomial form (see eq. 7 and 8). The panels show 2 standard error intervals (Newey-West corrected). P-values are for F-tests of the joint significance of the population share parameters.

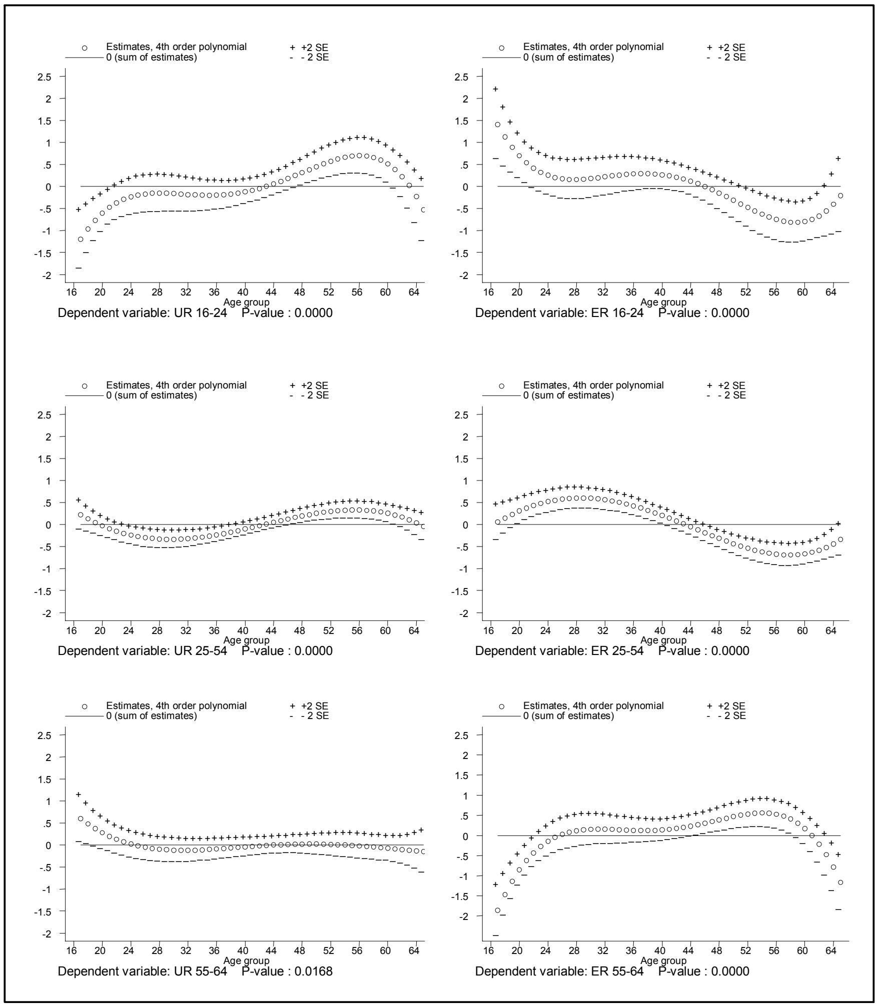

Figure 4b. Estimates of population share effects of the age groups 16, 17,..,64 on age-specific (16-24, 25-54 and 55-64) unemployment (UR) and employment (ER) rates. All estimates are based on IV models (instrument: 16 years lagged population shares, see eq. 9). Estimates are restricted to sum to zero and follow a fourth order polynomial form (see eq. 7 and 8). The panels show 2 standard error intervals (Newey-West corrected). P-values are for F-tests of the joint significance of the population share parameters.

As for the interpretation of the results it is clear that the adverse effects from large shares of 50-60 years old workers could be reconciled with the matching theory of Shimer (2001). That theory predicts that the labour market should perform worse the more well-matched individuals there are, and 50-60 year old workers are probably the most well-matched of all. However, the fact that the estimates displayed in *Figure 4* show signs of age effects other than the youth share effects on the labour market outcomes raises an important question regarding the results in Shimer (2001). The question is to what extent demographic changes that are correlated with the youth share (as well as the lagged birth rate that is used as the instrument) are driving the results. Such correlations are indeed bound to appear due to the fact that people tend to have children during a limited age-span, which in the Swedish case generates the pattern shown in *Figure 3*.

4 Partial effects

This section presents further evidence by studying the effects of demographic changes on earnings, sector specific earnings and employment and by decomposing the effect into shifts of, and movements along, the Beveridge curve.

4.1 Employment and earnings by sector

One possible explanation for the positive effects of large youth cohorts on youth labour market performance shown in *Section 3* is an increase local product demand in sectors that employ many young workers. A test of this hypothesis is to study the effects on employment and earnings in different industries.

The data used in this section is constructed from the same micro data as the data on employment used earlier on in the paper. However, Statistics Sweden generated the data separately for Dahlberg and Forslund (1999) and the last two years were added on afterwards. The sample period is therefore one year shorter (1985-98), and the data are divided into slightly different age groups: 18-24, 55-65 and all workers aged at least 16.

Table 5 displays employment-effects from an increase in the youth share on overall employment and separately for three sectors; manufacturing, construction as well as retail and wholesale. It is reasonable to think that manufacturing to a large extent serves a market outside the local labour market area whereas construction as well as retail and wholesale are more locally oriented. Thus, manufacturing should be less affected if the employment effect for young workers is driven by local product demand.

The most notable feature both in terms of youth employment and overall employment is that the manufacturing and mining sector have expanded. The effect is clearly strongest for the young workers. Construction and retail and wholesale employment are either negatively affected, or not affected at all.

The results show that the increase in employment mainly is manifested in the manufacturing sector. Thus, a construction boom, or any other expansion of local product demand, can not readily explain the results. This is in line with the results for the US presented in Shimer (2001).

Since the estimated effects show signs of an increase in the employment for young workers it is natural to ask for the effects on wages. Unfortunately, local wage-level data is not available. Thus, we are restricted to studying effects on annual labour *earnings* for different age groups.

Table 5. Estimates of sector specific youth share effects.

Estimate	Employment rate			ln(Earnings)		
	18-24	55-65	All (16+)	18-24	55-65	All (16+)
All sectors	1.660** (0.284)	-1.011** (0.201)	-0.093 (0.117)	-0.698 (0.367)	-0.035 (0.195)	-0.411** (0.151)
Manufacturing, mining	2.051** (0.307)	0.081 (0.120)	0.761** (0.128)	-0.708 (0.588)	-1.106** (0.354)	-0.533* (0.215)
Construction	-0.072 (0.068)	-0.107** (0.039)	-0.167** (0.032)	-1.845* (0.825)	-0.312 (0.739)	-0.067 (0.283)
Wholesale, retail and communications	-0.015 (0.182)	-0.001 (0.062)	0.063 (0.053)	-0.806 (0.527)	-0.507 (0.430)	-0.780** (0.169)
Observations	1526	1526	1526	1526	1526	1526

Note: Regressions are based on IV models (instrument: see eq., 2) that include fixed area and year effects (equation, 3). The sample consists of 109 local labour markets during 1985-98. Dependent variables are the employment rate and the log of average earnings of different age groups by sector. Newey-West corrected standard errors are in parentheses. *Statistical significance at 5 % level. **Statistical significance at 1 % level.

Estimates displayed in the top row of *Table 5* show, as expected (since young workers earn less), that an increase in the share of young workers is associated with a fall in average earnings. However, in contrast to the US experience of rising age-specific wages, we do not see a positive effect on age-specific earnings in the Swedish data, rather there are negative but insignificant estimates for both younger and older workers.[20]

The sector-specific earnings estimates for young workers show, just as the estimates for the average effect did, that young workers earnings are largely unaffected by the youth share except for a drop in construction earnings. Earnings for older workers and average earnings are decreased in the manufacturing sector.

It should be noted that the measure of annual earnings is far from perfect. Earnings by sector are calculated as the total annual earnings by individuals employed in the specific sector in November. This implies that variations in the number of weeks worked during the year will have a very large effect on the estimates. The sign of the bias depends on whether the fraction of November-employed workers that spend parts of the year without employment, or as employed in other sectors, is increased or decreased with the youth share. Andersson (1999) shows that job reallocation is counter-cyclical within Swedish manufacturing, suggesting that the bias is positive, though it is not obvious to what extent business cycle results can be generalised to this kind of supply chocks.

4.2 Tightness and the Beveridge-curve

The model in Shimer (2001) is based on a search theoretical framework. It modifies the standard matching model by introducing on-the-job search and match-specific productivity. Furthermore, there is random matching between all workers and firms instead of between unemployed workers and vacancies as in the standard model. The empirical observation that large youth cohorts are beneficial for all workers is explained as an increasing-returns-to-scale phenomena, where new entrants accept more matches, thus improving the matching process. This reduces firms costs of opening vacancies when youth cohorts are large and, as a result, the labour market will be tighter in equilibrium.

In a standard matching model (Pissarides, 2000), tightness $(\theta = \text{Vacancies}\,(\text{V}) / \text{Unemployed}\,(\text{U}))$ is determined from a free entry condition for firms and from wage bargaining. Equilibrium tightness will be a posi-

[20] Edin & Holmlund (1995) show, using time series data for Swedish manufacturing, that youth wages are decreased relative to prime aged wages when the youth share is increased. Their specification is however somewhat different in the sense that the dependent variable is relative wages and the only control variable is a time trend.

tive function of matching efficiency (ζ) and a negative function of the separation rate (s).

The model determines the unemployment rate at a given tightness from the flow equilibrium[21] (the *Beveridge curve*) in the labour market:

$$u = s / \left[s + \zeta \cdot p\left(\theta(\zeta, s)\right) \right] \tag{10}$$

where $\zeta p(\theta)$ is an unemployed workers probability of finding a job.

Thus, under the assumption that matching efficiency is a function of the youth share, it is possible to decompose the effects of demographic changes into two parts using a log-linear approximation. This gives one effect through changes in the tightness of the labour market and one effect for a given tightness (i.e. an effect through shifts in the Beveridge curve). The youth share will both increase tightness and shift the Beveridge-curve inward if it improves the matching efficiency on the labour market (i.e. if it has a positive effect on ζ).

Focusing on the youth share effect we may write:

$$\frac{d \ln UR^k}{d \ln YS} = \left. \frac{d \ln UR^k}{d \ln YS} \right|_{\theta} + \frac{d \ln UR^k}{d \ln \theta} \frac{d \ln \theta}{d \ln YS} . \tag{11}$$

Note that the expression on the left-hand side, i.e. the overall effect, is the coefficient (γ) that was estimated in *Sections 3.1* and *3.2*. For convenience we may denote the youth share effect on unemployment at a given tightness by η, the effect of tightness on age-specific unemployment ϕ and the youth share effect on tightness λ and thus rewrite equation (11) as:

$$\gamma^k = \eta^k + \phi^k \lambda . \tag{12}$$

It is possible to estimate the three right-hand side parameters from two equations. The effect on tightness is given by:

$$\ln(\theta)_{it} = \alpha_i + \beta_t + \lambda \ln(YS)_{it} + \varepsilon_{it} . \tag{13}$$

[21] The equilibrium condition is that the inflow into unemployment (1-u)s is equal the outflow from unemployment $u\zeta p(\theta)$.

This equation is common to all age groups (assuming that they all search on a common market). Secondly, we may estimate the effect of tightness on unemployment ϕ^k and the youth share effect for a given tightness η^k:

$$\ln(UR)_{it}^k = \alpha_i^k + \beta_t^k + \eta^k \ln(YS)_{it} + \phi^k \ln(\theta)_{it} + \varepsilon_{it}^k . \quad (14)$$

By estimating the η^k:s we get estimates of the youth share effects at a given tightness, i.e. of shifts in the Beveridge-curves.

Two alternative definitions of tightness are used: vacancies per unemployed and vacancies per labour force participant.[22] The standard definition of tightness is vacancies per unemployed but the model in Shimer (2001) assumes that matching takes place between vacancies and all labour force participants, employed or unemployed.

Estimates of youth share effects on tightness (λ) are displayed in the top row of *Table 6*. They show that an increase in the youth share gives a tighter labour market. The estimates are however insignificant at the 5 % level regardless of how tightness is defined. Using unemployment as the denominator yields a slightly lower p-value (6.5 %) than using the size of the labour force (7.2 %).

Further estimates in *Table 6* are based on equation (14), with the unemployment rate as the dependent variable and tightness and the youth share as independent variables in each regression. It is clear from the estimates that the youth share effect at a given tightness is very close to the overall effect. Thus, the main part of the effect on youth unemployment seems to work through a shift in the Beveridge-curve rather than through movements along the curve.

The estimates in *Table 6* give mixed support for the Shimer-model. On the one hand, we see both an increase in tightness and an inward shift of the Beveridge curve (for young workers), just as we would expect from improved matching efficiency. On the other hand, the effect through tightness is not the most important one (which can explain why older workers do not benefit at all) as was hypothesised by Shimer. Rather, the main effect is the shift in the Beveridge curve. Thus, the effect should work through factors that affect the flow equilibrium that underlies the Beveridge-curve, such as the search intensity of the young workers. Some caution is however warranted when interpret-

[22] Vacancies per working aged inhabitant in the area would give almost identical results.

ing the estimates since tightness is measured with error due to the fact that only vacancies reported to the unemployment office can be observed.

Table 6. Estimates of partial youth share effects.

Dependent variable	Independent variable	Overall effect		Partial effects		
		Equation (3)		Equations (13) and (14)	θ≡V/U	θ≡V/LF
θ (Tightness)	YS (Youth share)		--	λ	1.579 (0.856)	1.265 (0.702)
UR 16-24	YS	γ	-1.081* (0.432)	η	-0.703 (0.367)	-0.924* (0.426)
	θ		--	ϕ	-0.240** (0.017)	-0.125** (0.019)
UR 55-64	YS	γ	0.226 (0.399)	η	0.450 (0.363)	0.281 (0.398)
	θ		--	ϕ	-0.142** (0.020)	-0.044** (0.016)
UR 16-64	YS	γ	-0.324 (0.359)	η	-0.008 (0.297)	-0.209 (0.355)
	θ		--	ϕ	-0.200** (0.018)	-0.091** (0.015)
Observations			1635		1635	1635

Note: All estimates are based on IV models (instrument: log of eq., 2) with fixed area and year effects and Newey–West corrected standard errors. All variables enter in logarithms. Sample period is 1985-99. UR is the unemployment rate, θ is tightness, V vacancies, U the number of unemployed and LF the size of the labour force. Standard errors are in parentheses. *Statistical significance at 5 % level. **Statistical significance at 1 % level.

5 Concluding remarks

The paper has studied effects on the labour markets of changes in the age distribution using a panel of Swedish local labour markets between 1985 and 1999. The empirical results showed that labour market performance is affected by the composition of the working-aged population.

In contrast to the cohort-crowding hypothesis, the results show that young workers benefit from belonging to a large cohort. This is in line with results from the US presented in Shimer (2001). Large youth shares do however not appear to have any positive effects on the older workers, which is in contrast to the US experience. In fact, the results indicate that large youth cohorts may have an adverse effect on the oldest workers.

The estimated youth share effects are robust to models that simultaneously estimate the effects of other demographic changes. In addition, 50 to 60 year old workers are estimated to have an adverse effect on the outcomes of most workers, both in terms of higher unemployment and lower employment. This is consistent with the hypothesis in Shimer (2001) that well-matched workers are congesting the matching process. However, the fact that demographic changes unrelated to the youth share appear to have an effect on the labour market indicates that the US youth share estimates may change if these demographic changes are accounted for.

Some partial effects of changes in the youth share are derived in an attempt to get some guidance as to the relevance of possible explanations for the results. It is shown that it is unlikely that the positive effects for young workers is driven by local product demand effects since the major employment effect is in manufacturing, rather than in construction and other local services.

Some further support for a notion that a large youth share reduces youth unemployment through increased matching efficiency is found. The youth share is estimated to have a positive effect on tightness (although with a p-value just over 5 %), but most of the effect on youth unemployment appears to come from an inward shift in the Beveridge-curve. This is consistent with an explanation of increased matching efficiency for young workers.

The results presented in this paper are consistent with the hypothesis from Shimer (2001) that large youth cohorts tend to increase matching efficiency at the youth labour market. Some results also indicate that this is true at the prime aged labour market. Thus, it is perhaps anomalous that the reverse appears to be true at the labour market for the oldest age group.

One interesting feature that separates the older Swedish unemployed from most unemployed workers in the US as well as most young unemployed Swedish workers is the duration of an average unemployment spell. Long-term unemployment is much more common among the older workers than among younger workers in Sweden.[23] It is possible that the mechanisms underlying the experiences of the long-term unemployed differ from those of the short-term unemployed for whom the logic of the matching function may apply more readily. This may be one explanation for the differences in results but more re-

[23] See e.g. Ackum Agell et al (1995).

search is clearly needed to clarify the mechanisms underlying the results presented in this paper

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Appendix: Robustness of the youth share estimates

Given that the high degree of autocorrelation in the residuals may be of some concern, *Table A1* shows estimates of alternative specifications to further assess the robustness of the results. The first column is a replication of the Newey-West corrected model from *Table 3* in the body of the paper.

The second column shows estimates based on 5-year averages of the variables. This model produces results that are very similar to those of the original model. This is quite reassuring, since the youth share by construction is a slow moving variable.

Yet another solution to the autocorrelation problem is to introduce a lagged dependent variable. Estimates in the third column of *Table A1* show that the long run estimates from such a dynamic model are almost identical to the Newey-West corrected estimates in the first column. The same is true for the AR (1) corrected FGLS estimates in the fourth column of the table. It should be noted however that the estimates with a lagged dependent variable require a large T to be consistent (in this case T=14). With this caveat in mind, it is clear that the estimates are robust to four different treatments of the autocorrelation problem: Newey-West correction of the standard errors, AR (1) corrected FGLS-estimation, aggregation to 5-year averages and the inclusion of a lagged dependent variable.

The models have also been estimated in differences. This does change the results somewhat. The estimated youth share effects on the unemployment rates as well as the employment rates of young workers become insignificant. The unemployment rate estimate for 20-24 year olds and the employment rate estimate for 16-19 year olds also change sign. Including area specific trends by allowing for a fixed area effect after differencing the data, gives similar results.

Table A1. Estimates of the youth share effects, alternative treatments of the autocorrelation problem.

Dep. variable		*Basic model*	5 year averages	Lagged dep. variable (long run estimates)	FGLS	Differ-ences	Differences with area trends
Variable	Age group						
UR	16-19	*-0.805*** *(0.231)*	-0.676* (0.317)	-0.791** (0.284)	-0.741** (0.285)	-0.283 (0.722)	-0.060 (0.851)
	20-24	*-0.637*** *(0.217)*	-0.902** (0.285)	-0.708** (0.267)	-0.575* (0.257)	0.295 (0.468)	0.662 (0.550)
	25-54	*0.182** *(0.088)*	0.154 (0.113)	0.209 (0.114)	0.237* (0.099)	0.257 (0.148)	0.321 (0.174)
	55-64	*0.495*** *(0.168)*	0.551** (0.178)	0.454* (0.184)	0.540** (0.160)	0.702** (0.233)	0.892** (0.273)
	All (16-64)	*0.167* *(0.097)*	0.130 (0.115)	0.193 (0.119)	0.241* (0.104)	0.372* (0.154)	0.501** (0.180)
ER	16-19	*1.452*** *(0.234)*	1.516** (0.372)	1.534** (0.375)	0.967** (0.314)	-0.065 (0.431)	-0.584 (0.504)
	20-24	*1.927*** *(0.287)*	2.134** (0.490)	2.331** (0.523)	1.253** (0.352)	0.677 (0.420)	0.210 (0.484)
	25-54	*0.172* *(0.107)*	0.234 (0.162)	0.167 (0.204)	-0.153 (0.120)	-0.146 (0.132)	-0.330* (0.152)
	55-64	*-0.882*** *(0.183)*	-1.015** (0.261)	-0.766** (0.292)	-0.346 (0.176)	-0.339 (0.194)	-0.053 (0.221)
	All (16-64)	*0.065* *(0.097)*	0.120 (0.164)	0.026 (0.214)	-0.310** (0.116)	-0.350** (0.124)	-0.524** (0.142)
Standard errors		*NW*	Uncorrected	Delta	AR (1)	Uncorrected	Uncorrected
Observations		*1635*	327	1526	1526	1526	1526
Column		(1)	(2)	(3)	(4)	(5)	(6)

Note: Estimates are for the effects of the youth share defined as the share of 16-64 year old individuals that are 16-24 years old. Regressions are based on IV models (instrument: see equation 2) with fixed area (109 LLM:s) and year effects, see equation (3). Sample period is 1985-99. UR (ER) is the unemployment (employment to population) rate. *Column (1)* is a replication from *Table 3*, standard errors have been Newey West-corrected. *Column (2)* has the variables entering as averages over 5-year periods. *Column (3)* includes a lagged dependent variable, the estimates are for the long run effect, standard errors are calculated by the delta-method. *Column (4)* is estimated by the Cochrane-Orcutt procedure to correct for 1st order autocorrelation. *Column (5)* is estimated in first differences. *Column (6)* is estimated in first differences with area trends as fixed area effects after the first differencing. Standard errors are in parentheses. *Statistical significance at 5 % level. **Statistical significance at 1 % level.

Table A2 shows estimates based on a few alternative models. The first column shows OLS estimates of the youth share effect. The most notable features of this column is that the displayed estimates are smaller in size and less significant. In addition we see that the employment rate estimate for 20-24 year olds is positive in the OLS specification. This difference between the IV and OLS estimates is probably explained by the fact that many young individuals move before entering university, generating low participation rates in areas with high *actual* youth shares.

The second column of *Table A2* shows estimates from a model without area fixed effects. The purpose of estimating this model is to show to what extent the estimates are driven by the fixed area-effects. The results show that the effects for young workers are independent of whether or not these fixed effects are included, whereas the estimates for older workers change signs and become significant. The third column shows estimates that include area specific trends instead of year effects, and the estimates are very similar to those of the original model.[24]

The last three columns show estimates of a logarithmic model where the youth share and the instrument as well as the dependent variables enter in logarithms. Only the sign and significance of each estimate can be compared to the basic linear model since the interpretation of the estimates changes with the functional form. The logarithmic model is also estimated in differences with and without trends because of the sensitivity of the linear model to this alteration. All of these estimates support the impression that the youth unemployment rate is lower the higher the youth share is and that the effect on older workers have the reverse sign. The employment rate estimates are less robust to the differencing of the data, especially for the 16-19 year olds.

[24] An alternative to the rather crude trends is to introduce control variables based on the interaction between the area fixed effect and the average value of the dependent variable in the rest of the country (thus allowing for an area-specific impact of aggregate shocks). The inclusion of these control variables does not change the results.

Table A2. Estimates of the youth share effects, alternative specifications.

Dep. variable		OLS	No fixed area effects	Area trends	Logarithmic model		
Variable	Age group				Levels	Differences	Differ-ences with area trends
UR	16-19	-0.274 (0.161)	-0.932** (0.138)	-0.998** (0.129)	-2.720** (0.729)	-4.718** (1.787)	-5.198* (2.108)
	20-24	-0.278 (0.143)	-1.895** (0.158)	-1.205** (0.142)	-0.891* (0.438)	-1.071 (0.718)	-1.069 (0.843)
	25-54	0.089 (0.054)	-0.980** (0.082)	-0.130* (0.051)	-0.670 (0.381)	0.047 (0.544)	0.291 (0.638)
	55-64	0.073 (0.099)	-1.280** (0.125)	-0.116 (0.059)	0.226 (0.399)	1.413* (.690)	1.838* (0.814)
	All (16-64)	0.067 (0.059)	-1.098** (0.088)	-0.210** (0.057)	-0.324 (0.359)	0.329 (0.467)	0.598 (0.548)
ER	16-19	0.617** (0.173)	1.912** (0.217)	2.576** (0.156)	0.878** (0.208)	-0.439 (0.330)	-0.986* (0.385)
	20-24	-0.159 (0.244)	1.478** (0.235)	3.124** (0.183)	0.556** (0.092)	0.153 (0.128)	0.008 (0.148)
	25-54	-0.078 (0.068)	1.151** (0.122)	0.675** (0.064)	0.029 (0.027)	-0.039 (0.030)	-0.080* (0.035)
	55-64	-0.138 (0.106)	3.359** (0.225)	-0.226** (0.065)	-0.353** (0.067)	-0.110 (0.061)	-0.006 (0.069)
	All (16-64)	-0.256** (0.072)	1.684** (0.144)	0.771** (0.074)	-0.017 (0.030)	-0.107** (0.033)	-0.152** (0.038)
Standard errors		NW	NW	NW	NW	Uncorrected	Uncorrected
Observations		1635	1635	1635	1635	1526	1526
Column		(1)	(2)	(3)	(4)	(5)	(6)

Note: Estimates are for the effects of the youth share defined as the share of 16-64 year old individuals that are 16-24 years old. Regressions are, *except otherwise noted below*, based on IV models (instrument: see equation 2) with fixed area (109 LLM:s) and year effects, see equation (3). Sample period is 1985-99. UR (ER) is the unemployment (employment to population) rate. *Column (1)* is estimated by OLS. *Column (2)* is estimated without the fixed area effects. *Column (3)* includes area-specific trends instead of the fixed year effects. Columns (4) to (6) has the youth share, its instrument and the dependent variable entering in logarithms. *Column (5)* is estimated in first differences. *Column (6)* is estimated in first differences with area trends as fixed area effects after the first differencing. Standard errors are in parentheses. *NW* indicates that standard errors have been Newey-West corrected for 1st order autocorrelation. *Statistical significance at 5 % level. **Statistical significance at 1 % level.

A possible complication is that changes in the youth share may be spuriously correlated with structural change that disfavour some regions at particular times. A structural shock index was constructed in an attempt to control for this possibility.[25] The index was used as an additional control variable along with the year and area dummies. The inclusion of such an index did not affect any of the youth share estimates, but the index-estimates had an unexpected sign in some of the regressions indicating that it did not fully capture what it was intended to do (and hence the results are not displayed).

Table A3. Estimates of time-specific youth share effects.

Age group of dependent variable	Dependent variable: Unemployment rate			Dependent variable: Employment to population rate		
16-19	-0.712** (0.233)	-0.658** (0.248)	-1.187** (0.284)	1.781** (0.197)	0.526* (0.210)	1.845** (0.240)
20-24	-0.661** (0.178)	-0.504** (0.190)	-0.745** (0.217)	1.662** (0.240)	1.936** (0.255)	2.500** (0.292)
25-54	0.321** (0.062)	0.100 (0.066)	-0.026 (0.076)	-0.024 (0.077)	0.189* (0.082)	0.584** (0.094)
55-64	0.648** (0.100)	0.537** (0.107)	0.110 (0.122)	-1.026** (0.119)	-0.551** (0.127)	-0.964** (0.145)
All (16-64)	0.262** (0.065)	0.162* (0.069)	-0.036 (0.079)	0.007 (0.077)	0.012 (0.083)	0.255** (0.094)
Time period	1985-90	1991-95	1996-99	1985-90	1991-95	1996-99

Note: Estimates are for the effects of the youth share defined as the share of 16-64 year old individuals that are 16-24 years old. Regressions are based on IV models (instrument: see equation 2) with fixed area (109 LLM:s) and year effects, see equation (3). Sample period is 1985-99 and sample size is 1635. The youth share effect is allowed to vary between the 5-year periods. First order Newey-West corrected standard errors are in parentheses. *Statistical significance at 5 % level. **Statistical significance at 1 % level.

To assess whether the estimated parameters are stable over time a model where the youth share effect is allowed to vary between three five year periods is estimated. The results are displayed in *Table A3*. The results show that the estimates are very stable over time, especially for the young and the oldest

[25] The index was constructed in three steps: First a weight was calculated for each area (constant over time) for each industry based on the fraction of the total number of employed workers in that area that were employed in that particular industry. Second, a corresponding weight was calculated for each industry and year (constant over the areas). Third, the index was constructed as the covariance between the area's industry weights and the year's industry weights.

workers. This is quite reassuring given the large variation in the macro environment that is evident from *Figure 1* in the body of the paper.

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