

Microdata Evidence on Rent-Sharing

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Abstract

We examine the effect of firm profits on wages for individual workers while focusing on the empirical complications associated with estimating the extent of rent-sharing. Controlling for worker and firm fixed-effects and using several instruments to deal with the endogeneity of profits, we report results indicating that OLS-estimates strongly underestimate the effects of profits on wages. Moreover, the effect of profits on wages are estimated separately for firms with increasing and decreasing profits within a given time period. We find a positive and stable effect only in firms with increasing profits. This is in line with the idea that falling profits do not lead to wage cuts while increasing profits imply higher wages.

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1 Introduction

Empirical work on the effect of profits on wages has taken as a starting point the basic theoretical conjecture that firms' ability to pay is an important determinant of individual wages. According to bargaining models, individual wages are positively correlated with firm profits (see e.g. Oswald (1982, 1985) and Manning (1994)).

There is a body of empirical literature dealing with the impact of profits on wages based on aggregated data.¹ 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. Few recent studies use microdata to analyze the impact of profits on wages.² The purpose of this study is to examine the effect of firm-level profits on individual wages. We use two large matched employer-employee data sets. The first sample covers more than 170,000 Swedish employees for 1991 and 1995, matched with their employing firm's balance-sheet information. The second sample covers around a million employees and their employing firms for the period 1996-2000. The matched data contain detailed information on individual and firm characteristics, including individual unemployment experience as well as annual balance-sheet information and explicit survey information on product-demand elasticity.

The main problem in examining the impact of profits on wages is how to deal with the numerous empirical problems that arise in identifying this

¹See Katz and Summers (1989), and Blanchflower *et al.* (1996) on the US. See also Abowd and Lemieux (1993) and Christofides and Oswald (1992) who use Canadian contract data. For European studies, see Blanchflower *et al.* (1990), Holmlund and Zetterberg (1991), Nickell *et al.* (1994), Hildreth and Oswald (1997) and Estevão and Tevlin (2003).

²Abowd *et al.* (1999) use microdata on wages and profits for the private sector in France. Arai (2003) uses a small matched worker-firm sample for Sweden. Margolis and Salvanes (2001) examine microdata on males in manufacturing in France and Norway. Martins (2003) examines a large Portuguese matched data set.

effect. One needs to control for individual heterogeneity implying that repeated information on individuals are needed. Another issue is to separate the impact of profits from other group-effects at the firm level such as the effects associated with firms' wage policies. It is then essential to have information on all or at least many of the employees in a firm in order to examine the importance of firm effects.

The trickiest issue is, however, the endogeneity of profits, due to two sources of reverse causality. The effect of profits on wages can be underestimated due to the accounting relationship between wages and profits, implying that higher wages lead to lower profits. Moreover, according to theories of incentive pay and various versions of efficiency wage theory, higher pay imply higher profits.³ Besides lagged profits, previous studies have used instruments at the industry level, such as import selling prices (Abowd and Lemieux (1993)), energy costs (Blanchflower *et al.* (1996)) and input-output tables (Estevão and Tevlin (2003)).

A problem with the IV-method is that if one overlooks a theoretically minor source of correlation between the instrument and the error term in the second step regression, results can be biased and instrumentation does not solve the endogeneity problem. By using several instruments we do not have to rely on the risky strategy of relying on a single instrument only.

Our data cover repeated observations for individuals that allow us to control for individual heterogeneity. The large number of observations within each firm also make it possible to examine firm effects. Moreover, we have information on product demand elasticity at the establishment level as well as export revenue and energy costs at the firm level. These measures are proper candidates for

³For efficiency wage theories, see Akerlof and Yellen (1986).

working as valid instruments for profits since they should affect profits but not wages other than via profits.

The contribution of this paper is to provide evidence on the impact of firm-level profits on individual wages based on matched employer-employee data accounting for worker and firm heterogeneity as well as the endogeneity of profits by relying on several new instruments. The common practice in the literature is to use instruments for profits at aggregates levels. Our study use several set of instruments at the firm level.

The results of our study imply a positive effect of profits on wages that is robust across various model specifications. Our evidence provides strong support for the existence of rent-sharing as this effect on wages is found during the 1990s: a period characterized by very different business cycle phases. Using our instruments for profits yield results indicating that OLS-estimates largely underestimate the impact of profits on wages. Moreover, we find a positive and stable effect only in firms with increasing profits in line with the idea that falling profits do not lead to wage cuts while increasing profits imply higher 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 on wages are reported in Section 4. Individual and firm heterogeneity is dealt with in section 5 and the endogeneity issues are addressed in Section 6. Finally, the paper is concluded in Section 7.

2 Data

We use two set of matched employer-employee data based on the same source of registered information.⁴ We study firms with at least 30 employees. The origin of the 1991-1995 sample 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 and firms 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 1991 or 1995 are collected, forming a larger individual sample.⁵ 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 for individual characteristics from the two samples indicates no significant differences.

Balance-sheet information is available for the period 1987-1995.⁶ The data on employees are matched to official balance sheet data for the employing firms through the Swedish system of corporate registration numbers identifying the employing firm. 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 were removed.

The 1996-2000 sample use information from a large database consisting of register based data from Statistics Sweden. Two data sets from the database, one with information on individuals and one with information on firms, are used in this paper. These data sets are matched together by means of unique identity

⁴For a description of the data, see Appendix.

⁵For a description of the Swedish Establishment Survey (APU), see le Grand *et al.* (1996).

⁶These data were made available by Per Weidenman, MM Partner.

numbers. The firm-based data set contains financial data and other information at the firm level. The data set covers all firms, approximately 300,000 in number, in the entire Swedish economy. The individual-based data set contains detailed information from official registers on a very large representative sample of employed individuals, covering around 2 million employees each year.

Data on wages, human capital and job characteristics for both the 1991-1995 and the 1996-2000 data sets are from Statistics Sweden as well as from data collected by the Swedish Trade union Confederation and the Swedish Employers' Confederation. Wages are computed as full-time equivalent pre-tax monthly salaries. These data are matched with individual unemployment records for the period 1992-1994 from the National Labor Market Board (*AMS Händelsedatabas*). Our data allow us to track individuals from 1992 to 1994 and define a dichotomous variable equal to one when the individual is registered as unemployed some time during this period and zero otherwise. Using individuals' unemployment experience reasonably captures the relative fragility of individuals to employment shocks and thus, works as a control for individual heterogeneity.

As a measure of profits, we use accounting 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. For the 1991 and 1995 sample we have firm profits for all years from 1987 to 1995. To use all information on profits, we compute four year averages as a measure of profitability. For the 1996-2000 sample we use yearly profits.

We also have information on a measure of product-demand elasticity, firm export revenues and firm-level energy costs. These variables are used as in-

struments for profits. The information on product-demand elasticity originates from an establishment survey on a sub-sample of firms in 1991. Firms were asked to report a predicted sales response for the next 6 months to a hypothetical product price increase of 10 percent, ranging from essentially unchanged sales to a more than 10 % drop in sales. Reported answers to this question give us a *short-term* measure of product-demand elasticity. Energy costs, available for firms with at least 50 employees, are firms' total costs for energy while export revenues are revenues from sales to firms located outside Sweden. Energy costs and export revenues are available for the period 1996-2000.

3 Empirical Setup

We start from the following linear wage function as derived in previous studies (see e.g. Blanchflower *et al.* (1996) and Hildreth and Oswald (1997)).

$$w \approx \bar{w} + \frac{\phi}{1-\phi} \left(\frac{\pi}{n} \right) \quad (1)$$

Wages w are a function of workers' productivity related characteristics and unemployment risk as components of \bar{w} , profits per employee $\frac{\pi}{n}$ and the relative bargaining power $\frac{\phi}{1-\phi}$ of the employees. The wage equation above can be specified empirically as follows.

$$w_{ijt} = c + \alpha_i + \alpha_{jt} + \mathbf{X}'_{ijt} \boldsymbol{\beta} + \varepsilon_{ijt} \quad (2)$$

$$\alpha_{jt} = \gamma_0 + \pi_{jt} \gamma_1 + \bar{\mathbf{X}}'_{jt} \boldsymbol{\gamma}_2 + v_{jt} \quad (3)$$

Wages and a vector of individual characteristics are denoted as w_{ijt} and \mathbf{X}_{ijt} , respectively for individual i , in firm j at time t . The fixed overall intercept is denoted as c . This specification allows individual effects, α_i , as well as firm

effects, α_{jt} . While we throughout the paper assume individual effects as fixed, we elaborate with the firm effects starting from the specification in equation (3). The firm effect depend on firm profits π_{jt} and group-level variables computed as averages of individual characteristics included in \mathbf{X}_{ijt} and denoted as $\bar{\mathbf{X}}_{jt}$. Furthermore, there might exist firm effects that we cannot specify due to a lack of information. These effects are captured by the random term v_{jt} . This specification allows wages to vary across firms due to differences in firm profits, composition effects as well as a set of unknown variables captured by v_{jt} . The random errors are assumed to be normally distributed. The random error ε_{ijt} is assumed to be *iid* while v_{jt} is dependent within groups since it is common to every worker in the employing firm j . Initially, when studying firm (group) effects, such as the effects of skill composition and unemployment on wages, we assume random firm effects but later in the study move on to assume fixed firm effects when we are interested in removing these effects.

We first estimate the above model using data for cross sections on 1991, 1995 and 2000, setting $v_{jt} = 0$ and $\gamma_2 = 0$. The individual characteristics controlled for are gender, educational level, labor market experience, blue-collar status, seniority at the establishment level and industry affiliation. For the 1995 sample we also include individual unemployment records during 1992-1994. In a second step we allow for γ_2 and estimate the model by maximum likelihood in a mixed-effects model set-up that also yields predicted variances for the group-level errors.⁷

Cross-section estimation may suffer from biases due to omitted variables, correlated with the profit measure. Having data for several years and for all individuals in the firm would enable us to control for individual time-

⁷See Pinheiro and Bates (2000). Using the terminology of hierarchical modelling, equation (2) and (3) are the individual and firm level equations.

invariant heterogeneity as well as firm-effects in a single fixed-effect estimation (see Abowd *et al.* (1999)). A problem, however, is that the identification of firm-effects relies on workers who change employers. This is valid if workers' firm-changes are exogenous and random. This is not likely to be the case (see Gibbons and Katz (1992) for a discussion of this issue). We proceed instead as follows.

We estimate mixed-effects models for our cross section data explicitly examining the firm-group effects. The group effects imply that some firms pay higher wages than others and that wages can vary with profits, gender and skill composition, past group unemployment experience as well as a set of unknown factors captured by the firm error term.

We then proceed to control for worker heterogeneity and firm-effects. We deal with this potential problem by estimating individual fixed-effects 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, as well as using the 1996-2000 panel. This is done for all individuals as well as a sub-sample of individuals working in the same establishments across different years. The former only controls for individual effects while the latter controls for both time invariant individual- and firm-specific effects, thus accounting for a systematic sorting of individuals across firms.

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. We deal with the endogeneity of profits by using various instruments for profits in estimation on our matched worker-firm data. Our rich data allow for three different instruments yielding results that do not depend on the choice of a single instrument as is the

case in many previous studies. Instruments include lagged profits, a measure of product-demand elasticity, export revenues and energy costs.

Lagged profits can be used as instruments since firm profits exhibit large volatility and are thus not persistent over time. Product-demand elasticity, export revenues and energy costs at the firm-level have not previously been used in the literature as instruments for profits. Product demand elasticity directly captures the existence of product market rents. Firm profits should directly be affected by market power as measured by firm product-demand elasticity. Any effect of product-demand elasticity on wages should go through profits via a rent-sharing mechanism. Revenues in export markets are largely governed by demand in foreign markets. Foreign demand for firm products are most likely governed by factors that are not correlated to wages. Firm energy costs are exogenous and largely determined by internationally determined energy prices. Energy costs affects profits and should not directly affect wages which therefore makes it a suitable instrument for profits.

4 Basic Results

Results reported in Table 1 indicate that there are positive and significant effects of firms' ability to pay on individual wages. The implied elasticities range between 0.01 and 0.02 across various specifications and years.⁸ 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

⁸The 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.

⁹Christofides 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.

– Table 1 about here

Note that the period 1991-1995 is characterized by a large fall in GDP and an increase of total unemployment from 5 to 15 percent. The estimates in the 1995 specification, therefore reflect employer’s ability to pay, during a recessionary period and the effect of profits on wages in 1991 and 2000 represent boom periods. Despite the differences in economic conditions during these periods the obtained elasticities are remarkably similar across years.¹⁰

An 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.¹¹ 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 analysis are performed for all estimations reported in Tables 1-5.¹²

We also investigate whether the extent of rent-sharing varies across gender and occupational groups, since the bargaining power of workers may systematically vary across these groups. Results based on separate regressions for

Hildreth and Oswald (1997) report elasticities in the range of 0.02 to 0.04 for the UK. The corresponding range in the Arai (2003) study on Sweden is 0.01 to 0.02.

¹⁰Estimations of the impact of profits on wages in 1991 using profits per employee in 1987, 1988, 1989 and 1990, respectively, instead of using four-year averages, yield coefficients ranging between 0.04 and 0.05 that is to be compared with 0.05 for the average profits per employee variable. The average profits per employee is stable during this period ranging between 0.24 and 0.32 (100,000 SEK). The corresponding estimates for the 1991 to 1994 period yield similar coefficients and elasticities when using one year profits per employee in 1991 and 1993 while the estimates for 1992 and 1994 and the elasticities are considerably smaller. We believe that this is the result of the turbulent economic environment in 1992-1995 as described above. This is confirmed by the large volatility of profits per employee in these years ranging from 0.04 to 0.97 as compared with the period average of 0.33.

¹¹For effects of capital intensity on wages, see Dickens and Katz (1987) on industry data and Arai (1999) on matched worker-firm data.

¹²Results not reported in the tables can be obtained from the authors upon request.

men and women imply that the elasticities of wages with respect to profits are higher for men than for women. The difference in elasticity is not due to difference in average profits for employers of men and women but rather reflect the difference in estimated coefficients for profits.¹³

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. 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.

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.¹⁴ 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 when we add 14 industry dummies constructed such that they roughly capture the different bargaining areas (see columns 2, 5 and 7 in Table

¹³Arai (2003) reports similar results for 1991 in Sweden using observed hourly wages and the same measure for profits. See Nekby (2003) for an examination of gender differences in rent-sharing.

¹⁴See e.g. Stephan and Gerlach (2005) and Simón *et al.* (2006) for analysis on the relationship between collective bargaining and wages.

1). The major fraction of overall rents, however, are due to local bargaining, i.e. bargaining at the firm/establishment and individual level. These 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.¹⁵

5 Controlling for Worker and Firm Effects

Wages might vary across firms due to various sources of firm effects. These can stem from differences in gender and skill compositions, firms wage policy or the workers' experienced unemployment. To investigate the role of firm effects, we estimate our model including the firm random-effects specification shown in equations (2) and (3). Results reported in Table 2 imply that the our estimate for profits is robust to allowing for firm random intercepts (effects).

– Table 2 about here

Our findings indicate that despite controls for individual heterogeneity with respect to past experienced unemployment and other individual characteristics as well as industry affiliation, individual wages are negatively correlated to the share of workers at the establishment who have experienced unemployment. This result is in line with results reported in the wage-curve literature where a negative relation between wages and unemployment at various group-levels (nation, industry, region) are reported.¹⁶

Next, we estimate fixed-effects models, controlling for unobserved individual heterogeneity. Panels a and b in Table 3 present results for the 1991-1995 and 1996-2000 panels, respectively.

¹⁵See the quarterly reports of the National Institute of Economic Research (Konjunkturinstitutet).

¹⁶See e.g. Blanchflower and Oswald (1994).

– Table 3 about here

The individual fixed-effects model estimated on data for 1991 and 1995 yields a positive and significant profit coefficient (see panel a). Notice that almost all individual variables are time invariant and therefore not included in the equations.

An issue is to consider is whether the profit effects on wages are stronger in firms experiencing a profit increase in comparison to firms experiencing decreasing profits. Workers who's employing firm experiences falling profits can hardly be expected to experience falling wages, due to wage setting institutions which almost entirely exclude wage cuts. The previous estimates of rent-sharing represent an average effect for both profitable firms and firms with losses. The effect of profits on wages is symmetric above and under the zero level only if wages disclose symmetric flexibility both upwards and downwards. There are however both theoretical and empirical studies pointing at an asymmetry in wage responses to various shocks.¹⁷ To examine this issue we estimate the profit effect for firms that experience increasing and decreasing profits, respectively.

Estimating the fixed effects model on a sub-sample of workers experiencing an increase in profits in their employing firm, leads to a huge increase in the estimate of profits (see column 3 in Table 3). For individuals working in firms with decreasing profits we find a negative and significant profit effect (see column 2). These results indicate that behind the overall rent-sharing estimates lies distinct differences between firms with increasing or decreasing profits.

Fixed-effects estimation aggregates the profit effect for individuals who have worked in the same firm as well as for those who have changed employer. The

¹⁷See Agell and Lundborg (2003) for evidence on downward wage rigidity in Sweden.

problem of endogenous switches might be important here though many employer switches during the period 1991-1995 are exogenous due to high unemployment rates at the time. Identifying the firm-effects by relying on individuals who switch firms is hazardous when we have no information about the reason of mobility. We therefore restricted the estimation to workers remaining at the same firm, to obtain a within individual and within firm effect estimate. Our approach is therefore basically to estimate individual-firm spell-fixed effects (see e.g. Andrews *et al.* (2005)).¹⁸ Running the model on this group of workers yields basically the same results indicating no significant differences between workers who switched firms and estimates on only those who did not change employer between periods (see columns 4-6 in Table 3).

Estimates using the 1996-2000 data in panel *b* confirm the results obtained for 1991-1995. A positive and significant profit effect is found for firms experiencing an increase in profits, while the opposite is true for firms with non-increasing profits during the period 1996-2000. Results are robust to estimating the model on those individuals who remain in the same firm, i.e. estimating within individual and firm effects. The message of these results is that rents exist and are not due to fixed individual or firm effects.

6 Instrumenting Profits

High wages might lead to high profits, as predicted by efficiency wage theory, but higher wages reduce profits as measured in firms' balance sheet reports,

¹⁸Note that in the case of no mobility between firms, individual fixed-effects and individual-firm spell fixed-effects are identical. Since the structure of our data is such that information on employees originates from repeated samples of firms, there is little mobility between firms over time.

implying reverse causality from wages to profits. To deal with this problem, Abowd and Lemieux (1993) use international selling prices as instruments and find that the effect of profits on wages increased ten fold.¹⁹ This indicates that the rent-sharing effect might be underestimated due to the accounting relationship between wages and profits. Other instruments used in the previous literature are energy costs (Blanchflower *et al.* (1996)) and input-output tables (Estevão and Tevlin (2003)) at the industry level.

Though it is possible to empirically evaluate the correlation between an endogenous explanatory variable and an instrument, there are no possibilities to examine whether the instrument is uncorrelated with the error-term in the second-step regression. This means that much hangs on theoretical arguments for validity of an instrument. If we overlook a theoretically minor source of correlation between the instrument and the error term in the second step regression, results can be biased and instrumentation does not solve the endogeneity problem. By using several instruments we do not have to rely on the risky strategy of relying on a single instrument.

A common instrument in the literature is to use lagged values of profits as instrument (see e.g. Blanchflower *et al.* (1996) and Hildreth and Oswald (1997)). Therefore, in Table 4, we first report results for 1991 when profits are instrumented with lagged values of profits in 1987 and 1988. Results reported in columns 1 and 2 confirm that profits affect wages.²⁰

– Table 4 about here

¹⁹The increase in the elasticity might be due to using weak instruments (see Bound *et al.* (1995)). This cannot be verified since the first step regressions are not reported in Abowd and Lemieux (1993).

²⁰In Table 4, 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 on wages in 1995, we basically obtain the same results except when profits in 1992 are used as an instrument yielding insignificant results.

Results in column 3 in Table 4 indicate that using our measure of product demand elasticity as an instrument for profits, increases rent-sharing estimates by approximately 10 times as compared to our cross-section or fixed-effects estimates. According to the first step results in Table 4, our instrument is highly correlated with profits. Firms reporting greater product-demand elasticity have, as predicted, lower profits.

Our dataset covering the period 1996-2000 allows for several other instruments for profits to deal with the potential problem concerning endogeneity of profits. Restricting our sample to individuals working in the same firms, we can estimate fixed-effects models to control for time-invariant individual and firm heterogeneity, while using the set of time-varying instruments available in the data. Results of these estimations are reported in Table 5.

– Table 5 about here

As in the case of our estimated fixed-effect models in Table 3, we run our model on two sub-samples. We split the sample into observations associated with firms that exhibit increasing profits during the entire period 1996-2000 and those that exhibit non-increasing profits during this period. The general idea is, as mentioned above, that we should not expect a positive effect of profits on wages for firms with non-increasing profits due to downward wage-stickiness.

The first-step regressions show that foreign sales are strongly and positively correlated to profits, especially for firms with increasing profits. Notice that the estimated coefficient in column 2, although significant, is close to zero. Regarding energy costs as an instrument we find that energy costs are not correlated to profits in general but positively correlated with profits for firms with decreasing profits. This implies that energy costs are a weak instrument, confirmed by the radical increase in the estimate for profits (see Bound *et al.*

(1995)). We therefore rule out energy costs as a valid instrument.

Results on all firms and individuals indicate the existence of substantial effects of profits on wages that are much larger than the corresponding OLS estimates (see columns 1 and 4 in Table 5). Turning to the sub-sample of firms, we only find positive profits effects on wages in firms with increasing profits, consistent with the idea that downward wage-stickiness leads to rent-sharing effects only in firms with increasing profits. The general conclusion from Tables 4 and 5 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.

7 Conclusions

Using a large Swedish matched employer-employee panel data for the 1990's, we are able to simultaneously deal with the empirical problems associated with estimating the extent of rent-sharing.

A problem with identifying the impacts of profits on wages is the endogeneity of profits. There are two main potential sources of reverse causality from wages to profits. First, high wages might lead to high profits, as predicted by efficiency wage theory. Second, higher wages reduce profits by definition, as measured in firms' balance sheet reports. In addition to lagged profits, we use export revenues and product demand elasticity to deal with this problem.

Controlling for worker and firm fixed-effects and using several instruments to deal with the endogeneity of profits, we report results indicating that OLS-estimates strongly underestimate the effects of profits on wages. Our results indicate that the extent of rent-sharing increases with the level of education, experience and seniority and that rents are higher for men compared to women.

The major fraction of overall rents are due to local bargaining, i.e. bargaining at the firm/establishment and individual level.

Moreover, the effect of profits on wages are estimated separately for firms with increasing and decreasing profits within a given time period. We find a positive and stable effect only in firms with increasing profits. This is in line with the idea that falling profits do not lead to wage cuts while increasing profits imply higher wages.

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Tables

Table 1. Effect of profits on wages. Results from cross-section estimates for 1991, 1995 and 2000. Dependent variable is log monthly wage. Robust standard errors corrected for within-firm correlation in parantheses.

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 |
|------------------------------|--------------------|--------------------|--------------------|--------------------|-------------------|-------------------|-------------------|
| | 1991 | 1991 | 1995 | 1995 | 1995 | 2000 | 2000 |
| Profits/Employee | .050*** (.018) | .036*** (.019) | .027 *** (.010) | .018*** (.007) | .018*** (.007) | .008*** (.002) | .007*** (.001) |
| Female | -.14*** (.007) | -.14*** (.02) | -.15*** (.008) | -.15*** (.008) | -.15*** (.008) | -.17*** (.008) | -.15*** (.009) |
| Experience | .022*** (.001) | .022*** (.001) | .017*** (.001) | .017*** (.001) | .019*** (.001) | .020*** (.001) | .020*** (.001) |
| Experience ² /100 | -.040*** (.003) | -.039*** (.003) | -.027*** (.002) | -.027*** (.002) | -.03*** (.002) | -.03*** (.002) | .003*** (.001) |
| Seniority/10 | .005 (.005) | .003 (.004) | .020*** (.006) | .011*** (.006) | .025*** (.005) | | |
| Education level | YES | YES | YES | YES | YES | YES | YES |
| Blue Collar | -.097*** (.009) | -.099*** (.009) | -.15*** (.010) | -.16*** (.009) | -.16*** (.009) | -.14*** (.020) | -.17*** (.011) |
| Individual unemployment | | | -.064*** (.002) | -.068*** (.002) | | | |
| Industry | NO | YES | NO | YES | YES | NO | YES |
| R ² | 0.43 | 0.45 | 0.47 | 0.49 | 0.48 | 0.43 | 0.44 |
| N | 154,830 | 154,830 | 152,406 | 152,406 | 152,406 | 932,746 | 932,746 |

Notes: *** indicate significance at the 1%-level. Industry classification corresponds to 14 industries. Education level corresponds to 7 education level dummies.

Table 2. Random firm-intercept models. Standard errors in parentheses.

| | 1991 | 1995 |
|----------------------------------|------------------|-------------------|
| Intercept | 8.99*** (.07) | 9.04*** (.07) |
| Profits/Employee | .034*** (.01) | .020*** (.00) |
| Female | -.13*** (.00) | -.14*** (.00) |
| \overline{Female}_{Firm} | -.04* (.02) | -.07** (.02) |
| Blue Collar | -.08*** (.00) | -.15*** (.00) |
| $\overline{Blue\ Collar}_{Firm}$ | .03 (.03) | .08*** (.02) |
| Individual unemployment | | -.06*** (.00) |
| $\overline{Unemployment}_{Firm}$ | | -.028*** (.01) |
| Log likelihood | 41,197 | 30,278 |
| Bayesian IC | -82,156 | -60,294 |
| N | 154,830 | 151,583 |
| STD of random intercepts | .082 | .086 |
| 95% confidence intervals | (.076,.088) | (.081, .092) |

Notes: ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively. Other variables included are experience, experience squared, seniority at the establishment level and education level dummies.

Table 3. Effect of profits on wages. Results from individual and firm fixed-effects models.
Dependent variable is log monthly wage. Standard errors in parentheses.

| | 1 | 2 | 3 | 4 | 5 | 6 |
|---------------------------|--------------------|--------------------------------|-----------------------------|--------------------|--------------------------------|-----------------------------|
| | All | $\Delta(\frac{\pi}{n}) \leq 0$ | $\Delta(\frac{\pi}{n}) > 0$ | All | $\Delta(\frac{\pi}{n}) \leq 0$ | $\Delta(\frac{\pi}{n}) > 0$ |
| <i>a: 1991-1995 panel</i> | | | | | | |
| Profits/Employee | .0024*** (.001) | -.075*** (.002) | .025*** (.002) | -.007*** (.001) | -.075*** (.002) | .035*** (.002) |
| Same employer | NO | NO | NO | YES | YES | YES |
| Hausman | 277*** | 1,418*** | 6.49*** | 198*** | 1,207*** | 31*** |
| Breusch & Pagan | 60,258*** | 24,317*** | 25,664*** | 49,677*** | 24,853*** | 24,123*** |
| R ² (overall) | .003 | .006 | .0005 | .002 | .005 | .0005 |
| N | 83,876 | 41,282 | 38,192 | 68,763 | 37,485 | 31,278 |
| <i>b: 1996-2000 panel</i> | | | | | | |
| Profits/Employee | .004*** (.000) | -.013*** (.000) | .015*** (.000) | .004*** (.000) | -.014*** (.000) | .015*** (.000) |
| Same employer | NO | NO | NO | YES | YES | YES |
| Hausman | 17,221*** | 5,332*** | 3,913*** | 15,766 | 11,627*** | 3,380*** |
| Breusch & Pagan | 3.2e+06*** | 1.2e+06*** | 1.7e+06*** | 2.9e+06*** | 1.2e+06*** | 1.7e+06*** |
| R ² (overall) | .02 | .0005 | .05 | .02 | .0004 | .04 |
| N | 556,434 | 301,602 | 340,205 | 555,090 | 247,298 | 328,996 |

Notes: $(\frac{\pi}{n})$ denotes profits per employee. *** indicate significance at the 1%-level.

Industry classification corresponds to 14 industries.

Table 4. Effect of profits on wages. Results from instrumental variable estimates using various instruments. Dependent variable is log monthly wage in 1991. Robust standard errors in parentheses.

| | 1 | 2 | 3 |
|---------------------|----------------------|----------------------|---------------------------|
| Instrument: | Profits/Empl. in -87 | Profits/Empl. in -88 | Product demand elasticity |
| Profits/Employee | .030*** (.003) | .063*** (.002) | .33*** (.068) |
| Wu-Hausman test | 183*** | 13*** | 17*** |
| R^2 | 0.42 | 0.44 | 0.31 |
| N | 122,195 | 157,494 | 89,009 |
| 1st step estimates: | 0.59*** (0.01) | 0.51*** (0.00) | -1.01*** (0.09) |

Notes: i) *** indicate significance at the 1%-level. Other variables included in estimation are gender, education level, experience, experience square, seniority and blue-collar status. The estimated coefficients for these variables are essentially the same as those reported in previous tables when estimating on the same sample of individuals. Adding industry dummies reduces the estimate for profits but does not effect the level of significance.

ii) Columns 1-3 are based on subsamples of the original matched data due to missing values for our instruments. OLS estimates of profits per employee on the same sample of individuals as in columns 1-3 yield basically the same estimates as compared with estimates of the corresponding equations on the full sample of individuals.

iii) First step estimates are estimated coefficients where profits per employee are regressed on our various instruments.

iv) The Wu-Hausman test examines if there is a systematic difference between the OLS and the IV estimator. The null-hypothesis is that the measure of profits is exogenous.

Table 5. Effect of profits on wages. Results from individual and firm fixed-effects models using various instruments for the 1996-2000 panel. Dependent variable is log monthly wage. Robust standard errors in parentheses.

| | 1 | 2 | 3 | 4 | 5 | 6 |
|---------------------|---------------------------|--------------------------------|-----------------------------|-------------------------|--------------------------------|-----------------------------|
| Instrument: | F o r e i g n s a l e s | | | E n e r g y c o s t s | | |
| | All | $\Delta(\frac{\pi}{n}) \leq 0$ | $\Delta(\frac{\pi}{n}) > 0$ | All | $\Delta(\frac{\pi}{n}) \leq 0$ | $\Delta(\frac{\pi}{n}) > 0$ |
| Profits/Employee | .034*** (.000) | -.53*** (.000) | .030*** (.000) | .37*** (.000) | -.16*** (.000) | .066 (.000) |
| $R^2(overall)$ | 0.30 | 0.27 | 0.04 | 0.04 | 0.01 | 0.06 |
| N | 471,470 | 222,375 | 265,641 | 468,113 | 184,173 | 294,492 |
| 1st step estimates: | .15*** (.00) | -.01*** (.00) | .19*** (.00) | -.01 (.00) | .02*** (.00) | -.06 (.00) |

Notes: i) *** indicate significance at the 1%-level. All estimates are conditioning on same employer, implying within individual and firm effect estimates.

ii) The estimates are based on subsamples of the original matched data due to missing values for our instruments. OLS estimates of profits per employee on the same sample of individuals yield basically the same estimates as compared with estimates of the corresponding equations on the full sample of individuals.

iii) First step estimates are estimated coefficients where profits per employee are regressed on the various instruments.

Appendix: Data Description

Individual outcome variables:

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

Unemployment: Information on unemployment history during 1992-94 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* (9 years), *Upper Secondary School* (less than 3 years), *Upper Secondary School* (3 years), *Post Secondary School* (less than 3 years), *University Undergraduate Studies* (3 years or more, not including graduate studies) and *University Graduate Studies*.

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 are for the 1991-1995 data set defined according to the Population and Housing Census of 1990 (*FoB90*). These refer to occupation classification in 1990 and not necessarily the current employment. For 1996-2000, the classification is based on trade union affiliation.

Balance sheet information:

Profits are defined as annual profits after capital depreciation in Swedish kronor in 1990 prices. Available for the period 1987-95 from MM Partner and for 1996-2000 from SCB. In estimations on the matched sample, we remove firms with profit-per-employee below the 1st percentile and above the 99th percentile.

Number of employees refers to average number of employees available for the period 1987-95 from MM Partners and for 1996-2000 from Statistics Sweden).

Export revenues are defined as total export revenues from sales to firms located outside Sweden (SCB) and is available for the period 1996-2000.

Energy costs refer to total energy costs (SCB). Available for manufacturing firms with at least 50 employees during the period 1996-2000.

Information from the Swedish Establishment Survey 1991 (APU):

Product-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%.

Table A.1. Summary of the principal characteristics of the data sets.

| | 1991-1995 | 1996-2000 |
|----------------------|--|--|
| Coverage | Private sector, firms with at least 30 employees | Private sector, firms with at least 30 employees |
| Individuals per year | Around 150,000 | Around 900,000 |
| Sources | Statistics Sweden, National Labor Market Board, Swedish Establishment Survey, MM Partner | Statistics Sweden |
| Variables | Register and survey data | Register data |
| The profits variable | Average four-year profits based on at least two years | Annual profits |

Table A.2. Descriptive Statistics

| | 1991 | | 1995 | | 2000 | |
|--|---------|-----|---------|-----|---------|------|
| | Mean | SD | Mean | SD | Mean | SD |
| Log monthly wage ^a | 9.50 | .26 | 9.54 | .29 | 9.70 | .31 |
| Female | .26 | | .28 | | .36 | |
| Experience | 17 | 10 | 20 | 10 | 19 | 12 |
| Seniority | 8 | 7 | 11 | 7 | | |
| Blue Collar | .62 | | .52 | | .49 | |
| Education level: | | | | | | |
| Elementary School < 9 | .17 | | .12 | | .08 | |
| Compulsory School =9 | .14 | | .13 | | .12 | |
| Upper Secondary School < 3 | .33 | | .34 | | .33 | |
| Upper Secondary School = 3 | .16 | | .16 | | .22 | |
| Post Secondary School | .11 | | .14 | | .13 | |
| University undergraduate | .09 | | .11 | | .11 | |
| University graduate | .004 | | .006 | | .005 | |
| Individual unemployment | .14 | | | | | |
| 4-year average Profits/Employee ^b | .29 | .34 | .34 | .63 | | |
| Annual Profits/Employee ^b | | | | | 1.37 | 3.63 |
| Observations | 154,830 | | 152,406 | | 932,746 | |

Notes:

^a In Swedish Kronor in 1990 prices.^b In 100,000 Swedish Kronor in 1990 prices.