EMPIRICAL STUDIES ON WAGES, FIRM PERFORMANCE AND JOB TURNOVER

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To Pia and Anna

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Stockholm, August 2002

Fredrik Heyman

INTRODUCTION AND SUMMARY

This thesis consists of four studies in empirical labor economics. Each essay is self-contained and deals with various issues related to the functioning of the labor market and the interaction of firms and workers. Even though each essay is concerned with different specific questions, they do have a natural linkage. The first essay deals with labor flows, the second with labor flows and wage dispersion, the third with the relationship between wages and firm performance and, finally, the last chapter is concerned with firm performance and wage dispersion.

The questions being asked are related to employment contracts and wage setting: how are job and worker flows affected by temporary employment contracts? Are there differences in the cyclical pattern of labor flows between permanent and temporary contracts? Can differences in job flows be explained by differences in wage dispersion? Do employees in firms with higher profits earn more than employees in firms with lower profits? What is the relationship between the internal pay structure in firms and firm performance?

Human-capital models have for years dominated empirical work on issues such as wage determination and labor turnover. The focus has been on the workers' individual characteristics and their supply of labor. The past decade has, however, seen a gradual shift in the direction of taking into account both supply and demand factors, as well as the role of contracts and incentives, when analyzing issues related to the labor market. As stressed by Hamermesh (1999), this development in empirical labor economics has proceeded through the stimulus provided by new data sets, linking information on both employers and employees.

Using detailed information on firms and workers makes it possible to account for the fact that labor market outcomes to a large extent is the result of the actions taken on both sides of the market. In the words of Hamermesh (1999, p. 31): "Understanding interactions of workers and firms is the Holy Grail of labor economics. While we may attribute various outcomes to behavior on one side of the market or the other, in the end both blades of the supply-demand scissors must be viewed as determining what we observe."

The need for detailed micro data on both employers and employees when studying different topics in labor economics is also discussed in Abowd and Kramarz's (1999) contribution to the latest *Handbook of Labor Economics*. Their exposition presents topics where the creation of matched worker-firm data sets have lead to new results, including employment mobility, compensation structure and wage determination.

Micro data on both employers and workers are used to analyze the questions asked in the essays. By using disaggregated information, issues related to firm and individual heterogeneity can be studied. The first essay uses data from Short-Term Employment Statistics, containing direct information on worker turnover and employment stocks for a representative panel of around 10,000 establishments in the Swedish private sector. The second essay matches Short-Term Employment Statistics to Short-Term Wage Statistics. The latter data set contains wage statistics for a panel of more than 10,000 establishments and firms.

The third and fourth essays, finally, use a large and detailed matched employer-employee data set. The data consist of very detailed information on individual characteristics for a sample of over 170,000 Swedish employees, matched with the profits of their employing firm and the unemployment registers. These data are matched with the Swedish Establishment Survey (APU), containing explicit information on product market competition and product demand elasticity.

The first essay, *The Impact of Temporary Contracts on Gross Job and Worker Flows* (with Mahmood Arai), examines job and worker flow dynamics for temporary and permanent contracts. Microeconomic evidence from establishment data for many countries indicates that minor net changes in employment are a result of substantial gross job flows. A significant percentage of jobs are created and destructed simultaneously in both downturns and upturns.

The micro approach to job flows concerns changes in employment at the plant or firm level. Job creation refers to new jobs in expanding and new establishments and job destruction to lost jobs in contracting and exiting firms. Seminal papers in the gross job flow literature are Leonard (1987), Dunne *et al.* (1989) and Davis and Haltiwanger (1990, 1992). Results from studies on a variety of countries indicate that there is substantial job creation and job destruction in almost every industry.

Data used in earlier research on gross labor flows do not allow for a distinction between different types of employment contracts (an exception is Abowd *et al.* (1999a)). This distinction is especially important in Europe since several European countries discriminate between permanent and temporary contracts in their employment legislation. Lower termination costs associated with temporary contracts enable firms to reduce adjustment costs by using temporary workers as a buffer for employment adjustment. This implies larger volatility for temporary, as compared to permanent, contracts and high average rates of gross job flows as a result of the high volatility of temporary employment.

The lack of information on contract types might yield gross job flows that are difficult to interpret in terms of labor market flexibility. Comparable gross labor flows might mask important changes in the composition of employment types over time. Furthermore, similar gross labor flows can be observed across countries with radically different employment structures and labor market institutions.

This paper examines the impact of temporary contracts on job and worker flow dynamics. The data contain quarterly information on the stock of permanent and temporary contracts, as well as direct information on hires and separations for permanent and temporary workers. The information is from a representative sample of around 10,000 Swedish private establishments.

The results indicate that temporary contracts, covering only around 10 percent of all contracts, stand for half of all gross job (and worker) flows. This means that gross job (and worker) flow rates for temporary contracts are around 10 times larger than job (and worker) flows for permanent contracts. The predominant picture from previous research is that job turnover is counter-cyclical. Our results imply that job reallocation associated with temporary contracts is acyclical in both manufacturing and non-manufacturing sectors. For permanent contracts, job reallocation only exhibits a countercyclical pattern in manufacturing, characterized by a low fraction of temporary contracts. Services employing a higher fraction of temporary contracts exhibit no cyclical pattern in job reallocation, implying that establishments in services use temporary contracts as an adjustment buffer and can adjust its labor input more smoothly.

Using direct information on hires and separations, we find that, on average, permanent worker turnover is 8 percentage points larger than permanent job turnover. This

is much lower than the previously observed differences between total worker and job flows, thereby implying that the previously reported rates of worker turnover might be dominated by switches between employers of temporary workers.

The share of temporary contracts varies with the industry structure and changes as a result of sectoral shifts. This implies that cross-country comparisons, as well as studies of the dynamics of job and worker flows, based on aggregated time-series data, can be distorted by the impact of the fraction of temporary labor on gross labor flows. This, in turn, makes the distinction between permanent and temporary contracts crucial in analyzing job and worker flows, especially when labor protection laws discriminate between short-and long-term employment contracts.

The second essay, *Wage Dispersion and Allocation of Jobs*, investigates the relationship between job turnover and the distribution of wages. One key result from the literature on job creation and job destruction is the magnitude of gross job flows. US studies have found an annual job reallocation rate of around 20 percent, indicating that one job out of five is either destroyed or created over a twelve-month interval.

The high figures of job turnover reported for the US labor market might not be surprising if taking into account how deregulated the US labor market is. However, studies of several European labor markets have reported job flows of the same magnitude as for the US. Given the large institutional differences in terms of job security legislation and unionization, the similarity between Europe and the US may seem puzzling.

One possible explanation for similar labor reallocation rates across labor markets with very different employment-protection legislations is related to differences in wage setting institutions. Bertola and Rogerson (1997) argue that although job-security laws lead to lower job flows, their impact might be reduced if differences in wage-setting institutions have opposite effects. Wage setting institutions are, on average, more centralized in Europe, leading to greater uniformity of wages across industries and firms. A compressed wage structure implies that firms cannot adjust wages as a response to positive or negative demand shocks. Bertola and Rogerson's conclusion is that when labor protection laws and wages are jointly considered, the result might very well be that job flows in countries with high adjustment costs and a compressed wage structure mimic those in countries with low adjustment costs and decentralized wages.

Using establishment data on job turnover and wages for a panel of around 10,000 establishments in the Swedish private sector, the relationship between wage compression and job reallocation is studied at the industry level. To accommodate the Swedish institutional framework, the Bertola and Rogerson model is extended by introducing the presence of two types of employment contracts and also taking into account that total labor input is a function of both the number of employees and the hours worked.

Estimating industry fixed-effects models for 14 two-digit industries yield results indicating large sector differences regarding the effect of the degree of wage dispersion on job reallocation. In accordance with the Bertola and Rogerson hypothesis, this effect is positive in the manufacturing sector. Running separate regressions for job creation and job destruction shows a negative and significant effect of wage dispersion on job destruction, whereas it is insignificant in the job-creation equation. These results are in accordance with wages being more rigid downwards than upwards. The quantitative effect of the impact of wage dispersion on job turnover is limited, however. A one standard deviation increase in wage dispersion reduces the total job reallocation by around 10 percent. Turning to the non-manufacturing sector, the Bertola and Rogerson hypothesis is not supported.

Further results include (i) a strong positive effect of the industry-share of temporary employees on job reallocation and (ii) a negative relationship between the use of overtime and job turnover. If the fraction of temporary employees is taken as a proxy for adjustment costs for labor, then the results indicate that the largest part of the variation in gross job flows is explained by differences in the cost of adjusting labor, rather than by differences in wage dispersion.

In the third essay, *Wages, Profits and Individual Unemployment Risk: Evidence from Matched Worker-Firm Data* (with Mahmood Arai), the impact of firm performance on individual wages is studied.

Numerous studies have found that wages are distributed in a way that cannot be explained by differences in productivity and observable human capital characteristics. One empirical regularity that is not compatible with the classical model concerns the relationship between profits and wages, where several studies have found a positive and

significant effect.[1] The most widely suggested interpretation for this phenomenon is that employers and employees engage in rent-sharing, thereby splitting the profits created between themselves.

The estimated effects of rent-sharing might be underestimated due to the accounting relationship between wages and profits, which implies that higher wages lead to lower profits. On the other hand, according to theories of incentive pay and various versions of efficiency wages, higher pay implies higher profits and, in the presence of product market rents, a positive correlation between these can be due to rent-sharing, but may also be the result of the impact of high wages on profits.

The purpose of this study is to examine the extent of rent-sharing and the impact of individual and aggregated unemployment risk on wages of individual workers. The data used in earlier studies are aggregated on the worker and/or the firm side.[2] The aggregation on the employer part suppresses the within industry variation in the impact of profits and unemployment risk on wages and an aggregation on the worker part implies neglecting worker heterogeneity.

We use a sample of over 170,000 Swedish employees for 1991 and 1995 matched with their employing firm's profits and the unemployment registers. The matched data contain detailed information on individual characteristics, including their unemployment experience during 1992-1995 as well as annual profits as reported in the firms' balance-sheet reports. These data are matched with the Swedish Establishment Survey (APU), containing explicit information on product market competition and product demand elasticity.

The contribution of this paper is that it provides evidence on the wage determination, based on disaggregated individual and firm data dealing with the problems of firm and worker heterogeneity, and the endogeneity of profits. Moreover, a novelty is that the relation between worker aggregate unemployment experience and wages is estimated after controlling for individual heterogeneity that influences individuals' unemployment risk and wages. Due to our disaggregated data, we can examine the extent

[1] See e.g. Blanchflower *et al.* (1990, 1996), Christofides and Oswald (1992) and Hildreth and Oswald (1997).
[2] Abowd *et al.* (1999b) and Arai (1999) use microdata on wages and profits for the private sector. The data in Abowd *et al.* do not include unemployment, while Arai (1999) only uses regional unemployment on a small sample of employees and a limited panel structure.

of rent sharing between and within industries, as well as between and within firms within industries.

Our results imply positive effects of profits on wages, both in 1991 and 1995. The implied elasticities are between 0.008 and 0.025 in 1991, and between 0.005 and 0.012 in 1995. This means that our reported elasticities are within the range of previously reported elasticities using other sources of data for various countries. These elasticities imply that the wage inequality in Sweden due to the spread in profits is as high as 13% of the mean wages in 1991 according to Lester's range of pay.[3] These correlations are robust for controlling for time-invariant unobserved individual- and firm characteristics.

Using firm-reported short-term product market elasticity and the number of competitors as instruments for profits indicate that our estimated elasticities are underestimated. The elasticities implied by the IV-estimates suggest Lester's measure of wage inequality due to profits to be as high as 50% of the mean wages.

Finally, we investigate the impact of individual heterogeneity with respect to unemployment risk that might also affect wages. We include the individuals' unemployment event record in our regressions, and our results confirm that individuals with a higher unemployment risk also have lower wages. Aggregating individual unemployment experience during 1992-1995 at the firm- and various industry-levels yields aggregated unemployment-risk measures. Including these aggregated measures along with individual unemployment risk in our estimations show results suggesting that there exists a robust negative correlation between unemployment risk and wages at various aggregation levels.

The final essay, *Pay Inequality and Firm Performance: Evidence from Matched Employer-Employee Data*, tests several implications from tournament models. According to a variety of theories, the wage distribution both within and between firms can have important effects on individual productivity and firm performance.

One argument for high wage differentials, based on incentive effects, is found in Lazear and Rosen's (1981) tournament theory. Higher wage differentials lead to higher individual effort, and are therefore productivity enhancing. This, in turn, suggests that there

[3] The corresponding figure for 1995 is 10%.

is a positive relationship between wage dispersion and productivity. The opposite relationship is found in theories stressing fairness and cooperation between co-workers.[4]

Based on the theoretical literature on the effects of intra-firm wage differentials, this study empirically examines several implications from tournament theories. First of all the general relationship between within-firm pay inequality and firm performance. The analysis is carried out for both white-collar workers and managers, using different measures of intra-firm wage dispersion. In addition, three more specific predictions from tournament models are tested on managers. These are (i) a convex relationship between pay and job levels for managers, including an extra large increase at the very top of the hierarchy, (ii) a positive relationship between the number of executives (contestants) and wage dispersion for managers and (iii) a positive association between market demand uncertainty and managerial pay spread.

Previous studies have mainly focused on wage dispersion for managers. This has been a natural first step since tournament theory is mainly a theory on how wage distribution affects managerial behavior. Furthermore, suitable data for analyzing the relationship between wage distribution for wider occupational groups of workers and firm performance have not previously been available. Earlier evidence on wages for managers and on various aspects of predictions from tournament models includes Leonard (1990), Main *et al.* (1993) and Eriksson (1999). Winter-Ebmer and Zweimuller (1999) and Bingley and Eriksson (2001) present evidence on the relationship between pay dispersion and firm performance for broader occupational groups.

Except for Bingley and Eriksson and Winter-Ebmer and Zweimuller, previous studies do not control for differences in human capital accumulation, which means that a systematic sorting of workers (and managers) into firms with certain characteristics can partly drive results. This study explicitly controls for firm differences in human capital when testing several predictions from tournament theory.

The present study also adds to the previous literature in that it is the first to study aspects of tournament models, including the effect of wage dispersion on firm performance, on Swedish data. In an international perspective, the Swedish wage

[4] See e.g. Akerlof and Yellen (1990) and Levine (1991).

distribution is very compressed and characterized by strong unions and a higher degree of centralized wage negotiations than most other countries.

For white-collar workers, the results show a positive effect of intra-firm pay spread on firm performance for 1991 and 1995. This applies to different measures of wage dispersion, capturing both raw differences and differences corrected for the fact that part of the wage spread is due to differences in human capital accumulation. To take firm heterogeneity into account, difference equations are estimated on a panel of firms. Once more, consistent with tournament theory, a positive and significant effect of wage dispersion on profits is found. Quantitatively, an increase in the coefficient of variation of wages by one standard deviation increases firm profits by approximately 20 percent, evaluated at the mean of profits.

The results for managers are based on information on about 10,000 managers. For various measures of wage dispersion and specifications, a positive and significant association between managerial pay and profits is found.

No support is found for the hypothesis of a positive relationship between the number of managers (contestants) and wage spread. Instead, the results show a negative and significant effect of the number of executives and pay spread among managers.

Finally, consistent with tournament theory, higher wage dispersion is found in firms operating in volatile product markets characterized by a high degree of output uncertainty.

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Chapter 1

The Impact of Temporary Contracts on Gross Job and Worker Flows

I

The Impact of Temporary Contracts on Gross Job and Worker Flows

Mahmood Arai* and Fredrik Heyman†‡

Abstract

Based on direct information on hires and separations for 10,000 Swedish establishments during 11 years, we report new evidence that around half of gross job and worker flows stem from temporary contracts which account for only around 10 percent of employment. The share of temporary contracts is positively correlated to employment growth, indicating that firms use temporary jobs as a buffer in labor adjustment. Job reallocation for temporary contracts is acyclical. Service establishments with a high share of temporary employment exhibit acyclical job reallocation, while permanent jobs in manufacturing are countercyclical.

Keywords: Job creation and Job destruction, Worker flows, Dual labor markets, Temporary employment.

JEL Classification:J21, J23

1 Introduction

Studies of gross labor flows for the US and many European countries report similar average job creation and job destruction flows aggregating temporary and permanent contracts, despite the fact that labor markets in these countries vary greatly with respect to labor legislation restricting hiring and firing of workers on short- and long-term

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contracts.[1] Temporary employment is by definition characterized by strong volatility and thus, a high turnover of jobs and workers. When firms face high adjustment costs associated with permanent contracts, as is the case in Sweden and many other European countries, high levels of aggregate job or worker flows can be dominated by high levels of labor flows associated with temporary contracts. On the other hand, when labor legislation does not affect the adjustment costs associated with various types of contracts, as is the case in the US, aggregate flows are likely to be more evenly distributed across contract types. This suggests that similar levels of aggregate labor flows may mask differences in flows due to the differences in contract types. This, in turn, implies that a similar aggregate level of labor flows does not necessarily indicate a similar pattern of flexibility in the labor market.

Employment protection legislation in Sweden restricts hiring and firing of workers. In terms of termination costs of employment contracts, the labor law (LAS) is more restrictive for permanent than for temporary contracts. Time-limited contracts are only allowed for (i) one trial period of six months, (ii) seasonal or temporarily excessive work loads, (iii) replacement of employees on leave, (iv) workers over 67 years and a few other cases. This is not unique for Sweden. Labor protection laws in many European countries discriminate between short- and long-term contracts (see OECD (1993, 1996)). In the presence of large hiring and firing costs associated with permanent contracts, firms can reduce adjustment costs by using temporary workers as a buffer for employment adjustment.[2]

The purpose of this paper is to examine job and worker flow dynamics for temporary and permanent contracts. Our data contain quarterly direct information on hires and separations for permanent and temporary workers. The information is from a representative sample of approximately 10,000 Swedish private establishments covering the period 1989:2 – 1999:4. Data of this kind have not previously been used to study labor flow dynamics. Earlier research on gross labor flows has been based on data that do not permit distinguishing between different types of employment contracts (an exception is Abowd *et al.* (1999)).

[1] See Leonard (1987), Dunne *et al.* (1989), Davis and Haltiwanger (1990,1992) for job flows. Abowd *et al.* (1999), Anderson and Meyer (1994), Burda and Wyplosz (1994), Lane *et al.* (1996), Hamermesh *et al.* (1996) and Albaek and Sörensen (1998) study both job- and worker flows. For studies of gross job flows in Sweden, see Persson (1999) and Andersson (1999).

[2] See Saint-Paul (1996), Cabrales and Hopenhayn (1997) and Alonso-Borrego (1998) for analyses of temporary and permanent employment.

The only result on gross labor flows by contract types reported in previous research is found in Abowd *et al.*, indicating that in France, 2/3 of all hires are short-term contracts and more than half of all separations are due to the end of such contracts. This indicates that short-term contracts account for a substantial part of labor flows. This paper contributes to the literature by analyzing the impact of temporary contracts on gross job and worker flows during a sufficiently long period, thereby enabling an analysis of the cyclical variation of these flows.

Our results are as follows. Temporary contracts account for approximately 10 percent of all contracts but stand for half of all gross job (and worker) flows. This means that gross job (and worker) flow rates for temporary contracts are about 10 times larger than job flows (and worker) for permanent contracts.

The dominant picture from previous research is that job turnover is counter-cyclical. Our results imply that job reallocation associated with temporary contracts is acyclical in both the manufacturing and non-manufacturing sectors. For permanent contracts, a countercyclical pattern for job reallocation is found in manufacturing only.

We examine worker flows based on direct information on reported hires and separations and find that on an annual basis, permanent worker turnover is, on average, 8 percentage points larger than permanent job turnover. This is much lower than previously observed differences between total worker and job flows, implying that previously reported worker turnover rates are highly dominated by employer changes among temporary workers. The low excess worker reallocation for permanent jobs that we observe could reflect a low fraction of poor job-matches. For instance, if costs for labor turnover are high, more effort is made to secure a good match. Our low rate of excess worker reallocation for permanent contracts would then reflect good matches as a result of cautious hiring policies. Another explanation is that the employment protection law reduces the lay-off rate for low-productivity workers with permanent contracts. This, however, takes place at the cost of a higher turnover rate for temporary workers.

Considering the variation over time in worker turnover, it is reasonable to believe that workers try to improve their job matches in good times when jobs are plentiful. Our results indicate that worker turnover for all jobs is strongly procyclical in the entire private sector. Analyzing the cyclical pattern of worker turnover by industry, we report evidence indicating that the aggregate result reflects the procyclical pattern of worker turnover in the non-manufacturing sectors.

Our results for job reallocation and worker reallocation imply that excess worker

reallocation is strongly procyclical. In all cases, except for permanent contracts within manufacturing, the procyclical pattern of excess worker reallocation is a result of procyclical worker mobility. The procyclical pattern of excess worker turnover in manufacturing simply reflects the countercyclical pattern of job reallocation. The overall message is that excess worker mobility is worker-driven in services and job-driven in manufacturing.

The remainder of the paper is organized as follows. The data is described in Section 2 and measurement issues are discussed in Section 3. Section 4 presents the results for the pattern of gross job flows. Worker flows are investigated in Section 5 and Section 6 concludes.

2 Data

The data are obtained from the Short Term Employment Statistics (*Kortperiodisk Sysselsättningsstatistik*) collected by Statistics Sweden (SCB). These data contain quarterly information on worker turnover and employment stocks for a representative panel of around 10,000 establishments of all sizes in the non-agricultural private sector, during the period 1989:2 – 1999:4. The information on establishment employment as well as hires and separations is supplied for both permanent (time unlimited) and temporary (time limited) contracts for men and women, separately (see Appendix A for data description). Data are consistent with the Swedish Labor Force Surveys according to results reported by Statistics Sweden (SCB) and our own computations. For details, see Arai and Heyman (2000).

The structure of the survey is as follows. A representative sample is drawn from the population of private-sector establishments of all sizes in Sweden, stratified according to industry affiliation and establishment size. Establishments with more than 99 employees are sampled with probability one. In order to update the sample to include newly started establishments and avoid attrition due to exits, 10 percent of the sample are replaced every year for the period 1989-1994, and every six months starting in 1995.

The establishments are randomly divided into three equal groups. Every quarter, each group responds to questions on employment and worker turnover for one month each. The number of employees refers to a particular date in the month, while separations and hires refer to flows during the entire month. As an example, one third of the sampled establishments in the first quarter (second quarter) report information for

January (April), while the other two groups report the corresponding information for February (May) and March (June). This sampling strategy improves the precision of reported information on hires and separations in comparison to a strategy of collecting information on worker flows for the entire quarter. The establishments' response on hires and separations refers to a period of one month and are thus expected to yield reliable information.

We know of only two other studies using direct information on worker flows. Hamermesh (1996) with Dutch firm data and Abowd *et al.* (1999) with French establishment data, also have direct information on hires and separations. These data sets, however, cover only a few years each.[3] Our data, covering more than a decade, avoid the problem of capturing a particular state in the business cycle which may be the case when data cover only 2-4 years.

Note that the sampling strategy here should not be confused with monthly data since the information rendered does not refer to a particular, but rather a random, month. The quarterly flows are simply three times larger than the flow during a random month. The sample strategy yielding information on all months of a quarter is such that aggregate data on a sufficiently large number of establishments yield an unbiased estimate of hires and separations for a quarter, since the establishments are randomly divided into three groups reporting for different months.

3 Measuring Job and Worker Flows

The standard measure of job flows is the changes in the stock of employees over time. Job flows can also be computed from direct information on worker flows into and out of establishments. Our measures of the job creation rate (JCR) and the job destruction rate (JDR), based on direct information on hires (h_{et}) and separations(s_{et}) for establishment e with the number of employees n_{et} during period t in sector k, are as follows:

$$JC(D)R_{kt} = \Sigma_e(|h_{et} - s_{et}|)/n_{et}; \qquad if \quad h_{et} \geq (\leq) s_{et}.$$

Our measure can be compared to the standard measure of job creation (destruction)

[3] Abowd *et al.* use monthly hires and separation for a sample of around 2,000 establishments with 50 employees and more, during 1987-1990. Hamermesh *et al.* use Dutch data for a few years.

rates, given as follows:

$$JC(D)R_{kt} = \frac{\Sigma_e(|n_{et} - n_{e,t-1}|)}{\Sigma_e 0.5(n_{et} + n_{e,t-1})}; \qquad if \quad n_{et} \geq (\leq) n_{e,t-1}$$

Using our data, the standard job flow measures yield changes in the stock of employees in the period between the middle of two subsequent quarters, while our measure based on reported hires and separations refers to flows during a quarter. A comparison of flows calculated on these different sources of information serves as a test of the logical consistency of the data. The changes in these job flows are very similar. Considering permanent contracts, the two measures of gross job creation have a correlation of 0.77. The corresponding correlation for gross job destruction is 0.74. Net employment changes based on the two measures are essentially identical (see Arai and Heyman (2000) for details).[4]

Measuring worker flows in our data is straightforward. Hires and separations are directly measured in our data, while previous studies must rely on employer switches for identifying hires and separations.[5] These two measures are quite comparable when dealing with contracts of longer duration. Considering more time-limited contracts, however, the differences in the two measures can be considerable since the measure of worker flows based on employer switches misses all worker flows within measurement-dates .

To obtain the highest possible comparability between job and worker flows, especially regarding contract types, we rely on our direct measures of job and worker flows.

[4]Differences in gross flows based on the two measures might be due to the impact of measurement error in the two measures. The measurement errors sum up and generate a false flow when the errors in the stock of employees (or hires and separations) are not positively and perfectly correlated over time (or across hires and separations). When the errors are positively but not perfectly correlated, there will be false flows that amount to the differences in the errors. The errors in the flow-based measure stem from the same reporter at the same time, while the errors in the stock measure stem from two different points in time and possibly from two different reporters. This implies that the stock measure is more vulnerable to measurement errors as compared to the flow-based measure. The job flows for temporary contracts are higher when computed on the basis of net hires and separations, as compared to the change in the stock employees on temporary contracts. A reason for this difference is that the establishments are asked to report the stock of temporary workers on a certain day but exclude temporary workers, working on an hourly basis, absent on that particular day. These workers are included in the hires and separations data and influence the magnitude of job creation or destruction computed on the basis of net hires and separations.

[5]Exceptions are Hamermesh *et al.* (1996) and Abowd *et al.* (1999).

4 Job Flows

Annual job creation for permanent contracts ranges between over 10 percent at the peak of the economic boom in 1989 to below 5 percent at the bottom of the slump in 1993. Gross job destruction for permanent contracts varies between 8 percent in 1995 and 13 percent in 1992. This can be compared to job flow rates for temporary contracts that are, on average, 10 times larger (see Table 1 and Figures B1-B3 in Appendix B).

- Table 1 about here -

The very high flow rates for temporary contracts indicate that these contracts function as an adjustment buffer. To further examine this, we analyze the relationship between the share of temporary contracts and the variation in net employment over time. First, we find that the share of temporary contracts in expanding establishments is 13 percent while it is 8 percent in shrinking establishments. Moreover, we estimate a model with change in the share of temporary employment ($\Delta TEMP$) at the establishment level as the dependent variable and net employment change (ΔNET) as the explanatory variable controlling for establishment fixed effects ($\Delta TEMP_{et} = \beta \Delta NET_{et} + \nu_e + \varepsilon_{et}$ for establishment e in time t). The estimated coefficient is + 0.26 and is highly significant ($p = 0.000$). These results indicate that establishments use temporary contracts to expand and lay off workers on temporary contracts as they reduce employment.

The employment share of temporary jobs is around 10 percent in the private sector, varying between 6 percent in manufacturing and 12 percent in services. Annual job reallocation rates for permanent contracts are, on average, 13 percent in manufacturing compared to 20 percent in non-manufacturing. Examining job flows in 14 industries (6 in manufacturing and 8 in non-manufacturing), job reallocation is found to be largest in the Hotel and Restaurant, Construction and Service sectors (See Table 2). The lowest job reallocation rates are observed in the Food, Mining and Electricity sectors with a rather low fraction of temporary employment. Job-reallocation is positively correlated to the share of temporary contracts which exhibits a procyclical pattern according to our aggregated time-series data as well as our panel of 14 industries.

- Table 2 about here -

Previous studies examine the cyclical pattern of gross job flows based on raw correlations on aggregated time-series. Aggregated job reallocation for all contract types

can vary as a result of firms' adjustment during the business cycle and as a result of sectoral shifts. The aggregated measures are sensitive to changes in the relative employment shares of various industries with different shares of temporary employment. As an example, the share of manufacturing employment with only 6 percent temporary jobs (and thus low job reallocation rates) decreased from 39 percent in 1989 to 32 percent in 1999.

Raw correlations using industry time-series data indicate that job reallocation is pro-cyclical in Trade, Transport, Banking and Services while Metal and Machinery, Construction, Chemistry and Textile exhibit a significant counter-cyclical pattern. For other sectors, job reallocation is acyclical. The previous findings on countercyclical job reallocation are only supported for permanent contracts in the traditional manufacturing industries in our data. These findings are in line with Boeri (1996), suggesting that the countercyclical job reallocation is specific for manufacturing. Foote (1998) argues that the observed countercyclical job reallocation is compatible with a sluggish labor adjustment in combination with a decreasing employment trend.

We examine the cyclical pattern of gross job flows by means of our panel of 14 industries over 43 quarters. In this way, we exploit the rich variation in various industry cycles and can estimate overall as well as within-industry cyclical patterns of job flows. Results reported in Table 3 indicate that job reallocation for all types of contracts is acyclical in specifications with and without industry fixed-effects. The same pattern is observed for job reallocation for permanent contracts (see panel a, Table 3).

- Table 3 about here -

Separate regressions for manufacturing and services based on our panel of industries disclose a conflicting pattern. We find a stable counter-cyclical pattern of job reallocation for permanent contracts in manufacturing, while job reallocation in services exhibits an acyclical pattern.

Our conclusion is that industries that can adjust employment by using temporary workers are characterized by smooth job reallocation and thus, do not exhibit any cyclical pattern in job reallocation. The observed counter-cyclical job reallocation in manufacturing might reflect this sector's limited possibilities of using temporary contracts as an adjustment buffer leading to sluggish labor adjustment.

5 Worker Flows

Few previous studies deal with both job and worker flows. Studies using matched worker-establishment data all report extremely high rates of worker reallocation. Lane *et al.* (1996) report a quarterly worker reallocation rate of 24 percent in the manufacturing sector in the state of Maryland, indicating that roughly every worker experiences a hiring or separation during one year. Persson (1999) reports a 50 percent annual worker reallocation for the Swedish private sector. Albaek and Sörensen (1998), studying the Danish manufacturing sector, find an annual worker reallocation of 57 percent. These studies estimate hires and separations by identifying employer switches. This procedure is very sensitive to the fraction of temporary to total employment. Temporary workers switch jobs often and temporary jobs are often filled by different workers in different time periods.

Results based on directly observed hires and separations are reported in Table 1 and Figures B1-B3 in Appendix B. Note that our direct hires and separations cover all employer switches, while previous studies only cover a part of hires and separations using information on employer affiliations for two dates. On the other hand, given that permanent jobs usually last more than one year, computing employer switches for permanent workers yields a good estimate of total hires and separations.

Permanent hires exhibit high variation over time, ranging from nearly 18 percent in 1989 to a low of 6 percent in 1993. The mean hiring rate equals 12 percent. The corresponding figure for the separation rate is 14 percent, varying between 11 percent in 1996 and 18 percent in 1990. Despite a severe net employment contraction during 1991-94, some firms still hired at a rate equal to half the job destruction. These simultaneous hires and separations indicate heterogeneity in firms' labor demand. However, we find that expanding establishments hire around 80 percent of total hires and contracting establishments constitute 80 percent of total separations.

Hires and separations associated with temporary contracts are extremely high. The total worker reallocation is around 46 percent, which is comparable to the worker reallocation rate for all workers reported in earlier studies for Sweden and Denmark. This can be compared to a worker reallocation rate of 25 percent for permanent contracts reported above, implying that around half of the estimated worker turnover in earlier studies originates from a relatively small (10 percent), but mobile group of temporary workers.

Considering the variation in worker turnover over time, it is reasonable to believe that workers try to improve their job matches in good times when jobs are plentiful. Results in panel *a* Table 4 indicate that worker turnover is procyclical in the private sector. Results in panels *b* and *c* disclose that the overall results are dominated by non-manufacturing industries. Worker turnover for all jobs is strongly procyclical in services while worker mobility seems to be procyclical only within manufacturing industries (when controlling for industry fixed-effects). For permanent contracts, the procyclical worker turnover pattern is only found in Services.

- Table 4 about here -

Our results for job reallocation and worker reallocation imply that excess worker reallocation (the difference between worker and job reallocation) is strongly procyclical. In all cases, except for permanent contracts in manufacturing, the procyclical pattern of excess worker reallocation is a result of procyclical worker mobility. The procyclical pattern of excess worker turnover in manufacturing simply reflects the countercyclical pattern of job reallocation. The overall message is that excess worker mobility is worker driven in services and job driven in manufacturing.

We observe an excess worker reallocation of 8 percent for permanent jobs, implying that between 4 and 8 percent of permanent workers change jobs to improve their job-match, or to leave or enter employment. This can be compared to excess worker reallocation for temporary contracts that is, on average, approximately 80 percent annually, implying that between 40 and 80 percent of temporary workers experience jobs switches not induced by creation or destruction of temporary jobs.

Our overall low rate of excess job turnover for permanent contracts might reflect good matches as a result of cautious hiring policies due to high firing costs. Another explanation is that the employment protection law reduces the lay-off rate for low-productivity workers with permanent contracts. This takes place at the cost of a higher turnover rate for temporary workers, however. One conclusion is that the Swedish labor market exhibits a clear pattern of dualism associated with permanent and temporary employment.

6 Conclusions

The main message of the paper is that the distinction between permanent and temporary contracts is crucial in analyzing job and worker flows, especially when labor protection laws discriminate between short- and long-term contracts.

Using direct information on hires and separations, we report results indicating that previous findings on labor flows, aggregated over all contract types, are partly dominated by unstable temporary jobs and mobile temporary workers. Gross job and worker flows vary strongly in levels. Results imply that worker and job turnover for temporary contracts is around 10 times higher than worker and job turnover for permanent contracts, highlighting a dual structure of the Swedish labor market.

The share of temporary contracts varies with industry structure and changes as a result of sectoral shifts. This implies that cross-country comparisons, as well as examinations of the dynamics of job and worker flows for all contracts, based on aggregated time-series data, can be misleading by the impact of the share of temporary labor on gross labor flows.

We find no clear cyclical pattern of job reallocation with the exception of permanent contracts in manufacturing, characterized by a low fraction of temporary contracts. Services, employing a higher fraction of temporary contracts, exhibit no cyclical pattern in job reallocation, implying that establishments within the service sector use temporary contracts as an adjustment buffer and can adjust their labor input more smoothly .

Our results for job reallocation and worker reallocation imply that excess worker reallocation is strongly procyclical. The procyclical pattern of excess worker reallocation is a result of procyclical worker mobility in services. The procyclical pattern of excess worker turnover in manufacturing simply reflects the countercyclical pattern of job reallocation. Excess worker mobility is worker driven in services, and job driven in manufacturing.

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Tables

Table 1. Annual net and gross labor flow rates 1989-1999 (percent).

			All Jobs			Permanent Jobs			Temporary Jobs		
Year	Temp	Δ E	JRR	WRR	EWR	JRR	WRR	EWR	JRR	WRR	EWR
1989	9	0	30	55	25	19	34	15	167	246	78
1990	9	–1	30	53	23	21	35	14	159	235	75
1991	8	–2	27	41	14	18	24	06	156	227	70
1992	8	–3	28	40	12	18	22	03	169	256	86
1993	8	–2	28	40	11	16	20	04	177	249	71
1994	10	0	30	45	14	17	23	06	182	247	64
1995	10	0	29	46	16	17	24	07	170	239	68
1996	10	–1	28	42	14	15	20	05	170	240	70
1997	10	0	32	50	18	17	24	07	189	282	92
1998	10	0	31	53	21	18	27	08	163	261	98
1999	11	+1	31	52	21	19	28	09	150	239	89
Mean	9.5	-1	30	46	17	18	25	8	169	250	79
MAX	11	1	32	55	25	21	35	14	190	282	98
MIN	8	-3	27	40	11	15	20	3	150	228	64
SD	1	1	2	6	5	1.6	5	4	11	14	10

NOTES: Temp is the share of temporary contracts and ΔE is the net employment change. JRR , WRR and EWR are Job-, Worker- and Excess-Worker Reallocation Rates.

Table 2. Annual gross labor flow rates by industry 1989-1999 (percent).

		All Jobs			Permanent Jobs			Temporary Jobs		
Industry	Temp	JRR	WRR	EWR	JRR	WRR	EWR	JRR	WRR	EWR
Manufacturing	6	22	33	11	13	18	6	206	267	61
Mining	7	25	40	15	11	14	3	254	377	122
Chemistry	6	21	32	11	13	19	6	200	252	53
Wood & Paper	6	21	33	11	13	18	5	187	260	73
Textile	4	23	30	08	17	21	5	221	251	30
Machinery	4	20	30	10	13	19	6	224	272	47
Electricity	4	15	24	09	08	11	3	193	271	78
Non-Manufacturing	12	33	53	20	20	29	8	156	237	81
Construction	9	38	51	12	24	29	5	210	269	60
Real Estate	11	32	45	13	17	23	5	179	218	39
Food	14	36	57	21	12	18	6	207	288	80
Hotel & Rest.	27	67	137	70	33	55	21	186	365	180
Banking	7	20	29	09	12	18	6	150	174	24
Transport	10	30	51	21	19	30	10	153	241	87
Trade	11	31	48	17	20	28	8	145	212	67
Misc. Services	12	34	56	22	22	31	10	147	232	85

NOTES: Temp is the share of temporary contracts. JRR , WRR and EWR are Job-, Worker- and Excess-Worker Reallocation Rates.

Table 3. Cyclical pattern of job reallocation rate (JRR), 1989:2-1999:4. Employment weighted regressions. Dependent variable is JRR for different contracts. Robust standard errors in parentheses. Boldface coefficients are significant at conventional levels.

	All Jobs		Permanent Jobs	
a: Private Sector				
Δ Employment	0.21	−0.03	0.21	−0.04
	(0.29)	(0.15)	(0.14)	(0.08)
Industry Dummies	NO	YES	NO	YES
Quarterly Dummies	YES	YES	YES	YES
R^2	0.07	0.77	0.13	0.66
OBS	602	602	602	602
b: Manufacturing				
Δ Employment	-0.44	-0.11	**-0.29**	**-0.29**
	(0.30)	(0.17)	(0.08)	(0.08)
Industry Dummies	NO	YES	NO	YES
Quarterly Dummies	YES	YES	YES	YES
R^2	0.23	0.67	0.11	0.20
OBS	258	258	258	258
c: Non-manufacturing				
Δ Employment	-0.16	-0.04	0.12	0.06
	(0.34)	(0.20)	(0.16)	(0.11)
Industry Dummies	NO	YES	NO	YES
Quarterly Dummies	YES	YES	YES	YES
R^2	0.15	0.71	0.00	0.51
OBS	344	344	344	344

Table 4. Cyclical pattern of worker reallocation rate (WRR), 1989:2-1999:4. Employment weighted regressions. Dependent variable is WRR for different contracts. Robust standard errors in parentheses. Boldface coefficients are significant at conventional levels.

	All Jobs		Permanent Jobs	
a: Private Sector				
Δ Employment	**1.1**	**0.79**	**0.68**	**0.35**
	(0.46)	(0.20)	(0.22)	(0.14)
Industry Dummies	NO	YES	NO	YES
Quarterly Dummies	YES	YES	YES	YES
R^2	0.08	0.80	0.04	0.58
OBS	602	602	602	602
b: Manufacturing				
Δ Employment	-0.41	**0.52**	0.04	0.05
	(0.48)	(0.27)	(0.15)	(0.15)
Industry Dummies	NO	YES	NO	YES
Quarterly Dummies	YES	YES	YES	YES
R^2	0.20	0.64	0.03	0.06
OBS	258	258	258	258
c: Non-manufacturing				
Δ Employment	0.81	**0.94**	**0.61**	**0.57**
	(0.61)	(0.29)	(0.31)	(0.22)
Industry Dummies	NO	YES	NO	YES
Quarterly Dummies	YES	YES	YES	YES
R^2	0.06	0.77	0.03	0.47
OBS	344	344	344	344

Appendix A: Description of the Variables

Permanent employees: All employees with a time-unlimited employment contract in the middle of the survey month.

Self-employed and working shareholders are considered to be permanent workers. Only firms with at least one permanent employee is included.

Temporary employees: All employees with a time-limited employment contract in the middle of the survey month.

Employees employed on an hourly basis, not present on the actual date, are not included.

Permanent Hires: The number of individuals hired on a time-unlimited basis during the survey month.

Temporary Hires: The number of individuals hired on a time-limited basis during the survey month. Firms report hires on temporary contracts in the second quarter that are systematically higher than the quits in the second and third quarters. This implies a large increase in the share of temporary contracts that is not consistent with the changes in the stock of employees on temporary contracts, as measured in our data. A large number of temporary workers are hired during June-August and especially in July (the sumer vacation month in Sweden) to fill up vacancies during ordinary employees' vacation. Subsequent quits among these hired temporary workers are probably underreported. Temporary workers might not formally be registered as quitting after the end of the summer, since many of them remain in the pool of temporary workers on an hourly basis for some time. To correct for this, hires for temporary contracts were in 10 percent of the cases replaced by hires that are consistent with the average change in the stock of temporary workers during previous and the subsequent quarter if, $hire_t > 2quit_t + quit_{t+1}$ for all quarters.

Separations, Permanent Contracts: The number of individuals separated on a time-unlimited basis during the survey month.

Separations, Temporary Contracts: The number of individuals separated on a time-limited basis during the survey month.

Industry Classification: The 14 industry classification corresponds roughly to the two-digit system of industry classification (6 in manufacturing and 8 in non-manufacturing). Aggregation of some industries was necessary to match the industry classification (SNI69) for observations from the period 1989-1994 and the new system (SNI92) for the period 1995-1998. This leads to some minor misclassifications.

Appendix B: Figures

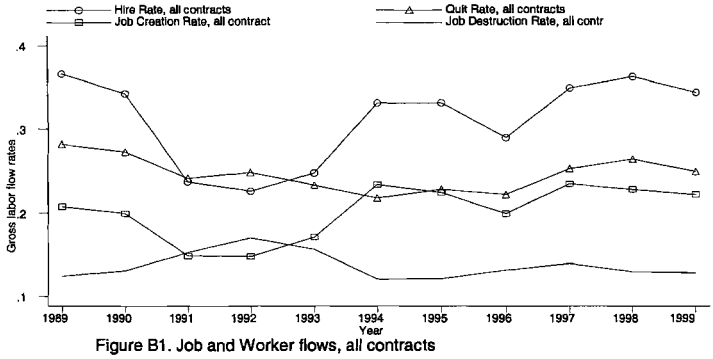

Figure B1. Job and Worker flows, *all* contracts

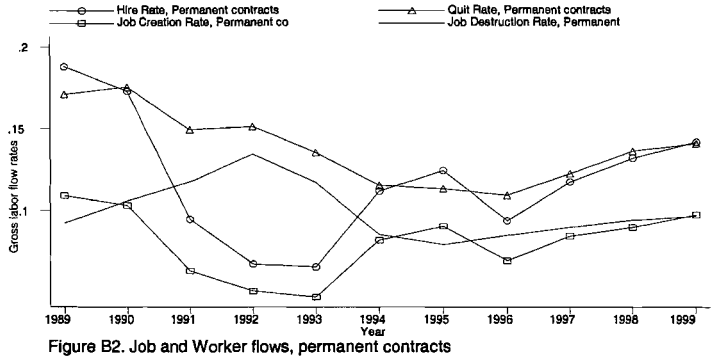

Figure B2. Job and Worker flows, permanent contracts

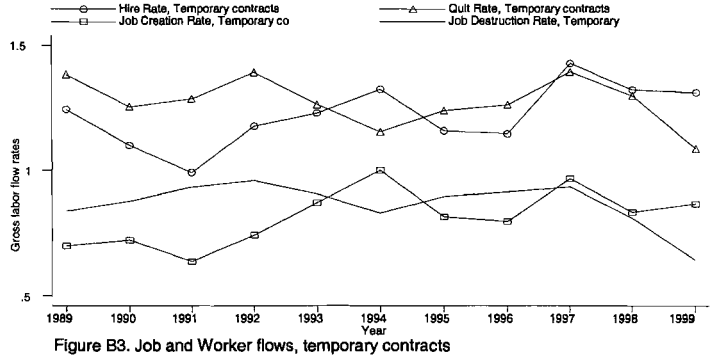

Figure B3. Job and Worker flows, temporary contracts

Chapter 2

Wage Dispersion and Allocation of Jobs

II

Wage Dispersion and Allocation of Jobs

Fredrik Heyman*

Abstract

This paper uses Swedish establishment-level panel data to test the hypothesis of a positive relation between the degree of wage compression and job reallocation as proposed by Bertola and Rogerson (1997). The effect of wage dispersion on job turnover is negative and significant in the manufacturing sector. The wage compression effect is stronger on job destruction than on job creation, suggesting that wages are more downward than upward rigid. Further results include (i) a strong positive relationship between the industry share of temporary employees and job turnover and (ii) a negative relationship between the amount of working-time flexibility and job reallocation.

Keywords: Job creation and job destruction, wage dispersion, temporary employment contracts.

JEL Classification:J21, J31, J63

1 Introduction

In the past decade, the rate of job reallocation in different markets has been estimated in numerous empirical studies.[1] Studies based on US and European data yield very similar results: the gross job turnover rate is approximately 20 percent in the observed countries. Given the large institutional differences in terms of job security legislation and unionization, the similarity between Europe and the US may seem surprising.

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[1]See Dunne *et al.* (1989), Leonard (1987) and Davis and Haltiwanger (1992,1997,1999) for job flows. Abowd *et al.* (1999), Anderson and Meyer (1994), Lane *et al.* (1996), Hamermesh *et al.* (1996) and Albeack and Sörensen (1998) study both job- and worker flows. Three Swedish studies are Andersson (1999), Arai and Heyman (2002) and Persson (1998).

One possible explanation for similar labor reallocation rates across labor markets with very different employment-protection legislations concerns differences in wage setting institutions. Bertola and Rogerson (1997) argue that although job-security laws lead to lower job flows, their impact might be reduced if other institutional differences have opposite effects. One such difference is wage setting. Wage setting institutions are, in general, much more centralized in Europe than in the US which, in turn, leads to greater uniformity of wages across industries and firms. A compressed wage structure implies that firms cannot adjust wages as a response to positive or negative demand shocks. Instead, they will have to adjust either the number of employees or the number of hours worked by the workforce. This implies, all else equal, that there is a positive relationship between the degree of wage compression and the magnitude of gross job flows.[2]

Bertola and Rogerson's conclusion is that when labor protection laws and wage setting institutions are considered jointly, the result might very well be that job flows in countries with high adjustment costs and a compressed wage structure mimic those in countries with low adjustment costs and decentralized wages. If wage-setting institutions have a significant effect on the reallocation of jobs, this may partly explain the reported similarities between European and US job reallocation rates.

Evidence presented in OECD (1993, 1996) and Blau and Kahn (1996) is consistent with the notion that wages are more compressed in Europe than in the US. It also indicates that wage setting institutions and the degree of wage compression may very well be an important factor behind observed similarities between European and US gross job flows. However, the Bertola and Rogerson model has not been empirically tested.

The purpose of this paper is to empirically test the Bertola and Rogerson hypothesis of a positive relationship between the degree of wage compression and job turnover on Swedish panel data. In order to accommodate the Swedish institutional framework, the Bertola and Rogerson model is extended by introducing the presence of two types of employment contracts and also taking into account that total labor input is a function of both the number of employees and the hours worked.

The first extension will allow for distinction between different types of employment contracts. The presence of fixed-term contracts with low adjustment costs makes

[2] A similar relationship can be found in Moene and Wallerstein (1997). They investigate the effects on growth of wage compression through centralized bargaining.

employment more responsive to shocks in labor demand and hence, more volatile. However, volatile temporary employment contracts can coexist with more stable employment for employees with permanent contracts. This can possibly lead to a segmented labor market where one group of workers with unstable employment acts as a buffer for changes in labor demand. The second extension takes into account that in response to a negative shock, firms can choose between reducing the number of employees and paying the relevant adjustment costs, or adjusting the number of hours worked by the existing employees.

Although the Bertola and Rogerson model is constructed to explain differences between economies, this paper will use Swedish data to analyze the relationship between wage compression and job reallocation at the industry level, i.e. explore industry-variation within Sweden. The advantage with this approach compared to a comparative study between different countries, is that controlling for differences that may affect job reallocation rates, in addition to the wage distribution, is more manageable between industries, or over time, in the same countries rather than between economies.[3] The data contain quarterly information on establishment employment turnover and wages for a panel of more than 10,000 establishments in the Swedish private sector, covering the time period 1992:3 to 1999:2.

The results suggest substantial sector heterogeneity. In the preferred model, industry heterogeneity is controlled for by using fixed effects. The effect of wage dispersion on job turnover is negative and significant in the manufacturing sector. This supports the Bertola and Rogerson hypothesis that a more compressed wage structure will result in higher job reallocation rates. Estimating separate models for job creation and destruction show a negative and significant effect of wage dispersion on job destruction, whereas the effect is insignificant in the job creation-equation. These results are consistent with wages being more rigid downwards than upwards. No support for the Bertola and Rogerson model is found in the non-manufacturing sector.

An issue to consider is endogeneity of the wage dispersion variable. I investigate this by performing IV-estimation. However, results from Hausman tests show that exogeneity can not be rejected.

[3]Moreover, due to data quality considerations such as country differences in sample coverage, definitions of establishments, the ability to link establishments over time and sector coverage, cross-country comparisons on gross job flows and wage dispersion are problematic. These problems are discussed in Davis and Haltiwanger (1999).

Further results include (i) a strong positive relationship between the industry-share of temporary employees and job turnover and (ii) a negative relationship between the amount of working-time flexibility and job reallocation.

The remainder of the paper is organized as follows: Section 2 gives a brief description of the Swedish labor legislation and also describes the Swedish wage-setting system. A theoretical model linking wage setting, employment protection legislation and turnover of jobs is presented in Section 3. Section 4 describes the data. The empirical specification is given in Section 5. Section 6 reports estimation results and discusses their implications for the observed stylized fact that European and US gross job flows are of the same magnitude. Section 7 concludes.

2 Labor Market Legislation and Wage Formation in Sweden

2.1 Labor Market Legislation

The Swedish Employment Protection Act (LAS) has traditionally been built on two principles: permanent contracts for an indefinite period of time as the normal type of employment and dismissals based on a just cause only. The legislation also contains rules on notification periods and priority rules in case of layoffs.[4] The last two decades have, however, seen a gradual liberalization of the labor laws. For instance, in 1982 the use of fixed-term contracts was relaxed with the introduction of a trial period. Strict priority rules such as last in, first out, have also been relaxed.

The Swedish labor legislation is much more restrictive for permanent than for temporary contracts. In order to restrict the use of temporary contracts, LAS gives a list of cases in which fixed-term contracts are allowed.[5] The most important cases where time-limited contracts are permitted are (i) one trial period of six months, (ii) seasonal

[4]In comparison, the US system is characterized by little statutory regulation at the federal level and varying legislation between states. The principle of employers' right to hire and fire at will lies at the basis of American labor legislation. Furthermore, employers are basically free to determine the length of prior notification, since there are no statutory provisions. Only in a few states are dismissals restricted by statutory requirements of just cause. A federal law, giving some protection to workers in case of mass layoffs, is found in the so-called Plant Closing Act from 1988. Finally, around 25 percent of the American workers are protected by provisions in collective agreements.

[5]Additional possibilities for fixed-term contracts may be created in collective agreements between employers' and employees' organizations.

or temporarily excessive work loads, (iii) replacement of employees on leave and a few other cases. In some cases, there are limitations to the duration of the contract. Employees are, for example, obliged to transform a temporary contract into a permanent one if the employee has had time-limited contracts for 3 years in a 5-year period.

In 1997, the rules concerning fixed-term contracts were relaxed in so far that these could be made without specifying an objective reason, under the condition that no more than five workers were employed on a fixed-term basis at the same time. Furthermore, the maximum duration of temporary contracts was extended to 18 months for newly established firms.

As concerns the termination costs of employment contracts, LAS is more restrictive for permanent than for temporary contracts. Under these circumstances, an increase in employment in a firm is likely to start by an increase in the number of temporary workers. In this way, firms obtain an option to transform a fraction of temporary contracts into permanent ones if there is a decrease in the initial uncertainty about demands. Analogously, in a downturn, temporary contracts are the first to be terminated. The lower termination costs associated with temporary contracts enable firms to reduce adjustment costs by using temporary workers as a buffer for employment adjustment. This implies larger volatility for temporary contracts as compared to permanent contracts.[6]

The possibility to use flexible workers in the production process differs between firms in different industries. Table 1 displays a large sector-variation in the fraction of temporary workers. Service industries like Trade, Real estate and Other services have the highest fraction, whereas the lowest fraction is found in traditional manufacturing industries such as Machinery, Electricity and Textiles. These fractions are fairly stable over time.

2.2 Wage Formation

Beginning in the 1950's, Swedish wage formation was heavily influenced by the principle of a solidarity wage policy for several decades. Solidarity wage policy with its emphasis on "equal pay for equal work" was one of the cornerstones of the so-called Swedish model. From an industrial policy perspective, the idea behind "equal pay for equal

[6]See Saint-Paul (1996), Cabrales and Hopenhayn (1997), Alonso-Borrego (1998), Boeri (1999), Goux *et al.* (2001) and Holmlund and Storrie (2002) for analyses of temporary and permanent employment.

work" was to set pressure on weak firms, and gradually force them out of the market, thereby enhancing industry productivity. Active labor market policy had the role of allocating labor from declining to expanding industries and regions. A solidaristic wage policy was also made possible by a highly centralized wage bargaining system.

During the period 1956-83, centrally negotiated wage agreements were made by the two main parties on the Swedish labor market: the Swedish Trade Union Confederation (LO) and the Swedish Employers' Confederation (SAF). These framework wage agreements were then followed by negotiations at both the industry and local levels. Centrally negotiated wage levels were more or less binding and often included special low wage provisions, aimed at increasing the relative wage of workers at the lower end of the wage distribution. As a result of the egalitarian wage policy, wage dispersion in Sweden, measured as the total variance of blue-collar workers, declined by 75 percent between 1962 and 1983 (see Hibbs and Locking (2000)). As pointed out by Hibbs and Locking (2000), the Swedish wage distribution was so compressed that a relative wage increase of around 30 percent was sufficient to carry a worker from the 10th to the 90th decile of the wage distribution.

The decline in wage inequality ended in the 1980's as a result of the breakdown of centrally negotiated wage agreements between LO and SAF. Wage agreements after 1983 have primarily involved industry-level negotiations. This implies that Sweden has gone from a tri-level bargaining system to a two-level system with industry- and plant-level bargaining. Furthermore, the egalitarian view on wage setting was gradually dissolved and practically disappeared around 1990. The effect of these changes in the wage-formation process on relative wages was quite dramatic: between 1983 and 1993, the relative wage dispersion increased by approximately 50 percent (see Edin and Holmlund (1992) and Hibbs and Locking (2000)). This means that wage dispersion in the early 1990's was of the same order of magnitude as that in the mid-1970's. The trend towards increased wage inequality continued in the 1990's, hand in hand with a more decentralized wage bargaining system. However, in an international perspective, the Swedish wage distribution is still very compressed.

3 Theoretical Model

In the model, there are two types of workers: those with permanent contracts (l_P) and those with temporary contracts (l_T). The contracts differ in hiring and firing

costs. Adjustment costs are higher for permanent contracts than for temporary ones. Given that the employment legislation concerning permanent and temporary contracts is identical in all industries, I assume that the possibilities to use temporary workers in the production process is exogenous. This means that firms in industries with a high fraction of temporary workers can use temporary contracts as a buffer for shifts in labor demand, to a higher degree than firms with a low fraction of temporary employees. Consequently, costs for adjusting the labor force become lower for firms with a high fraction of temporary employees than for those with a low fraction of such workers.

In every period, firms are subject to an idiosyncratic demand (or productivity) shock. With a certain probability the firm is either in a "good" state, θ_G, or in a "bad" state, θ_B, with $\theta_B < \theta_G$. As in Bertola (1990), I assume the state of each firm to be characterized by a two-state Markov process. The transition probability, p, is symmetric with p being the probability of a firm moving from the good (bad) state to the bad (good) state. Each firm's state is uncorrelated with the state of the other firms, so in every period $p/2$ firms switch from the good to the bad state at the same time as $p/2$ firms move from the bad to the good state.

Firms maximize the discounted value of current and future profits. For each time period and state $i \in \{G, B\}$, the profits of a representative firm in industry k is given by

$$\Pi_k^i = (\theta^i L - \frac{1}{2}\beta L^2) - w^i L - AC_P - AC_T, \tag{1}$$

where the term in parenthesis is the firm's revenue. $L = L_P + L_T$ is total labor input consisting of permanent and temporary labor. AC_P and AC_T are adjustment costs for permanent and temporary workers, respectively. In every period, the firm sets employment so that the expected marginal revenue product is equal to the wage plus adjustment-costs. The marginal revenue product is equal to θ^i - βL^i. Wages are exogenous but state-contingent in the model, which means that $w = w^G$ in "good" times whereas $w = w^B$ in "bad" times, with $w^G > w^B$.[7]

The model can apply to any type of wage-setting institutions, since the degree of wage dispersion will determine job turnover. In a highly decentralized system, wages will be determined by the state of the individual firm, giving rise to wages closely

[7]In accordance with Swedish wage formation, wages are the same for permanent and temporary employees.

linked to the current state. This will not be the case in a centralized system where the relationship between individual productivity and wages is weaker, and therefore, the difference between w^G and w^B becomes smaller.

L in equation (1) can be seen as total labor input, which includes both the number of workers and the number of hours worked. Subsequently, total labor input can be decomposed as the product of the number of employees and the amount of hours they work per period, that is $L = hl$. Assuming that working hours are only flexible upwards, the number of working hours, h, in the two states is given by

$$\begin{aligned} h^G &= h(1+\sigma) && \text{if } \theta = \theta^G \\ h^B &= h && \text{if } \theta = \theta^B, \end{aligned} \tag{2}$$

where h is the negotiated number of hours per employee and σ the amount of overtime employers can use during expansions, $\sigma \in (0, \sigma^{MAX}]$.[8] We can now rewrite the firm's marginal revenue product with respect to L:

$$\begin{aligned} MRP_L^G &= \theta^G - \beta h(1+\sigma)l^G \\ MRP_L^B &= \theta^B - \beta h(1+\sigma)l^B. \end{aligned} \tag{3}$$

To solve for optimal labor demand in the two states, we must first define the firm's value function in the two states. The value functions for firms in expansion (V^G) and recession (V^B) are given by:

$$\begin{aligned} V^G &= \theta^G - \beta h(1+\sigma)l^G - w^G + \frac{1}{1+r}\left[(1-p)V^G + pV^B\right] \\ V^B &= \theta^B - \beta h l^B - w^B + \frac{1}{1+r}\left[pV^G + (1-p)V^B\right]. \end{aligned} \tag{4}$$

The optimal employment policy for firms in expansion implies that the marginal increase in revenue from hiring Δl people is equal to the marginal cost of hiring.

[8] In Europe, the scope for substituting hours for workers is in reality asymmetric. Employers cannot decrease the number of hours worked for the workforce, and thereby save on the wage per employee. In many European countries the extent to which overtime is used is determined through negotiations between employer organizations and unions or through legislation.

Analogously, for firms in recession, the optimal employment policy is to equate the shadow loss of revenues from dismissing workers to the firing cost. The total cost of hiring (firing) is equal to $\Delta l_P H_P + \Delta l_T H_T$ ($\Delta l_P F_P + \Delta l_T F_T$) where Δl_P and Δl_T are the change in the number of permanent and temporary employees, respectively. H_P, H_T, F_P and F_T are the hiring-and firing costs for the two types of workers with $H_P > H_T$ and $F_P > F_T$. This implies that firms with a higher fraction of temporary employees can more easily adjust to changes in demand by using their stock of temporary workers. Substituting the optimal employment policies into (4) yield the following expression for labor demand in the two states:

$$\begin{aligned} l^G &= \frac{1}{\beta h(1+\sigma)}\left[(\theta^G - w^G) - \frac{r+p}{1+r}H - \frac{p}{1+r}F\right] \\ l^B &= \frac{1}{\beta h}\left[(\theta^B - w^B) + \frac{p}{1+r}H + \frac{r+p}{1+r}F\right], \end{aligned} \tag{5}$$

where $H \equiv \Delta l_P H_P + \Delta l_T H_T$ and $F \equiv \Delta l_P F_P + \Delta l_T F_T$. In steady state, 50 percent of the firms are in the good state while the remaining 50 percent are in the bad state. Given the symmetric Markov process, job creation equals employment growth in expanding industries $(l_G - l_B)$ times the number of firms moving from recession to expansion $(p/2)$. Analogously, job destruction is equal to $|(p/2)(l_B - l_G)|$. Summing job creation and job destruction yields job reallocation. The job reallocation rate (JRR) is given by:

$$JRR = \frac{1}{N}\left[\frac{p}{2}(l^G - l^B) + |\frac{p}{2}(l^B - l^G)|\right] = \frac{p}{N}(l^G - l^B), \tag{6}$$

where N is total employment. Substituting (5) into (6) yields the following expression for the job reallocation rate:

$$JRR = \frac{p\left[(\theta^G - \theta^B) - (w^G - w^B) - \frac{r+2p}{1+r}(H+F) - \sigma\left(\left(\theta^B - w^B\right) + \frac{p}{1+r}H + \frac{r+p}{1+r}F\right)\right]}{\beta h(1+\sigma)N}. \tag{7}$$

Equation (7) gives several qualitative predictions. First, we see that job reallocation is increasing in the degree of wage compression. It is also evident that job reallocation is increasing in the probability of switching states and decreasing in the amount of hours worked and overtime. A more frequent use of overtime in expansions will lead

to less new hires and less turnover of labor. Furthermore, job reallocation is increasing in the difference between θ^G and θ^B, a measure of the volatility of the business cycles. Finally, the level of job reallocation is negatively related to the cost of hiring and firing the two types of workers which, in turn, depends on the fraction of temporary employees. Firms with a high fraction can more easily adapt to changes in demand by adjusting its stock of temporary employees, since the hiring and firing costs are lower for temporary than for permanent contracts.

4 Data and Descriptive Statistics

The data are collected from Short Term Wage Statistics (*Konjunkturstatistik, löner för privat sektor*) and Short Term Employment Statistics (*Kortperiodisk Sysselsättningsstatistik*) collected by Statistics Sweden (SCB). Each data set contain information on employment turnover and wages for a panel of more than 10,000 establishments in the non-agriculture private sector. The two data sets cannot be matched at the establishment level. However, by means of industry sic-codes, the data can be matched at different industry levels.

The Short Term Wage Statistics collects information on wages for blue- and white-collar workers.[9] Data are quarterly for the period 1991-95 and monthly from 1996 and onwards. A representative sample is drawn from the population of private sector establishments with at least five employees, stratified according to industry affiliation and employment size. The sample covers establishments/plants in machinery and mining (sic-codes C+D) and firms in the service sectors. For blue-collar workers, the survey asks questions about number of hours worked, number of overtime hours worked, *average hourly earnings* excluding and including retroactive wage supplements and number of blue-collar employees. For white-collar workers the variables are negotiated *monthly salary* excluding and including commissions and retroactive wage supplements and number of white-collar employees. 5-digit sic-codes are available for all firms and establishments.

The real wages were more or less unchanged during the period 1992-95, but have thereafter increased. The real wage increases after 1995 are mostly due to the low inflation in this time period. During this period, inflation decreased from above 10

[9] The two groups of workers are defined according to their union affiliation.

percent in 1991 to around zero in 1999. The annual mean real wage increase for the entire period is 2.2 percent for blue-collar workers and 2.7 percent for white-collar workers. Table 1 shows descriptive statistics for 14 industries (6 in manufacturing and 8 in non-manufacturing), and displays inter-industry wage differentials. For blue-collar workers, the highest wages are found in Mining and Wood, whereas the lowest wages are in Hotel and Banking. White-collar workers receive the highest wages in Banking and Chemistry and the lowest in Transport and Hotel.

- Table 1 about here -

The coefficient of variation, CV $(= std(W)/mean(W))$, is used as a measure of total and industry wage dispersion. Each group is weighted by its share of the total wage sum. When computing industry variation in wages, I use the share of wages for white- and blue-collar workers for each industry and time-period, V_{itW} and V_{itB}, respectively. Hence, the following measures for variation in wages for industry i at time t will be used:

$$CV_{it} = V_{itB}\frac{std(W_{itB})}{mean(W_{itB})} + V_{itW}\frac{std(W_{itW})}{mean(W_{itW})}, \tag{8}$$

where t=1992:3, ... ,1999:2 and i=1, ... ,14.[10] The mean coefficient of variation for the whole period is 0.14 for blue-collar workers and 0.17 for white-collar workers. Industry means are presented in Table 1.

The Short Term Employment Statistics contain quarterly information on the stock of permanent and temporary contracts as well as direct information on hires and separations for permanent and temporary workers. The data are obtained from a random sample from the population of private-sector establishments of all sizes in Sweden, stratified according to industry affiliation and establishment size. The establishments are randomly divided into three equal groups. Every quarter, each group responds to questions on employment and worker turnover for one month in the quarter each. The information on the number of employees refers to a particular date in the month, while separations and hires refer to flows during the entire month. As an example, one third of the sampled establishments in the second quarter reports information for

[10] This measure of wage compression is an approximation to the to $(w^G - w^B)$ in equation (8). Data considerations have made this operationalization necessary.

April, while the other two groups report the corresponding information for May and June. The information on establishment employment as well as hires and separations is supplied for both permanent (time unlimited) and temporary (time limited) contracts, separately for men and for women (see Arai and Heyman (2002) for more details on the data).

Job flows are computed on the basis of the changes in number of the employees (n) at establishment (e) over time (t).[11] Job Creation (JCR) and Job Destruction (JDR) rates for sector i at time t are given by:

$$JC(D)R_{it} = \frac{\Sigma_e(|n_{et} - n_{e,t-1}|)}{\Sigma_e 0.5(n_{et} + n_{e,t-1})}; \qquad if \quad n_{et} \geq (\leq) n_{e,t-1}. \tag{9}$$

Net employment growth is equal to the difference between JCR and JDR, whereas job reallocation is equal to the sum of JCR and JDR. Table 2 presents mean quarterly gross job flow rates for the period 1992-99.

- Table 2 about here -

Job creation is, on average, 4 percent and varies between 2 and 6 percent. The corresponding figure for job destruction is 5 percent, varying between 3 and 6 percent. The mean job reallocation rate is 9 percent. Gross job flows are higher in non-manufacturing than in manufacturing. If we study individual industries among 14 two-digit industries, we see that the highest job reallocation rates are observed in Hotel, Construction and Real estate (see Table 1). The lowest job reallocation rates are observed in Textiles, Chemistry and Metal and manufacturing.

5 Empirical Specification

I use the following specification to test the predictions from equation (7) empirically:

$$JRR_{it} = \beta_0 + \beta_1 WDISP_{it} + \beta_2 TEMP_{it} + \beta_3 OVERT_{it} + \beta_4 NET_{it} + \nu_i + \varepsilon_{it}, \tag{10}$$

[11] In order to be compatible to the wage data, job flows are calculated for establishment with at least five employees.

where JRR_{it} is job reallocation in industry i at time t, $WDISP$ is wage dispersion, $TEMP$ is the fraction of temporary employees in relation to total employment and $OVERT$ is the fraction of overtime hours in relation to the total number of hours worked. All equations include NET, net employment change, reflecting the current state of the business cycle. ν_i is a time-invariant industry-specific effect and ε_{it} is the usual error term.

Equation (7) states job reallocation as a function of wage dispersion, adjustment costs and the use of overtime. The coefficient of variation is used as a measure of wage dispersion. I use the fraction of temporary employment as a proxy for labor adjustment costs. A high fraction of temporary labor will lead to lower adjustment costs when there is a change in the firms' stock of employees. All variables are industry-sector means (6 manufacturing and 8 non-manufacturing industries), roughly corresponding to the two-digit system of industry classification.[12]

Since downward wage rigidity is likely to be more common, wage dispersion is expected to have a larger effect on job destruction than on job creation.[13]. Therefore, I will also estimate equation (10) with job creation and job destruction separately as dependent variables.

The fixed-effect model assumes the industry specific effect, v_i, to be non-stochastic. The random-effect model assumes that the industry-specific error term is random and drawn from a common distribution independent of the explanatory variables. The random-effect estimator is a weighted average of the estimates produced by the between and within estimators. This means that both cross-sectional and time-series variation are taken into account. A priori, it is not obvious which model should be used.

The Hausman test is used for discriminating between the fixed- and the random-effect models (see Baltagi (1995)). This is a test for correlation between the industry-

[12]In 1993, the Swedish industry classification changed systems from SNI69 to SNI92. The 14 industries analyzed in this paper are constructed so as to enable comparisons over time. To check for robustness to industry classification, all equations are re-estimated using the new industry classification. This corresponds to data for 47 industries in the period 1993 to 1999. With a few exceptions, the results remain unchanged.

[13]Recently, several papers have used survey data consisting of interviews with managers and labor representatives to explore the mechanism behind nominal wage rigidity and why nominal wages are so insensitive to macroeconomic shocks (see e.g. Agell and Lundborg (1999) and Bewley (1998)). In the Agell and Lundborg (1999) study, covering firms in the Swedish manufacturing sector, only 2 firms out of 153 had experienced nominal wage cuts during the 1990's. The low figure is despite the largest economic downturn in Sweden since the 1930's, with total unemployment increasing from 4 percent in 1991 to more than 12 percent in 1993.

specific effects and the independent variables. If the correlation between v_i and the regressors is different from zero, the GLS estimator is biased and inconsistent. The fixed-effect estimator is, however, still consistent.

An issue to consider is endogeneity of the wage dispersion variable. This could arise if for instance firm and industry characteristics in industries with high job flows influence the distribution of wages. Industry-differences in e.g. market structure, union power and the possibilities to use temporary employees as well as technological and organizational differences may affect both job flows and the distribution of wages in a systematic way. To deal with the possibility of endogeneity in the wage dispersion variable, IV-estimations and Hausman tests for endogeneity are presented.[14] Lagged values of wage dispersion is used as an instrument for wage dispersion.

6 Results

Results are presented in Tables 3 and 4. Table 3 shows the results from the estimates of equation (10) for the entire private sector, as well as for the manufacturing and service sectors separately. Furthermore, estimations are also presented using the 90-10th percentile ratio as an alternative measure of wage dispersion. Table 4 shows results when job creation and job destruction are used separately as dependent variables. This table also presents results from IV-estimations.

- Table 3 about here -

The Hausman tests presented in Tables 3 and 4 and in unreported estimated equations shows that the random-effects estimator can be rejected. The positive correlation between the regressors and the individual-specific effects means that there exist unobservable effects that differ between industries. This, in turn, leads to a non-zero correlation between ε_{it} and $\mathbf{X}_{it}$, i.e. the regressors are endogenously determined. By performing the fixed-effects technique, the ν_i are wiped out, leaving the within estimator unbiased and consistent for β.

Considering the results for job reallocation, it appears that for the entire private sector (column 1), $WDISP$ is not significant. The other variables all have expected

[14] The Hausman test tests if there is a significant difference between the OLS- and the IV-estimator. Both estimators are consistent in the case of no correlation between the regressors and the errors. However, only the OLS-estimator is efficient. If the null hypothesis is false, only the IV-estimator is consistent.

signs and are significantly different from zero. The positive sign for $TEMP$ indicates a higher job turnover in firms in industries with a high fraction of temporary employees. These firms can more easily adapt to changes in labor demand by changing the number of temporary employees.

The coefficient for the overtime variable, $OVERT$, is negative, suggesting that job turnover is lower in industries where the number of working hours is relatively flexible. Rather than changing the number of employees, firms can adjust the number of hours worked per employee.

The descriptive statistics presented in Tables 1 and 2 point to large differences between industries in manufacturing and non-manufacturing. Therefore, there are reasons to believe that differences between the two sectors influence the estimated coefficients. An insignificant effect of wage dispersion on job turnover at the aggregate level might mask important sector heterogeneity.

To check for sector heterogeneity, separate regressions are estimated for manufacturing and non-manufacturing, respectively. These regressions are reported in columns 2 and 3. The effect of wage dispersion on job turnover is negative and significant in manufacturing. The estimate of $WDISP$ suggests that in the manufacturing sector, a more compressed wage structure will lead to a higher job reallocation rate, a result in accordance with the Bertola and Rogerson hypothesis. The point-estimate is equal to -0.33, implying that a one standard deviation increase in $WDISP$ reduces JRR by 0.007, which amounts to 12 percent of the median of JRR. The corresponding effect is 14 percent for an industry in the 10th percentile of the job reallocation distribution.

Interaction terms between the wage dispersion variable and the share of temporary employees, overtime and net employment change, are never significant. Hence, I find no evidence for that the effect of wage dispersion differ systematically between firms with different fractions of temporary employees or a different use of overtime. Furthermore, results are unchanged when I add a time dummy for periods when rules for fixed-term contracts were relaxed.

Turning to the non-manufacturing sector, the effect of wage dispersion is reversed. Column 3 displays that there is a positive and significant correlation between $WDISP$ and JRR in the service sector.

Hence, sector differences seem to play an important role in determining the relationship between the degree of wage compression and job reallocation. For instance, technological and organizational differences between, on average, larger capital-intensive

firms in manufacturing and smaller firms in the service sectors may affect the extent to which jobs are reallocated.[15] Furthermore, unions have traditionally been weaker in the service sector than in the manufacturing industry. These differences may affect the extent to which the wage distribution has an impact on job turnover.

The coefficients for the fraction of temporary employees and overtime have expected signs and are significant in both sectors. Net employment change is negative and significant in manufacturing, thereby reflecting a countercyclical pattern in job reallocation. This is not the case in the non-manufacturing sector where the coefficient for employment growth is insignificant.[16]

An often used measure for wage dispersion is the 90-10th percentile ratio. To check for robustness of the results, I also estimate equations using this ratio instead of the coefficient of variation. Results are displayed in columns 4–6 in Table 3.

Results for the wage dispersion variable remain qualitatively unchanged. The effect of wage dispersion on job turnover is negative in manufacturing, whereas it is positive in non-manufacturing. The estimated coefficient is equal to –0.09 in the manufacturing sector, which means that an increase in wage dispersion with one standard deviation reduces job reallocation by 0.005. Given that the median job reallocation rate is equal to 0.056, the estimate indicates that a one standard deviation increase in wage dispersion reduces total job turnover in the median industry by approximately 9 percent.

Table 4 display results when job creation and job destruction are used separately as dependent variables.

- Table 4 about here -

The wage dispersion variable is not significant, neither in the full sample nor in the non-manufacturing sector. In manufacturing, $WDISP$ is negative for both job creation and job destruction, having a higher coefficient value (in absolute terms) for job destruction. However, the variable is only significant for job destruction.

For the manufacturing sector, results support that wages being more downward than upward rigid, thereby leading to higher rates of job turnover in economic downturns than in economic upturns. Hence, asymmetric wage rigidity, in the sense of more

[15] Arai and Heyman (2002) find clear differences both in levels and in the cyclical pattern for job reallocation between manufacturing and service sectors.

[16] This result is in line with Boeri (1996), stressing that countercyclical job turnover mainly concerns the manufacturing industry.

downward than upward rigid wages, seems to influence the way in which the wage distribution affects job turnover. More specifically, the combination of a compressed wage structure, more downward than upward rigid wages, and a deep recession in the Swedish manufacturing sector in the early 1990's may explain the observed sector differences regarding the relationship between the wage structure and the reallocation of jobs. The sector differences result in support for the Bertola and Rogerson hypothesis of a negative relationship only in the manufacturing sector. However, the economic significance is rather small; only a small fraction of the variation in job turnover is explained by the degree of wage compression.

Finally, to exploit the question of endogeneity in wage dispersion, instrumental variable regressions are estimated together with Hausman tests for endogeneity. Columns 7-9 in Table 4 display the results. In all equations, one year lags of $WDISP$ are used as instrument for $WDISP$.[17] As can be seen from Table 4, IV-estimation does not change the results for wage dispersion. The wage dispersion variable is negative and significant in both the equation with total job reallocation as the dependent variable (column 7), and the equation with job destruction as the dependent variable (columns 8 and 9). The other explanatory variables remain qualitatively unchanged. The similar results obtained from OLS and IV are confirmed by the Hausman tests. These show that there is no significant difference between the OLS-estimator and the IV-estimator. This suggests that exogeneity of $WDISP$ cannot be rejected, and we can rely on results obtained in the industry fixed-effects models discussed above.

7 Summary and Conclusions

One puzzle from the job creation and job destruction literature is the similar pattern of flows observed in both the US and Europe. Given that the labor market is more regulated in Europe than in the US, we would expect significantly higher job flows in the US than in Europe. Bertola and Rogerson propose one explanation for the observed similarities. They argue that differences in wage-setting institutions play an important role and that there exists a positive relationship between the degree of wage-compression and gross job flows. This means that job flows in countries with high

[17] Results are robust to time-structure choice. I have also instrumented CV in a random-effects model without any changes in results. Furthermore, a Hausman-test comparing the random-effects model, with and without instruments, is not rejected.

adjustment costs and a compressed wage structure can be similar to those in countries with low adjustment costs and a high wage dispersion.

This paper is use Swedish establishment data on job turnover and wages to test the Bertola and Rogerson hypothesis. The estimation technique is industry fixed-effects estimation on 14 two-digit industries. Results indicate large sector differences regarding the effect of the degree of wage dispersion on job reallocation. In accordance with Bertola and Rogerson, the effect is positive in the manufacturing sector. Furthermore, estimating separate regressions for job creation and job destruction yields a negative and significant effect of wage dispersion on job destruction and an insignificant effect on job creation. These results are in accordance with wages being more downward than upward rigid.

The quantitative effect of the impact of wage dispersion on job turnover is, however, relatively small. A one standard deviation increase in wage dispersion reduces the total job reallocation by around 10 percent. This means that for Sweden, the Bertola and Rogerson hypothesis can not explain industry-variation in job reallocation Turning to the non-manufacturing sector, the Bertola and Rogerson hypothesis is not supported.

Further results include (i) a very strong positive effect of the industry-share of temporary employees on job reallocation and (ii) a negative relationship between the use of overtime and job turnover. The latter result suggests that job reallocation is lower in industries where the number of hours worked is flexible.

If the fraction of temporary employees is taken as a proxy for adjustment costs for labor, then the results indicate that the largest part of the variation in gross job flows is explained by differences in the cost of adjusting labor, rather than by differences in wage dispersion. As much as 25 percent of the variation in job flows is explained by variation in the industry-share of temporary employment. This result is in accordance with Arai and Heyman (2002), stressing the importance in distinguishing between permanent and temporary contracts when studying job flows.

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›les

le 1. Descriptive statistics. 14 industries, 1992:3–1999:2.

		Real Wage Blue-collar workers	Real Wage White-collar workers	CV Blue-collar Workers	CV White-collar Workers	CV All Workers	p(90/10) Blue-collar Workers	p(90/10) White-collar Workers	p(90/10) All workers	Fraction overtime	Fraction Temporary Employees
cing:	Mean	74.75	19954	0.15	0.23	0.23	1.44	1.64	1,64	0.03	0.07
	STD	5.24	1463	0.03	0.02	0.02	0.08	0.09	0,09	0.01	0.02
›truct.:	Mean	92.80	19087	0.11	0.16	0.13	1.28	1.46	1,34	0.02	0.11
	STD	4.31	894	0.01	0.01	0.01	0.03	0.03	0,02	0.00	0.03
tr. Etc.:	Mean	92.23	18189	0.11	0.12	0.12	1.28	1.30	1,29	0.04	0.04
	STD	5.98	1141	0.02	0.02	0.02	0.05	0.03	0,03	0.01	0.02
Estate:	Mean	79.41	18072	0.12	0.19	0.16	1.33	1.42	1,38	0.02	0.12
	STD	5.35	876	0.02	0.02	0.01	0.05	0.06	0,04	0.00	0.02
i:	Mean	85.21	18276	0.11	0.17	0.13	1.32	1.39	1,35	0.03	0.15
	STD	5.17	1502	0.01	0.03	0.01	0.04	0.05	0,04	0.00	0.03
:l-Res.:	Mean	72.21	16787	0.13	0.15	0.13	1.36	1.39	1,37	0.01	0.28
	STD	3.23	915	0.02	0.03	0.02	0.06	0.06	0,05	0.01	0.04
_nistry:	Mean	90.74	20099	0.13	0.14	0.14	1.37	1.38	1,38	0.04	0.06
	STD	6.41	1633	0.01	0.01	0.01	0.03	0.05	0,04	0.00	0.01
hinery:	Mean	88.80	19537	0.12	0.16	0.13	1.32	1.37	1,35	0.04	0.05
	STD	5.94	1457	0.01	0.01	0.01	0.03	0.05	0,04	0.01	0.01
ing:	Mean	106.34	19301	0.13	0.13	0.13	1.37	1.40	1,38	0.06	0.08
	STD	7.61	1480	0.01	0.03	0.01	0.04	0.07	0,04	0.01	0.04
Serv.:	Mean	80.67	17990	0.18	0.22	0.22	1.50	1.71	1,68	0.02	0.14
	STD	4.25	1156	0.02	0.01	0.01	0.03	0.04	0,04	0.00	0.01
:ile:	Mean	76.61	17483	0.12	0.16	0.13	1.30	1.48	1,35	0.02	0.04
	STD	4.85	953	0.01	0.01	0.01	0.03	0.06	0,02	0.00	0.01
le:	Mean	80.08	17820	0.13	0.20	0.17	1.33	1.55	1,46	0.02	0.12
	STD	5.23	1133	0.01	0.01	0.01	0.02	0.03	0,03	0.00	0.01
›sport:	Mean	87.86	16718	0.16	0.19	0.17	1.37	1.47	1,42	0.04	0.12
	STD	5.98	1384	0.02	0.02	0.01	0.03	0.04	0,03	0.01	0.02
›d etc.:	Mean	96.14	18413	0.17	0.18	0.17	1.49	1.42	1,46	0.04	0.06
	STD	6.82	1533	0.01	0.02	0.01	0.05	0.07	0,05	0.00	0.01
ufact.:	Mean	90.64	18851	0.13	0.15	0.14	1.36	1.41	1.38	0.04	0.07
	STD	11.06	1672	0.02	0.03	0.02	0.07	0.07	0.06	0.01	0.04
manu.:	Mean	82.50	18077	0.14	0.18	0.17	1.36	1.49	1.45	0.03	0.12
	STD	8.75	1510	0.03	0.04	0.04	0.08	0.14	0.14	0.01	0.07

le 2. Descriptive statistics. Quarterly gross job flow rates, 1992:3–1999:2.

		Job Creation	Job Destruction	Job Reallocation	Net Empl. Change
/ate sector	Mean	0.04	0.04	0.08	-0.01
	STD	0.02	0.03	0.04	0.01
nufacturing	Mean	0.03	0.03	0.06	-0.01
	STD	0.02	0.02	0.02	0.01
ı-manufacturing	Mean	0.05	0.05	0.10	0.00
	STD	0.03	0.03	0.04	0.01

Table 3. Effects of wage dispersion, fraction temporary employees and use of overtime on job flows. Industry fixed-effects regressions. Dependent variable is job reallocation rate (*JRR*). Robust standard errors in parenthesis.

	1	2	3	4	5	6
	Job reallocation rate (*JRR*)					
Wage dispersion	0.09	0.26**	-0.33***	0.02	0.09**	-0.09***
	(0.09)	(0.12)	(0.11)	(0.03)	(0.04)	(0.03)
Fraction temp. employees	0.30***	0.25***	0.38****	0.30***	0.22**	0.38***
	(0.06)	(0.09)	(0.06)	(0.06)	(0.10)	(0.06)
Overtime	-0.56*	-0.54***	-0.56*	-0.55***	-0.53***	-0.62***
	(0.16)	(0.21)	(0.32)	(0.16)	(0.21)	(0.32)
Net employment change	-0.15**	-0.11	-0.15*	-0.16**	-0.16	-0.16*
	(0.07)	(0.11)	(0.10)	(0.08)	(0.11)	(0.10)
Industry	All	Non-manuf.	Manuf.	All	Non-manuf.	Manuf.
Measure of wage dispersion	CV	CV	CV	p(90/10)	p(90/10)	p(90/10)
Hausman test	4.54	14.3***	7.6*	6.7	12.8***	8.0*
Breusch and Pagan test	459***	67.4***	1.5	466***	94***	1.8
R^2	0.78	0.76	0.52	0.78	0.76	0.51
N	392	224	168	392	224	168
T	28	28	28	28	28	28

Notes:
(i) * indicate significance at the 10%-level, ** at the 5%-level and *** at the 1%-level.
(ii) Industry classification corresponds to 14 industries (6 in manufacturing and 8 in non-manufacturing).
(iii) CV=coefficient of variation. p(90/10)= 90-10th percentile ratio.
(iv) The Breusch and Pagan LM-test is a test for individual (industry) random effects. The null-hypothesis is that the variance in the industry-specific effects is equal to zero. Rejection of the null shows that industry-specific effects are present.

Table 4. Effects of wage dispersion, fraction temporary employees and use of overtime on job flows. Industry fixed-effects regressions. Dependent variables are job creation rate (*JCR*) and job destruction rate (*JDR*). Robust standard errors in parenthesis.

	1	2	3	4	5	6	7	8	9
	Job creation rate (*JCR*)			Job destruction rate (*JDR*)			*JRR*	*JCR*	*JDR*
ge Dispersion	0.05	0.13	-0.10	0.04	0.14	-0.23*	-0.57**	-0.09	-0.48*
	(0.07)	(0.09)	(0.07)	(0.09)	(0.11)	(0.12)	(0.26)	(0.18)	(0.29)
ction temp. employees	0.55*	0.50***	0.57***	-0.24***	-0.25***	-0.19***	0.40***	0.63***	-0.23***
	(0.05)	(0.07)	(0.08)	(0.05)	(0.07)	(0.06)	(0.07)	(0.09)	(0.07)
ertime	0.14	0.10	0.52**	-0.70***	-0.64***	-1.08***	-0.82**	0.74***	-1.56***
	(0.13)	(0.16)	(0.29)	(0.17)	(0.22)	(0.36)	(0.42)	(0.29)	(0.47)
employment change	0.21*	0.34***	-0.02	-0.37***	-0.45***	-0.13	-0.18	0.10	-0.28**
	(0.06)	(0.08)	(0.08)	(0.08)	(0.11)	(0.11)	(0.14)	(0.10)	(0.14)
ustry	All	Non-manuf.	Manuf.	All	Non-manuf.	Manuf.	Manuf.	Manuf.	Manuf.
asman test	63.6***	37.7***	141***	79.2***	50.5***	45.8***	14.3	14.3	4.9
usch and Pagan test	122***	43.5***	16.7***	67.7***	3.9**	6.3***			
	0.74	0.72	0.69	0.54	0.56	0.32	0.51	0.72	0.34
est	50.56	40.75	21.67	25.17	18.03	10.84	13.94	21.38	6.51
	392	224	168	392	224	168	144	144	144
	28	28	28	28	28	28	28	28	28

otes:

* indicate significance at the 10%-level, ** at the 5%-level and *** at the 1%-level.

) CV=coefficient of variation is used as a measure for wage dispersion. Results remain qualitatively unchanged when p(90/10) is used as a measure for wage dispersion.

i) Industry classification corresponds to 14 industries (6 in manufacturing and 8 in non-manufacturing).

) The Breusch and Pagan LM-test is a test for individual (industry) random effects. The null-hypothesis is that the variance in the industry-specific effects is equal to zero. Rejection of the null shows that industry-specific effects are present.

) Columns 7-9 are estimated using instrumental variable analysis. The Hasuman test in columns 7-9 tests if there is a systematic difference between the OLS- and the IV-estimator.

Chapter 3

Wages, Profits and Individual Unemployment Risk: Evidence from Matched Worker-Firm Data

III

Wages, Profits and Individual Unemployment Risk: Evidence from Matched Worker-Firm Data

Mahmood Arai* and Fredrik Heyman‡

Abstract

We present new evidence on the extent of rent-sharing, based on a large panel of matched worker-firm data for Sweden. Controlling for worker and firm heterogeneity, as well as examining the problem of endogeneity of profits, we report evidence implying the existence of rent-sharing. Another result is that unemployment risk, aggregated at the firm and various industry levels, has a negative effect on individual workers' wages after controlling for individual differences in unemployment risk.

Keywords: Wages, Profits, Rent-sharing, Wage-curve

JEL Classification: D31, J31

1 Introduction

Empirical work on the impact of firm performance and labor market conditions on wages has departed from the basic theoretical conjuncture that firms' ability to pay and workers' possibilities of finding jobs in the event of unemployment, are two important determinants of individual wages. According to bargaining models, individual wages are positively correlated to firm profits and vary negatively with workers' unemployment risk. A positive correlation between profits and wages is also compatible with a competitive model with friction. In addition, incentive theories of wages and efficiency-wage hypotheses suggest that high wages lead to high profits.[1]

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[1]For bargaining models, see Oswald (1982, 1985) and Manning (1994). For a positive correlation between profits and wages in a competitive framework with frictions, see Blanchflower *et al.* (1996). For efficiency wage theories, see Akerlof and Yellen (1986).

There is a body of empirical literature dealing with the impact of profits on wages based on aggregated data.[2] The data used in these studies are aggregated on the worker and/or the firm side, suppressing within industry variation and/or neglecting worker heterogeneity.[3] The estimated elasticity of wages with respect to profits in these studies, based on a number of varying data sources, is amazingly similar across countries, ranging between 0.01 and 0.05.

The purpose of this study is to examine the extent of rent-sharing and the impact of individual and aggregated unemployment risk on the wages of individual workers. We use a sample of over 170,000 Swedish employees for 1991 and 1995, matched with their employing firm's balance-sheet information. The matched data contain detailed information on individual characteristics, including individual unemployment experience from 1992-1995, as well as annual balance-sheet information for the period 1987-1995. These data are matched with the Swedish Establishment Survey (APU), which contains explicit information on product market competition and product demand elasticity.

The contribution of this paper is to provide evidence on the impact of profits and unemployment risk on wages based on disaggregated individual and firm data. As such, the study explicitly deals with the problems of firm and worker heterogeneity, as well as the issue of endogeneity of profits. A novelty is that the relationship between aggregate unemployment risk and wages is estimated taking into account that marginal workers have both lower wages and a higher unemployment risk.

The results of our study imply a positive effect of profits on wages that are robust across various model specifications and exist for both 1991 and 1995. The results for 1991 indicate that individual wages are positively correlated to firm average profits for 1987-1990. A similar pattern is observed regarding the impact of profits for 1991-1994 on individual wages in 1995.

Our evidence provides strong support for the existence of rent-sharing as this effect on wages is found for both 1991 and 1995, two periods characterized by very different business cycle phases. The unemployment rate in Sweden was extremely low in the late

[2]See Katz and Summers (1989), and Blanchflower *et al.* (1996) on the US. Abowd and Lemieux (1993) and Christofides and Oswald (1992) use Canadian contract data. For European studies, see Blanchflower *et al.* (1990), Holmlund and Zetterberg (1991), Nickell *et al.* (1994), and Hildreth and Oswald (1997).

[3]Abowd *et al.*(1999) and Arai (1999) use microdata on wages and profits for the private sector. Margolis and Salvanes (2001) examine microdata on males in manufacturing. The data in Abowd *et al.* and Margolis and Salvanes do not include unemployment, while Arai (1999) only uses regional unemployment on a small sample of employees with a limited panel dimension.

1980's, less than 2 percent while the profits reported for the 1991-1994 period reflect firm performance during the deepest recession in Swedish history since the 1930's. The implied elasticities are between 0.008 and 0.025 in 1991 and between 0.005 and 0.012 in 1995, falling within the range of previously reported elasticities using other sources of data in various other countries. Moreover, we observe that 75 percent of the effects of profits on wages are within industry effects, indicating that rent-sharing takes place mainly at local-level bargaining in Sweden. The significant impact of profits on wages remains intact when controlling for time-invariant individual- and firm-specific heterogeneity. Our elasticities imply that, due to the spread in profits, wage inequality in Sweden is as high as 13 percent of the mean wages in 1991, according to Lester's range of pay (Lester (1952)).[4]

Reported elasticities may, however, be an underestimation of the extent of rent-sharing due to the accounting relationship between wages and profits, implying that higher wages lead to lower profits. On the other hand, according to theories of incentive pay and various versions of efficiency wage theory, higher pay implies higher profits. These problems can be dealt with using instruments for profits in estimations. Besides lagged profits, previous studies have used instruments such as energy costs (Blanchflower *et al.* (1996)) and import selling prices (Abowd and Lemieux (1993)). Abowd and Lemieux report elasticities increasing by 10 times when profits are instrumented. Using employer-reported short term product market elasticity and the number of competitors as instruments for profits yields results indicating that our estimated elasticities are underestimated. The elasticities implied by the IV-estimates suggest that Lester's measure of wage inequality due to profits is as high as 50 percent of the mean wages.

We also investigate the impact of individual heterogeneity with respect to unemployment risk on wages. To our knowledge, no previous study investigates the impact of unemployment risk on wages at the individual and establishment level. Previous wage curve studies do usually not include profits and use unemployment measures aggregated in various dimensions - by industry, region or country (see Blanchflower and Oswald (1994)). We include individuals' unemployment records from 1992-1995 as well as aggregated individual unemployment experience, for the same time period, at the firm and various industry levels in our regressions. Results confirm a stable and neg-

[4]Lester's range of pay is calculated as the product of the elasticity of wages with respect to profits and four standard deviations of profits (the range) divided by the average profits.

ative correlation between unemployment and wages along with a significant positive effect of profits on wages.

The remainder of the paper is organized as follows. Data are described in Section 2 and the empirical setup is discussed in Section 3. Basic results on the effect of profits as well as individual and aggregate unemployment risk on wages are reported in Section 4 where we also report variations in rents across worker groups as well as within and between industries. Individual heterogeneity and endogeneity issues are analyzed in Section 5. Finally, the paper is concluded in Section 6.

2 Data

The origin of the sample used for estimation in this study is the 1991 Swedish Level of Living Survey (LNU) which is a 1/1000 random sample of the Swedish population between the ages 18 and 65. All individuals in LNU 1991 are matched with their employing establishments through unique organization numbers. These establishments then form the basis of the Swedish Establishment Survey (APU) where a large number of administrative data for *all* individuals working in APU-establishments in 1987, 1991, or 1995 are collected, forming a larger individual sample. To ensure that our sample is representative, it is compared to another randomly drawn sample of individuals in 1991 and 1995. A comparison of sample means from the two samples indicates no significant differences.

The data on employees are matched to official balance sheet data for the APU firms through the Swedish system of corporate registration numbers. Data on wages, human capital and job characteristics are from Statistics Sweden as well as from data collected by the Swedish Trade union Confederation (LO) and the Swedish Employers' Confederation (SAF). Wages are computed as full-time equivalent pre-tax monthly salaries. These data are matched with individual unemployment records for the period 1992-1995 from the National Labour Market Board (*AMS Händelsedatabas*). Our data allow us to track individuals from 1992 to 1995 and define a dichotomous variable equal to one when the individual is registered as unemployed some time during this period and 0 otherwise. Furthermore, we construct another variable equal to the fraction of individuals at the establishment level having experienced unemployment during the 1992-1995 period. This variable measures the aggregated unemployment risk at the firm level. For a detailed description of the data, see Appendix.

Balance-sheet information is available for the period 1987-1995. Before matching individuals to firms in the matched worker-firm sample, those firms in the balance-sheet data that were observed for less than two years or had less than 2 employees were removed.

As a measure of profits, we use annual profits, after capital depreciation, per employee. This profit measure is clearly observable for both the employer and the employees, as well as for other parties outside the firm. This, together with the fact that it is a widely used measure of firm performance, makes it a suitable variable for investigating the relationship between profits and wages. Due to high variability in firm performance, we use average profits over time as a measure of long-run profitability.

3 Empirical Setup

Consider the following specification:

$$W_{ijt} = c + \mathbf{X}'_{ijt}\beta_0 + P_{ijt}\beta_1 + \mathbf{U}'_{ijt}\beta_2 + \varepsilon_{ijt};$$

$$\mathbf{U}'_{ijt}\beta_2 = \sigma U_{i,1992-1995} + \xi \bar{U}_{j,1992-1995},$$

$$P_{ijt} = \Sigma_{t-4}^{t-1} p_{jt} / \Sigma_{t-4}^{t-1} e_{jt},$$

$$\varepsilon_{ijt} = \upsilon_i + \upsilon_j + \upsilon_t + u_{ijt}.$$

W_{ijt}, $\mathbf{X}_{ijt}$, P_{ijt} and U_{ijt} denote wages, vectors of individual characteristics, average past profits per employee and unemployment risk variables for individual i in firm j during year t $(= 1991, 1995)$. The individual characteristics used are gender, educational level, labor market experience, and seniority in the establishment. The error components υ_i, υ_j and υ_t are individual, firm and time effects respectively and u_{ijt} is the random error term.

Using our unemployment measures combined with the 1995 data simply captures experienced unemployment and would represent expected unemployment risk under perfectly adaptive expectations. The interpretation of our unemployment measures combined with the 1991 data is not straightforward, however. Individuals could not have foreseen the crises of the early 1990's when bargaining on wages prior to 1991, and could not have based their negotiations on adequately predicted future unemployment risks. Using actual unemployment records might, however, reasonably capture the rel-

ative fragility of individuals to employment shocks and thus, their relative bargaining success. The unemployment experience during 1992-1995 used in the estimation on 1991 data corresponds to unemployment risk based on perfect foresight. The workers' expectations can reasonably be assumed to be somewhere in between perfectly adaptive expectations and perfect foresight. Obtaining similar estimates of expected unemployment risk measured in these two ways would be an indication of a stable effect of expected unemployment on wages.

Cross-section estimation may suffer from biases due to omitted variables, correlated with the profit measure. To deal with this potential problem, we estimate difference equations to examine the impact of changes in profits between the periods 1987-1990 and 1991-1994 on changes in individual wages between 1991 and 1995. This is done both for individuals working in the same establishments in 1991 and 1995 and for those who changed employers between these years. The former controls for time invariant individual- and firm-specific effects ($\nu_i + \nu_j$) accounting for systematic sorting of individuals across firms, while the latter only controls for individual effects (ν_i).

Another important issue to consider is the endogeneity of profits. Wages affect profits due to the accounting relationship, which leads to an underestimation of the impact of profits on wages. Moreover, according to incentive theories of wages, high wages may lead to high profits. Our past four-year averages of profit per employee reduces the potential simultaneity bias between wages and profits. We deal with the endogeneity of profits by using various new instruments for profits in estimation on our matched worker-firm data. Instruments include lagged profits, a measure of product demand elasticity and a measure indicating the degree of competition in the product market. The latter two instruments directly capture the existence of product market rents.

An additional issue to consider is returns to capital. We deal with this by examining the robustness of our results to the inclusion of controls for the capital-to-labor ratio (value of equipments per employee) and by comparing profits for firms with comparable capital-to-labor ratios.[5] We also investigate to what degree rent-sharing varies across groups of workers and takes place between industries, between firms within industries and between individuals within industries and firms.

[5] For effects of capital intensity on wages, see Dickens and Katz (1987) on industry data and Arai (1999) on matched worker-firm data.

4 Results

Results reported in Table 1 indicate that there are positive and significant effects of firms' ability to pay, measured in terms of average past four year accounting profits per employee, on individual wages in 1991 and 1995. The implied elasticities for 1991 range between 0.008 and 0.025 across various specifications.[6] The corresponding figures for 1995 are 0.005 and 0.012. These elasticities are in line with previously reported elasticities which are surprisingly uniform across countries and time periods using various sources of data. Previous studies report elasticities ranging between 0.01-0.05.[7]

– Table 1 about here. –

The lower elasticities for 1995 are a reflection the exceptional recession in Sweden in the early 1990's. The period 1991-1995 is characterized by a large fall in GDP and an increase in total unemployment from 5 to 15 percent. The average profits for 1991-1994, used in the 1995 specification, therefore reflect employers' ability to pay during a recessionary period, on the wages of workers in 1995. In light of this economic crisis and the concomitant increased fear of unemployment among workers, we would not expect a higher degree of rent-extraction. For this reason, we focus on the profit estimates for 1991.

The estimated coefficient for average profits reported in column 1, Table 1 is 0.09 and given the mean value of profits per employee of 0.27, the elasticity of wages with respect to profits is 0.024. This elasticity remains intact when including a dummy variable for females, but decreases by forty percent when we account for individual differences in human capital and include a blue-collar dummy (see column 2). Hence, systematic sorting of highly educated and experienced workers into more profitable firms accounts for half of the observed coefficient for average profits.

An issue to consider is that the profit per employee measure does not vary for workers in the same firm. To control for group effects, we estimate models correcting for within group correlated errors as proposed by Moulton (1990) and run between-firm

[6] The elasticities are computed by multiplying the estimated coefficient for profits by profit means. This elasticity is approximately equal to the elasticities obtained when estimating log wages on log profits.

[7] Christofides and Oswald (1992) report elasticities of around 0.01 for Canada. The elasticities in the Blanchflower *at al.* (1996) study on the US range between 0.02 and 0.05. Hildreth and Oswald (1997) report elasticities in the range of 0.02 to 0.04 for the UK. The corresponding range in the Arai (1999) study on Sweden is 0.01 to 0.02.

regressions. Accounting for group effects leads to increased standard errors, but the profit coefficient remains significant at the 1 percent level. We also include the capital-to-labor ratio and firm size in our models while taking group effects into account. The profit effect is robust to the inclusion of these variables. To check the robustness of our results with respect to labor supply, we also estimated the same models including only workers working more than 50 or 75 percent of full-time. These experiments, not reported in the paper, leave the results unchanged. The sensitivity analyses are performed for all estimations reported in Table 1-5.[8]

Variation Across Worker Groups

We also investigate whether the extent of rent-sharing varies across gender and occupational groups, since the bargaining power of workers may vary across these groups. Results are presented in Table 2.

– Table 2 about here. –

In column 1, an interaction variable between females and profits is added. This estimation examines whether the effects of profits on wages vary across men and women, holding the other coefficients constant. The estimate of the interaction variable is negative and significant, implying that the extent of rent-sharing is substantially lower for women. However, an interaction specification is not appropriate for examining a gender rent-gap since there are significant differences in returns to human capital between men and women. To take this into account and examine rent-sharing differentials with respect to gender, we run separate regressions for men and women (see columns 2 and 3).

Results imply that the elasticities of wages with respect to profits are 0.017 for men and 0.010 for women, indicating significant differences in rents for these two groups of workers. To check whether these rent differentials reflect the systematic sorting of men and women across industries, we also add 2-digit industry dummies. Adding the industry dummies reduces the impact of profits for both men and women, but does not affect the rent-gap implying lower rents for women within industries.[9]

[8] Results not reported in the tables can be obtained from the authors up on request.

[9] Arai (1999) reports similar results for 1991 in Sweden using observed hourly wages and the same measure for profits.

Around 80 percent of blue-collar workers and approximately 70 percent of white-collar workers are unionized in Sweden. If rent-sharing were only a consequence of the degree of unionization, we would expect to observe higher rents for blue-collar workers. Examining the variation of rents across occupational groups, classified as white- and blue-collar workers, shows significantly lower rents for blue-collar workers (see column 4, Table 2). Running the same regression, and adding industry dummies, yields results implying that the lower rents for blue-collar workers are not due to the systematic sorting of these workers into different industries. Experiments with a similar model including an interaction between profits and manufacturing disclose that workers in manufacturing receive significantly lower rents as compared to those in services. These results suggest that the existence of rent-sharing is not related to the degree of unionization. The lower rents for blue-collar workers might partly reflect the differences in human capital across groups. Our regressions including interaction variables between profits and human capital variables indicate that the extent of rent-sharing increases with the level of education, experience and seniority. The individual bargaining power might also increase with human capital and lead to higher rents for workers characterized by higher skills.

Within and Between Industry Effects

Another question to consider is the extent to which rent-sharing takes place within industries. This is interesting, given the Swedish collective wage-bargaining system with the industry level as the highest level of centralization. However, due to a higher degree of coordination of unions and employers' organizations, substantial differences across industries might not be expected. This view is confirmed by our results indicating that the effect of profits drops by roughly 25 percent when we add 14 industry dummies constructed such that they roughly capture the different bargaining areas (see columns 3 and 6 in Table 1 for 1991 and 1995, respectively).

Obviously, the magnitude of within industry effects depends on the level of industry aggregation. Our 14 industries are more aggregated than the actual bargaining levels in Sweden. For example, The Swedish Trade union Confederation (LO) is involved in approximately 25 negotiations. Using a finer industry classification of 55 industries (2-digit SIC) reduces the within industry effect from 75 to 55 percent. Actual industry-level wage bargaining in Sweden corresponds to a level of aggregation falling in-between

the above mentioned levels of industry classification. This means that the fraction of overall rents due to local bargaining lies somewhere between 55 and 75 percent. These local effects consist of bargaining at the firm/establishment and the individual level.

A remaining question is then how large a part of the within industry effects can be attributed to between-firm within industry effects and how large a part to rent-sharing between individuals within firms in a given industry. To examine these issues, we estimate between-firm within industry effects by running WLS regressions on transformed data of firm averages, controlling for industry dummies. The between-firm effects are about 70 percent of the within industry effects and correspond to a specification where we also control for blue-collar status, and thus represent rent-differentials within broad occupational groups.

Our results are in line with what is usually perceived as the magnitude of wage drift in Sweden, indicating the relative importance of local bargaining in Sweden.[10] The major part of rent-sharing is attributed to firm/establishment and individual level bargaining. According to our results, more than a half of rent-sharing takes place within industries. Two thirds of these effects are attributed to firm differences and the remaining one third is the result of within firm variation.

Unemployment Risk

In the previous sections, we examined the impact of firms' ability to pay on wages without taking into account how unemployment levels might affect the extent to which high profits lead to high wages. The extent of rent-sharing also depends on individuals' fear (risk) of unemployment. The bargaining outcome partially depends on the probability of obtaining a new job in the event of job-loss. This is especially important when comparing the extent of rent-sharing between two time periods characterized by extremely different levels of aggregate unemployment.

Previous studies on the wage curve report a stable statistical relation between unemployment and wages, where unemployment is measured on aggregated industry, region, or national levels (see Blanchflower and Oswald (1994)). Few micro data studies

[10]See the quarterly reports of the National Institute of Economic Research (Konjunkturinstitutet) for 1991-1995.

estimate both the impact of profits and unemployment on wages.[11] The unemployment risk for a group of workers is not uniformly distributed across firms and workers, however. To examine the heterogeneity across firms and individuals with respect to unemployment and thereby study the effect of unemployment on wages, unemployment risks must be linked to individual employees and firms. Since firms, by definition, do not contain unemployed workers, unemployment must necessarily be measured as the expected unemployment risk for a worker or a group of workers within a firm.

Individual heterogeneity concerning unemployment risk may affect wages in two ways. First, individuals with a high expected unemployment risk might have a weaker bargaining power. Second, the behavior of individuals in a wage bargaining situation can be influenced by the expected unemployment risk for other workers within the same firm. The first mechanism is related to individual wage bargaining and the second to firm and establishment wage bargaining.

Results on the impact of unemployment risk at the individual and aggregate levels on wages for 1991 and 1995 are reported in Table 3.[12] The unemployment variable measures *expected (perfect foresight)* relative to the unemployment risk in the 1991 estimation and *expected (adaptive)* unemployment when estimating on data for 1995. Our various specifications indicate that both the individual and the firm unemployment variable have a negative impact on wages.[13] The results are similar when estimating this specification for 1991 and 1995. The impact of the individual unemployment measure on wages means that low-skilled workers have lower wages and usually experience higher unemployment risks.

– Table 3 about here. –

Results in Table 3 indicate that there is a negative effect of aggregate firm and industry unemployment risk on individual wages, once we have controlled for firm profits, human capital and individual heterogeneity in unemployment risks, measured as expected or experienced unemployment. This allows us to interpret the effect of

[11] See Blanchflower *et al.* (1996) based on industry unemployment and profits, and Arai (1999) based on firm profits and regional unemployment.

[12] Since our measure of unemployment risk is defined for 1992-1995 and Hausman tests suggest using the fixed effects specification, only allowing time varying regressors, we are restricted to estimating cross-sections for 1991 and 1995.

[13] We also experimented with specifications (not reported here), only including a firm unemployment measure where the firm effect decreases from -0.02 to -0.03 for 1991 and from -0.04 to -0.05 in 1995.

the aggregate unemployment variable as the effect of unemployment on wages in a firm/establishment bargaining context. The effect of unemployment on wages is somewhat stronger in 1995 compared to 1991. This might reflect the crises of the early nineties and/or the different efficiency of our unemployment measures in capturing individual unemployment risk. The elasticities of wages with respect to unemployment cannot easily be compared to previous estimates on the unemployment effect on wages, using more aggregated unemployment data. What we find important here is that the worker wage seems to be negatively correlated to co-workers' unemployment experiences. Such an effect most likely goes through the local wage bargaining process.

A good indicator of employment uncertainty associated with jobs in a firm is the firms' profit levels in the previous years. This implies that profits and an aggregated unemployment measure at the firm level are strongly correlated. This is confirmed by a negative and highly significant raw correlation between profits and aggregated firm unemployment. The Pearson correlation is -0.21 for 1991 and -0.14 for 1995. Adding the unemployment measures to our basic specification for 1991 leads to a minor drop in the profits estimate from 0.06 to 0.05. The corresponding figures for 1995 are 0.03 and 0.02, respectively. However, these differences are not significantly different from zero.

Our results confirm a stable and negative correlation between aggregate unemployment and wages along with significant positive effects of profits. The estimates for profits and unemployment vary somewhat across specifications, but are basically of the same magnitude regardless of the level of aggregation. The effect of our aggregate measures differs from earlier evidence on the wage curve in that the estimates represent effects after controlling for firms' ability to pay and individual worker heterogeneity in unemployment risk.

5 Individual Heterogeneity and Endogeneity

Cross-section estimations, such as those reported above, ignore several potential problems. Unobserved individual heterogeneity may lead to overestimation of the profit effect on wages if high ability workers earn higher wages and are sorted into high profit firms. The fact that we cannot control for all relevant productivity measures in our cross-section estimation implies that our profit estimates may suffer from an omitted ability bias. In order to deal with this problem, we run fixed-effect models.

By running individual fixed-effects regressions, we are able to control for unobserved individual heterogeneity.

The individual fixed-effects model estimated on data for 1991 and 1995 yields a positive and significant profit coefficient (see Table 4). Notice that almost all individual variables are time invariant and therefore not included. The results of the Hausman test indicate that the individual effects are correlated with profits per employee, thus rejecting the random effects model given the specification. This is expected in light of our cross-section results that workers with high levels of human capital are sorted into profitable firms.

– Table 4 about here. –

As mentioned above, the period 1991-1995, constitutes the severest economic crisis in Sweden since the 1930's. During such a recession, it is not self-evident that wage increases should strongly follow profit increases in light of the general employment insecurity due to shut-downs and increasing unemployment.

Fixed-effects estimation aggregates the profit effect for individuals who were working in the same firm in 1991 and 1995, as well as for those who changed employer. It is reasonable to assume that employer switches during this period are exogenous due to the high unemployment rates at the time. Results on gross worker flows for this period in Sweden indicate that both worker turnover and excess worker reallocation, i.e. the difference between worker and job turnover, decreased drastically during the early 1990's (see Arai and Heyman (2000)). This implies few voluntary job switches during this period. Another issue is to check whether the profit effects on wages are stronger in firms experiencing a profit increase between the two periods.[14]

Estimating the fixed effects model on a sub-sample of workers experiencing an increase in profits in their employing firm, leads to a three times higher estimate of profits with a t-statistics of 21.34, indicating a high significance level (see column 2 in Table 4). Furthermore, we restricted the estimation to workers remaining at the same firm between the two years, to obtain a within individual and firm effect estimate. Running the model on this group of workers basically yields the same results, indicating that

[14]Our profit measure does not refer to the difference in profits between 1991 and 1995 but represents the difference in average profits between the two periods 1987-1990 and 1991-1994. Workers whose employing firm in the later period has lower profits than in the former period can hardly be expected to experience falling wages due to wage setting institutions almost entirely excluding wage cuts.

there are no significant differences between workers who switched firms and those who did not (see column 3 in Table 4). The message of these results is that rents exist and are not due to fixed individual or firm effects.

Instrumenting Profits

Next, we turn to an examination of the possibility that high wages lead to high profits, as predicted by efficiency wage theory together with imperfect competition in the product market, or that higher wages reduce profits as measured in the firms' balance sheet reports. Abowd and Lemieux (1993) use international selling prices as instruments and find that the effect of profits on wages increases by ten times. At least, this indicates that the rent-sharing effect might be underestimated due to the accounting relationship between wages and profits. Another instrument used in the previous literature is energy costs (Blanchflower *et al.* (1996)). Studying the effect of profits on wages in 1991, we first instrument average profits with lagged values of profits separately for different lags. Results reported in columns 1 and 2 confirm that profits affect wages.[15]

– Table 5 about here. –

Furthermore, we use new instruments for profits based on establishment level data matched with our data, which include explicit information on demand elasticity in the product market and the degree of competition. Firms were asked to report a predicted sales response for the next 6 months to a hypothetical product price increase of 10 percent. Four categories were available ranging from essentially unchanged sales to a more than 10 % drop in sales. Reported answers to this question give us a *short-term* product demand elasticity.

Another indicator of firm market power, available in our data, is the number of important competitors in the product market which is measured in four classes (see Appendix for details). Results in columns 3-5 in Table 5 indicate that using these two variables as instruments for profits in separate specifications, rent-sharing estimates increase radically by approximately ten times as compared to our cross-section or

[15] In Table 5, we only report the results when profits are instrumented with lagged profits in 1987 and 1988. Using profits in 1989 and 1990 as instruments yield similar results. Using lagged profits in 1991, 1992, 1993 or 1994 as instruments for estimating the effect of profits in 1991-1994 on wages in 1995, we basically obtain the same results except when profits in 1992 are used as an instrument yielding insignificant results.

fixed-effects estimates. Our conclusion is that using observed profits to estimate rents tends to underestimate the impact of rent-sharing, due to the accounting relationship between wages and profits. The implied elasticities from these estimations are as high as 0.1, which implies that around half of the wage inequality in Sweden is due to the spread of profits according to Lester's range of pay.

6 Conclusions

In this paper, we report elasticities of wages with respect to profits, based on a large matched worker-firm dataset, in the range of 1 and 3 percent. These elasticities vary across groups, being lower for women and blue-collar workers. The significant and positive effect of profits on wages is robust to controls for worker and firm fixed-effects, the impact of unemployment risk on wages and the endogeneity of profits. The major part of these rents are found within industries and two thirds of the within industry effect can be attributed to between firm effects, while the remainder is due to within firm effects.

We report elasticities of wages with respect to profits implying that, due to the spread in profits, wage inequality in Sweden is as high as 13 percent of mean wages in 1991, according to Lester's range of pay. Using a measure of product demand elasticity as an instrument for profits leads to an increase in the profit estimate and yields elasticities in the magnitude of 10 percent, implying a Lester's range of pay of 50 percent.

To investigate the impact of workers' unemployment risk on wages, we construct a measure of individual unemployment risk by using information on workers' unemployment experience during the period 1992-1995. We reported results suggesting that aggregate unemployment risk at the firm as well as at 3-, 4- and 5-digit industry levels is negatively correlated with individual wages. These results add to previous empirical work on the wage curve in that they examine the relationship between aggregate unemployment and wages, taking into account that marginal workers have both lower wages and a higher unemployment risk. Moreover, these results examine the sensitivity of the impact of unemployment on wages at various aggregation levels from the firm to industry levels. The evidence presented here indicates that profits and unemployment affect wages.

The message of these results is that profits and unemployment affect wages in

economies with very different institutional settings. Our results for Sweden are similar to those reported for the US and UK, despite the fact that Sweden constitutes an extreme with regard to degree of unionization and extended labor protection laws as compared to these countries. Rent-sharing seems to be an integral part of wage setting in capitalist economies, regardless of institutional setting.

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Tables

Table 1. Effect of profit on wages. OLS estimates for 1991 and 1995. Dependent variable is log monthly wage. Robust standard errors in parentheses.

	1	2	3	4	5	6
	1991	1991	1991	1995	1995	1995
Profits/Employee	.093***	.054***	.040 ***	.038***	.026***	.018***
	(.002)	(.002)	(.002)	(.001)	(9E-04)	(9E-04)
Female		-.14***	-.13***		-.15***	-.15***
		(.001)	(.001)		(.001)	(.001)
Experience		.022***	.022***		.019***	.020***
		(.0002)	(.0002)		(.0002)	(.0002)
Experience^2/100		-.04***	-.04***		-.03***	-.03***
		(.0006)	(0.0006)		(0.0006)	(0.0006)
Seniority/10		.008***	.007***		.031***	.024***
		(.0008)	(.0008)		(.0008)	(.0009)
Education level	NO	YES	YES	NO	YES	YES
Blue Collar		-.10***	-.11***		-.16***	-.17***
		(.0014)	(.0013)		(.0013)	(.0013)
Industry	NO	NO	YES	NO	NO	YES
R^2	0.01	0.44	0.45	0.01	0.48	0.49
N	175,023	172,280	172,280	178,259	176,009	176,009

Notes: *** indicate significance at the 1%-level. Industry classification corresponds to 14 industries.

Table 2. Variation in the effect of profits on wages across gender and occupation. OLS estimates for 1991. Dependent variable is log monthly wage. Robust standard errors in parentheses.

	1	2	3	4
	All	Men	Women	All
Profits/Employee	.063***	.061***	.040***	.066***
	(.002)	(.002)	(.002)	(.003)
Female	-.13			-.14
	(.001)			(.001)
Experience	.022***	.024***	.017***	.022***
	(.0002)	(.0003)	(.0004)	(.0002)
Experience^2/100	-.04***	-.046***	-.03***	-.04***
	(.0006)	(.0007)	(.001)	(.0006)
Seniority/10	.074***	.007***	.012***	.008***
	(.0008)	(.001)	(.001)	(.0008)
Education level	YES	YES	YES	YES
Blue Collar	-.10***	-.15***	-.11***	-.010***
	(.001)	(.002)	(.001)	(.002)
(Profits/Employee)* female	-.03***			
	(.003)			
(Profits/Employee)*blue collar				-.02***
				(.003)
Industry	NO	NO	NO	NO
R^2	0.44	0.43	0.33	0.44
N	172,280	127,012	45,268	172,280

Notes: *** indicate significance at the 1%-level. Industry classification corresponds to 14 industries. Estimating these models on data for 1995 qualitatively yields the same results.

Table 3. Effects of profits and unemployment on different aggregation levels on wages. OLS estimates for 1991 and 1995. Dependent variable is log monthly wage. Standard errors corrected for within firm (or 3-digit industry in 3 and 6) correlated errors in parentheses.

	1	2	3	4	5	6
	1991	1991	1991	1995	1995	1995
Profits/Employee	.048**		.045***	.022***		.023***
	(.02)		(.002)	(.007)		(.006)
Female	-.12***	-.12***	-.12***	-.12***	-.12***	-.12***
	(.01)	(.006)	(.01)	(.01)	(.007)	(.01)
Experience	.02***	.02***	.02***	.02***	02***	.02***
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Experience^2/100	-.04***	-.04***	-. 04***	-.03***	-.03***	-.03***
	(.003)	(.003)	(.003)	(.002)	(.002)	(.003)
Seniority/10	.001	.002	.001	.007	.008	.007
	(.005)	(.005)	(.006	(.005)	(.005)	(.007)
Education level	YES	YES	YES	YES	YES	YES
Unemployment						
Firm (log)	-.02**	-.02***		-.04***	-.04***	
	(.007)	(.007)		(.006)	(.007)	
Industry (log)			-.02**			-.05***
			(.01)			(.01)
Individual	-.04***	-.04***	-.04***	-.08***	-.08***	-.08***
	(.004)	(.004)	(.004)	(.005)	(.005)	(.007)
R^2	0.43	0.43	0.43	0.45	0.44	0.45
N	163,722	163,722	166,215	167,272	167,272	170,172

Notes: ** indicate significance at the 5%-level and *** at the 1%-level. Industry unemployment is aggregated at 3-digit industry levels. Using aggregated unemployment at 4- and 5-digit industry levels yields basically the same results. These results are robust to including a variable for blue-collar workers, interactions between profits per employee and blue-collar workers as well as females. The interaction terms are not significantly different from the corresponding coefficients reported in Table 2.

Table 4. Effects of profits on wages. Results from individual fixed-effects models for the 1991-95 panel. Dependent variable is log monthly wage. Standard errors in parentheses.

	1	2	3
	All	Δ(Profits/Employee)> 0	Δ(Profits/Employee) > 0
Profits/Employee	.0069***	.022***	.034 ***
	(9E-04)	(.002)	(.002)
Conditioning on same employer -91 and -95	NO	NO	YES
Hausman test	354***	38***	13***
Breusch & Pagan test	65,542***	33,066***	25,364***
R^2 (overall)	0.004	0.004	0.001
N	187,364	100,022	66,374

Notes: *** indicate significance at the 1%-level.

Table 5. Effects of profits on wages. Instrumental variable estimates using various instruments. Dependent variable is log monthly wage in 1991. Robust standard errors in parentheses.

	1	2	3	4	5
Instrument:	Profits/Empl. in -87	Profits/Empl. in -88	Demand elasticity	No. of competitors	Instruments in 3, 4
Profits/Empl.	.011***	.058***	.27***	.51***	.40***
	(.003)	(.002)	(.066)	(.021)	(.014)
R^2	0.43	0.45	0.35	0.09	0.25
N	116,389	149,860	85,288	100,032	84,797

Notes: i) *** indicate significance at the 1%-level. Other variables included are gender, education level, experience, experience square, seniority, blue-collar, individual unemployment and aggregated unemployment at the firm level. Adding industry dummies reduces the estimates for profits but leaves the results qualitatively the same.
ii) Columns 3, 4 and 5 are based on a subsample of the original matched data due to missing values for our instruments. The estimate for 4-year average profits based on the remaining sample is not significantly different from the estimate when using the original sample. The instruments in column 3, 4 and 5 are not available for 1995.
iii) We cannot reject the validity of our instruments in specification 5. Regressing the second stage residuals on the instruments yields an R^2 of 0.0000. The χ^2 distributed statistics $R^2 * N$ is used as the test statistics for testing the validity of the used instruments.

Appendix: Data Description

Definition of variables

Individual outcome variables:

Wages: Monthly pre-tax full-time equivalent wages in 1990 prices (using CPI) based on Swedish Trade Union Confederation (LO) and the Swedish Employers' Confederation (SAF) wage data and completed with the income registers from Statistics Sweden (SCB).

Unemployment: Information on unemployment history during 1992-95 according to the National Labour Market Board's Event Database (*AMS Händelsedatabas*) containing individual records of all individuals who have registered as unemployed at the labor offices. Registering as unemployed is a necessary condition for being eligible for unemployment benefits as well as having the possibility of participating in labor market programs.

Demography variables:

Gender and **Age** are from SCB's Population Census (*Registret över totalbefolkningen*).

Human Capital variables:

Education level dummies are based on the 2-digit level of the Swedish Education Nomenclature (SUN-codes) from the Swedish Education Register (*Utbildningsregistret*). These are *Elementary School* (less than 9 years), *Compulsory School*, *Upper Secondary School*$<$ 3 (at most 2 years), *Upper Secondary School* $\geq$ 3 , *Long Upper Secondary School* (3-4 years), *College* (Shorter University Education) and *University*.

Experience is number of years on the labor market according to the Employment Register (*Sysselsättningsregistret*).

Seniority is number of years at the establishment based on tracing the individual back to 1986 in the Employment Register (*Sysselsättningsregistret*). Individuals with more than 6 years of seniority are given the mean seniority in Sweden according to the Level of Living Survey in 1991, i.e. 16 years.

Industry and Occupational Groups:

Industry dummies based on the 2-digit SIC (*SNI69*). Own classification of 14 industries as well as three- and five-digit industry classification.

Blue- and White-collar worker according to the Population and Housing Census of 1990 (*FoB90*). These refer to occupation classification in 1990 and not necessarily the current employment.

Balance sheet information:

Profits (Swedish kronor in 1990 prices) are defined as annual profits after capital depreciation. Available for the period 1987-95 (MM Partners). In estimations on the matched sample, we remove 143 firms in 1991 with a four-year average (1987-1990) profit-per-employee below -53,300 (the 1st percentile) and above 261,000 (the 99th percentile). For the data from 1995 we remove 119 firms with four-year (1991-1994)

average profit-per-employee below -128,000 (the 1st percentile) and above 890,000 (the 99th percentile). In one case, an annual observation of 18,000,000,000 for 1995 was replaced by the averages in the other years. These extreme values are most likely due to measurement errors in the profits or firm-size variables. The remaining sample consists of 6,932 firms in 1991 and 5,757 in 1995.
Number of employees refers to average number of employees available for the period 1987-95 (MM Partners).

Information from the Swedish Establishment Survey (APU):
Demand elasticity. If your company increased its prices by 10 percent, how would demand be affected in six months? (i) Stay the same (or increase), (ii) Reduced, around 5%, (iii) Reduced, around 10%, (iv) Reduced, more than 10%.
Number of competitors. How many important competitors does your firm have? (i) 1, (ii) 2-5, (iii) 6-10, (iv) > 10.

Table A.1. Sample Means for individuals in the matched sample.

	1991			1995		
	N	Mean	SD	N	Mean	SD
Log monthly wage	175 023	9.43	.26	178 259	9.52	.30
Female	175 023	.26		178 259	.29	
Experience	175 023	17	9.97	178 259	19	10.26
Seniority	173 226	8	6.66	176 499	10	6.92
Blue Collar	175 023	.64		178 259	.53	
Education level:						
Elementary School < 9	174 059	.16		177 761	.11	
Compulsory School =9	174 059	.14		177 761	.13	
Upper Secondary School < 3	174 059	.35		177 761	.34	
Upper Secondary School 3	174 059	.16		177 761	.17	
Upper Secondary School > 3	174 059	.11		177 761	.14	
College < 3	174 059	.08		177 761	.11	
University	174 059	.004		177 761	.006	
Individual unemployment	168 982	.27		172 274	.21	
Profits/Employee, 100,000 SEK	175 023	.27	0.34	178 259	.33	0.63
Firm size	175 023	6081	9456	178 259	3561	4940

Chapter 4

Pay Inequality and Firm Performance: Evidence from Matched Employer-Employee Data

IV

Pay Inequality and Firm Performance: Evidence from Matched Employer-Employee Data

Fredrik Heyman*

Abstract

This paper uses a large matched employer-employee data set for Sweden to study several predictions from tournament theory. The main results are: (i) a positive and significant effect of intra-firm wage dispersion on profits and average pay using various measures of wage dispersion, (ii) a positive relationship between market demand uncertainty and wage dispersion and (iii) a negative effect of the number of managers (contestants) on managerial pay spread. The first two results are in accordance with predictions from tournament theory, while the last one is not. The results are robust to controllling for firm differences in human capital and firm fixed-effects.

Keywords: Wage dispersion, Firm performance, Tournament models, Matched employer-employee data

JEL Classification*:* J31, J41

1 Introduction

An important factor in wage bargaining between employers and employees concerns relative wages. Different groups of workers compare wages both internally, within their own plants or firms and externally, with workers in other firms or industries. Comparisons can be made both with workers with similar tasks, education or positions in the firm hierarchy as well as with wages for executives and managers. One example of the latter is the debate in many countries on CEO compensation. The growing gap between employee- and executive compensation has been criticized as unethical with negative effects on work morale.

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According to a variety of theories, described in more detail below, the wage distribution both within and between firms can have important effects on individual productivity and firm performance. One argument for high wage differentials is based on incentive effects. Higher wage differentials lead to higher individual effort, and are therefore productivity enhancing. Lazear and Rosen's (1981) tournament theory argues that if wages are based on relative productivity, then workers with higher productivity will be rewarded with higher wages. This, in turn, will increase the equilibrium effort and lead to a positive relationship between wage dispersion and productivity.

The positive effect of pay spread on firm performance hinges on the assumption that employers determine wages by themselves. This implies that they can set the wage distribution within the firm so that it maximizes the incentives for individual effort. This may apply to executive compensation, but not to broader occupational groups. For instance, wages for white-collar workers are determined through negotiations between employers and unions in Sweden. Given that unions strive for more equalized wages, the final outcome of the wage distribution within firms becomes a function of the relative strength of the two parties.

The opposite relationship between intra-firm wage dispersion and firm performance is found in theories stressing fairness and cooperation between co-workers. Akerlof and Yellen (1990) argue that individual effort is reduced if the wage is regarded as unfair. A similar argument based on cohesiveness is found in Levine (1991).

As discussed by e.g. Prendergast (1996) and Eriksson (1999), many results from the empirical literature on issues related to the wage structure within firms are consistent with different theories. This also applies to various predictions from tournament models. As a consequence, it is difficult to distinguish between alternative explanations. This study follows Eriksson in testing several predictions from tournament theory on the same data, and not just if results are in accordance with one or two hypotheses. First of all, the general relationship between within-firm pay inequality and firm performance is studied. The analysis is carried out for both white-collar workers and managers, using different measures of intra-firm wage dispersion. In addition, three more specific implications from tournament models are tested: (i) a convex relationship between pay and job levels for managers including a particularly large increase at the very top of the hierarchy; (ii) a positive relationship between the number of executives (contestants) and wage dispersion for managers and (iii) a positive association between market demand uncertainty and managerial pay spread.

Estimation is based on a large matched employer-employee data set for Sweden consisting of very detailed information on approximately 170,000 individuals, including around 10,000 managers. Each individual is assigned a unique organization-number, mapping each worker to his/her establishment. These establishments then form the basis of the Swedish Establishment Survey (APU). A large number of administrative data for *all* individuals working in APU-establishments constitutes the larger individual sample. These data are then matched to firm official balance sheet data through the Swedish system of corporate registration numbers. The matched data contain detailed information on individual characteristics as well as their employing firms and, as such, the data are well suited for studying various aspects of firms' internal wage structure.

The main results are: (i) a positive and significant effect of intra-firm wage dispersion on profits and average pay, which applies to both managers and white-collar workers; (ii) a positive relationship between the degree of noisy business environment, measured as variation in sales, and wage dispersion and (iii) a negative effect of the number of managers (contestants) on managerial pay spread. The first two results are in accordance with predictions from tournament theory, while the last is not.

Previous studies have mainly focused on wage dispersion for managers. This has been a natural first step since tournament theory is primarily a theory on how wage distribution affects managerial behavior. Furthermore, suitable data for analyzing the relationship between wage distribution for wider occupational groups of workers and firm performance have previously not been available. With the creation of matched worker-firm data sets, the question of the effects of intra-firm wage spread can be addressed.

Earlier evidence on wages for managers and various aspects of predictions from tournament models includes O'Reilly *et al.* (1988), Leonard (1990), Main *et al.* (1993) and Eriksson (1999). Winter-Ebmer and Zweimuller (1999) and Bingley and Eriksson (2001) present evidence on the relationship between pay dispersion and firm performance for broader occupational groups. Except for Bingley and Eriksson and Winter-Ebmer and Zweimuller, previous studies do not control for differences in human capital accumulation, however. This means that the results can partly be driven by a systematic sorting of workers (and managers) into firms with certain characteristics. This study is the first to explicitly control for firm differences in human capital, when testing several predictions from tournament theory.

The present study also adds to the previous literature in that it is the first to study

aspects of tournament models, including the effect of wage dispersion on firm performance, on Swedish data. In an international perspective, the Swedish wage distribution is very compressed and characterized by strong unions and a higher degree of centralized wage negotiations than most other countries.[1] Given these characteristics, it is of interest to compare results to those for countries with more dispersed wages. One issue to consider is whether the characteristics of the Swedish wage structure influence the findings in many studies of a positive relationship between managerial pay spread and firm performance.

The remainder of the paper is organized as follows. The theoretical background and previous empirical evidence are presented in Sections 2 and 3, respectively. Section 4 describes the data and the empirical setup. The results are presented in Section 5. Finally, the paper is concluded in Section 6.

2 Theoretical Background

2.1 Predictions From Tournament Models

A central question in labor economics concerns wage determination. In the classical competitive framework, wages are determined by individual productivity. However, numerous studies have found wages to be distributed in a way that cannot be explained by productivity differences. There are several reasons why observed wages may be more equally distributed than productivity differences between employees. If individual productivity cannot be observed, or this is too costly, wage differences cannot reflect differences in productivity between employees. Another explanation is that employees are more risk averse than employers, so that workers are willing to accept wages that are more compressed than the actual productivity distribution.

These explanations treat individual productivity as exogenous in the sense of not being related to the actual pay distribution. However, there are reasons to believe that the determination of wages within and between firms affects worker behavior and thereby, their productivity. Theories linking wage dispersion and productivity (and firm performance) give rise to conflicting predictions of whether this relationship is positive or negative.

[1] See OECD (1996) for an overview of earnings inequality in OECD countries.

A common argument against an egalitarian wage distribution is that it has negative incentive effects. According to this line of argument, high wage spreads lead to increased effort and also increase the employee motivation to engage in, for instance, more education and on-the-job training. These incentive effects would then result in higher productivity and improved firm performance.

The tournament model analyzes the effect of wages on incentives in the presence of costly monitoring of individual productivity and effort (see Lazear and Rosen (1981)). The model postulates that it is efficient to reward workers according to their relative performance rather than to absolute performance. The wage gap between different jobs is interpreted as the tournament prize. A high wage gap between employees (players) provides incentives for workers to do their best, which means that the equilibrium effort is increasing in the difference between winning or losing the prize (Equation 6 in Lazear and Rosen (1981)).

Another prediction from tournament models is that wage differentials are expected to increase, the higher one moves up the hierarchy (see Rosen (1986)). The reason for this is that winning a contest at every level below the highest, does not only give the prize, but also the possibility to win further prizes higher up the hierarchy (an option value). There are no further prizes at the highest level so to induce enough incentives at that stage, the prize structure must include an extraordinarily large increase at the very top of the hierarchy. This means that the optimal compensation schedule implies a convex relationship between wages and hierarchal job level, including additional weight on top-ranking prizes (Equations (14)-(16) in Rosen (1986)).

In an extension of tournament models with many contestants, McLaughlin (1988) analyzes the effect of the number of contestants (n) on the prize structure, effort and incentives. He shows that in the case of risk-neutral contestants, the prize spread increases with the number of contestants (Equation 47 in McLaughlin (1988)). The intuition behind this result is that if n is large, a marginal increase in effort only has a small effect on the probability of winning. Therefore, a big prize spread is required to induce effort. Empirically, this means that controlling for other economic determinants of managerial pay, the wage gap between the CEO and the vice presidents should be positively related to the number of vice presidents.

Another implication of tournament models concerns the effect of market uncertainty on the optimal pay structure. Various versions of tournament models show that the greater is the importance of variability of demand, the lower is the optimal effort level.

If luck is an important factor in determining output, employees will be less motivated to try hard to win the promotion. This means that in markets where stochastic shocks in output are important, the wage gap must be sufficiently high to counterbalance the negative effect on effort of the random component (Equation 6 in Lazear and Rosen (1981)). Empirically, this implies that we will observe large wage spreads in markets characterized by a high degree of demand and output uncertainty.

To sum up, tournament theories give rise to the following four testable predictions:

Prediction 1: A positive relationship between wage dispersion and firm performance.

Prediction 2: A convex relationship between wages and job levels, including an extra large increase at the top of the hierarchy.

Prediction 3: A positive relationship between the number of contestants in a tournament and wage dispersion.

Prediction 4: A positive relationship between the degree of market demand volatility ("noisy business environment") and wage dispersion.

2.2 Predictions From Alternative Models of Wage Determination

The prediction of a positive relationship between wage dispersion and firm performance is controversial from a theoretical perspective. One argument against using relative wages for enhancing productivity, based on the risk for uncooperative behavior, is presented in Lazear (1989). He shows that when employee compensation is based on relative comparisons, workers may engage in uncooperative behavior *vis-à-vis* fellow workers. Relative compensation implies that workers can increase their chances of winning a contest by either increasing their own effort, or negatively affecting the productivity of co-workers. If the latter behavior is frequent in a firm, increasing wage compression can be productivity enhancing. The importance of unproductive uncooperative behavior is related to the organization and composition of the work force. The higher the share of very competitive "aggressive" individuals in a firm, the higher are the positive effects of a flat wage distribution.

A link between wage distribution, job morale and productivity can also be found in equity and relative deprivation theories (see Cowherd and Levine (1992) and references

therein). According to equity theory of human behavior in social exchange, wages should be distributed so that the ratio between the value of labor input (e.g. effort) and output (e.g. wage) is perceived as fair, which is the case if it is similar to that of a relevant comparison group. However, the perception of an unfair ratio has negative consequences for the firm in terms of decreasing individual effort or individuals leaving the organization as a result of not receiving fair wages. Based on equity theory, Akerlof and Yellen (1990) present a model seeking to explain why a compressed wage structure can be productivity enhancing. According to their fair-wage hypothesis, employees withdraw effort, hence becoming less productive, as their wage becomes lower than the "fair" wage.

The relative deprivation theory is closely related to equity theory. According to relative deprivation theory, individuals experience deprivation when comparing their wages to those of a reference group, and find that they receive less. The relevant reference group can differ between individuals and groups. For instance, the reference group can be workers in the same occupation within the firm, managers in the same firm or workers in dissimilar occupations within the firm. A wage structure that is too dispersed, thus leaving groups of workers dissatisfied with their relative wages, can result in absenteeism, strikes or sabotage.

Finally, Levine (1991) presents a model where firms want to reduce wage differentials based on efficiency wage considerations. The reason being that a more compressed wage structure increases cohesiveness (and productivity) within a firm. This paper defines cohesiveness as "the propensity to obey group norms because approval of the group is valued". To maintain cohesiveness, firms will pay an efficiency wage to those at the low end of the wage distribution.

3 Previous Studies

Most empirical studies investigating the effect of firm pay structure on firm performance have focused on compensation to top executives. A seminal paper is Leonard (1990). He studies the effects of executive compensation policy on performance for a sample of large US firms and finds no statistical relationship between the standard deviation of pay and the steepness of pay for managers and corporate performance. Consistent with tournament theory, wage differentials between management levels are found to be larger at higher levels in the hierarchy. Ang *et al.* (1998) present similar results

regarding the effect of executive wage dispersion on firm performance. They investigate the pay gap between CEO's and other members of top management teams for a sample of Israeli firms, and find no support for the tournament model.

Main *et al.* (1993) and Eriksson (1999) find a positive relationship between managerial pay dispersion and firm performance. Using survey data on the pay for top executives in 200 US firms, Main *et al.* (1993) investigate the role of the pay distribution among the top-management team. They find a positive and significant relationship between wage dispersion among executives, measured as the coefficient of variation, and the return on assets. However, the positive effect is insignificant when they instead use stock market returns as a measure of firm performance. Consistent with tournament theory, they also find a positive and significant effect of wage dispersion on average wages. Similar results are found in Eriksson (1999) who uses information on managers for a panel of Danish firms. He finds a weak positive effect of the coefficient of variation in wages for managers on firm performance, measured as profits per sales. Furthermore, consistent with tournament theory, Eriksson finds the mean wage for managers to be higher in firms with more variation in executive compensation.

Several papers testing predictions from tournament theory study how wage differentials between managers vary along the corporate ladder. Leonard (1990) and Main *et al.* (1993) find a convex pay structure including an extraordinarily large increase at the top of the hierarchy. Eriksson (1999) reports increasing pay differences, but finds no additional reward at the top. Additional evidence, consistent with a convex wage structure, is found in Lazear (1992) and Baker *et al.* (1994), both analyzing detailed data from two different large US firms.

Evidence on the effect of the number of contestants on wage dispersion is scarce. Studies considering the relationship between the managerial pay structure and the number of managers show mixed results. O'Reilly *et al.* (1988) find a negative relationship whereas Main *et al.* (1993) and Eriksson (1999) both find this relationship to be positive.

The prediction of Lazear and Rosen's (1981) tournament model of a positive relationship between the degree of market demand volatility and wage dispersion has only been tested by Eriksson (1999). He reports a positive and statistically significant relationship between the coefficient of variation of firm sales and the pay spread for managers.

As an alternative measure of firm performance, Cowherd and Levine (1992) consider

at the effects of wage equality between lower-level employees and managers on product quality. Using data on 102 US establishments, they find that pay equality is positively related to product quality. Their results are consistent with predictions from equity and relative deprivation theories, stressing the importance of the within firm wage structure on managerial goals, effort and cooperation.

All papers presented above study the pay structure for managers, while other papers use wage data for broader groups of workers. Using Swedish data on individual wages and aggregated industry information on productivity, Hibbs and Locking (2000) investigate the relationship between wage dispersion and productive efficiency. They find a positive effect of within-plant and within-industry wage dispersion on industry productivity (alternatively measured as real value added or labor productivity). The opposite is true for between-plant and between-industry wage dispersion, which are negatively related to productivity.[2]

The studies presented above do not take firm differences in human capital into account.[3] To my knowledge, only Bingley and Eriksson (2001) use such data to test predictions from tournament theory on the effects of wage spread and skewness for the whole wage distribution on employee productivity and firm performance in Denmark. In this analysis, the authors explicitly take firm differences into account in the composition of human capital. Sickness absenteeism is used as a proxy for individual effort and total factor productivity as a measure of firm productivity. Firm productivity is increasing in wage dispersion for white-collar workers up to a point, after which it becomes counter-productive. Only weak productivity effects are found for blue-collar workers.

A related paper also analyzing the effects of the entire firm wage distribution is Winter-Ebmer and Zweimüller (1999). They test theories on the relationship between within-firm pay structure and firm performance on wage data for Austrian firms. However, they lack direct information on firm performance and instead, they proxy firm performance with the total firm wage level. The results are similar to those of Bingley and Eriksson (2001).

[2]The between-industry wage dispersion results are consistent with theories stressing the positive structural effects of a compressed wage structure. This is done by shifting labor and capital from less productive to more productive firms and industries (see Moene and Wallerstein (1997)).

[3]Eriksson (1999) has information on human capital characteristics, but only uses these data when studying the convexity of the managerial pay structure.

4 Data and Empirical Specification

The data on individuals originate from the Swedish Level of Living Surveys (LNU) in 1991, a representative survey of non-agricultural workers aged 18-64. Each individual has a unique organization-number, mapping each worker to his or her employer. These firms then form the basis of the Swedish Establishment Survey (APU). By matching the organization number for the firms in APU with employment data from Statistics Sweden, information on all individuals working in these firms sometime during 1987, 1991 or 1995 is available. Individuals working in non-agricultural private APU-establishments in 1991 and 1995 constitute the worker data set. Each year contains approximately 170,000 employees.

For these individuals, rich information is obtained by matching data from several Swedish data sources. Data on wages and job characteristics are provided by Statistics Sweden (*SCB*) and from data collected by the Swedish Trade Union Confederation (*LO*) and the Swedish Employers' Confederation (*SAF*). Information on employment, including total labor market experience and seniority, originates from the Swedish Employment Register. Data on individual characteristics such as age, gender and birth origin are from the Population Census from *SCB* (*Registret över totalbefolkningen*). Detailed information on education, including grades from high school, are from the Swedish Education Register (*Utbildningsregistret*). See the Appendix for a detailed description of the data.

Managers are classified according to the international standard classification of occupations (ISCO-88). ISCO-88 divides corporate managers into three subgroups: directors and chief executives, production and operations department managers and other department managers. Classification codes for managers are only available in 1995. Data for 1995 on executives consist of about 10,000 managers in 560 firms, for all of whom information on individual characteristics are available.

The unique organization number for each employing firm is utilized to match the individuals with balance sheet data for the firms where they work. Balance-sheet information is available for the period 1987-1996. Before matching individuals and firms, those firms in the balance-sheet data that are observed for less than two years or with less than 2 employees are removed. The balance-sheet information data for 1991 and 1995 are transformed into four-year averages. The reason for this is twofold. First, measurement errors in variables, such as profits per employee, are reduced when

four-year averages are uses. Second, due to high variability in both firm performance variables and firm size, four-year averages yield a better measure of long-run profitability, removing the transitory variation in profits.

Annual profits after capital depreciation per employee are used as a measure of profits. This direct measure of firm performance has not previously been used to investigate the relationship between wage structure and firm performance. 146 firms with a four-year average profit-per-employee below -65,000 SEK in 1991 (the 1st percentile) and above 273,000 SEK (the 99th percentile) are removed. For the data from 1995, I remove 124 firms with a four-year average profit-per-employee below -94,000 SEK (the 1st percentile) and above 687,000 SEK (the 99th percentile). These extreme values are most likely due to measurement errors in the profits or firm-size variables.

When analyzing the effect of intra-firm wage dispersion on firm performance, it is important to compute wage dispersion for a relevant group of employees. There are several ways of measuring wage dispersion within the firm. Wage dispersion can be defined for different groups of workers, such as between white- and blue-collar workers, or between managers and the rest of the work force. It can also be defined for similar workers in terms of various observable productivities. In the latter case, the residual inequality, after controlling for human capital variables, is the relevant measure of wage dispersion.

This study analyzes the wage distribution for both unequal and observationally equal employees. The former include the white/blue collar wage gap and various measures of wage dispersion for white-collar workers. The latter concept refers to unexplained residual inequality for white-collar workers and wage dispersion for managers.[4] To compute conditional wage differentials for white-collar workers, I follow Winter-Ebmer and Zweimuller (1999) and Bingley and Eriksson (2001) and estimate individual wage equations for each year, 1991 and 1995, and each firm as

$$\ln W_{ijt} = \beta_0 + \beta_1' \mathbf{X}_{ijt} + \varepsilon_{ijt} \ ,$$

where W_{ijt} is the full-time equivalent monthly wage for worker i in firm j at time t; $\mathbf{X}_{ijt}$ is a vector of individual characteristics including gender, education, labor market experience, labor market experience squared and tenure and ε_{ijt} is the usual error

[4]Within firms, the group of managers is, in many respects, a much more homogeneous group than white-collar workers, a category including employees with a variety of tasks.

term. Estimations are based on firms with at least five white-collar employees.

The variance in the residuals for each firm and each year is used as a measure of conditional wage dispersion. This measure is the residual inequality after controlling for observable human-capital characteristics and is then used as an independent variable in employee-weighted regressions on firm performance. Other measures of wage dispersion for white-collar workers that will be used are the coefficient of variation of wages, the 90-10th percentile wage ratio and the white/blue-collar wage ratio. These measures represent raw measures of wage dispersion and do not account for individual differences in human capital accumulation, such as education and labor market experience.

All equations include control for firm differences in human capital of the work force and for industry and firm size. Firm differences in human capital are accounted for by including the share of the work force that (i) have more than 5 years of labor market experience, (ii) have more than 3 years of tenure, (iii) have attended at least long upper secondary school and (iv) are younger than 25 and older than 50 years, respectively. The share of women, the share of white-collar workers and the share of workers with foreign origins are also included.

The effect of pay spread for white-collar workers on firm performance will be estimated on both cross section for 1991 and 1995 as well as first-difference equations for 1991-1995. The latter method deals with the effect of unobserved firm heterogeneity. In the analyses of managers, the coefficient of variation in managerial wages, the wage difference and the wage ratio between the CEO and other managers will be used.

One problem in testing whether wage dispersion increases with the number of contestants is how the contestants should be defined. Since I lack information identifying vice presidents, I follow Main *et al.* (1993) and Eriksson (1999) in defining the contestants to be all individuals reported as being part of the management team. This group is approximately equal to those who could be seen as active in the tournament. Finally, in testing the effects of output uncertainty, the coefficient of variation in firm sales during the 1992-1995 period is used.

5 Results

5.1 The Effect of Wage Dispersion on Firm Performance

5.1.1 *White-collar workers*

Tables 1 and 2 show results on the effect of wage dispersion on firm performance for 1991 and 1995. Columns 1 through 4 show the results where firm performance is used as dependent variable along with different measures of wage dispersion. Columns 5 and 6 show results where the wage rate is used as dependent variable.

- Tables 1 and 2 about here -

Column 1 in Tables 1 and 2 shows the effect of conditional wage dispersion (residual inequality) on profits to be positive and significant. The positive relationship between wage dispersion for observationally equal employees and firm performance is consistent with tournament theory. Quantitatively, for 1991, a one standard deviation increase in pay dispersion is associated with a 3,970 SEK increase in firm profits per employee. Given that the mean profits in 1991 is 29,021 SEK, the estimate indicates that a one standard deviation increase in conditional wage dispersion increases profits per employee by approximately 15 percent, evaluated at the mean. The corresponding figure for 1995 is 37 percent.[5] These two periods are characterized by different business cycle phases. In the 1991-1995 period, Sweden experienced its deepest recession since the 1930s, with a substantial fall in GDP and an increase in total unemployment from 5 to 15 percent.

The results for alternative measures of wage dispersion are presented in columns 2-4. These are the coefficient of variation in pay, the 90-10th percentile ratio and the white/blue-collar wage ratio. It is evident from these columns that the results are qualitatively robust for different measures of wage dispersion. Regardless of measure used, wage dispersion is positively related to firm performance, the only exception being the coefficient for the white/blue-collar wage ratio that is insignificant in 1995.

[5]The quantitative difference between the two periods is partly due to differences in firm sample. Estimating the effect of conditional wage dispersion on firm performance for a panel of firms present in both 1991 and 1995 yields no significant difference between the two periods.

Results are also robust for including various other variables in the equations. For instance, the capital-labor ratio may affect the relationship between pay structure and firm performance. It can also influence workers' possibilities of extracting rents from the firm, depending on the size of labor costs in relation to the cost of capital.[6] A positive relationship between the capital-labor ratio and wages can also influence the intra-firm wage distribution and profits. However, including firms' capital-labor ratio does not change the effect of wage dispersion on firm performance. I have also estimated models using the log of wage dispersion to take a convex relationship into consideration. With a few exceptions, the results remain unchanged.

Columns 5 and 6 show results on the relationship between mean wages and wage dispersion. According to tournament models, wages are, on average, higher in firms with more dispersed wages. The estimated coefficient for the conditional wage dispersion variable and the coefficient of variation are both significant and positive, thereby being consistent with tournament theory.

All equations presented in the paper have also been estimated using a sub-sample of firms with at least 50 employees. The reason for this sensitivity analysis is that there may be systematic organizational differences between small and large firms that can influence the results. Unreported results show very similar qualitative effects for the sample of large firms, as compared to those reported in the tables. Quantitatively, the estimated coefficients are, in most cases, larger for larger firms.

The cross-section estimates presented above may be biased if variables correlated with wage dispersion are omitted. Unobserved firm heterogeneity may lead to an overestimation of the effect of wage dispersion on firm performance if, for instance, high ability workers and managers are sorted into high profit firms where wages are higher and more dispersed. In order to deal with unobserved firm heterogeneity, difference equations are estimated on a panel of firms present in both 1991 and 1995. Results from the first-difference estimates are presented in Table 3.

- Table 3 about here -

A Hausman test is used to discriminate between the fixed-effects and the random-effects models. This is a test for correlation between the firm-specific effects and the independent variables. Results of the Hausman tests indicate that the firm-effects

[6] Arai (1999) finds a positive relationship between firms' capital-labor ratio and individual wages.

are not correlated with the independent variables in the equations with profits as the dependent variable. This means that the random-effects estimator is efficient, taking both cross-section and time-series variation into account.

An important issue to consider is sorting of employees. Results would be driven by some sorting mechanism if employees with certain characteristics are systematically sorted into firms with a certain wage distribution. However, the Hausman test shows that exogeneity of the wage dispersion variable cannot be rejected. This, in turn, means that sorting of workers is not a problem and I can rely on consistent cross-section estimates.

The Breusch and Pagan LM-test is used to test for individual random-effects. The null-hypothesis is that the variance in the firm-specific effects equals to zero. A rejection of the null shows that firm-specific effects are present. As can be seen in Table 3, the estimated test statistic is significantly different from zero.

Results from the estimated random-effects models show a positive and statistically significant effect of wage dispersion on profits. For the conditional wage dispersion measure, the quantitative effect is in the range of the estimates obtained in cross-section estimations for 1991 and 1995. Similar results are found for the other pay spread variables presented in columns 2-4. For instance, an increase in the coefficient of variation in wages by one standard deviation increases firm profits by approximately 20 percent, evaluated at the mean of profits. Regressions of average pay on wage dispersion, again controlling for firm differences in human capital accumulation and firm size, show significantly lower wages in firms with less dispersed wages (see columns 5 and 6 in Table 3).[7] This is, once more, consistent with tournament theory.

5.1.2 *Managers*

Table 4 shows results on the relationship between managerial wage spread and profits (column 1-5) and the association between average pay for managers and wage dispersion (columns 6 and 7). All equations include control for human capital, industry and firm size.

- Table 4 about here -

[7] Equations with average pay as the dependent variable are estimated with a fixed-effects model. Results of the Hausman test indicate rejection of the random-effects model, given the specification.

Column 1 in Table 4 shows a significant positive association between the coefficient of variation in CEO wages and profits per employee. Quantitatively, the estimated coefficient means that a one standard deviation increase in the coefficient of variation of wages for managers is associated with an approximately 29,000 SEK increase in mean profits per employee for those firms in 1995 with information on managers. This amounts to 62 percent of the mean profits per employee in the sample.[8] Columns 3 and 5 show results when alternative measures of executive wage spread are used. These are the wage difference between the CEO and other managers in the firm, and the wage ratio between the CEO and other managers, respectively.[9] Once more, results show a positive effect of managerial pay spread on profits.

As stressed by Main *et al.* (1993), Lazear's tournament model suggests control for the average pay of managers and the degree of task independence among executives. To take the former into consideration, equations are estimated including firms' mean wages. This is done in order to take the part of the positive effect of pay dispersion on profits that can be due to a relationship between mean wages and profits into account. A positive relationship between wages and profits is compatible with various versions of efficiency-wage models, predicting higher profits for firms paying higher wages (see Akerlof and Yellen (1986)). Results controlling for the average pay among managers, reported in columns 2 and 4, show that inclusion of this variable does not change the estimated coefficient for the coefficient of variation variable.[10]

Considering the issue of team interdependence, wage dispersion must be modified if competition among managers leads to negative effects, such as sabotage or uncooperative ("hawkish") behavior.[11] To take team interdependence into account, Main *et al.* (1993) and Eriksson (1999) include the proportion of executives holding jobs with profit center head titles as a measure of executive team interdependence. Due to

[8]It should be noted that a one standard deviation increase in wage dispersion is associated with a large increase in wage dispersion; around 30 percent evaluated at the mean.

[9]I have also experimented with other measures of wage dispersion that do not alter the results.

[10]The estimated coefficient for log average pay is negative and significant in the regression with coefficient of variation as a measure for wage dispersion, whereas it is positive, but insignificant, when the wage gap between the CEO and other managers is used. Besides the difference in definition of wage spread, the reason behind this seems to be the differences in the sample of firms. Estimating equation 2 on the same sample as in equation 4, leaves the estimated coefficient for lag average wage insignificant. However, the coefficient of variation is still positive and significant. It should also be noted that the mean wage and the measures of managerial wage dispersion are highly correlated, as is confirmed by significant raw correlations.

[11]In the terminology of Lazear, the effect of pay dispersion among managers depends on whether the firm's managers mainly consist of "hawks" or "doves".

lack of information on profit center head titles, this could not be done in this study. However, neither Main *et al.* (1993) nor Eriksson (1999) find a significant effect of the team interdependence variable.

Finally, results on the relationship between average pay for managers and wage dispersion are mixed. I do find a positive and significant effect when the coefficient of variation in managerial pay is used as an independent variable (column 5), but the opposite is found when the wage difference between the CEO and other managers is used as a measure of executive pay spread (see column 6).

5.2 Basic Managerial Pay Structure

As described above, the tournament model suggests that the wage gap between executives (players) in one rank and those in the next should be sufficiently large to create incentives for managers to do their best. This pay gap (the prize of the tournament) is expected to increase, the higher one moves up the hierarchy, thereby implying a convex wage structure for managers.

- Table 5 about here -

Table 5 presents the mean wages and the percentage increase in compensation for four managerial levels. As can be seen from the table, the mean wage increases when moving up the hierarchy. Starting from the highest level, managers on level 1 have a 70 percent higher mean wage than those on level 2. The corresponding wage increases between levels 4 and 3 and between 3 and 2 are 43 and 14 percent, respectively. Wages increase when moving up the corporate ladder, and there also seems to exist an extra high wage gap at the very top. However, data do not reveal a convex pay structure for managers. The results in Table 5 can be compared with Main *et al.* (1993) and Leonard (1990), who find a convex pay structure including an extraordinarily large increase at the top of the hierarchy and with Eriksson (1999), reporting increasing pay differences but no additional reward at the top.

The wage differences in Table 5 do not account for differences in human capital between managers, nor for differences in firm size and industry affiliation. It may be the case that observed pay gaps are due to differences in individual and firm characteristics and that there exists a systematic sorting into different management levels. Column 1

in Table 6 shows results from estimating a wage equation on executive job levels and individual and firm characteristics.

- Table 6 about here -

The estimated wage differences between levels are now somewhat lower, but still, moving from the second highest to the highest level is associated with the largest percentage increase. The percentage pay differences between levels, starting from the lowest level, are 32, 8 and 54 percent, respectively. Including level dummies leads to a large increase in explanatory power. 42 percent of the variance in pay is explained by a model controlling for individual-, firm- and industry factors, as compared to 62 percent when executive level dummies are included.

Executive pay is increasing in experience and schooling and is lower for women than for men. It is also higher in large firms. However, managers are not rewarded for tenure within the firm, the coefficient for seniority is in fact negative. This effect is consistent with the notion that the most able managers are the first to be promoted, while the others remain longer in the firm and receive lower wages. The same pattern is observed in columns 3 and 4, where separate equations are estimated for the two highest management levels.

Human capital and firm characteristics also affect the prize of the tournament, as measured by the difference in pay between the CEO and other executives. Column 5 and 6 show the wage gap to be positively related to the size of the firm and negatively related to firm tenure. It is, however, not significantly affected by education and experience.

5.3 The Effect of the Number of Contestants

In an extension of tournament models with many contestants, McLaughlin (1988) shows that, in the case of risk-neutral contestants, the prize spread increases with the number of contestants. Empirically, this means that after controlling for other economic determinants of managerial pay, the more vice presidents, the larger the observed wage gap between the CEO and the vice presidents. The effect of the number of contestants on managerial pay spread is presented in Table 7.

- Table 7 about here -

Column 1 shows results for a specification with only the number of contestants and firm size as independent variables. The dependent variable is the log difference between CEO compensation and the average wage for the other executives. The coefficient for the number of contestants is negative and highly significant. This result is clearly at odds with the prediction in tournament models with risk-neutral players of a positive relationship between the number of participating contestants and the managerial pay gap. The results are not altered when industry dummies are added to control for fixed industry effects (column 2) or controls for differences in human capital accumulation between firms (column 3). The point-estimates for the effect of the number of contestants in columns 1-3 are not significantly different.

The same pattern also emerges when the coefficient of variation in executive pay is used as dependent variable (see columns 4-6 in Table 7). The results in Table 7 are in line what is found by O'Reilly *et al.* (1988), who also finds a negative relationship. A positive association between the number of contestants and managerial pay spread is presented in Main *et al.* (1993) and Eriksson (1999).

The negative relationship between the number of firm managers and managerial wage dispersion is consistent with wage-setting theories stressing the importance of fairness and/or cohesiveness (see the references given in Section 2). If, for instance, department managers compare their wage to that of the CEO and perceive the relative wage as unfair, they may put less effort into their work and, hence become less productive. In this case, a compressed wage structure in firms with a large number of managers is productivity enhancing. A similar argument can be based on the risk for uncooperative behavior, if managerial wages are very dispersed (see Lazear (1989)).

Finally, drawing conclusions on the effect of the number of contestants on the wage spread, it is important to keep in mind that the theoretical prediction of a positive relationship hinges on the assumption of contestants' risk behavior. McLaughlin (1988) shows that in the case of risk-averse contestants, the effect on the wage spread is ambiguous; the optimal wage spread going to zero in the limit.

5.4 The Effect of Market Demand Volatility on Pay

Various versions of tournament models show that the greater is the importance of variability in demand, the lower is the optimal effort level. This means that in markets with a large stochastic output component, the wage gap must be sufficiently high to counterbalance the negative effect on effort from the random component. Empirically, this implies that we will observe large wage spreads in markets characterized by a high degree of demand and output uncertainty. Results from tests on this aspect of tournament theory are presented in Table 8.

- Table 8 about here -

The dependent variable in column 1 is the pay gap between the CEO and the average pay for other managers. Consistent with tournament theory, the coefficient for the coefficient of variation in firm sales is positive and significant, implying higher wage dispersion in firms operating in volatile markets. The results are robust for including control for variation in human capital among firms (column 2). The point-estimates in columns 1 and 2 are very similar, suggesting that firm-differences in observable human capital do not influence the results. The result on the effect of market demand volatility is also robust for including the number of contestants (see column 3).

To make the results comparable to Eriksson (1999), Table 8 also shows results when the coefficient of variation in executive compensation is used as a dependent variable. As can be seen from columns 4-6, results on the effect of noisy business environment on wage dispersion remain qualitatively unchanged. Quantitatively, the coefficient implies that a one standard deviation increase in the variable for firm sales uncertainty is associated with a 6-11 percent increase in the pay spread for managers. This effect is higher than what is found in Eriksson (1999). He reports that an increase in the coefficient of variation in firm sales by 1 standard deviation increases the coefficient of variation in pay by 3 percentage points.

6 Summary and Conclusions

This paper uses a large matched worker-firm data set for Sweden to test several predictions from tournament theory. First of all, the relationship between within-firm pay

inequality and firm performance is studied. In addition, three more specific implications from tournament models are tested. The analysis is carried out for both white-collar workers and managers, using different measures of intra-firm wage dispersion.

For white-collar workers, results show a positive effect of intra-firm pay spread on firm performance for 1991 and 1995. This applies to different measures of wage dispersion, capturing both raw differences as well as differences corrected for part of the wage spread being due to differences in human capital accumulation. Using detailed information on individual characteristics, all equations include controls for firm differences in human capital of the work force and for industry and firm size. To take unobserved firm heterogeneity and the possibility of systematic sorting of employees into account, difference equations are estimated on a panel of firms present in both 1991 and 1995. Once more, consistent with tournament theory, the first-difference estimates show a positive and significant effect of wage dispersion on profits. Quantitatively, an increase in the coefficient of variation in wages by one standard deviation increases firm profits by approximately 20 percent, evaluated at the mean of profits. Results from Hausman tests show that exogeneity of the wage dispersion variable cannot be rejected and that there exists no systematic sorting of employees. Hence, there seems to be a causal effect of pay spread on firm performance, both in cross-section and first-difference estimations.

The results for managers are based on information on about 10,000 managers in 560 firms. For various measures of wage dispersion and specifications, a positive and significant association between managerial pay and profits is found. Based on the data on managers, several other hypotheses from tournament theory are also tested. These are (i) a convex relationship between pay and job levels for managers, (ii) a positive relationship between the number of executives (contestants) and wage dispersion for managers and (iii) a positive association between market demand uncertainty and managerial pay spread.

Regarding the first of these predictions, results show that wages increase as one moves up the corporate hierarchy. There also seems to exist a particularly high wage gap at the very top. However, data do not reveal a convex pay structure for managers.

No support is found for the hypothesis of a positive relationship between the number of managers (contestants) and wage spread. Instead, results show a negative and significant effect of the number of executives on pay spread among managers, a result consistent with wage-setting theories stressing the importance of fairness and cooper-

ation.

Finally, consistent with tournament theory, a higher wage dispersion is found in firms operating in volatile product markets, characterized by a high degree of output uncertainty.

Most studies that empirically test aspects of tournament models have used US data on executive compensation. The present paper is the first to test predictions from tournament theory on Swedish data. Given the large differences between US and Swedish labor markets, this study adds to the empirical literature on the effects of how wages are distributed within firms. Despite the differences in wage-setting institutions and the distribution of wages, results are similar to those in previous studies.

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Tables

Table 1. The effect of firm-level wage dispersion for white-collar workers on profits per employee and log average wage in 1991. Firm-size WLS estimations. Standard errors in parentheses.

	1	2	3	4	5	6
	Profits per employee				Log average wage	
Residual inequality	283.4**				1.53***	
	(116.0)				(.20)	
Coefficient of variation		725.1*				5.19***
		(399.2)				(.69)
90-10th percentile ratio			259.2**			
			(127.9)			
White/blue-collar ratio				1551.4***		
				(211.3)		
Log firm size	10.3***	10.3***	10.3***	9.1***	.02***	.02***
	(1.37)	(1.37)	(1.37)	(1.93)	(.00)	(.00)
Human Capital	YES	YES	YES	YES	YES	YES
Industry	YES	YES	YES	YES	YES	YES
R^2 (overall)	0.33	0.32	0.32	0.49	0.63	0.63
No. of employees	63 798	63 862	61 862	53 699	63 798	63 862
No. of firms	525	531	531	260	525	531

Notes: *** indicate significance at the 1%-level, ** significance at the 5%-level and * significance at the 10%-level. Human capital corresponds to control for workers' experience, seniority, education, gender, age, blue-collar workers and birth origin. The industry classification corresponds to 14 industries. F-tests for the joint insignificance of the human-capital variables are rejected in all equations, except equation (4). F-tests for the joint insignificance of the industry dummies are rejected in all equations.

Table 2. The effect of firm-level wage dispersion for white-collar workers on profits per employee and log average wage in 1995. Firm-size WLS estimations. Standard errors in parentheses.

	1	2	3	4	5	6
	Profits per employee				Log average wage	
Residual inequality	926.4***				2.55***	
	(167.2)				(.15)	
Coefficient of variation		3843.9***				8.03***
		(562.0)				(.50)
90-10th percentile ratio			1035.2***			
			(183.5)			
White/blue-collar ratio				366.5		
				(456.7)		
Log firm size	14.0***	13.2***	14.8***	22.9***	.00	-.00
	(2.05)	(2.02)	(2.04)	(4.77)	(.00)	(.00)
Human Capital	YES	YES	YES	YES	YES	YES
Industry	YES	YES	YES	YES	YES	YES
R^2 (overall)	0.20	0.21	0.20	0.23	0.65	0.63
No.of employees	97 746	97 900	97 900	75 212	97 746	97 900
No. of firms	1 067	1 080	1 080	432	1 067	1 080

Notes: *** indicate significance at the 1%-level, ** significance at the 5%-level and * significance at the 10%-level. Human capital corresponds to control for workers' experience, seniority, education, gender, age, blue-collar workers and birth origin. The industry classification corresponds to 14 industries. F-tests for the joint insignificance of the human-capital variables are rejected in all equations. The same applies to F-tests for the joint insignificance of the industry dummies.

Table 3. The effect of firm-level wage dispersion for white-collar workers on profits per employee and log average wage. Results from first–difference estimations for 1991-1995. Standard errors in parentheses.

	1	2	3	4	5	6
	Profits per employee				Log average wage	
Residual inequality	275.1***				1.27***	
	(93.3)				(.17)	
Coefficient of variation		561.7*				6.32***
		(345.7)				(.59)
90-10th percentile ratio			187.7*			
			(114.5)			
White/blue-collar ratio				753.3***		
				(262.8)		
Log firm size	3.5	3.1	3.3	5.1	-.03*	-.03*
	(2.7)	(2.7)	(2.7)	(4.0)	(.01)	(.01)
Human Capital	YES	YES	YES	YES	YES	YES
Hausman test	11.2	11.1	10.8	9.8	31.9***	25.2***
Breusch and Pagan test	33.2***	33.9***	34.4***	18.6***	80.9***	95.7***
R^2 (overall)	0.06	0.05	0.04	0.09	0.35	0.42
No. of firms	267	268	268	171	267	268

Notes: *** indicate significance at the 1%-level, ** significance at the 5%-level and * significance at the 10%-level. Human capital corresponds to control for workers' experience, seniority, education, gender, age, blue-collar workers and birth origin. The industry classification corresponds to 14 industries. Equations 1-4 are estimated with a random-effects model and equations 5 and 6 with a fixed-effects model. F-tests for the joint insignificance of the human-capital variables are rejected in all equations, except equation (4).

Table 4. The effect of firm-level wage dispersion for managers on profits per employee and log average wage in 1995. Firm-size WLS estimations. Standard errors in parentheses.

	1	2	3	4	5	6	7
	Profits per employee					Log average wage	
Coefficient of variation	3233.2***	3372.2***				2.72***	
	(485.8)	(489.3)				(.83)	
CEO - other managers' diff.			127.8***	132.5***			-.19***
			(26.6)	(28.7)			(.04)
CEO - other managers' ratio					1258.1***		
					(258.4)		
Log average wage		-51.2**		25.0			
		(25.3)		(56.8)			
Log firm size	11.2***	10.4***	24.4***	25.0***	24.3***	-.02***	-.02***
	(3.1)	(3.2)	(5.8)	(5.9)	(5.7)	(.01)	(.01)
Human Capital	YES	YES	YES	YES	YES	YES	YES
Industry	YES	YES	YES	YES	YES	YES	YES
R^2 (overall)	0.26	0.27	0.45	0.45	0.45	0.42	0.58
No. of employees	87 473	87 473	56 038	56 038	56 038	87 473	56 038
No. of firms	561	561	185	185	185	561	185

Notes: *** indicate significance at the 1%-level, ** significance at the 5%-level and * significance at the 10%-level. Human capital corresponds to control for workers' experience, seniority, education, gender, age, blue-collar workers and birth origin. The industry classification corresponds to 14 industries. F-tests for the joint insignificance of the human-capital variables are rejected in all equations. The same applies to F-tests for the joint insignificance of the industry dummies.

Table 5. Average pay for different management levels (SEK) and percentage difference between levels.

Level	Number of observations	Average Pay	Standard Deviation	Difference
1	517	44 413	17 261	69.7
2	3 175	26 174	8 545	14.2
3	2 112	22 923	7 619	43.1
4	4 394	16 024	3 014	

Table 6. The effect of individual and firm characteristics on wage differences between managerial levels and on wage dispersion. Robust standard errors in parentheses.

	1	2	3	4	5	6
	Log Monthly Wage				Wage Dispersion	
	All Managers	All Managers	Level 1 Managers	Level 2 Managers	Level 1 Managers	Level 2 Managers
Level 1	.77***	.78***				
	(.02)	(.02)				
Level 2	.35***	.35***				
	(.01)	(.01)				
Level 3	.25***	.28***				
	(.001)	(.007)				
Log firm size		.00**	.03***	-.01***	.05***	.00
		(.00)	(.01)	(.00)	(.01)	(.00)
Female	-.16***	-.15***	-.28***	-.18***	-.09**	.01
	(.01)	(.01)	(.05)	(.01)	(.04)	(.01)
Experience	.04***	.03***	.01	.04***	.01	-.01**
	(.00)	(.00)	(.01)	(.00)	(.01)	(.00)
Experience^2/100	-.06***	-.006***	-.00	-.05***	-.02	.02***
	(.00)	(.00)	(.03)	(.01)	(.02)	(.001)
Seniority/10	-.01**	-.02***	-.09***	-.03***	-.08***	-.01
	(.00)	(.00)	(.02)	(.017)	(.02)	(.01)
Education level	YES	YES	YES	YES	YES	YES
Industry	NO	YES	YES	YES	YES	YES
R^2	0.61	0.62	0.24	0.28	0.34	0.15
N	9 996	9 996	516	3 159	495	1 815

Notes: *** indicate significance at the 1%-level and ** at the 5%-level. The industry classification corresponds to 14 industries. The dependent variable in columns 5 and 6 is the wage gap between the CEO and other managers. F-tests for the joint insignificance of the industry dummies are rejected in all equations.

Table 7. The effect of the number of contestants on wage dispersion for managers. Firm-size WLS estimations. Dependent variable is wage dispersion for managers, measured as the wage gap between the CEO and other managers and the coefficient of variation for log executive pay. Standard errors in parentheses.

	1	2	3	4	5	6
	Wage gap			Coefficient of variation		
Number of managers/100	-.08***	-.09***	-.12**	-.001***	-.001***	-.001***
	(.01)	(.00)	(.01)	(.000)	(.000)	(.000)
Log firm size	.10***	.14***	.13***	.003***	.003***	.003***
	(.02)	(.02)	(.02)	(.000)	(.000)	(.000)
Human Capital	NO	NO	YES	NO	NO	YES
Industry	NO	YES	YES	NO	YES	YES
R^2 (overall)	0.15	0.46	0.54	0.13	0.20	0.26
No. of employees	56 038	56 038	56 038	87 458	87 458	87 458
No. of firms	185	185	185	560	560	560

Notes: *** indicate significance at the 1%-level and ** at the 5%-level. Human capital corresponds to control for workers' experience, seniority, education, gender, age, blue-collar workers and birth origin. The industry classification corresponds to 14 industries. The dependent variable in columns 1-3 is the wage gap between the CEO and other managers and in column 4-6, the coefficient of variation for log executive pay. F-tests for the joint insignificance of the human-capital variables are rejected in equations (3) and (6). The same applies to F-tests for the joint insignificance of the industry dummies in equations (2), (3), (5) and (6).

Table 8. The effect of market demand volatility on wage dispersion for managers. Firm-size WLS estimations. Dependent variable is wage dispersion for managers, measured as the wage gap between the CEO and other managers and the coefficient of variation for log executive pay. Standard errors in parentheses.

	1	2	3	4	5	6
	Wage gap			Coefficient of variation		
CV of Sales/Employee	.242***	.327***	.242***	.008***	.007***	.007***
	(.079)	(.086)	(.073)	(.002)	(.002)	(.002)
Log firm size	.040***	.012	.117	.001***	.001***	.002***
	(.014)	(.017)	(.019)	(.000)	(.000)	(.000)
Number of managers/100			-.113***			-.001***
			(.014)			(.000)
Human Capital	NO	YES	YES	NO	YES	YES
Industry	YES	YES	YES	YES	YES	YES
R^2 (overall)	0.30	0.39	0.57	0.19	0.27	0.29
No. of employees	56 021	56 021	56 021	87 456	87 456	87 441
No. of firms	184	184	184	560	560	559

Notes: *** indicate significance at the 1%-level. Human capital corresponds to control for workers' experience, seniority, gender, age, blue-collar workers and birth origin. The industry classification corresponds to 14 industries. The dependent variable in columns 1-3 is the wage gap between the CEO and other managers and in column 4-6, the coefficient of variation for log executive pay. F-tests for the joint insignificance of the human-capital variables are rejected in equations (2), (3), (5) and (6). The same applies to F-tests for the joint insignificance of the industry dummies in all equations.

Appendix: Data Description

Definition of variables

Individual outcome variables:
Wages: Monthly pre-tax full-time equivalent wages in 1990 prices (using CPI) based on Swedish Trade Union Confederation (*LO*) and the Swedish Employers' Confederation (*SAF*) wage data and completed with the income registers from Statistics Sweden (*SCB*).

Demography variables:
Gender, Age and Birth Origin are from SCB's Population Census (*Registret över totalbefolkningen*).

Human Capital variables:
Education level dummies are based on 2 digit level of the Swedish Education Nomenclature (SUN-codes) from the Swedish Education Register (*Utbildningsregistret*). These are *Compulsory School* (less than 9 years), *Comprehensive School* (9 years), *Upper Secondary School* (2 years at most), *Upper Secondary School* (3 years), *Long Upper Secondary School* (more than 4 years), *College* (Shorter University Education) and *University.*
Experience is number of years on the labor market according to the Employment Register (*Sysselsättningsregistret*).
Seniority is number of years at the establishment based on tracing the individual back to 1986 in the Employment Register (*Sysselsättningsregistret*). The variable is left censored at 5.5 years. Individuals with more than 6 years of seniority are given the mean seniority in Sweden according to the Level of Living Survey, i.e. 16 years.

Industry and Occupational Groups:
Industry dummies based on the 2-digit SIC (*SNI69* and *SNI92*). Own classification of 14 industries.
Blue- and White-collar worker according to the Population and Housing Census of 1990 (*FoB90*). These refer to the occupation classification in 1990 and not necessarily to the current employment.

Balance sheet information:
Profits (Swedish kronor) are defined as annual profits after capital depreciation. Available for the period 1987-95 (MM Partners).
Number of employees refer to average number of employees. Available for the period 1987-95 (MM Partners).

Table A.1. Sample Means.

	White-collar workers						Managers		
	1991			1995			1995		
	N	Mean	SD	N	Mean	SD	N	Mean	SD
Female	63 862	.30		97 900	.34		10 198	.11	
Experience	63 862	20	9.17	97 900	21	9.70	10 198	26	8.04
Seniority	63 862	8	6.68	97 900	10	7.00	10 198	11	6.56
Education level:									
Compulsory School < 9	63 767	.06		97 789	.04		10 198	.08	
Comprehensive School =9	63 767	.06		97 789	.06		10 198	.06	
Upper Secondary School < 3	63 767	.20		97 789	.26		10 198	.22	
Upper Secondary School 3	63 767	.24		97 789	.21		10 198	.24	
Upper Secondary School > 3	63 767	.21		97 789	.22		10 198	.17	
College < 3	63 767	.22		97 789	.20		10 198	.22	
University	63 767	.01		97 789	.01		10 198	.02	
Log monthly wage	63 862	9.63	.31	97 900	9.65	.32	10 198	9.93	.37
Residual inequality	63 798	.04	.01	97 746	.05	.02			
White/blue collar ratio	53 699	1.02	.02	75 212	1.03	.01			
90-10th percentile ratio	63 862	1.08	.01	97 900	1.07	.02			
Coefficient of variation in wages	63 862	.03	.004	97 900	.03	.01	10 198	.03	.01
CEO-other managers wage diff.							10 198	.78	.29
CEO-other managers wage ratio							10 198	1.08	.03
Coeff. of variation of Sales/Empl.							10 195	.26	.25
Profits/Employee, 100.000 SEK	63 862	.29	.42	97 900	.45	1.04	10 198	.47	.97
Log firm size	63 862	7.75	1.49	97 900	7.62	1.80	10 198	7.23	1.69

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