DISCUSSION PAPER SERIES

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IZA DP No. 1376

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November 2004

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

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Discussion Paper No. 1376
November 2004

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IZA Discussion Paper No. 1376
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ABSTRACT

Rent Sharing Before and After the Wage Bill*

Many biases plague the estimation of rent sharing in labour markets. Using a Portuguese matched employer-employee panel, these biases are addressed in this paper in three complementary ways: 1) Controlling directly for the fact that firms that share more rents will, ceteris paribus, have lower net-of-wages profits. 2) Instrumenting profits via interactions between the exchange rate and the share of exports in firms' total sales. 3) Considering firm or firm/worker spell fixed effects and highlighting the role of downward wage rigidity. These approaches clarify conflicting findings in the literature and result, in our preferred specification, in a Lester range of pay dispersion of 56%, also shown to be robust to a number of competitive interpretations.

JEL Classification: C33, J31, J41

Keywords: rent sharing, instrumental variables, matched employer-employee data, fixed effects

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* I am indebted to Ian Walker and Robin Naylor for their insight and support. I also thank the feedback from Mahmood Arai, Wiji Arulampalam, Paul Bingley, Alex Bryson, Carl Campbell, Felix FitzRoy, Francis Kramarz, Alcides Martins, Derek Neal, Andrew Oswald, Pedro Portugal, Jonathan Thomas, and seminar participants at the University of Warwick, ESPE (New York), EARIE (Helsinki), EEA Summer School (London), CAED (London), and EALE (Seville). Financial support from "Fundação para a Ciência e a Tecnologia" (POCTI/ECO/33089/99 and SFRH/BD/934/2000) and the British Council and logistical support from the Bank of Portugal are gratefully acknowledged.

I. Introduction

Do firms share rents with their workers? A standard competitive model predicts that there is no relationship between workers' wages and the profits of their firms. Firms that face a sudden, unexpected increase in profits – because, for instance, market conditions improve substantially – have no reason, from the point of view of the competitive paradigm, to share some of these rents with their workers. The latter are simply paid the opportunity cost of their time, which is determined in the labour market and therefore not affected by the profitability of the firm.

However, a number of alternative, non-competitive models predict a positive correlation between rents and wages of comparable workers. For instance, bargaining models find that workers will receive wages in excess of their best alternative, and that this difference will depend positively on their firms' rents. Similar results are obtained in fairness and risk-sharing models.

Given these conflicting theoretical results, empirical studies have an important role in illuminating this debate. Unfortunately, the estimation of rent-sharing effects incurs a number of potential biases that have prevented a satisfactory solution to this matter. These biases include that due to the accounting relationship between profits and wages (so that higher rent-sharing will simultaneously decrease profits and increase wages and thus lead to the underestimation of rent-sharing effects); the potentially simultaneous determination of profits, wages and employment; the correlation between profits and missing variables that capture workers' ability; and measurement error.

This paper addresses these biases by exploiting a Portuguese matched employer-employee panel for the 1993-1995 period. First of all, we address the downward bias induced by the accounting relationship. This is achieved by drawing on a different measure of profits that also follows from a standard bargaining model but which we argue is more appropriate for the purpose of estimating rent sharing. This new variable, "gross" profits per worker (or "net" profits per worker plus average yearly wages per firm) allows one to control directly for the bias explained above. As far as we know, such a variable has never been used before. Given that our data set includes information (namely wages) for *all* workers of each firm, by making weak assumptions about employer taxes, we were able to compute wage bills per firm and total profits before wage costs.

Secondly, we use an instrumental-variable technique to uncover the endogeneous nature of profits. Our instrument, again warranted by the theoretical model, is obtained from the interaction between exchange rates (an exogenous price shifter) and the share of exports in total sales of each firm. Differences in this instrument across firms and years bring about the exogenous variability in profits used to identify the rent-sharing parameter. Other instruments also based on international trade can be found in Abowd and Lemieux [1993], who use prices of imports and exports, and Teal [1996], who uses exchange-rate variation.[1]

Finally, we draw on the longitudinal nature of the data to control for firm or firm/worker fixed effects, following a stream of the literature that includes Blanchflower, Oswald and Sanfey [1996], Hildreth and Oswald [1997] and Bronars and Famulari [2001]. Although we also consider a large set of time-variant and time-invariant controls, both at the worker and the firm level, there is still scope for unobserved factors to impact simultaneously upon profits and wages and render the rent-sharing coefficients inconsistent. Unobserved worker ability or unobserved organisation type are important examples of such variables. To the extent that these variables are time invariant, our fixed effects models will capture their effect.

As mentioned above, on top of the different measure of rents used, this study also contributes to the rent-sharing literature by combining controls for unobserved time-invariant variables and the instrumentation of profits. This is a new stream of research that includes to our knowledge only three contributions: Margolis and Salvanes [2001], Arai [2003] and Kramarz [2003]. Overall, these papers have documented smaller estimates of rent sharing than those typically obtained in the literature that focuses on the endogeneity of profits; and either smaller or similar results to those of the stream of the literature that controls for time-invariant characteristics.

In Margolis and Salvanes [2001], the authors examine the degree of rent sharing in France and Norway, using large matched employer-employee panel data sets and progressively adding further controls to the wage equations. In their final specification, which includes controls for industries, business cycle effects, fixed worker and firm effects and an instrument, Margolis and

[1] Different instruments include past technological innovations – Van Reenen (1996) – and output movements in the sector to which an industry sells – Estevão and Tevlin (2003).

Salvanes eliminate the rent-sharing coefficient in France but not in Norway. However, one concern about the results of Margolis and Salvanes (2001) is that they use what they consider to be “weak” instruments (sales). This could explain their insignificant results for the case of France.

Swedish data is used in Arai [2003], who examines a panel of workers and finds Lester ranges of between 12% and 24%. (Lester ranges are a measure of wage dispersion related to rent sharing which is customarily employed in this literature and will also be adopted in this paper. These ranges are defined as the elasticity of wages with respect to profits multiplied by four times the ratio between the standard deviation of profits and mean profits.) Arai’s results support bargaining interpretations of the wages-profits correlations rather than those based on supervision efficiency-wages models or short-run demand frictions. [2]

Finally, Kramarz [2003] considers French matched data. Special attention is placed on the twofold impact of imports: decreasing the workers’ outside options (due to outsourcing decisions) but improving workers’ bargaining outcomes (due to the hold-up that can arise after firms invest in importing schemes). Rents are instrumented with lagged prices of US exports. The results indicate that, for most workers, the effect of deteriorating outside options is stronger than that of the import investments hold-up. The bargaining power parameter is estimated at 0.20.

As to our findings in the present paper, a first result concerns the use of gross profits. Not only do we confirm the anticipated downward bias brought by the accounting relationship mentioned above, but we also find that the difference between the two regressors (gross and net profits) fades away as more controls (including firm or firm/worker fixed effects) are considered. This is consistent with the results that weak instruments may do more harm than good (see Bound, Jaeger and Baker, 1995) and that the use of extra controls strengthens the role of the instruments in identifying the equation.

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[2] In a related paper, Arai and Heyman [2001], a large Swedish matched panel with information for 1991 and 1995 is used and robust evidence of rent sharing is found. As in the other papers that use IV referred above, rent-sharing estimates increase substantially when profits are instrumented (with survey evidence on the degree of product-market competition faced by each firm): Lester ranges go up from 14% to 50%. However, no information could be found on the quality of the instruments used. Moreover, since the instrument is available for one year only, no evidence is

More specifically, we find that estimates using instrumental variables tend to overestimate the amount of rent sharing if controls for firm or worker characteristics are missing. This upward bias also occurs if the measure of rent sharing used (typically net profits) is less correlated with the instrument than our alternative measure (gross profits). These results support the suspicions of Oswald [1996], who regarded some IV estimates as too large to be credible. In Abowd and Lemieux [1993], for instance, instrumented Lester ranges exceed 90% while their OLS counterparts are negligible.

Overall, we find evidence of a significant and substantial amount of rent sharing in our data. In our preferred specification, we find a Lester range of 56%. Our results are shown to be robust to a number of competitive interpretations, such as industry-specific shocks, differences in capital intensity and the use of overtime. Evidence about the impact of downward wage rigidity is also presented. Furthermore, we also find that groups of workers one may expect to have more bargaining power inside the firm benefit more from rent sharing, as suggested by bargaining models of rent sharing but not fairness models, for instance.

The structure of the paper is as follows: Section 2 presents a bargaining model that motivates the empirical work done in the paper. Section 3 introduces the data set and the instrument used. Section 4 present the results and Section 5 addresses their robustness. Finally, Section 6 concludes.

II. Theory

One way to explore theoretically the extent of rent sharing in a labour market involves modelling wage and employment determination as a Nash bargaining problem, in which employers and workers choose employment and wage levels. In this framework, common in the literature, wages are derived from the solution to the following problem:

$$(1) \qquad \mathrm{Max}_{\mathrm{w,N}} \left[\phi \log \{(\mathrm{w} - x)\mathrm{N}\} + (1-\phi) \log \pi\right]$$

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presented on the degree of rent sharing when one controls simultaneously for fixed effects and the endogeneity of profits.

where w represents the wage rate, N the employment level, x is the alternative wage and ϕ the bargaining power of workers. π are ('net') profits, which are defined as θF(N,K)–wN–rK, in which θ is a demand shifter, F(.) the production function (assumed, without loss of generality, to depend only on labour and capital), r the interest rate, and K the capital stock.

From the first order condition with respect to wages and after some algebra, one obtains an empirically testable wage equation:

$$(2) \qquad w = x + \frac{\phi}{1-\phi}\frac{\theta F(N,K) - rK - wN}{\mathrm{N}}.$$

This specification suggests that wages should be regressed on a measure of the alternative wage of workers and on average revenues per worker net of wages and capital costs. However, should one estimate this specification, one must acknowledge the many problems involved in identifying the coefficient of interest. One issue is that wages affect both the left- and the right-hand side of the equation, given the accounting relationship between wages and (net-of-wages) profits. This induces a downward bias in the estimation of the rent-sharing parameter.

To deal with this bias, we consider an alternative specification that can also be obtained from the first order condition with respect to wages:

$$(3) \qquad \mathrm{w} = x + \phi\,(\frac{\theta F(N,K) - rK}{\mathrm{N}} - x).$$

According to the latter specification, wages should be regressed, as before, on a measure of the reservation wage and, differently from before, on average quasi-rents. This specification, followed in Abowd and Lemieux [1993] and Estevão and Tevlin [2003], seems more appropriate as it focuses on the difference between revenues per worker and the alternative wage each worker could obtain, which then measures the rents that may or may not be shared by the firm. One problem with this specification, however, is that one has to estimate in a first step the alternative wage of each worker, a process that typically involves some untested assumptions.

In order to deal with the problems of the previous two specifications, this paper will follow a slightly modified version of (3), where total revenues and the reservation wage are two separate terms:

$$\text{(4)} \qquad \mathrm{w} = (1\text{-}\phi)\, x + \phi \, \frac{\theta F(N,K) - rK}{\mathrm{N}}.$$

This specification allows the model to estimate simultaneously the degree of rent sharing (captured by the parameter ϕ) and the role of the workers' characteristics in the determination of their alternative wage. We will thus focus on equation (4), contrasting its results with those based on equation (2). In the former case, the key regressor will be total revenues per worker minus non-labour costs (which in the model are represented by capital costs only). This regressor will be proxied by accounting profits per worker plus the average wage.

Other biases may still affect our results. One such bias would arise if the assumption of "strongly efficient" contracts followed in equation (1) is relaxed. This assumption posits that firms and workers (or unions) decide simultaneously on wages and employment. However, it may be more reasonable to assume instead that unions determine wages and then firms choose employment levels, as in "right-to-manage" models.[3] In this case, employment is determined by the contracted wage and not by the alternative wage, as in "strongly efficient" contracts. This result suggests that rents will be highly endogeneous, as employment and wages are simultaneously determined.

Another potential source of bias is related to efficiency-wages models. Again, if productivity depends on wages, then rents will also depend on wages. Additionally, a control for average profitability in a micro-level wage regression may pick up some of the firm or worker heterogeneity not captured by other variables considered in the specification. This missing-variable problem is then likely to bias upwards the profitability coefficient, to the extent that, for instance, workers better skilled along unobservable dimensions are more likely to be allocated to more profitable firms.

Finally, further bias will occur if there is measurement error in the rents per worker variable. Typically, measurement error attenuates estimates towards zero, particularly with differenced data (Griliches and Hausman [1985]). This would make the case of rent sharing even stronger. However, this result may be inverted if measurement error is non-random, for instance if firms spread losses across periods so to reduce their tax liabilities. (See Margolis and Salvanes [2001] for a discussion of this case.)

Fortunately, these biases may be removed if one uses exogenous variation in profitability induced by movements of the θ parameter.[4] As equations (2)-(4) make clear, any source of variation of profits that does not impact directly upon wages is a valid instrument. However, the IV methodology requires some caution, as various authors, including Bound, Jaeger and Baker [1995] and Staiger and Stock [1997], have shown that the biases induced by weak instruments may be more serious than the biases one incurs when not accounting for simultaneity at all. This "weak instruments" result is another *a priori* reason why we favour a gross profits specification, as instruments that are based on movements of the θ parameter will exhibit stronger correlations with gross than net profits.

III. Data

The main data source used in this study is a matched employer-employee panel, Personnel Records ('Quadros de Pessoal'), which covers all employees in Portugal (and their firms) since the early 1980's. These data, resulting from compulsory country-wide surveys, include several variables about firms (industry, location, firm size, domestic/foreign ownership, sales, equity, etc) and several variables about each one of all employees at each firm (schooling, age, tenure, gender, different measures of earnings, hours worked, etc). Identifiers for both firm and employees are also present, allowing one to construct a matched panel.

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[3] These models (also called "monopoly union" models) produce inefficient contracts, in the sense that different employment-wages combinations exist that would Pareto-improve upon the model's equilibrium (see McDonald and Solow [1981]).

The Personnel Records data set does not include information on profits (or exports). These latter data were obtained from a survey of annual reports of large firms headquartered in Northern Portugal. This survey [Jornal de Notícias, 1994, 1995 and 1996] covers only the period 1993-1995, thus constraining our analysis to those three years. The two data sources were then matched, producing the data set used in this study. The new sample therefore represents the population of employees of large manufacturing-sector firms headquartered in Northern Portugal (Appendix 1 presents a more detailed description of each data source and of the method used to merge the data.)

After dropping observations with missing cells, our data set draws on more than 44,000 workers and more than 75,000 workers-year. There are 91 firms, which correspond to 197 firms-year. The most important industry is the textiles and clothing industry, which includes more than 60% of workers in each year.[5] Some descriptive statistics of the key variables used are presented, in terms of workers-year, in Appendix 2.

As mentioned before, the measurement of profits is a topic of particular interest in this paper. The first profits variable presented in Tables A1 and A2, 'Net Profits (per Worker)', corresponds to the standard version available in the literature. The second, 'Gross Profits (per Worker)', is obtained after adding the wage bill per worker (or the average wage per worker) in each firm and is the variable used in the empirical implementation of equation (4). This wage bill is computed from aggregating the information of all workers in each firm.[6] As wages are available for one month only, we assume that the total wage bill of each worker is 14 times that, plus the employer-paid taxes.[7] We find that gross profits per worker are between four and six times bigger than net profits (depending on whether we look at worker or firm averages, respectively). These

[4] This approach may nevertheless work poorly for some specific production functions – see Abowd and Lemieux [1993] and Nickell [1999].
[5] This is an industry that presents very low levels of union power and which is at the bottom of wage premiums as found in studies of inter-industry wage differentials both in Portugal (see Hartog, Pereira and Vieira, 2001) and in other countries (see Krueger and Summers, 1988, for the US). However, and despite its competitive features (at least as far as its product market is concerned), Martins (2003) shows that this industry, in Portugal and in the 1991-95 period, exhibits significant and non-transitory inter-firm wage differences across observably homogeneous workers.
[6] In some cases, a small share of the workforce of a firm cannot be considered because of missing cells. In this event, the average wage bill of the whole firm is extrapolated from the information on workers for which information on wages is available.
[7] Employer tax rates are 23.75% for basic wages, overtime pay and tenure-related pay and half that (11.875%) for other payments. Employers in Portugal are also required to pay their workers 14 months of wages per year: in each

magnitudes underline the importance of wage bills in the total costs faced by firms and their potential influence in biasing rent sharing estimates.

Finally, we also present statistics about the share of exports in total sales. The average share (within firms that exhibit a positive level of exports) is of 48% for workers and 41% for firms. It is this variable, the share of exports in total sales, interacted with exchange rates, that is used as an instrument for profits. The rationale for this choice is that increases (decreases) in exchange rates will make exports more (less) expensive, thus creating an exogenous impact, of a longitudinal nature, upon each firm's demand for its products. Moreover, such exchange-rate variability is likely to impact on firms differently, depending on the openness of each firm to international trade, which is captured by the share of exports in total sales of each firm.

The period and country considered in this paper are particularly well suited for this instrument. Portugal is a small open economy – and thus unable to affect the international prices of the large majority of the products the country trades and the 1993-95 period proved to be a turbulent time from the point of view of exchange rates. During these years, and particularly between 1992 and 1993, the European Monetary System witnessed large fluctuations in its Exchange Rate Mechanism currencies, following the macroeconomic imbalances created by the German reunification.

As far as the Portuguese currency is concerned, and as documented in Figure 1 in terms of the effective exchange rate, there was a substantial depreciation over the first six months of 1993, a short time after the Escudo joined the Exchange Rate Mechanism (ERM) of the European Monetary System (April 1992). A first devaluation (of 6%) occurred in September 1992, a second (of 6.5%) in May 1993 – after which the ERM bands were widened from 6% to 15% – and a third (of 3.5%) in March 1995. As the figure shows, the value of the currency, measured in effective exchange rates (i.e. weighted by trade shares), dropped considerably over 1993 and up to mid-1994. The escudo then picked up some of its value until the end of 1995.[8]

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one of two months per year, typically in June and December, two months of wages (the standard wage plus either the "Summer" or "Christmas" subsidies) are paid.

IV. Results

1. OLS Results, Pooled Data

The first set of results, presented in this subsection, focuses on key relationships between the two types of profits and wages, under different sets of control variables. At this stage, we simply pool the data for the same workers in different years. Our goal here is only to take a first look at the biases induced by controlling for net, rather than gross, profits per worker. The wage equation considered is:

(5) $$y_{it} = X_{it}\beta_1 + F_{it}\beta_2 + \beta_3\pi_{j(i,t),t} + \varepsilon_{it},$$

where y_{it} denotes the logarithm of real hourly wages of worker i in period t. X_{it} is a set of human capital variables: six dummies denoting different schooling levels, a quartic in experience, a quadratic in tenure – measured in months –, and a gender dummy. F_{it} is a set of firm characteristics: six industry dummies, three region dummies, controls for firm size – in terms of the log number of employees and log real sales, and a foreign-ownership dummy.[9] $\pi_{j(i,t),t}$ denotes (either net or gross) real profits per worker in period t at the firm (j) of worker i in period t. Finally, ε_{it} is an error term following the standard assumptions. Standard errors are corrected to take into account heteroskedasticity related to the fact that most workers are present in the sample more than one period.

The rent-sharing coefficients for net and gross profits are presented in the first two columns of Table 1, for specifications with no other regressors except for two year dummies. As expected, we find that the first coefficient (.04), for net profits, is substantially smaller than the second (.107), for gross profits. Moreover, the measure of goodness of fit indicates that gross profits play

[8] The values used for the exchange rate of each year were obtained from averaging the monthly effective exchange rates indices across each year: 103.03 in 1993, 98.08 in 1994 and 100.78 in 1995 (higher indices mean a stronger Escudo vis-à-vis the other currencies).

[9] The inclusion of firm controls in this specification is not obvious from the point of view of a simple competitive model of the labour market. From that perspective, only individual characteristics should matter in wage determination, except if firm controls captured compensating differentials. However, the inclusion of firm controls can be warranted if firm characteristics pick up some extra worker traits that affect wages and are also correlated with firm profitability. Such an extended specification will therefore amount to a more stringent test of rent sharing.

a much better role in predicting wages than net profits. The profit-elasticities of wages are also very different, at 1.4% for net profits and 22.1% for gross profits.[10] However, these two elasticities are not strictly comparable, as they refer to percentage increases of two very different types of profits: as shown before, net profits are substantially smaller than gross profits. For this reason, Lester ranges are a better way of comparing the degree of rent sharing across different specifications. Using this method, the Lester range is 23% for net profits and 65% for gross profits.

In the second pair of columns of Table 1, we replicate this analysis but adding controls for worker characteristics. We find, for both measures of profits, smaller coefficients, elasticities and Lester ranges. This means that, as expected, more skilled workers are employed in firms with higher profits. Another result is that the three measures of rent sharing remain smaller when net profits are used.

Similar results are obtained in the last pair of columns of this Table, which add controls for firm characteristics to the previous specification. Once again all measures of rent sharing decline, indicating a correlation between firm characteristics and profits, and all measures of rent sharing remain bigger under gross profits. The coefficient for net profits even becomes negative under this specification, leading to a Lester range of –3% while the corresponding figure under gross profits is 23%. These results confirm the prediction that rent sharing is underestimated and may even be wrong-signed when one does not account for the fact that, *ceteris paribus*, higher wages translate into lower profits.[11]

As a test of the robustness of the results, we also considered specifications that include one-year lags of the profit variables. This is motivated by the fact that past profitability will be pre-determined at the year under study. Moreover, wages in the 'Personnel Records' data set refer to specific months (March in 1993 and October in 1994 and 1995) while profits refer to the full year. As an additional test of robustness, and in order to facilitate the comparison of these results with those of the following subsections, we run the same regressions with the sub-sample of workers whose firms export (this is the sample considered when the instrument is used). We find,

[10] Mean elasticities in a log-level regression are given by the product of the coefficient and the mean of the regressor.
[11] Moreover, the gross profit coefficients remain significant if one considers clustering at the firm level (rather than at the worker level, as done here) while the net profit coefficients become insignificant.

under both the specification with lags and that with the exporting firms sub-sample, the same ranking and similar values for the profits coefficients as in the main regressions. (These results are not reported but are available upon request.)

2. Instrumental Variables, Pooled Data

In this sub-section, we address the simultaneity between wages and profits via a 2SLS technique. In the top part of Table 2, we present the coefficients (and standard errors) of the instrument in the auxiliary regression that predicts profits per worker.[12] In the first two columns, which refer to specifications with controls for human capital but not firm characteristics, the instrument is found to be highly significant and with the expected negative sign. This negative sign means that the greater the openness of the firm to the external market (as measured by the share of exports in total sales), the greater the (negative) impact of an increase (i.e. appreciation) of the exchange rate of the escudo upon the firm's profits, or vice-versa.

The two key indicators of instrument quality (as suggested by Bound, Jaeger and Baker, 1995) are very favourable.[13] Firstly, the partial R^2 of the instruments is considerable, at .02 for net profits and .06 for gross profits. Secondly, the F-statistic of this instrument produces extremely high values, 1,463 for net profits and 3,781 for gross profits. (The rule-of-thumb thresholds for this statistic suggested by Staiger and Stock, 1997, are between 10 and 25.) Moreover, and although these indicators of instrument quality are very good, it is clear that, as predicted, the instrument fits much better in the regression with gross profits. Both the partial R^2 statistic and the F-statistic are more than twice as big in the latter regression.

As to the main regression, we find that the profits coefficients have both increased considerably after instrumentation, but especially in the case of net profits. In this case, Lester ranges jump from 18% to 121% while for gross profits, these ranges increase from 41% to 78%. Our interpretation of this upward bias for net profits is that, since the instruments typically used in the literature act as exogenous demand shifters, they will be much better predictors of profits before

[12] As mentioned before, we use a sub-sample of exporting firms. This is due to the possible endogeneity of the exporting/not exporting decision.

the wage bill (i.e. total revenues minus the costs of non-labour inputs only) than of profits after the wage bill (i.e. total revenues minus total costs, including those of labour).

In the two remaining columns of Table 2 we replicate the 2SLS results in specifications that also include firm characteristics. We find that, in this case, the results become unstable, particularly in the net specification, where the Lester range falls to –49%. This is related to the poor performance of the instrument, as measured by the partial R-squared and the F statistic, which probably arises because the firm controls are strongly correlated with the instruments, leaving little explanatory power for the latter and decreasing the precision of the profit regressors.

3. Instrumental Variables, Firm Effects

In this sub-section, we explicitly take into account the panel nature of the data set and estimate models of the following type:

(6) $$y_{it} = X_{it}\beta_1 + F_{it}\beta_2 + \beta_3\pi_{j(i,t),t} + \lambda_{j(i,t)} + \varepsilon_{it}\,,$$

where $\lambda_{j(i,t)}$ denotes a fixed effect for the firm of worker i at period t.

The inclusion of firm fixed effects implies that any evidence of rent sharing will now be derived from within-firm differences in profits across time. This restriction is the price one pays for the benefit of accounting for any time-invariant differences across firms. Such differences may include different working practices (e.g. monitoring vs. incentives) that may impact differently the firm's profitability.

The results are presented in Table 3. In column A, when profits are not instrumented and net profits are considered, one obtains a negative rent-sharing coefficient. This result is induced by the downward bias incurred whenever net profits are used. However, this bias is not counterweighted as before by the much higher profitability of firms that pay higher wages, as estimation focus only on within-firm differences. This problem is addressed in column B, where

[13] These indicators are, however, in many cases, not available in the rent sharing literature that uses instruments. Another point is that the exclusion restriction cannot be statistically tested in our analysis as the equation is just

the consideration of gross profits renders the just-mentioned downward bias irrelevant. As expected, in this new specification, rent sharing is positive, with a Lester range of 8%.

In the following columns of Table 3, the simultaneous nature of profits is again addressed. In columns C and D, for specifications that include controls for human capital, the instrument again performs well, with the predicted negative sign, partial R^2's of .0047 and .0077 and F-statistics of 272 and 448. The rent-sharing coefficients are precisely determined, corresponding to Lester ranges of 39% and 34%, respectively. The latter values are much smaller than those obtained for the equivalent specifications of Table 2. The differences between gross and net profits also decline substantially.

In columns E and F, controls for firm characteristics are added. Unlike in the same specification of Table 2, now the instruments perform very well, with the expected signs, partial R^2's of .034 and .045 and F-statistics above 2,000 in both cases. Lester ranges are 24% and 23% for net and gross profits, respectively. We therefore find that Lester ranges fall further with respect to the specification without firm controls and the difference between net and gross profits is no longer significant.

4. Instrumental Variables, Firm-Worker Spell Effects

In this sub-section, we consider firm-worker spell fixed effects. The motivation for this analysis is that, on top of the unobservable differences across firms, the workforce of each firm may also vary in unobservable ways that are also correlated with profitability. We then estimate models of the following type, which explicitly control for such heterogeneity:

$$y_{it} = X_{it}\beta_1 + F_{it}\beta_2 + \beta_3\pi_{j(i,t),t} + \nu_{ij} + \varepsilon_{it}\,, \qquad (7)$$

where ν_{ij} denotes the worker-firm spell fixed effect, i.e. $\nu_{ij} = \theta_i + \lambda_{j(i,t)}$, in which θ_i is the worker fixed effect and $\lambda_{j(i,t)}$ is the firm fixed effect as before. Then, by mean-differencing equation (7), with respect to the spell means, one obtains:

identified (there is one instrument and one right-hand-side endogeneous variable in our wage equation).

$$(8) \qquad y_{it} - \bar{y}_s = (X_{it} - \bar{X}_s)\beta_1 + (F_{it} - \bar{F}_s)\beta_2 + \beta_3(\pi_{j(i,t),t} - \bar{\pi}_{j(i,t),s}) + (\varepsilon_{it} - \bar{\varepsilon}_s),$$

in which each barred variable represents the mean of that variable for each spell (defined as a worker-firm match) over time. Since both worker and firm heterogeneity are controlled for in this equation, the rent sharing parameter (β_3) can be estimated consistently, which was not necessarily the case in the previous sections. This result underlines the importance of drawing on panel data covering both employers and employees in order to rigorously study the labour market.[14]

The results are presented in Table 4. One finds now that non-instrumented results are positive but very small, with Lester ranges of 1% for net profits and 10% for gross profits. However, when one takes into account the remaining sources of endogeneity via the use of the instrument, in columns C and D, the coefficients and Lester ranges are substantially smaller than in other equivalent specifications. Additionally, in the most complete specification of Table 3, there are not significant differences between net and gross profits. In particular, the Lester ranges in this case are both 15%–14% with controls for human capital only and 11%–12% with controls for human capital and firms characteristics.

One possible reason for the small Lester ranges in the specifications with spell fixed effects is that the Portuguese employment law (as that of other European countries – see European Industrial Relations Observatory [2001]) makes it very difficult for employers to cut (basic) pay.[15] This feature of the labour market would generate an asymmetry in the relationship between individual wage growth (within a given firm) and profit growth (the variables considered in spell fixed effects estimation) under the case of rent sharing. Under these circumstances, when profits

[14] A related stream of research, following the seminal paper by Abowd, Kramarz and Margolis (1999), estimates worker and firm fixed effects (rather than simply controlling for them) and examines their correlation. This type of approach is not followed here as our focus lies instead on the consistent estimation of the coefficient of a time-varying variable. See Abowd, Creecy and Kramarz (2002) for a description of an updated version of this method and Andrews, Schank and Upward (2004) for a critical analysis; see also Woodcock (2003). Another related point is that since there is relatively little mobility of workers between the firms covered in the sample, our spell fixed effects specification overlaps quite closely with an alternative specification that considers only worker fixed effects.

[15] Only in a few circumstances is this allowed in Portugal. According to Article 21st, Law ("Decreto-Lei") 49,408, 24 November 1969, "The employer is forbidden to: … c) cut pay (except in the cases foreseen in the law, after authorisation from the Ministry of Employment, should the worker agree)." The exceptions referred to are when a worker is temporarily promoted to a higher hierarchy level (as a replacement, for instance) and then moved back to the original position, when the worker becomes less productive for health reasons and, in exceptional cases, when the worker agrees to take a pay cut because the firm may become bankrupt due to lack of demand and/or increasing costs.

increased wages would also increase but when profits fell wages would not fall (or, at least, not in nominal terms). This would bias downward the rent-sharing coefficient and possibly explain the very low Lester ranges documented in Table 4.[16]

To clarify this situation, we develop a simple two-period extension of the model presented in Section 2. Allowing only the wage and the shock to vary over the two periods (i.e. making the simplifying assumptions of unchanged inputs, interest rate, outside wage and bargaining power), the wages in each period i (i=1, 2), in the simple case without downward wage rigidity, would be:[17]

$$\text{(9)} \qquad \mathrm{w}_{\mathrm{i}} = x + \phi \left(\frac{\theta_i F(N,K) - rK}{\mathrm{N}} - x \right).$$

However, introducing downward rigidity, the wage in the second period would become:[18]

$$\text{(10)} \qquad \mathrm{w}_2 = x + \phi \left[\frac{\max(\theta_1, \theta_2) F(N,K) - rK}{\mathrm{N}} - x \right]$$

This implies that the change in wages over the two periods would be effectively censored at zero if the second period shock is worse than the first period shock while wages would evolve following the prediction of the simple rent sharing model only if the firm faces a better shock in the second period. Therefore $dw=\phi.[F(N,K)/N].d\theta$ if $d\theta>0$ and $dw=0$ if $d\theta<0$ (in which $dw=w_2-w_1$ and $d\theta=\theta_2-\theta_1$). Without taking this into account, one would estimate a weighted average of the two cases, which would be more biased towards zero the greater the share of firms experiencing decreasing shocks.

[16] A similar type of asymmetry may also be found in the model of implicit contracts with costless mobility described in Beaudry and DiNardo (1991). This model, in which wages increase when unemployment falls but wages do not decrease when unemployment increases, is found to fit US data better than alternative models (spot labour markets or implicit contracts with costly mobility). Another point concerns the nominal nature of the legal rigidity discussed here: we argue that inflation is not likely to make this constraint non-binding since we will show that the *real* wage increase due to rent sharing (when profits increase) can be very substantial, implying nominal wage increases much above inflation rates.

[17] Here we are considering equation (3). The results are unchanged using the other equations.

We follow Arai and Heyman [2001] and test this asymmetry hypothesis by restricting the sample to those workers whose firms exhibit positive growth of their (total nominal) profits. The results, presented in Table 4b, support our interpretation as we find much bigger profits coefficients in this case. In the most complete specification, presented in column D, the Lester range is 56%, much larger than the 12% obtained before. (Arai and Heyman [2001] obtain a similar increase with their Swedish data.) Paradoxically, the "competitive" result of Table 4, which documents little rent sharing, is, according to our model, driven by a rigidity introduced by a labour-market institution.

V. Robustness and Interpretation

There are several ways to test the robustness of the results and, in particular, the extent to which they warrant a non-competitive interpretation of the labour market. In this section, this will be pursued by controlling for extra variables and by considering different dependent variables. Moreover, even if rent sharing can explain these results, other models than bargaining may also be consistent with these findings. In order to shed light on this matter, we will also examine differences in rent sharing between groups of workers with possibly different levels of bargaining power.

As before, one may consider different specifications and sample definitions. In this section, we follow that of Table 4b, column D, which we believe is the most appropriate, for the reasons explained before. This specification includes controls for human capital and firm characteristics, firm/worker spell fixed effects and instrumented gross profits, while the sample is restricted to workers whose firm's total nominal profits increase over adjacent years.[19]

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[18] We disregard the more complex outcomes that would arise if we allowed for intertemporal bargaining. Such type of bargaining may face important commitment and enforceability problems, compounded by the finite or even short-run horizons of unions and managers.

[19] All regressions presented next were also conducted dropping this sample restriction. Consistently with the previous results, the Lester ranges in these cases – available upon request – were always below their counterparts obtained in this section but with the same qualitative results. An additional robustness test consisted in deriving the amount of measurement error required to justify the difference between the OLS, panel and IV results reported. The results (again available upon request) were also consistent with the findings.

1. Controls for Industry-Year Interactions and Capital Intensity

Up until now, we have assumed that economic shocks hit different industries homogeneously across time. However, our evidence of rent sharing may be driven by the employment and wage adjustments of firms in different industries to economic shocks, if they face positively sloped short-run labour supply curves. This could generate a spurious correlation between wages and profits, even though the most complete specifications already include a control for firm size.

We therefore allow economic shocks to impact on different industries differently across the period covered, by including interactions between industry and year dummies in our specification. The first column of Table 5a shows that this has no sizeable impact on estimated profits coefficients or Lester ranges – these are now 65%, compared to 56% before. We also control for changes in employment (either total or percentage differences) in the pooled specifications of equation (4) and again find that the coefficients are qualitatively unchanged.[20]

Another potential objection to a rent-sharing interpretation of these findings lies on the lack of controls for capital intensity. As Bronars and Famulari [2001] argue, capital-intensive firms will hire workers with greater observed and unobserved skills if capital and skilled labour are complements. If the regressions include no control for different degrees of capital intensity across firms, rent-sharing coefficients may simply be picking up the impact of higher unobserved ability.

Although our estimates already control for time-invariant individual/firm-specific unobserved factors, it is possible that capital intensity has a relevant time-variant dimension. We test for this by controlling for the equity level of each firm, which is probably the best proxy for capital available in our data. We find that this variable (used alternatively in per worker and real terms) enters the regression significantly (and positively), but the rent-sharing coefficient and the Lester range remain approximately unchanged at .093 and 59%, respectively.[21]

[20] The results are available upon request. We do not include controls for changes in employment in specifications with fixed effects given that in these cases the latter coefficients would be about the curvature of labour supply schedule and not its slope, which is the parameter of interest.
[21] We also consider a different proxy for capital intensity, the ratio of non-wage costs (including capital costs but also costs of other inputs except labour) to wage costs. This proxy is possibly subject to more noise, as non-capital and

2. Overtime Pay and "Other Payments"

When faced with positive demand shocks, firms may respond not by hiring more staff but by introducing or increasing overtime. Under the latter cases, the new hourly wage rate of each worker will necessarily increase, as the overtime rate must necessarily be higher than the rate for standard hours of work. This wage gap between normal and overtime work may then drive the evidence of rent sharing documented in this and other papers that consider total pay (that is, basic plus overtime pay) as the dependent variable.[22] Of course, it may also be argued that, even if the higher wages paid were driven uniquely by the steeper wage schedule for overtime hours, the subsequent overall wage increase would still reflect the employer's preference for overtime instead of alternative options, including hiring additional workers at the going (standard hours) rate. To that extent, overtime itself may not necessarily be considered as rent sharing.

Having underlined this caveat, this sub-section tests the "overtime hypothesis" – that rent sharing is simply due to the necessarily higher wage rates when overtime is used – by not considering overtime pay and overtime hours in our dependent variable. By running the same specifications as before with this new dependent variable, we find an economically negligible fall in rent sharing, from 56% to 55% (see the third column of Table 5a), indicating that overtime is not responsible for our evidence of rent sharing. However, if we focus instead on basic wages only (removing the remaining pay categories – subsidies, tenure-related pay and "other payments"), we find a substantial decline of rent sharing, to 22% (see the fourth column). This new figure is however significant and still indicates a sizeable amount of rent sharing in the labour market.

We found that the pay component most responsible for this large decline is "other payments", a residual category whose main component is precisely profit-sharing payments. Indeed, when

non-labour costs may differ considerably across firms. In any case, we again find rent-sharing coefficients of a similar magnitude, sign and significance. The results are not reported but are available upon request.

[22] The only study we are aware of that looks at differences between normal and total pay is Fakhfakh and FitzRoy (2004). Using French cross-section data and basic wages, these authors also find significant levels of rent sharing, although of a very low magnitude. Their elasticities are 1.4% using basic wages and about twice as much using total wages. However, they do not account for the many possible sources of endogeneity of profits. These factors may influence the different estimates of rent sharing obtained for total and basic wages if they are correlated with

taking hourly "other payments" as our single dependent variable, we find extremely large Lester ranges of 194% (workers in firms with increasing profits) and 197% (all workers) – see Table 5b. These figures should however be considered taking into account that "other payments" are a relatively small share of basic pay.[23]

These results are also consistent with our views of the implications of downward wage rigidity. Since employers are aware of these restrictions, which affect basic pay but not other pay categories, they will have an incentive to direct most rent sharing to flexible pay categories. And since pay in these categories can either increase or decrease between periods,[24] we no longer expect an asymmetry between the subset of firms with expanding profits and the entire set of firms when focusing our analysis on "other payments" only. Indeed, we find remarkably similar Lester ranges for each group of firms.

Overall, we conclude from this analysis that flexible forms of rent sharing are much more responsive than alternative forms that preclude downward adjustments. The results also highlight the importance of the time dimension in rent sharing, as the overall benefit workers may obtain from rent sharing will depend considerably on the reversibility of such payments.

3. Tenure, Education and Gender Differences

The motivation for the tests presented in this sub-section lies on evaluating bargaining interpretations of rent sharing in opposition to those stemming from efficiency wages or fairness models. For instance, if highly educated workers have a stronger bargaining power in firms (because they are less easily replaceable, for instance), they would also presumably benefit more from any rent-sharing agreements than other groups of workers. This prediction will not however hold in other models also consistent with rent sharing, such as fairness models.

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overtime. For instance, high-ability workers may be more likely to engage in overtime than their low ability colleagues.

[23] Basic pay corresponds to an average of 475 escudos per hour while other payments are only 70 escudos per hour. Moreover, hourly overtime pay averages at 1,200 escudos per hour but affects only about 10% of the workforce: the remaining 90% do not work overtime. On the other hand, about 80% of workers receive "other payments".

[24] The only constraint in this case is, of course, that "other payments" must be nonnegative. Another approach to this result, under the framework of the literature on incentives (see Oyer, 2004), is to observe that the downward wage rigidity constraints serve as limits to the degree that workers may insure their firms.

We conduct the tests by splitting the sample between high- and low-educated workers, high- and low-tenure workers, and men and women, and then running different wage regressions for each group of workers. (Again, the specification and sample considered is that of column D of Table 4b.) The thresholds we choose when splitting the samples into different subgroups are more/less than 36 months of tenure (high/low tenure)[25] and completion or not of secondary school (high/low education).

The results, presented in Table 5c, indicate not only sizeable differences in rent sharing between the different groups but also rankings of rent sharing levels as predicted by bargaining models. High-tenure workers have significant levels of rent sharing, with a Lester range of 59%, while rent sharing is not significant for the low-tenure workers (although the point estimate suggests a sizeable, but in any case lower, Lester range of 38%). Differences between workers with different schooling attainment are even starker. While the Lester range for highly-educated workers is 110%, the same range for low-education workers is less than half that value (52%).

Finally, rent sharing is also very different for men and women. The Lester range for the former group is 83% while that for the latter is only 15% (and not significant). This finding suggests that (differences in) bargaining power may explain a large share of gender wage discrimination. Black and Strahan (2001) find similar evidence of rents being shared mostly with men when studying the wage impacts of the deregulation of the US banking industry.[26]

VI. Conclusions

It is known that the estimation of rent-sharing may suffer from a large number of biases. This paper tackles this problem in three complementary ways, drawing on a large Portuguese matched employer-employee panel. Firstly, we control directly for the fact that firms that share more rents will, *ceteris paribus*, have lower net profits. Secondly, we instrument profits via interactions between exchange rates and the share of exports in total sales of firms. Finally, we control for

[25] The threshold was chosen in order to reflect the standard duration of probationary contracts, i.e. the standard time period after which, according to Portuguese employment law, workers in temporary contracts must either be transferred to permanent positions or be dismissed.
[26] See also Nekby [2003] and the references therein.

additional sources of heterogeneity, by considering specifications with firm or worker/firm spell fixed effects.

A first finding concerns the use of gross profits (i.e. profits before the payment of the wage bill but after all other production costs). Not only do we confirm the anticipated downward bias brought by the accounting relationship mentioned above, but we also find that the difference between the two regressors fades away as more controls (including worker or spell fixed effects) are considered.

We also find that estimates using instrumental variables tend to overestimate the amount of rent sharing if controls for firm or worker characteristics are missing. This upward bias also occurs if the measure of rent sharing used (typically net profits) is less correlated with the instrument than our alternative measure (gross profits). This is consistent with the result that weak instruments may do more harm than good and that the use of extra controls strengthens the role of the instruments in identifying the equation. Moreover, these results confirm the suspicions of Oswald [1996], who regarded some IV estimates as too large to be believable. In Abowd and Lemieux [1993], for instance, Lester ranges (a measure of wage dispersion between firms with "high" and "low" profits) increase by about ten times from OLS to IV, leading to a particularly large role of profits differences across firms in explaining wage inequality.

Overall, we find in our data evidence of a significant and substantial amount of rent sharing. In our preferred specification, we find a Lester range of 56%. This figure generally exceeds those of the few other studies that use matched worker-firm panels (Margolis and Salvanes, 2001, Arai, 2003, and Kramarz, 2003). A factor that may have magnified our results is the small number of years covered by our panel, as it is possible that the strong evidence of rent sharing documented here fades away in a longer time period. In such a time frame, workers are more likely to move between firms, competing away the rents earned by workers in firms experiencing positive demand shocks.

However, while this argument may carry some weight, our evidence of rent sharing is shown to be robust to a number of other competitive interpretations, such as industry-specific shocks, differences in capital intensity and the use of overtime. Our estimates also take into account the downward bias induced by downward rigidity in pay, as determined by employment law, which we show can be an additional driver of the underestimation of rent sharing. Moreover, we find that groups of workers one may expect to have more bargaining power inside the firm (workers with high levels of tenure or education and men) benefit more from rent sharing. These results represent further support to the case of rent sharing and, in particular, its bargaining interpretation.

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Appendix 1 – The Data Set

A. The 'Quadros de Pessoal' (QP) Data Set

The QP data set is an employer-based survey of both firm and employee characteristics, which has been covering the Portuguese economy since 1982 on an annual basis. This survey is run by the Ministry of Employment, based on a law that makes it compulsory for every Portuguese firm with at least one employee to hand out the required data. One key goal underpinning the introduction of this survey was to allow the Ministry of Employment to check the compliance of different aspects of Portuguese labour law.

These data include an extensive set of characteristics concerning the firm, the establishment (if relevant) and all the firm's employees. Moreover, since the mid-1980's an individual identifier (which is derived from the worker's National Insurance number) and a firm identifier are also available. These identifiers allow one to follow workers and firms over time, provided the former remain as employees (i.e. do not leave the labour market or become self-employed, for instance). Furthermore, each set of characteristics of each individual includes a reference to the firm for which the individual is working in each year, allowing the two dimensions of the data set to be fully matched.

The fact that the forms prepared by the Ministry of Employment are filled in by the employers should guarantee a high degree of quality and comparability of the data. Furthermore, the record or table for each firm or establishment, with information on each worker (most notably his or her pay and number of hours of work) is to be displayed in a public place at each establishment. This requirement allows the Ministry of Employment to check whether labour regulations (e.g., irregular extra time) are being met. This requisite should ensure a further layer of quality to the data set.

The annual samples used in this study concern the manufacturing industries and were subject to a sampling ratio of about 80%, where large firms are over-represented. For instance, in 1995, the universe of the manufacturing sector considers 845,000 workers and 37,500 firms while the sample includes information on approximately 677,000 workers and 12,800 firms. This corresponds to a ratio of 80% of workers but only 34% of firms.

B. The 'Jornal de Noticias' (JN) Survey

This survey is published annually by 'Jornal de Notícias', a leading Portuguese newspaper. The survey presents business information about the top 500 firms (in 1993) or 1,000 firms (in 1994 and 1995) located in the Northern part of Portugal. (Several firms in the 1994 survey do not present information on profits, which lead to their elimination.) The ranking of each firm was determined by their sales volume. The variables included during the period 1993-1995 are sales volume, equity, number of workers, exports, accounting variables and financial ratios and lagged values of some of these variables. We considered only firms in the manufacturing sector.

C. Matching the Two Data Sources

Two variables available in both sources (QP and JN) were strictly comparable and thus used directly in the merging process. These variables were the geographical location of the firm and its industry code. Other variables available in both sources (employment, sales and equity) were subject to some measurement error and thus had to be considered more carefully.

The source of such measurement error is related to the different time of the year during which the data is collected for each one of the two sources. While the QP data are about March (in 1993) and October (in 1994 and 1995), the 'Jornal de Notícias' refers to the full year and is thus likely to represent the characteristics of the firm by 31st December of each year.

The possible number of matches between firms in each data set (after restricting it to firms sharing a given geographical location and industry code) was 49,591. An algorithm for selecting a smaller number of possible matches was then implemented. This procedure borrowed from matching theory and in particular the 'Deferred Acceptance' algorithm by Gale and Shapley [1962]. Our version of this algorithm involved creating a loss function defined in terms of the weighted differences between the values of each one of the latter three matching variables (employment, sales and equity) across a maximum of three years in each data set. This loss function was then used to evaluate all possible matches. (An alternative and more standard approach, in particular for larger data sets, can be found in Fellegi and Sunter, 1969.)

From these results, we determined the best match for each firm in the JN data set. QP firms would then be grouped with the best match within the set of choices available. Paired firms would be selected and removed from the sample. This process would then be replicated until all high-quality matches would be found. These matches were selected as those above a threshold in terms of the quality of the match as determined by a maximum value of the loss function. The set of firms obtained from this process was then subject to a new round of inspection and elimination, as subjectively determined by the author. Firms-year for which relevant information from either one of the two data sources was missing were also dropped, after which the final sample of 91 firms and 197 firms-year used in the paper was obtained.

Appendix 2 - Descriptive Statistics

Table A1 - Descriptive Statistics, 1993-1995, Workers

	Obs.	Mean	C.V.	Min.	Max.
Schooling	75565	5.40	51.2%	0	16
Experience	75565	26.31	45.5%	0	77
Tenure	75565	17.03	67.9%	0	76.1
Female	75565	0.41		0	1
Log Hourly Wages	75565	6.20	8.3%	4.65	9.16
Log Firm Size	75565	6.56	14.0%	3.53	8.00
Log Sales	75565	8.89	10.2%	7.00	10.61
Foreign Firm	75565	0.13		0	1
Net Profits (Per Worker)	75565	0.33	398.3%	-3.54	26.24
Gross Profits (Per Worker)	75565	2.07	73.7%	-1.47	27.58
Share of Exports	57531	0.48	69.1%	0.00	1.00

Notes:
All monetary variables are in 1993 prices.
The share of exports refers to total sales.
Tenure is measured in months divided by 10.
C.V. denotes the coefficient of variation.

Table A2 - Descriptive Statistics, 1993-1995, Firms

	Obs.	Mean	C.V.	Min.	Max.
Schooling	197	5.81	23.5%	1.37	12.21
Experience	197	24.52	22.8%	12.85	36.72
Tenure	197	14.29	40.7%	2.85	28.46
Female	197	0.37		0.03	0.98
Log Hourly Wages	197	6.24	5.2%	5.76	7.51
Log Firm Size	197	5.61	17.7%	3.53	8.00
Log Sales	197	8.28	10.0%	7.00	10.61
Foreign Firm	197	0.14		0	1
Net Profits (Per Worker)	197	0.59	383.8%	-3.54	26.24
Gross Profits (Per Worker)	197	2.39	98.7%	-1.47	27.58
Share of Exports	141	0.41	86.2%	0.00	1.00

Tables and Figures

Table 1 - OLS results, Pooled data
Dependent variable: Log hourly wages.

	A	B	C	D	E	F
Net Profits	0.040		0.031		-0.005	
	[0.002]**		[0.002]**		[0.002]**	
Gross Profits		0.107		0.068		0.038
		[0.003]**		[0.002]**		[0.002]**
Controls:						
Human capital	No		Yes		Yes	
Firm characteristics	No		No		Yes	
Firm dummies	No		No		No	
Worker dummies	No		No		No	
Observations	75565	75565	75565	75565	75565	75565
Adjusted R-squared	0.012	0.100	0.421	0.453	0.468	0.475
Elasticity	0.014	0.221	0.011	0.140	-0.002	0.078
Lester range	22.7%	65.1%	17.6%	41.4%	-2.8%	23.1%

Notes:
Robust standard errors in brackets (using Stata's "cluster" option, at the worker level).
+ significant at 10%; * significant at 5%; ** significant at 1%
'Human capital' controls are six dummies for education levels, a quartic in (Mincer) experience, a quadratic in tenure, and a gender dummy. 'Firm characteristics' are seven dummies for industries, log number of workers, log real sales, a foreign firm dummy, and three region dummies. Two dummies for years are also included in all specifications.

Table 2 - 2SLS results, Pooled data
Dependent variable: Log hourly wages.

	A	B	C	D
Auxilliary regression				
Instrument	-0.0069	-0.0122	0.0021	-0.0007
	[0.0002]**	[0.0002]**	[0.0002]**	[0.0003]**
Partial R2	0.0248	0.0617	0.0013	0.0001
F-statistic	1463.8	3780.85	76.19	7.35
Main regression				
Net Profits	0.215		-0.086	
	[0.009]**		[0.037]*	
Gross Profits		0.122		0.266
		[0.004]**		[0.128]*
Controls:				
Human capital	Yes		Yes	
Firm characteristics	No		Yes	
Firm dummies	No		No	
Worker dummies	No		No	
Observations	57531	57531	57531	57531
Elasticity	0.078	0.254	-0.031	0.553
Lester range	121.3%	78.0%	-48.5%	170.1%

Notes:
Robust standard errors in brackets (GMM estimator)
+ significant at 10%; * significant at 5%; ** significant at 1%
'Human capital' controls are six dummies for education levels, a quartic in (Mincer) experience, a quadratic in tenure, and a gender dummy. 'Firm characteristics' are seven dummies for industries, log number of workers, log real sales, a foreign firm dummy, and three region dummies.
Two dummies for years are also included in all specifications.
The instrument used is the interaction between exchange rates and the shares of exports in total sales.

Table 3 - 2SLS results, Firm fixed effects
Dependent variable: Log hourly wages.

	A	B	C	D	E	F
Auxilliary regression						
Instrument			-0.010	-0.013	-0.016	-0.019
			[0.0006]**	[0.0006]**	[0.0004]**	[0.0004]**
Partial R2			0.0047	0.0077	0.034	0.0447
F-statistic			271.52	448.14	2024.35	2690.33
Main regression						
Net Profits	-0.002		0.069		0.043	
	[0.001]+		[0.021]**		[0.013]**	
Gross Profits		0.013		0.053		0.036
		[0.002]**		[0.016]**		[0.011]**
Controls:						
Human capital	Yes		Yes		Yes	
Firm characteristics	No		No		Yes	
Firm dummies	Yes		Yes		Yes	
Worker dummies	No		No		No	
Observations	75565	75565	57531	57531	57531	57531
Elasticity	-0.001	0.027	0.025	0.110	0.016	0.075
Lester range	-1.1%	7.9%	38.9%	33.9%	24.3%	23.0%

Notes:
Robust standard errors in brackets (GMM estimator). + significant at 10%; * significant at 5%; ** significant at 1%
'Human capital' controls are six dummies for education levels, a quartic in (Mincer) experience,
a quadratic in tenure, and a gender dummy. 'Firm characteristics' are seven dummies for
industries, log number of workers, log real sales, a foreign firm dummy, and three region dummies.
Two dummies for years are also included in all specifications.
The instrument used is the interaction between exchange rates and the shares of exports in total sales.

Table 4 - 2SLS results, Spell fixed effects
Dependent variable: Log hourly wages.

	A	B	C	D	E	F
Auxilliary regression						
Instrument			-0,01	-0,012	-0,017	-0,018
			[0.001]**	[0.001]**	[0.001]**	[0.001]**
Main regression						
Net Profits	0.002		0,026		0,02	
	[0.001]		[0.018]		[0.012]+	
Gross Profits		0.016		0,022		0,019
		[0.001]**		[0.015]		[0.010]+
Controls:						
Human capital	Yes		Yes		Yes	
Firm characteristics	No		No		Yes	
Firm dummies	No		No		No	
Worker dummies	Yes		Yes		Yes	
Observations	75565	75565	57531	57531	57531	57531
Workers	41918	41918	35767	35767	35767	35810
Elasticity	0,001	0,033	0,009	0,046	0,007	0,039
Lester range	1,1%	9,7%	14,7%	14,1%	11,3%	12,2%

Notes:
Robust standard errors in brackets (GMM estimator).
+ significant at 10%; * significant at 5%; ** significant at 1%
'Human capital' controls are six dummies for education levels, a quartic in (Mincer) experience,
a quadratic in tenure, and a gender dummy. 'Firm characteristics' are seven dummies for
industries, log number of workers, log real sales, a foreign firm dummy, and three region dummies.
Two dummies for years are also included in all specifications.
The instrument used is the interaction between exchange rates and the shares of exports in
total sales.

Table 4b - 2SLS results, Spell fixed effects, Subsample
Dependent variable: Log hourly wages.
Sample: workers in firms whose total nominal total profits increased.

	A	B	C	D
Auxilliary regression				
Instrument	-0,029	-0,05	-0,039	-0,061
	[0.002]**	[0.002]**	[0.001]**	[0.001]**
Main regression				
Net Profits	0.015		0.142	
	[0.002]**		[0.014]**	
Gross Profits		0.044		0.089
		[0.002]**		[0.009]**
Controls:				
Human capital	Yes		Yes	
Firm characteristics	No		Yes	
Firm dummies	No		No	
Worker dummies	Yes		Yes	
Observations	45444	45444	35606	35606
Workers	22892	22892	21543	21543
R-squared	0.021	0.037		
Elasticity	0,004	0,090	0,043	0,184
Lester range	7,4%	26,4%	74,0%	56,3%

Notes:

Robust standard errors in brackets (GMM estimator).

+ significant at 10%; * significant at 5%; ** significant at 1%
'Human capital' controls are six dummies for education levels, a quartic in (Mincer) experience, a quadratic in tenure, and a gender dummy. 'Firm characteristics' are seven dummies for industries, log number of workers, log real sales, a foreign firm dummy, and three region dummies.

Two dummies for years are also included in all specifications.
The instrument used is the interaction between exchange rates and the shares of exports in total sales.

Table 5a - 2SLS results, Spell fixed effects, Subsample

Dependent variable: Log hourly wages.

Sample: workers in firms whose total nominal total profits increased.

	Interactions Year-Industry	Capital per worker	All wages except Overtime	Basic wages only
Gross Profits	0,103	0,093	0,087	0,035
	[0.011]**	[0.009]**	[0.009]**	[0.008]**
Observations	35606	35606	35526	35528
Workers	21543	21543	21519	21521
Elasticity	0,213	0,192	0,180	0,072
Lester range	65,1%	58,8%	55,0%	22,1%

Notes:

Robust standard errors in brackets (GMM estimator). + significant at 10%; * significant at 5%; ** significant at 1%

Human capital and firm characteristics controls and two year dummies are included in all specifications.

The instrument used is the interaction between exchange rates and the shares of exports in total sales.

Table 5b - 2SLS results, Spell fixed effects
Dep. Variable: Log hourly "other payments"

	Workers with increasing profits	All workers
Gross Profits	0.307	0.331
	[0.026]**	[0.031]**
Observations	26402	41782
Workers	17878	27769
Elasticity	0.634	0.691
Lester range	194.0%	197.2%

Notes:

Same as in the previous Table.
"Other payments" is the wage category that includes profit-related pay.
"Workers with increasing profits" denotes workers in firms whose total nominal profits increased.

Table 5c - 2SLS results, Spell fixed effects, Subsample

Dependent variable: Log hourly wages.

Sample: workers in firms whose total nominal total profits increased.

	Tenure		Education		Gender	
	High	**Low**	**High**	**Low**	**Male**	**Female**
Gross Profits	0,092	0,071	0,141	0,085	0,121	0,028
	[0.009]**	[0.061]	[0.028]**	[0.009]**	[0.009]**	[0.018]
Observations	32122	3484	2806	32800	20282	15324
Workers	19887	2611	1875	19844	13218	9761
Elasticity	0,191	0,143	0,381	0,171	0,261	0,055
Lester range	59,1%	37,7%	110,1%	52,1%	83,4%	15,2%

Notes:

Same as in the previous Tables.

High tenure is defined as more than 36 months. High education is defined as at least 11 years of schooling.

Figure - Escudo's Nominal Effective Exchange Rate, 1990-1996

Source: Bank of Portugal; Base: 1987.