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

by

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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.

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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 to a competitive model with friction. In addition, incentive theories of wages and efficiency-wage hypotheses suggest that high wages lead to high profits.[1] 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

[1]For bargaining models see Oswald (1982, 1985). 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).

[2]See Katz and Summers (1989), and Blanchflower *et al.* (1996) on the US. Abowd and Lemieux (1993) and Christofides & 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) uses only regional unemployment on a small sample of employees with a limited panel dimension.

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 in providing evidence on the impact of profits and unemployment risk on wages based on disaggregated individual and firm data. As such, this study deals explicitly with the problems of firm and worker heterogeneity as well as the issue of endogeneity of profits. A novelty in this paper is that the relationship between aggregate unemployment risk and wages is estimated taking into account that marginal workers have both lower wages and 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 from 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 is found on wages for both 1991 and 1995, two periods characterized by very different business cycle phases. The unemployment rate during the late 1980's in Sweden was extremely low, 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 we control for time-invariant individual and firm

specific heterogeneity. Our elasticities imply that wage inequality in Sweden due to the spread in profits is as high as 14 percent of 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 profit 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 by using instruments for profits in estimation. Previous studies have, apart from lagged profits, used instruments such as energy costs (Blanchflower *et al.*(1996)) and import selling prices (Abowd and Lemieux (1993)). Abowd and Lemieux report elasticities that increase 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 mean wages.

Finally we 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 usually do 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 negative correla-

[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 average profits.

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

The remainder of the paper is as follows. Data are described in Section 2 and the empirical setup is discussed in Section 3. Basic results on the effect of profits on wages are reported in Section 4 where we also report variation of rents across worker groups as well as within and between industries. Individual heterogeneity and endogeneity issues are analyzed in Section 5. The relation between individual and aggregated unemployment risk with wages is analyzed in Section 6 and, finally, the paper is concluded in Section 7.

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 to their employing establishments by means of unique organization numbers. These establishments then form the basis of the Swedish Establishment Survey (APU) in which a large number of administrative data for *all* individuals working in APU-establishments 1987, 1991, or 1995 are collected forming a larger individual sample.[5] These data are matched to official balance sheet data for the APU firms by means of the Swedish system of corporate registration numbers.

Data on wages and job characteristics are from Statistics Sweden as well as from data collected by the Central Organization of Blue-Collar Workers (LO) and the Central Confederation of Private Employers (SAF). Wages are computed as full-time equivalent pre-tax monthly salaries. These data are matched

[5]To assure that our sample is representative, the sample is compared with another randomly drawn sample of individuals in 1991 and 1995. Comparing sample means from the two samples indicates no significant differences.

with individual unemployment records for the period 1992-1995 from the AMS Event Database (*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 who have 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 for profits we use annual profits, after capital depreciation, per employee. This profit measure is clearly observable for both the employer and 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:

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

$$\varepsilon_{ijt} = \upsilon_i + \upsilon_j + \upsilon_t + u_{ijt},$$
$$\mathbf{U}'_{ijt}\boldsymbol{\beta_2} = \sigma U_{i,1992-1995} + \xi \bar{U}_{j,1992-1995},$$
$$P_{ijt} = \Sigma_{t-4}^{t-1} p_{jt} / \Sigma_{t-4}^{t-1} e_{jt}.$$

where W_{ijt}, $\mathbf{X}_{ijt}$, P_{ijt} and $\boldsymbol{U}_{ijt}$ denote wages, vectors of individual characteristics, average past profits and unemployment risk variables for individual i in firm j during year t $(= 1991, 1995)$. The individual characteristics used are age, 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.

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. We do this for both individuals who are 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. This 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 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 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 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.[6] 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.

4 Effects of Profits on Wages

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.[7] The implied elasticities for 1991 range between 0.008 and 0.025 across various specifications.[8] 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.[9]

The lower elasticities for 1995 are a reflection of an exceptional recession that occurred during the early 1990's in Sweden. The period 1991-1995 is characterized by a large fall in GDP and an increase of total unemployment from 5 to 15 percent. Average profits for 1991-1994, used in the 1995 specification, therefore reflect employer's 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 work-

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

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

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

[9]Christofides and Oswald (1992) report elasticities of around 0.01 for Canada. The elasticities in the Oswald and Sanfeys (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 UK. The corresponding range in Arai (1999) study on Sweden is 0.01 to 0.02.

ers, we would not expect a higher degree of rent-extraction. For this reason, we focus on the profit estimates for 1991.

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

	1	2	3	4	5	6
	1991	1991	1991	1995	1995	1995
Profit/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
Number of obs.	175 023	172 280	172 280	178 259	176 009	176 009

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

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 is intact when including a dummy variable for females but decreases by fifty 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 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 into account group effects. The profit effect is robust to 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 who work more than 50 or 75 percent of full-time. These experiments, not reported in the paper, leave the results unchanged.[10]

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.

In column 1, an interaction variable between female and profits is added. The estimate of this 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 to examine rent-sharing differentials with respect to gender, we run separate regressions for men and women(see columns 2 and 3).

[10]These sensitivity analyses are performed for all estimations reported in Table 1-5.

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

	1	2	3	4
	All	Men	Women	All
Profit/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
Number of obs.	172 280	127 012	45 268	172 280

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

Results imply that the elasticities of wages with respect to profits are 0.017 for men and 0.010 for women indicating substantial 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 lowers the

impact of profits for both men and women but does not affect the rent-gap implying lower rents for women within industries.[11]

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 collective bargaining, 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, indicate lower rents for blue-collar workers (see column 4, Table 2). Running the same regression, adding industry dummies yields results implying that the lower rents for blue collar workers is not due to the systematic sorting of blue collar 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 in comparison to workers 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 level of education, experience and seniority. 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 collective bargaining system in Sweden where wages are bargained at the industry level as the highest level of centralization. However, due to a higher degree of coordination of unions and employer or-

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

ganizations, one might not expect substantial differences across industries. This view is confirmed by our results indicating that the effect of profits drops roughly 25 percent when we add 14 industry dummies constructed such that they roughly capture the different bargaining areas (see column 3 and 6 in Table 1 for 1991 and 1995, respectively).

Obviously, the magnitude of within industry effects depend on the level of industry aggregation. Our 14 industries are more aggregated than actual bargaining levels in Sweden. For example the Swedish Central Organization of Blue-Collar Workers (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 percent 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 individual level.

A remaining question then is how large a part of 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. These effects correspond to a specification where we also control for blue-collar status and thus represent rent-differentials within broad occupational groups. Excluding the blue- collar dummy reduces the share of the local bargaining effect attributed to individual bargaining to around 60 percent.

Our results are in line with what is usually perceived as the magnitude of wage drift in Sweden indicating the relative im-

portance of local bargaining in Sweden.[12]. 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 third of these effects are attributed to firm differences and the remaining 1/3 is the result of within firm variation.

5 Individual Heterogeneity and Endogeneity

Cross section estimation, such as those reported above, ignores 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 3).[13] The period 1991-1995, as mentioned above, represents 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 those individuals who were working in the same firm 1991 and 1995, as well as for those who changed employer. It is reasonable to as-

[12]The wage drift is calculated as a residual difference between the centrally negotiated wages and the final wage outcome. See National Institute of Economic Research (Konjunkturinstitutet) quarterly reports for 1991-1995.

[13]Notice that almost all individual variables are time invariant and therefore not included.

sume 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 as well as 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 profit effects on wages are stronger in firms experiencing a profit increase between the two periods.[14]

Table 3. Effect of profit 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	ΔProfit/Employee> 0	ΔProfit/Employee > 0
Profit/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
Number of obs.	187 364	100 022	66 374

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

Estimating the fixed effects model on a sub-sample of workers who experienced an increase in profits in their employing firm, leads to a four times higher estimate of profits with a t-statistics

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

of 18.64, indicating a high significance level (see column 2 in Table 3). Furthermore we restricted the estimation to workers who remained 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 yields basically the same results indicating that there are no significant differences between workers who switched firms and those who didn't (see column 3 in Table 3). The message of these results is that rents exist and are not due to fixed individual and 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 rises by ten times. This at least 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)). We run regressions instrumenting average profits with lagged values of profits separately for different lags. Results reported in columns 1-4 in Table 4 confirm that lagged profits affect wages.

Furthermore, we use new instruments for profits based on establishment level data matched with our data, that includes 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 gives us

a *short term* product demand elasticity ranging between 0 and above 1 as an indicator of firm's product market power.

Table 4. Effects of profit 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	6
Instrument:→	Profit/ Empl. in -87	Profit/ Empl. in -88	Profit/ Empl. in -89	Profit/ Empl. in -90	Demand elasticity	Demand elasticity and no. of competitors
Profit/Employee	.066***	.11***	.099***	.082***	.28***	.36***
	(.004)	(.003)	(.003)	(.003)	(.020)	(.02)
Female					-.14***	-.13***
					(.002)	(.002)
Experience					.02***	.02***
					(.0003)	(.0003)
Experience^2/100					-.04***	-.04***
					(0.0001)	(0.0001)
Seniority/10					-.004	-.007
					(.001)	(.001)
Education level	NO	NO	NO	NO	YES	YES
Blue collar					-.089***	-.085***
					(.002)	(.002)
Industry	NO	NO	NO	NO	YES	YES
R^2	0.01	0.02	0.01	0.01	0.37	0.32
Number of obs.	124 364	160 070	169 902	174 297	89 009	88 498

Notes:
i) *** indicate significance at 1%-level. Industry classification corresponds to 14 industries.
ii) Results in 1-4 are robust for including individual characteristics and industry affiliation. Column 5 and 6 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.
iii) Using lagged instruments for 1991,1992,1993 or 1994 for estimating the effect of profits in 1991-1994 on wages in 1995 we obtain basically the same results except when profit in 1992 is used as instrument. The instruments in column 5 and 6 are not available for 1995.

Another indicator, available in our data, of firm market power is the number of important competitors in the product market which is measured in four classes: $\{1, 2-5, 6-10, >10\}$. Using these two variables as instruments for profits in separate specifications, rent sharing estimates increase radically by approximately ten times. The implied elasticities from these estimations are as high as 0.1. This implies that more than half of the wage inequality in Sweden is due to the spread of profits according to Lester' s 'range of pay'. Our conclusion is that using observed profits to estimate rents tends to underestimate the impact of rent sharing.

6 Unemployment Risk

In 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 depends also 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 that are 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 microdata studies estimate both the impact of profits and unemployment on wages.[15] The unemployment risk for a group of workers is however not uniformly distributed across firms and workers. To examine the heterogeneity across firms and individuals with respect to unemployment and thereby to study the effect of un-

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

employment on wages, one needs to link unemployment risks to individual employees and to firms. Since by definition firms 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 manners. First, individuals with a high expected unemployment risk might have 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 is associated with firm and establishment wage bargaining.

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, however, straightforward. Individuals could not have foreseen the crises of the early 1990's when bargaining on wages prior to 1991, and therefore could not have based their negotiations on adequately predicted future unemployment risks. Using actual unemployment records, however, might reasonably capture the relative fragility of individuals to employment shocks and thus their relative bargaining success. The unemployment experience during 1992-1995 used in 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.

Table 5. Effects of profits and unemployment on different aggregation levels on wages. OLS estimates for cross sections in 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 parenthesis.

	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 level (log)	-.02**	-.02***		-.04***	-.04***	
	(.007)	(.007)		(.006)	(.007)	
Industry level[a](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
No. of obs.	163 722	163 722	166 215	167 272	167 272	170 172

Notes:
** indicate significance at 5%-level and *** at 1%-level.
[a] Results are for unemployment aggregated at 3-digit industry levels. Using aggregated unemployment at 5-digit industry level yields basically the same results.

Results on the impact of unemployment risk at the individual and aggregate levels on wages for 1991 and 1995 are reported in Table 5. Our various specifications indicate that both the individual and firm unemployment variable is negatively and

significantly correlated to wages.[16] The results are similar when estimating this specification for 1991 and 1995, the unemployment variable measures *expected (perfect foresight)* relative unemployment risk in the 1991 estimation and *expected (adaptive)* unemployment when estimating on data for 1995. The impact of the individual unemployment measure on wages means that low-skilled workers have lower wages and usually experience higher unemployment risks.

Results in Table 5 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 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 during the previous years. This implies that profits and an aggregated unemployment measure at the firm level are strongly correlated. This is confirmed by negative and highly significant raw correlation between prof-

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

its and aggregated firm unemployment. The Pearson correlation is -0.21 for 1991 and -0.14 for 1995. The corresponding rank correlation is -0.20 and -0.24 for 1991 and 1995 respectively. Adding the unemployment measures to our basic specification for 1991 leads to a drop of the profits estimate from 0.06 to 0.05. The corresponding figures for 1995 are 0.03 and 0.02 (compare Table 1 and Table 5).

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. 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 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 indicate that profits and unemployment affect wages.

7 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 0.01 and 0.03. 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 wage inequality in Sweden due to the spread in profits

is as high as 14 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 yield 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 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 to individual wages. The effect of our aggregate measures differ 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.

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 results reported for the US and UK despite the fact that Sweden constitutes an extreme in regards to degree of unionization and extended labor protection laws in comparison 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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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 Labor 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 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* ≥ 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 having 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 at the current employment.

Balance sheet information:
Profits (Swedish kronor in 1990 prices) is 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 four-year average (1987-1990) profit-per-employee below -53,300 (the 1st percentile) and above 261,000 (the 99th percentile) 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 were replaced by 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 6932 firms in 1991 and 5757 in 1995.
Number of employees refer 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 raised prices with 10 percent, how would then 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	33.9	178 259	.33	63.5
Size	175 023	6081	9456	178 259	3561	4940

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