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ABSTRACT

Bargaining and the Gender Wage Gap: A Direct Assessment*

An influential recent literature argues that women are less likely to initiate bargaining with their employers and are (often) less effective negotiators than men. We use longitudinal wage data from Portugal, matched to balance sheet information on employers, to measure the relative bargaining power of men and women and assess the impact of the gender gap in bargaining strength on the male-female wage gap. We show that a model with additive fixed effects for workers and *gender-specific* fixed effects for firms provides a close approximation to the wage structure for both men and women. Building on this model we present three complementary approaches to identifying the impact of differential bargaining strength. First, we perform a simple decomposition by assigning the firm-specific wage premiums for one gender to the other. Second, we relate the wage premiums for men and women to measures of employer profitability. Third, we show that changes in firm-specific profitability have a smaller effect on the wage growth of female than male employees. All three approaches suggest that women are paid only 85-90% of the premiums that men earn at more profitable firms. Overall, we estimate that the shortfall in women's relative bargaining power explains around 3 percentage points – or 10-15% – of the gender wage gap in Portugal.

JEL Classification: J16, J31, J71

Keywords: wage differentials, discrimination, gender, linked employer-employee data

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Despite efforts to outlaw gender discrimination and the closing of measured skill gaps between the sexes, women in virtually every country continue to earn less than men (Altonji and Blank, 1999; Blau and Kahn, 2000; OECD, 2012). The stubborn persistence of the gender wage gap has led researchers to shift attention away from traditional factors such as discrimination toward more subtle explanations based on psychological attributes and bargaining behavior. An important strand of this new research, building on evidence from both lab experiments (Bowles et al., 2005, Bowles et al., 2007; Small et al. 2007; Rigdon, 2012) and field settings (Babcock et al. 2006; Save-Soderbergh, 2007; Castillo et al., 2012), argues that women are less likely to initiate bargaining and are (often) less effective negotiators than men.[1]

Although this work makes a credible case that women and men differ in their willingness and ability to bargain over wages, the quantitative impact of these differences on the overall gender wage gap is unclear. We provide some of the first systematic evidence on this question using rich administrative data from Portugal that combines detailed information on hourly wages with balance sheet data for employers.[2] The recent negotiation literature asserts that women obtain a smaller share of the match surplus associated with their employment relationships than do men. A fundamental obstacle to assessing this hypothesis is that the surplus in any given match is unobserved, making it difficult to infer whether women are in worse matches than men or have lower bargaining power. A growing body of research, however, suggests there is a large component of wage variation associated with the sharing of firm-wide rents.[3] Building on this literature, we consider several approaches to assessing

[1]See Bertrand (2011) for a review. Related bodies of work suggest that women are less competitive and less confident than men, and as a result tend to choose educational tracks, occupations, and jobs that are associated with lower wages, e.g., Gneezy et al (2003), Niederle and Vesterlund (2007), and Buser et al. (2013).

[2]An earlier study by Nekby (2003) relates male and female wages to measured profitability in a cross section of Swedish firms, but does not address the potential selectivity issues caused by non-random sorting of men and women with different unobserved skill characteristics to more profitable firms.

[3]Modern empirical research on this topic starts with the seminal contribution of Abowd, Kramarz, and Margolis, 1999. See also Lentz and Mortensen (2010), Barth et al. (2012), and Moscarini and Postel-Vinay (2012). Card, Heining and Kline (2013) present an overview of recent studies and show that widening firm-specific wage differentials play an important role in the rise in wage inequality in West Germany.

the effect of gender differences in bargaining that exploit the matched longitudinal features of our data.

First, we fit gender-specific models of wage determination with fixed effects for workers and firms. Building on techniques developed in Card, Heining, and Kline (2013), we present a series of specification checks indicating that the additive worker-firm model provides a good approximation to the wage structure for each gender. Assuming that the firm effects are proportional to the shares of firm-wide rents received by men and women, simple comparisons of these effects identify the relative bargaining power of men and women. We show that, given an appropriate normalization on the firm effects, it is possible to decompose the gender wage gap into a component due to differential bargaining power and a component due to differential sorting of men and women to firms. We find that women capture a smaller fraction of firm-wide rents than men, and that this difference in bargaining power explains about 3 percentage points (or 10-15%) of the gender wage gap. A larger share (around 20%) of the wage gap is explained by the fact that women tend to be employed at lower-rent firms.

This simple decomposition ignores factors other than rent-sharing that may also contribute to firm-specific wage premiums (such as compensating differentials for working conditions). In a second approach, we directly estimate the relationship between the gender-specific wage premiums paid by a firm and a simple measure of firm profitability: value added per worker. We find that the estimated wage premiums for both men and women are strongly correlated with value added per worker, with a smaller effect for women, implying that women benefit less from working at more profitable firms. The estimated differences in bargaining strength imply a similar 3 percentage point impact on the overall gender wage gap.

In a third design, we examine the effects of changes in firm-specific profitability (measured by changes in value added per worker) on the wage changes of men and women who remain with the firm over a multi-year period. This approach, which mirrors the research design employed by much of the modern rent-sharing literature (e.g., van Reenen, 1996; Carlsson, Messina, and Skans, 2011; Card, Devicienti and Maida, 2013), has the advantage of not relying on the exogenous mobility assumptions underlying the standard worker-firm decomposition. To the extent that firms insure workers against transitory shocks to profitability (Guiso et al., 2005) this design may yield attenuated estimates of rent sharing over the relatively short time horizon available in our data. Despite some evidence of such attenuation, we again find that women's wages are less responsive than men's to changes in the profitability of their employer, with approximately the same ratio of implied gender specific bargaining power as found in the previous two designs.

The recent negotiation literature has argued that gender differences in the conduct of bargaining may have a bigger impact in certain environments – for example, in settings where there is more situational ambiguity (Bowles et al., 2005) or less information about other workers' wage outcomes (Rigdon, 2012). We compare the relative effect of changes in profitability on men's and women's wages in different industries, and between larger and smaller firms and those with different shares of female employees. We also contrast firms with higher and lower dispersion in male wages. We find some evidence that women's relative bargaining power is smaller in the manufacturing sector, and at larger firms with a greater share of female workers.

We conclude that, in the aggregate, differences in relative bargaining power explain about 10-15% of the gender wage gap in Portugal. Our findings contribute to a growing literature that emphasizes the fundamental role of firms in the wage determination process (Lentz and Mortensen, 2010). An open question for future research is the extent to which differences in the average wage premiums paid to men and women by the same firm are due to differences in the behavior of female employees, versus differences in how employers interact with their female workers.

1 Institutional Setting and Data Overview

Our analysis relies on a unique matched employer-employee data set from Portugal, containing high-quality data on worker's wages and firm profitability. Three features of the Portuguese labor market suggest to us that our findings may be broadly generalizable to other settings. First, women in Portugal have relatively high labor force participation rates, comparable to the rates in the U.S. and Northern Europe – e.g., 58% for all adult women in 2010 (ILO, 2012). The participation rate of women between the ages of 25 and 45 is particularly high (over 85%) reflecting the strong commitment to work among recent cohorts of women (INE, 2013). Second, the vast majority of women in Portugal (over 90% of those in private sector jobs) work full time, suggesting that wage comparisons between men and women are unlikely to be driven by differences between full time and part time jobs. Third, the gender wage gap in Portugal is within a few percentage points of the gaps in the U.S. and U.K., and very close to the OECD-wide average.[4]

An important institutional fact about the Portuguese labor market is that nearly all jobs are covered by collective bargaining contracts. Indeed, until very recently the contracts negotiated between major employer groups and trade unions were automatically extended to all workplaces in the same industry and region. Since these contracts are gender-neutral (i.e., they set wages for jobs regardless of gender), they may have some equalizing impact on the relative pay of women in Portugal. As shown in Cardoso and Portugal (2005), however, both male and female workers typically earn a significant wage premium over their base pay rate (a so-called "wage cushion"). Moreover, employers have wide latitude in assigning workers to job categories, and in promoting workers to higher categories. We suspect that the combination of these features lessens the impact of collective bargaining on the gender pay gap and leaves substantial room for factors like willingness to negotiate and bargaining skill to affect wages.

[4]The OECD "Family database" shows the gender gap in median full time earnings was 16% in Portugal, 19% in the U.S., and 16% on average across 26 OECD countries (OECD, 2012).

1.1 Data Sources

Our wage data are derived from the Quadros de Pessoal (QP), an annual census of private sector employees conducted by the Portuguese Ministry of Employment. Firms with at least one paid employee are required to submit information on their full workforce as of the survey reference week (in October). Government employees and individuals working as independent contractors are excluded from coverage.[5] There is also no information on people who are unemployed or out of the labor force during the reference week. Over our 8-year sample period from 2002 to 2009, the QP contains 20 million observations on 4.5 million different individuals.

The QP asks firms to report each employee's gender, date of birth, occupation, education, date of hire, regular monthly salary, other wage payments, and hours of work. Information is also collected on the industry, location, and founding date of the firm, shareholder equity, and gross sales (in the preceding calendar year). We construct an hourly wage by dividing a worker's salary and regular earnings supplements by his or her normal hours of work. The ability to measure a point-in-time hourly wage is a unique strength of the QP and substantially reduces the potential impact of differential hours of work between men and women on the gender wage gap.[6] We deflate all nominal quantities using the Consumer Price Index (2009=100).

We augment the wage information in QP with firm-specific balance sheet information for non-banking firms from Bureau van Dijk's "SABI"(Sistema de Analisis de Balances Ibericos) database. Businesses in Portugal are required to file balance sheet information and profit and loss statements each year.[7] These data are publicly available and are collected by various agencies and compiled by Bureau van Dijk into the SABI database. Information in SABI

[5] Firm owners are included in the data set but do not report wages, and so are excluded from our analysis.

[6] Differences in hours of work between men and women play a major role in explaining earnings differences in the U.S., particularly among the highly skilled. Wood, Corcoran and Courant (1993) and Bertrand, Goldin and Katz (2010) find important hours gaps between male and female lawyers and MBA graduates, respectively.

[7] These are filed with the Conservatoria do Registro Comercial. The same agency also keeps track of changes in ownership and organizational structure of firms. Based on informal discussions with firm owners we believe the penalties for non-filing are small, possibly accounting for missing data for many firms.

includes the firm's name, address, industry, and founding date, as well as various balance sheet entries (including sales and the value of intermediate inputs) and total employment. SABI data are available from 2000 onward, but coverage of the database widened substantially in 2005 and information on employment is only relatively complete after 2006.

Since the QP does not include firm names (or tax identification codes) that would permit a direct match with SABI, we use a combination of variables that are common to both data sets to conduct a "fuzzy" match. Specifically, we use detailed location, industry, firm creation date, annual sales and end-of-year shareholder capital as matching variables. As described in the Data Appendix, we successfully matched about 53% of firms that appear in the QP between 2002 and 2009 to a firm with at least one year's information in SABI. More information on our matching procedure and the match rates for various subgroups is presented in the Data Appendix. The match rate is relatively constant for firms with between 11 and 500 employees (around 68%), but lower for firms with 10 or fewer workers (51%) and for the largest firms (over 500 workers) many of which are in the banking sector. Overall, we have current year employer financial data for about 66% of the person-year observations in our QP sample from the 2006-2009 sub-period when information in SABI is relatively complete.

1.2 Analysis Samples

Our primary analysis sample consists of person-year observations for individuals who are between 19 and 65 years of age, have more than one year of potential labor market experience, and are employed as a paid worker in the reference period. After excluding data for individuals with inconsistent employment histories (see the Data Appendix) we arrive at a data set containing 16.3 million person-year observations on 3.9 million workers – 2.1 million men and 1.7 million women – described in columns 1 and 2 of Table 1. As noted in Table A1 of the Data Appendix the characteristics of this sample are very similar to the characteristics of the full QP sample.

A comparison between columns 1 and 2 in Table 1 shows that female workers in Portugal are slightly younger than their male counterparts (rows 1-3) but better educated (rows 4-8). Despite the education advantage women earn about 18% less per hour (row 9) – very similar to the gender gap in median hourly wages in the U.S. in 2007 (EPI, 2010). Women also work slightly fewer hours per month, though the difference (3%) is small by international standards.[8] The dispersion in monthly hours is larger for women than men (row 12), while the dispersion in hourly wages is smaller (row 10). Rows 13 and 14 show the fractions of the sample in the two largest urban areas in the country (Lisbon and Oporto), which together account for about one-half of all workers in Portugal.

Rows 15 and 16 show average firm size for men and women and the average fraction of female employees at men's and women's workplaces. As is true in the U.S. and the U.K., women in Portugal work at somewhat larger firms than men.[9] More striking is the wide disparity (70% vs. 24%) in the share of female employees at women's and men's employers (row 16), suggesting that there is significant gender segregation across firms in Portugal.[10] Indeed, male workers at all-male firms account for 21% of male person-years in our analysis sample, while female workers at all-female firms account for 19%.[11]

Wages of workers at gender segregated firms are relatively low: the mean log wage for men at all-male firms is 1.28 (31% below the average for all men) while the mean wage for women at all-female firms is 1.19 (22% below the average for all women). Surprisingly to us, these averages imply that that the gender wage gap between workers at fully segregated firms is substantially smaller than the gap in the labor market as a whole (9% versus 18%).

[8]Data reported by the OECD (2012) for Portugal (based on labor force survey data that includes the government sector and independent contract workers excluded from QP) shows part-time employment rates for men and women of 8% and 14%, respectively. The same source shows part-time employment rates for men and women in the US of 8% and 17%.

[9]Papps (2012) and Mumford and Smith (2008) report roughly 10% larger workplace sizes for women than men in the U.S. and U.K., respectively.

[10]Mumford and Smith (2008, online Appendix Table A2) report that in the U.K. in 2004 the average fraction of female employees at the workplace was 70% for women and 34% for men, which implies somewhat less segregation by gender than in Portugal.

[11]Construction and trade account for 43% and 20%, respectively, of the person-year observations at all-male jobs. All-female workplaces are prevalent in trade (23% of person-years at all female firms), health services (17%), hotels (14%), and textiles (13%).

While the average gender wage gap in the 2000's in Portugal was about 18%, the gap was gradually narrowing over the decade, as shown in Figure 1. Between 2002 and 2008, real wages of men were nearly constant, while real wages of women were rising slightly. In 2009 – the first year of the subsequent major recession in Portugal – nominal wages of both men and women grew by about 3% (the same pace as in the previous 6 years) but the rate of inflation dropped from around 3% to -1%, yielding a jump in real wages for both genders. Overall, the gender gap in log hourly wages narrowed from 21% in 2002 to 16% in 2009.

As described in more detail below, we estimate gender-specific wage determination models with worker and firm fixed effects on subsamples of people employed at firms in the "largest connected sets"of firms that employ men or women.[12] The characteristics of the men and women who are observed working at these firms are shown in columns 3 and 4 of Table 1. Overall, 91% of all person-year observations for male workers and 88% of all person-year observations for female workers are included in the largest connected sets. The included workers have very similar demographic characteristics and labor market outcomes as those in our overall analysis sample. In particular, the mean gender wage gap is only 1 point wider (0.19 versus 0.18) for men and women in the largest connected sets than in our overall analysis sample.

In much of our analysis we further limit attention to workers who are employed at firms that are in the connected sets for both men and women. (These firms, by definition, employ both men and women). This "doubly connected"sample –described in columns 5 and 6 of Table 1 – includes 66% of the person years of male workers in our analysis sample, and 69% of the person years of female workers. Individuals in the doubly connected set have higher education than in the workforce as a whole, and also have higher average wages: 12% higher for men and 7% higher for women. The gender wage gap is somewhat larger

[12] See Abowd, Kramarz and Margolis (1999) for discussion of connected sets. A firm is "directly connected" to another firm if there is at least one worker who is observed working at both firms (at different points of time). A firm is indirectly connected to another if there is a sequence of directly connected firms linking the two. The largest connected set of firms is the largest set of firms that are directly or indirectly connected to each other.

in this sample than in our overall sample (23% versus 18%) reflecting the omission of the gender-segregated firms, which as noted earlier have a relatively small gender gap. Firms in the doubly connected set are a little larger (row 15) and less gender-segregated than the population of firms in Portugal, as evidenced by the smaller disparity between the average fractions of female employees at men's and women's employers (row 16).

Finally, columns 7 and 8 of Table 1 show the characteristics of women and men whose employer in a given year can be matched to current year financial data. The age and education levels of these workers are quite similar to those in our full analysis sample. Mean wages of men at firms with financial data are 2 log points lower than in the full analysis sample, while mean wages of women at firms with financial data are 3 log points lower, implying a slightly larger gender wage gap among these workers than in our overall analysis sample (19% versus 18%). As shown in row 17, there is a similar 18 log point gap in mean value added per worker between male jobs and female jobs, suggesting that some of the gender wage gap may be due to the sorting of female workers to less profitable firms - an effect we discuss in more detail below.

2 Modeling Framework

We turn now to developing a simple econometric model that allows us to evaluate the effect of gender differences in bargaining aptitude and willingness to negotiate on the observed wages of women and men. Consider a longitudinal setting where we observe the wages of individual workers (indexed by i) in multiple periods (indexed by t). In period t individual i is observed working at a specific firm $j = j(i,t)$, that is uniquely specified given the identify of the worker and the time period.[13] To simplify notation we denote the firm simply by j. We also denote the gender of worker i by $g \in \{F, M\}$.

We posit a rent-sharing model where, in each period, the worker's log wage (w_{it}) equals her market alternative (a_{it}) plus a gender-specific share ($\gamma^g \in [0,1]$) of the surplus S_{ijt} generated

[13] In the small number of cases of dual job holding, we assign each worker his or her highest-earning job.

by the job match between worker i and firm j in period t:

$$w_{it} = a_{it} + \gamma^g S_{ijt}. \tag{1}$$

The recent negotiation literature argues that $\gamma^F < \gamma^M$, which will lead to lower wages among women, particularly those in employment relationships with a higher surplus.

We decompose the surplus S_{ijt} into three components:

$$S_{ijt} = \bar{S}_j + v_{jt} + m_{ij}. \tag{2}$$

The first component, $\bar{S}_j$, is the average level of surplus across all workers and all time periods at firm j. The second component v_{jt} represents a firm-wide time-varying component of the surplus, attributable to factors like changes in output market conditions or input prices. The third component is a time-invariant match-specific component, attributable to idiosyncratic features of the worker and the firm (i.e., any unique source of complementarity). We assume that the pure match components at each firm have mean zero for each gender.

We also decompose the market alternative wage a_{it} of the worker into a time invariant intercept α_i, a component due to a linear combination of time varying covariates X_{it} (e.g., labor market experience and changing market returns to education), and a time varying component ε_{it}:

$$a_{it} = \alpha_i + X'_{it}\beta^g + \varepsilon_{it}, \tag{3}$$

where β^g is a gender specific vector of coefficients on the time varying factors. We assume that ε_{it} has mean zero for each individual in each period.

Together, these specifications imply a wage model of the form:

$$w_{it} = \alpha_i + \psi_j^g + X'_{it}\beta^g + r_{it}. \tag{4}$$

where $\psi_j^g \equiv \gamma^g \bar{S}_j$, and $r_{it} \equiv \gamma^g v_{jt} + \gamma^g m_{ij} + \varepsilon_{it}$ is a composite error.[14] For OLS estimation of (4) to yield unbiased estimates of the worker and firm effects, we need for the following three conditions to hold: i) $E[r_{it}X_{it}|g] = 0$ for every gender g, ii) $E[r_{it}|i] = 0$ for every worker i, and iii) $E[r_{it}|j, g] = 0$ for every firm j and gender g. Condition iii is often referred to as an "exogenous mobility" restriction, which will hold if the sorting of workers to firms depends only on the person effects and the time-invariant firm-effects, and is independent of the residual components of wages. We discuss this condition at length below and provide some evidence that it is satisfied.

For each gender, equation (4) is equivalent to the two-way fixed effects model of wage determination considered by Abowd, Kramarz and Margolis (1999), and many subsequent authors (see Card, Heining and Kline, 2013 for a brief review of this literature). For our purposes, the key prediction is that the firm effects for female and male workers at firm j are related in a very simple way:

$$\psi_j^F = (\gamma^F/\gamma^M)\psi_j^M.$$

In particular, if women obtain a smaller share of the surplus than men, the firm effects for women will be proportional to the firm effects for men, with a factor of proportionality $\gamma^F/\gamma^M < 1$. This has a number of testable implications for the relative variation in wages of men and women between firms and within firms over time.

[14] More generally, γ could vary across people with γ^F and γ^M being the mean values for women and men. This "random coefficient" formulation would add another component to the residual of equation (4) representing the deviation of the person-specific bargaining parameter from the gender mean, multiplied by surplus associated with the job at the individual's employer. Another extension could allow differential sharing of the permanent and transitory components of the job-match surplus. Guiso et al. (2005) find that wages are much less sensitive to transitory profit changes than to permanent shifts, suggesting that it may be reasonable to allow a different bargaining share for the component v_{jt} in the residual r_{it}.

3 Between Firm Wage Differentials

3.1 Model Predictions

Equation (4) has straightforward implications for the wage changes of men and women who move between any pair of origin and destination firms. Under the assumption that job mobility is independent of the idiosyncratic match components of job surplus (i.e., the m_{ij} terms),[15] the expected wage change for males who move from firm 1 to firm 2 between periods $t-1$ and t is:

$$E\left[\Delta w_{it} | g = M, j\left(i, t-1\right) = 1, j\left(i, t\right) = 2\right] = \Delta X_{it}\beta^M + \gamma^M\left(\bar{S}_2 - \bar{S}_1 + v_{2,t} - v_{1,t-1}\right) \quad (5)$$

where Δ denotes the first difference operator. The expected wage change for females who make the same job transition is:

$$E\left[\Delta w_{it} | g = F, j\left(i, t-1\right) = 1, j\left(i, t\right) = 2\right] = \Delta X_{it}\beta^F + \gamma^F\left(\bar{S}_2 - \bar{S}_1 + v_{2,t} - v_{1,t-1}\right) \quad (6)$$

After adjusting for the impacts of the time-varying X_{it}, the expected wage change for female job movers is proportional to the expected change for men who make the same transition, with a factor of proportionality γ^F/γ^M. Intuitively, if women get a smaller share of the job surplus, then between-firm moves from less-profitable to more-profitable firms will benefit men more than women, while movements in the opposite direction will hurt women less than men. We can test this prediction by examining the average wages changes of groups of male and female job movers who move between matched subsets of firms, and relating the wage changes for women to the corresponding changes for men.

[15] See Card, Heining and Kline (2013) for a detailed discussion. If job mobility is unrelated to the individual component of the match surplus, then wage changes of workers who move from one group of firms to another will be equal in magnitude and opposite in sign to the wage changes of those who move in the opposite direction. We provide evidence of (approximate) symmetry in wage changes below.

3.2 A Simple Descriptive Analysis of Gains to Mobility

As a starting point for our analysis we conduct a study of wage mobility along the lines suggested by equations (5) and (6), using workers in the doubly connected sample described in columns 5-6 of Table 1. We begin by fitting a wage prediction model to male workers in the sample, using a basic set of firm characteristics: industry (20 categories), average firm size and its square, and dummies for location in Lisbon or Oporto. We then use the predicted values from this model to assign all worker-firm-year observations into "job quality"quartiles.[16] Finally, we construct average wages in the years before and after a move for male and female job-changers who are observed moving between different origin and destination quartiles. To ensure a balanced sample, we limit attention to movers with at least two years of wage data at the origin firm and two years at the destination firm.

Figure 2a shows the wage profiles for males and females who start at the lowest-quartile firms and move to other firms in quartiles 1, 2, 3, and 4. Figure 2b shows the corresponding profiles for workers who start in the highest quartile firms. A number of interesting patterns are apparent in the two figures. First, all the wage profiles are upward sloping, reflecting the fact that most job movers are relatively young and are experiencing some real wage growth. Second, the entire profile of wages for women in a given origin-destination group lies below the profile for men in the same mobility group, reflecting the existence of relatively large gender wage gaps even conditioning on the "quality" of both the origin and destination firm of job changers. Third, job changes up the firm quality distribution (e.g., from quartile 1 to quartile 4) are associated with positive wages gains relative to changes in the opposite direction. Fourth, the wage profiles of different origin-destination groups are approximately parallel in the years before and after a move. There is little indication that wages of people who will mover to higher quality firms are trending upward faster in the period before their move than those who move to lower quality firms.

Table 2 presents more detailed information on wages in the years before and after a job

[16] Since predicted wages are based on time-invariant firm-level variables, jobs at any particular firm are assigned the same quartile for male and female workers in all years.

change for males and females in all 16 origin/destination groups. We show the data for movers from quartile 1 to quartile 2, for example, under the subheading "Q1 to Q2". For each origin/destination group we also show the wage change from the last year on the old job to the first year on the new job (in the rows labeled "wage change") and a trend-adjusted wage change (in bold font), which adjusts the wage changes for workers in different origin-destination groups by subtracting off the wage change for workers who switch jobs but remain in the same quartile as they started. We also show the number of observations in each origin-destination group, and the fraction of job changers from a given origin quartile who end in each of the four destination groups.

Inspection of the trend adjusted wage changes in Table 2 confirms that job changers who join a firm in a higher predicted wage quartile (based on its industry, size, and location) experience a relative wage gain, while those who move to a firm in a lower predicted wage quartile experience a relative wage loss. Interestingly, these relative gains and losses are approximately symmetric. For example, the relative wage gain for males who start in a quartile 1 firm and move to a quartile 4 firm is 9% on average, while the relative wage loss for those who move in the opposite direction is -13%. Symmetry of the losses and gains for movers in opposite directions is predicted by the simple model of equations (5) and (6), which assumes that mobility is independent of the worker-specific job-match components of pay.

A second important observation is that although we have based the firm rankings in Figure 2 and Table 2 on predicted male wages, the wage gains of women who make a particular job transition (e.g., from quartile 1 to quartile 4 firms) are similar to the relative gains of men. Figure 3 plots the wage gains for women in each of the 16 origin-destination cells in Table 2 against the corresponding wage gains for men. Most of the points are below the 45-degree line, suggesting that the average wage gain of women for any job change is 1-2 percentage points lower than the corresponding wage gain for men.[17] Across the 16 origin-destination

[17] Loprest (1992) noted that that job mobility has a 2-3 percentage point smaller effect on wage growth for female than male workers, using longitudinal data from the NLSY. She also showed that the slower growth was not due to differences in the job characteristics of the origin or destination jobs.

groups, however, the slope of the relation between the female and male wage gains (shown by the solid line in the figure) is only slightly less than 1. (The estimated slope is 0.96, with a 95% confidence interval of 0.81 to 1.12). Thus, it appears that the relative benefits of moving to a better-paying firm are not too different for men and women. Figure 4 plots the trend-adjusted relative wage changes for women against the corresponding changes for men. Again, it appears that women benefit almost as much as men from a given job change, though with only 12 trend-adjusted wage changes we cannot rule out a fairly wide range for the relative benefit of women. (The 95% confidence interval for the estimated slope ranges from 0.86 to 1.06).

A final interesting feature of the data in Table 2 is that the *relative* fractions of men and women moving from a given origin group to each of the four alternative destination groups are quite similar.[18] In the case of workers who start at quartile 1 firms, for example, 64.7% of both women and men move to other firms in quartile 1, while 5.6% of men and 4.9% of women move to quartile 4 firms. This similarity suggests that the mobility process is broadly similar for men and women in the Portuguese labor market. Nevertheless, as noted, women tend to gain 1-2 percent less for each job-to-job transition than do men, contributing to a widening of the gender gap with experience. Whether this lower return to mobility is due to a gender gap in bargaining skill is unclear. The fact that the return is about the same regardless of the size of the wage change experienced by men who make the same type of change suggests to us that it may be due to other factors – for example, a difference between men and women in the fraction of job changes that are made to accommodate a change in the spouse's job location.[19]

[18] We define the quartiles so one-quarter of all person-year observations are in each quartile. On average slightly more men are in quartile 4 than quartile 1 (25.7% versus 24.1%) whereas for women the opposite is true (26.1% in quartile 1 versus 24.1% in quartile 4).

[19] Mincer (1978) hypothesized that a larger fraction of men than women are "tied movers" whose change in location is driven by a change in the location of their spouse's job. We cannot match couples in the QP to test this hypothesis directly.

3.3 Analysis Based on Worker-Firm Models

3.3.1 Normalization Issues

We turn now to a more comprehensive analysis of gender differences in pay premiums across firms. Building directly on equation (4), we fit models that include person effects (α_i), gender-specific firm effects (ψ_j^g), and a set of time-varying observable covariates with gender-specific coefficients. The worker and firm effects for each gender group are only separately identified within a connected set of firms linked by worker mobility (see Abowd, Creecy, and Kramarz, 2002). To simplify estimation, we restrict our analysis to the largest connected set of establishments for each gender. This allows us to identify the worker and firm effects for each gender, subject to a single normalization for each group.[20]

Because we are interested in comparing firm effects across genders, we need to impose a common normalization across the two groups so that the firm effects are measured on the same scale. While, in principle, we could normalize any doubly connected firm j_0 that employs both men and women to have a firm effect of zero for each gender, our model suggests that if firm j_0 has high surplus, women will be underpaid at this reference firm (because $\gamma^F < \gamma^M$). As a result, the mean difference in firm effects between men and women *relative to firm* j_0 will tend to understate the true extent to which women are underpaid. To avoid this problem, we seek a group of low surplus reference firms, which, according to our model, ought to pay both men and women roughly their outside wage a_{it}. To accomplish this, we make the normalizing assumption that the average of the firm effects for each gender across all the firms in the lowest vingtile of predicted firm quality (based on the index used in Figures 2a and 2b and Table 2) is zero.[21] The normalized firm effects ($\widetilde{\psi}_j^g$) therefore identify the gender-specific wage premiums paid at a given firm, relative to the average premium paid

[20] Standard approaches in the literature are to either set one of the firm effects to zero or to normalize the sum of the firm effects (or the sum of the person effects) to zero.

[21] In practice, we accomplish this by estimating our model via OLS separately for each gender omitting a dummy for the largest firm in the doubly connected set. We then renormalize these effects by subtracting off the mean estimated gender specific firm effect in the bottom vingtile.

by firms in the reference group of low-wage firms ($\bar{\psi}_0^g$):

$$\widetilde{\psi}_j^g = \psi_j^g - \bar{\psi}_0^g.$$

Making the further assumption that jobs at the lowest-wage firms provide zero surplus on average (i.e., $\bar{\psi}_0^M = \bar{\psi}_0^F = 0$) the normalized gender-specific firm effects provide estimates of rent-sharing *levels*. That is:

$$\widetilde{\psi}_j^g = \psi_j^g = \gamma^g S_j,$$

which allows us to compare firm effects across genders on a common scale. To the extent that firms in the bottom vingtile actually pay men more than women (so that $\bar{\psi}_0^M > \bar{\psi}_0^F$), our approach will underestimate the contribution of firm effects to the gender wage gap.

3.3.2 Endogenous Mobility

A concern with estimation of models like (4) by OLS is that the residual components of wages may be correlated with specific patterns of mobility, leading to biases in the estimated worker or firm effects. As discussed at length by Card, Heining, and Kline (2013), it is possible to assess the relevance of many sorts of endogenous mobility empirically by looking at a series of specification diagnostics. The model of wage determination outlined in Section 2 implies that the error term r_{it} contains three components: (1) a term ε_{it} representing the deviation of an individual's alternative wage a_{it} in period t from its average value in the sample period; (2) a term $\gamma^g v_{jt}$ representing the employee share of any transitory fluctuation in the average surplus available at the firm; (3) a term $\gamma^g m_{ij}$ representing the employee share of the idiosyncratic match effect for individual i at firm j. Each of these components is potentially correlated with the sequence of wage premiums received by a worker, and could therefore lead to biases in the estimated firm effects. We discuss each of the three components in turn.

The most important concern with the first component ε_{it} is the presence of a slowly evolving component in the alternative wage for person i, reflecting health shocks, learning about unobserved abilities, or other random but persistent shocks.[22] The equilibrium assignment process in the labor market may lead to a tendency for people whose alternative wage is falling to move down the firm-quality distribution (i.e., to firms with lower wage premiums), while those whose alternative wage is rising tend to move up the firm quality distribution. This form of mobility would lead to an expected fall in wages prior to a move to a lower quality firm, and an expected rise in wages prior to a move to a higher quality firm, that would lead to biased estimated of the firm effects. While this sort of selection is possible, the data in Figure 2 do not provide much support for it. The absence of systematic trends in wages prior to a move for workers who transition to better or worse firms in Figures 2a and 2b suggests that there is not a large persistent component in wages that is correlated with the direction of firm-to-firm mobility.[23]

Likewise, if there is a slowly evolving component of firm-wide profitability that is correlated with the direction of mobility (up or down the firm quality distribution) of workers who leave the firm, OLS estimates of the firm effects in equation (4) will be biased. The presence of such components will lead to different trends in wages prior to a move for workers who transition to higher- or lower-paying firms. Given the absence of such trends in Figures 2a and 2b, however, we believe that the component of the wage error associated with these fluctuations ($\gamma^g v_{jt}$) is small.

A final concern is that mobility is related to the person-specific job match component, $\gamma^g m_{ij}$. However, as noted in the discussion of Table 2, the near symmetry in the wage changes of workers who move up and down the firm quality distribution suggests that the idiosyncratic job match component of wages is not a major driver of mobility. Moreover, as we show below, comparisons between models based on equation (4) and generalized models

[22]Note that purely transitory fluctuations in the alternative wage that are correlated with the event of moving, but not with the relative size of the wage differentials at the origin and destination firms, will not lead to biases in OLS estimation of the model.

[23]It is possible however that worker mobility results in part from public learning about signals that are revealed very quickly and hence are missed by our event study diagnostics (e.g. as in Gibbons and Katz, 1992). While this is a possibility, we note that recent empirical work has found that employer learning tends to take much longer than a year (Altonji and Pierret, 2001, Lange, 2007).

with unrestricted job match dummies (which fully absorb person, firm, and any job match effects) reveal that the idiosyncratic job match component of wages is relatively small for both men and women in Portugal. Thus, we believe that this component is not a major source of bias in estimation of models based on (4).

These facts corroborate the recent findings of Card, Heining, and Kline (2013) in West German wage data and suggest that the exogenous mobility restrictions underlying additive worker-firm decompositions may hold in many other settings.[24] We note in passing that the so-called "exogenous mobility" restrictions still allow for very rich patterns of sorting of workers to firms. In particular, workers may sort dynamically in an unrestricted way based upon any time invariant characteristic of the worker and any time invariant characteristic of the firm without violating the conditions identifying the worker firm decomposition.

3.3.3 Estimation Results

Table 3 summarizes the parameter estimates and fit of our models for male and female workers observed between 2002 and 2009. The models include fixed effects for workers and firms (a total of 2.1 million effects for men and 1.7 million effects for women) as well as year dummies, fully interacted with education dummies (for the 4 education groups shown in Table 2) and quadratic and cubic terms in age interacted with education dummies.[25] The first panel of the table shows the standard deviations of the estimated person and firm effects and the covariate indexes $\left(X_{it}'\widehat{\beta}^{g}\right)$ for each observation, as well as the correlation of the person and firm effects, the residual standard deviation of the model, and the adjusted R-squared statistics. For both male and female workers, the standard deviations of the person effects are nearly twice as big as the standard deviations of the firm effects, implying that a relatively large share of wage inequality for both genders is attributed to permanent differences in the skills of different workers. The correlations between the estimated person and firm effects are both

[24] See also Macis and Schivardi (2013) for evidence that the exogenous mobility assumptions are satisfied in Italy.

[25] Estimates were computed using a preconditioned conjugate gradient algorithm as in Card, Heining, and Kline (2013).

positive, implying that higher skilled men and women are disproportionately employed at firms that pay all their workers a bigger wage premium. Such positive assortative matching has been found in West Germany by Card, Heining and Kline (2013), and in several other recent studies of European wage determination.[26]

The middle panel of Table 3 shows fit statistics for a generalized model of wage determination that includes unrestricted dummies for each job match. This model provides a slightly better fit to the wage data for both men and women, with adjusted R-squared statistics that are about 1 percentage point higher (e.g., 0.951 versus 0.940 for women). Comparing the residual standard error of the job match model to the corresponding standard error for the model with worker and firm effects we can construct an estimate of the standard deviation of the permanent job match effects (the m_{ij}) that are absorbed in the job match model but included in the residual of (4). The estimates are 0.062 for men and 0.054 for women – only about one-quarter as big as the standard deviations of the firm effects for the two genders. We conclude that the firm-wide component of job match surplus is considerably larger than the purely idiosyncratic component.

We have also conducted a series of additional specification checks of the fit of our basic models for men and women based on (4). These are summarized in the Appendix. In one check, we examine the mean residuals for subgroups of observations classified by the decile of the estimated person effect and the decile of the estimated firm effect. We find that the mean residuals are very small in all 100 cells for both genders, suggesting that the additive structure of (4) provides a good approximation to the wage-setting process in Portugal. As a second check, we examined the wage profiles of workers who transition between groups of firms, classified by the quartile of the (gender-specific) estimated firm effect. These results confirm the main patterns discussed in Figures 2a and 2b. In particular, we find that wages of men and women who move between jobs follow very similar trends prior to the move, regardless of whether they move up or down the firm quality distribution.

[26] See e.g., Skans et al. (2008) and Bagger et al. (2012).

The bottom panel of the figure presents the main components of a simple decomposition of the variance of wages across workers implied by the fitted version of equation (4):

$$\begin{aligned} Var\left(w_{it}\right) &= Var\left(\hat{\alpha}_i\right) + Var\left(\hat{\psi}_j^g\right) + 2Cov\left(\hat{\alpha}_i, \hat{\psi}_j^g\right) + Var\left(X_{it}'\hat{\beta}^g\right) \\ &\quad + Var\left(\hat{r}_{it}\right) + 2Cov\left(\hat{\alpha}_i + \hat{\psi}_j^g, X_{it}'\hat{\beta}^g\right). \end{aligned} \tag{7}$$

Among both male and female workers, person effects accounts for about 60% of overall wage variation, firm effects account for about 20%, and the covariation in worker and firm effects accounts for an additional 10%. In both cases the contribution of the measured covariates (including the main effects and the covariances with the person and firm effects) is relatively small, and the residual component is also small, reflecting the relatively high R-squared coefficients for the underlying models.

3.3.4 Implications for the Gender Wage Gap

A key implication of the hypothesis that women receive a smaller share than men of the surplus associated with their job is that the wage differential paid by a given firm to its male employees (over and above the premium paid at the "no-rent"firms whose premium is normalized to 0) will be larger in magnitude than the premium paid to its female employees – i.e., that the estimated male firm effects from equation (4) will be larger in magnitude than the estimated female firm effects. Since the firm effects for individual firms are estimated with error, it is difficult to test this inequality for any particular firm. By grouping firms into cells, however, it is possible to compare the average male and female firm effects for one group of firms versus another.

Figure 5 plots the mean values of the female firm effects against the corresponding means of the male firm effects in the cells defined by vingtiles of the index of "job quality" used in Figure 2 and Table 2, restricting attention to firms in the dual-connected set. Notice that the points lie along a line that is flatter than the 45 degree line (the estimated slope is 0.87, with a standard error of 0.03). The implication of this pattern is that female employees earn only about 90% of the rent premiums paid to their male co-workers.

To assess the implied impact of these smaller wage premiums we present a series of Oaxaca (1973)-style decompositions in Table 4.[27] The first row in the table shows the mean log wages of male and female workers in the dual-connected set, along with the associated gender wage gap, which is 23.4 log points. Rows 2-5 show various counterfactuals. In row 2 we assign the estimated *male* wage premium to the female workers at each firm.[28] Under this scenario, women's wages would rise by about 3.5 log points, and the gender wage gap would fall to about 20 log points. Thus, the smaller degree of rent sharing by women than men accounts for about 15% of the observed gender wage gap. Row 3 reverses the base group and assigns the estimated *female* wage premiums to the male workers at each firm. Under this scenario, men's wages would fall by 2.3 log points and the gender wage gap would narrow to 21 log points, implying that 10% of the observed gender wage gap is attributable to differential bargaining.

Rows 4 and 5 consider scenarios in which we hold constant the wage premiums earned by men and women at each firm, but assume that women have the same distribution across firms as men (row 4) or that men have the same distribution across firms as women (row 5).[29] Assigning women the men's distribution leads to a 4.7 log point rise in female wages – enough to narrow the gender wage gap to 18.8%. Reversing the process and assigning men the women's distribution leads to a 3.5 drop in male wages – enough to narrow the gender gap to 19.9%.

Overall, the decompositions in Table 4 lead to two main conclusions. First, the lower

[27] See Fortin, Lemieux, and Firpo (2011) for a recent review of decomposition techniques. Our approach is somewhat novel in that it relies on a theoretically motivated normalization across the two groups in order for the decomposition to be meaningful. This has some similarities to the reference group problems in Oaxaca decompositions discussed (among others) by Oaxaca and Ransom (1994) and Fortin (1998).

[28] Since the firm effects are just the coefficients of indicator variables for the current employer, this is the familiar decomposition step of assigning the "coefficients" of the firm dummies for men to women.

[29] We perform this calculation by reweighting the female workers at each firm by a weight that is equal to the overall fraction of male workers at the firm, divided by the overall fraction of female workers at the firm. This is the discrete version of the technique discussed by DiNardo, Lemieux and Fortin (1996).

degree of rent sharing by women illustrated graphically in Figure 5 leads to a net reduction in female wages of 2.3 to 3.5 percentage points, relative to a scenario in which women received the same wage premiums at each firm as their male coworkers. This effect is modest in size, but enough to account for 10-15% of the gender wage gap in Portugal. Second, a somewhat larger share of the gender gap (15-20%) is attributable for the fact that women are disproportionately employed at firms that pay both men and women relatively low wages. This latter finding agrees with the results of Cardoso, Guimarães and Portugal (2012), who conclude using different methods that roughly a fifth of the gender wage gap in Portugal is attributable to the differential sorting of women across firms.[30]

It is worth emphasizing that the nature of our fixed effects procedure ensures the firm specific wage premiums underlying the analysis in Table 4 have already been purged of any permanent differences in worker skill or ability across workers of either gender. Moreover, all the firms used in the analysis hire both men and women, and are connected to other firms that hire men and women in the economy by worker mobility – hence, the decompositions utilize a common support. Nevertheless, there are two limitations worth noting. Most importantly, the decompositions are based on the assumption that the rent premiums at the bottom 5% of firms in our job quality index are on average zero for both gender groups. To the extent that workers at these firms receive some rents and female employees earn a smaller share than males, the results in Table 4 are lower-bound estimates of the impact of women's lower bargaining power. Secondly, the decompositions only pertain to workers at dual-connected firms, and exclude in particular the employees of gender-segregated firms. Since the wage gap between segregated firms is relatively small (9%), however, we believe the first-order concern is the gender gap at firms that hire both men and women.

[30] Cardoso, Guimarães and Portugal (2012) estimate a worker-firm model imposing that the firm effects are identical across the genders. As our analysis shows, this neglects an important additional source of gender difference in pay.

3.4 Firm Effects and Profitability

The results in Table 3 confirm that firm-specific pay premiums tend to be larger for male workers than female workers, contributing to the gender wage gap in Portugal. A potential concern with these decompositions is that they implicitly ignore any sources of firm-specific pay premiums that are attributable to gender-neutral factors, such as compensating differentials for unfavorable working conditions or the cost of living in nearby neighborhoods. To address this concern we look directly at the relationship between the estimated wage premiums paid by firms to their male and female workers and a simple measure of profitability based on value-added per worker.[31]

Letting VA_{jt} denote the log of value added per worker at firm j in period t, define $\overline{VA}_j$ as the average of VA_{jt} over the years of non missing data available for firm j. Figures 6a and 6b show the relationships between average value added per worker and the estimated firm and person effects for men and women, using observations in our dual-connected sample that can be matched to an observation on average value added per worker for the employer. To construct these figures we divided $\overline{VA}_j$ into 20 cells, with approximately 5% of worker-year observations in each cell. We then calculated the mean values of the estimated male and female firm and worker effects for worker-year observations in each cell. Figure 6a shows that the estimated firm effects for men and women are both positively correlated with our measure of profitability. Consistent with the pattern in Figure 5, the firm effects for men exhibit a greater slope across cells, implying that women gain somewhat less than men from working at more profitable firms. In fact, the relative slope of the female effects is 0.89, which is very close to the slope of the relationship in Figure 5.

Figure 6b shows that the estimated person effects for men and women are also positively correlated with average value added per worker at their employer, though the person effects for female employees are much below the values of the effects for men in the same $\overline{VA}_j$ cell.

[31] Of course, the anaysis in this section assumes value added per worker is not correlated with unfavorable working conditions or cost of living, which may not be the case. The next section's analysis of within firm changes in value added and wages circumvents these concerns.

The implication of these patterns is that most all of the variation in average wages of men and women across firms in different $\overline{VA}_j$ cells is attributed to differences in "worker quality" (as measured by the person effects in equation (4)). Only a relatively small share of the gap is attributed to differences in the wage premiums received by male and female employees at the firms in different cells.[32]

We evaluate the relationships between value added per worker and the estimated firm and worker fixed effects more formally in Table 5. Here, we present regression models of the form:

$$\hat{\psi}_j^g = \kappa^g + \theta^g \overline{VA}_j + \varsigma_j^g \tag{8}$$

where $\hat{\psi}_j^g$ denotes the estimated (normalized) firm effect for firm j and gender group g and ς_j^g is a gender-specific error. We estimate these models at the person-year level, assigning each worker the estimated gender specific fixed effect for his or her employer in a given year, and cluster standard errors by firm. To the extent that male workers receive a larger share of the rents from more productive firms we expect $\theta^M > \theta^F$. The estimates in columns 1 and 3 (from models with no other firm-level controls) confirm that θ^M is bigger than θ^F, consistent with the visual evidence in Figure 6a. Estimates from models with controls for industry, firm size, and major city (columns 2 and 4), are a little smaller in magnitude for both genders but again suggest that θ^M is bigger that θ^F.

A rough estimate of the implied effect of differential rent sharing on the overall gender wage gap can be obtained by multiplying the estimated gap between θ^M and θ^F (which is about 0.03) by the average value of $\overline{VA}_j$ relative to its value at firms that pay no rent premium. Assuming these are the firms in the lowest cell in Figure 6a, the average excess $\overline{VA}_j$ at higher rent firms is approximately 1, and the implied gender wage gap attributable to differential rent sharing is 0.03 (i.e., 3 percentage points). By comparison the decompositions

[32] The mean log wage of the male or female workers at a given set of firms is equal to the sum of the estimated firm effects plus the estimated person effects plus the estimated value of the covariate index. Ignoring the terms involving the covariates (which are all small), this means that the mean log wage gap between women and men at a given set of firms can be decomposed into the difference in gender-specific firm effects, plus the difference in person effects.

in Table 4 imply that differential rent sharing accounts for a gender gap of 2.3 percentage points (using the female wage structure as the base) or 3.5 percentage points (using the male wage structure as the base). The two approaches therefore yield very similar estimates.

4 Within Firm Changes in Profitability and Wages

Our analysis so far has relied upon a decomposition of wages into worker and firm effects. As already discussed, such decompositions are identified by worker mobility across firms. But our model suggests a lower value of the rent sharing parameter for women than men also has implications for the evolution of wages of male and female workers who remain with the same firm over time. In this section we use observations from the last 4 years of our analysis sample (2006-2009) to measure the effects of changes in firm-specific profitability on the wages of male and female job stayers. In addition to exploiting a different source of variation, this analysis has the advantage of not relying on the exogenous mobility assumptions underlying the worker-firm decomposition of wages.

We begin in Table 6 with an overview of the subsample of workers from the dual-connected set who are continuously employed for the same firm between 2006 and 2009, and whose employer has financial information available for each year from SABI. This sample contains 290,000 men and 204,000 women employed at 33,600 firms. The men and women in the sample have about the same age, education and wages as men and women in our overall analysis sample. The gender wage gap in the sample of stayers is also roughly comparable to the gap in our overall sample, with a value of 23 log points in 2006 and 22 points in 2009. The firms in the sample are slightly larger than average for the economy as a whole but quite similar to those in the full dual connected set.

Figure 7 illustrates the relationship between the changes in firm-specific profitability between 2006 and 2009 and the corresponding changes in wages of male and female job stayers. We group firms into 20 roughly equally sized groups based on the change in log value added per worker ($\triangle VA_j = VA_{j,2009} - VA_{j,2006}$), and for each group construct the mean log wage changes of the job stayers. As shown in the graph, mean wage changes are strongly correlated with $\triangle VA_j$: across cells the slope of the best fitting line is 0.059 for men and 0.046 for women. Consistent with the cross-sectional relationships between profitability and firm-specific wage premiums shown in Figure 6a, the relationship is flatter for women, implying a smaller degree of rent sharing for female employees than males.

Table 7 presents a series of models estimated separately for men and women that relate changes in employee wages to the corresponding change in log value added per worker at his or her employer:

$$\triangle w_i = w_{i2009} - w_{i2006} = \widetilde{X}_i' \phi^g + \lambda^g \triangle VA_j + \xi_i \tag{9}$$

where $\widetilde{X}_i$ is a vector of demographic controls and ξ_i is a composite error. Columns 1 and 5 show simple univariate OLS models, while columns 2 and 6 add a quadratic function of the worker's age in 2006 as additional controls. In these specifications the elasticities of wages with respect to value added are relatively small – 0.028 or 0.029 for men and 0.018 or 0.019 for women – but precisely estimated. Moreover, as in Figure 7, the elasticity for women is smaller than for men. The ratio of the elasticities in columns 6 and 2, for example, is 0.64.

In comparison with the estimated effects of profitability on firm-specific wage premiums in Table 5, the estimated coefficients in columns 1-2 and 5-6 of Table 7 are relatively small. One important reason for this difference is measurement error in value added per worker, will be magnified in a first differenced specification, particularly when the difference is taken over only a three year span. In an effort to address this problem, we use the change in value added per worker between 2007 and 2008 as an instrument for the change between 2006 and 2009. As shown in columns 3 and 7, the change from 2007 to 2008 is a powerful predictor of the change over the longer horizon, with t-statistics of around 60 (clustered by firm). The resulting IV estimates, presented in columns 4 and 8, yield an elasticity for men of 0.056 and for women of 0.050. Again, the effect of profitability on wages is smaller for women than men, with a relative magnitude of 0.89 (standard error=0.23).

Even using the IV strategy the estimated effects of changes in profitability on changes in wages in Table 7 are only about one-half as big as the elasticities in Table 5. An explanation for the gap is that, contrary to equation (1), wages are less responsive to transitory fluctuations in the match surplus between worker i and firm j than to permanent differences in this surplus. Indeed, an analysis by Guiso et al. (2005) of wages and firm profitability based on Social Security earnings record for Italian firms suggests that wages are only weakly affected by year-to-year changes in profitability. Part of the dampened response to short-run changes in profitability may be due to a reluctance to raise (or lower) wages in response to temporary changes in profitability that will be quickly reversed. In addition, institutional features like multi-year collective bargaining contracts and lags in the individual negotiation process may slow the adjustment of wages to movements in profitability.

These considerations suggest that the elasticities of wages with respect to changes in profitability over a 3-year horizon probably understate the longer-run responsive to permanent differences in the match surplus associated with a particular job. Nevertheless, it is interesting that even over a relatively short horizon we find that the wages of female employees are less responsive to changes in their employer's profitability than wages of their male colleagues. Indeed, our estimate of the ratio λ^F/λ^M is roughly 0.89, which is very close to the estimated ratio from our previous designs. Taken together with the evidence from the decompositions in Table 4 and the estimated elasticities in Table 5, we believe there is a strong case in support of the hypotheses that gender differences in bargaining lead to a systematic wage penalty for female workers.

4.1 Heterogeneity

An important insight from the recent negotiation literature is that gender differences in willingness and ability to bargain can have different impacts in different environments. Bowles et al. (2005), for example, argue that gender differences are magnified in settings where there is more situational ambiguity, while Rigdon (2012) argues that lack of social information on co-worker pay is disadvantageous for women. We investigated the potential role for environmental differences in the relative bargaining power of women by fitting equation (9) by instrumental variables to subgroups of men and women, and examining the relative magnitude of the estimated rent-sharing coefficients, λ^Fversus λ^M. A selection of our results is presented in Table 8.

For reference, the first row of the table reproduces the IV results for the entire sample of male and female job stayers from columns 4 and 8 of Table 7. The next set of rows show results by firm size. To aid in interpreting the estimates, we show the fractions of men and women in the different size categories (in columns 2 and 4), and the ratio of the estimated female to male elasticities (in column 5). Among very small firms (under 10 employees in the QP), we find that rent-sharing effects are very small for both women and men. If anything, the estimate of λ is slightly larger for women, but the ratio is very imprecise. For middle-sized firms (11-99 workers) and larger firms (100 or more workers) the estimates of λ^g and their relative magnitude are quite similar to the results for the sample as a whole.

A more interesting pattern emerges when we stratify firms by the fraction of female employees. Among the firms with a relatively low share of female workers (30% or less), the estimated rent-sharing parameters are very similar for male and female employees. Perhaps surprisingly, women appear have lower relative bargaining power at firms where their share of the workforce is larger.

In an effort to develop a proxy for the degree of situational ambiguity in a workplace, we calculated the standard deviation of wages among male workers who were present in the workplace in 2006. We then divided firms into two groups: those with higher dispersion in male wages and those with lower dispersion. Contrary to our initial expectations, we find that that women's relative bargaining power appears to be greater at firms with higher wage inequality among male workers.

We also estimating equation (9) by industry. Three large sectors – manufacturing, trade, and non financial services – account for 75% of male workers and 90% of female workers in the sample, and are reported in Table 8. We find that the bargaining disadvantage of women is relative large in manufacturing, where the elasticity of men's wages with respect to profitability is nearly twice as big as the elasticity of women's wages. In trade, neither gender group has a large response to profitability. In non-financial services the estimated rent sharing coefficients for men and women are both relatively large, but imprecise.

Overall, we conclude that there is likely some important heterogeneity in the relative degree of rent sharing by women versus men. Women appear to be at the greatest disadvantage in larger firms, in manufacturing industries, and in firms with a higher fraction of female employees. Unfortunately, as shown by the standard errors in column 5 of the table, our ability to precisely estimate the relative bargaining power of women is limited, and none of the estimates of the relative ratio of female to male bargaining power are significantly different from 1, though the ratio in manufacturing is marginally so.

5 Summary and Conclusions

A growing body of new research has suggested that the gender gap in wages may be driven in part by differences in the ability and willingness of women to bargain with their employers over wages. As noted by Bertrand (2011), the real test for this class of theories is how much of the overall gender gap it can explain. We have developed a novel way of assessing these theories, using matched longitudinal data that includes wage outcomes for the same worker observed at different employers and firm-level financial information. The key to our approach is the recognition that there is a large and persistent firm-level component in the match surplus that drives bargaining over individual wages. To the extent that women obtain a smaller share of the surplus in their employment relationships, the average wage premium paid to the female workers at a given firm will fall short of the average premium paid to their male co-workers. In fact, we find that women are paid only 85-90% of the wage premium that men earn at more profitable firms. Averaging the shortfall across firms, we conclude that differences in relative bargaining power explain about 10-15% of the gender wage gap in Portugal. We corroborate this estimate using alternative approaches that relate the wage premiums earned by men and women at different firms to a direct measure of firm profitability and by studying the relationship between shocks to firm profitability and the wage growth of incumbent employees. Again, we conclude that 10-15% of the gender wage gap can be attributed to the lower bargaining share earned by female employees.

Our findings contribute to a growing literature that emphasizes the fundamental role of firms in the wage determination process (Lentz and Mortensen, 2010). The efficiency wage literature of the 1980s suggested that employers may offer firm-specific wage premiums, though most of the supporting empirical evidence was based on industry-level data (e.g., Krueger and Summers, 1988; Katz and Summers, 1989). More recent research has followed Abowd, Kramarz and Margolis (1999) in using comprehensive matched worker-firm data. Our findings provide additional evidence on the importance of firm-specific wage policies, and show that a significant share of the gender wage gap arises directly through the firm-level component of wages.

An important question for future research is the extent to which differences in the average wage premiums paid to men and women by the same firm are due to differences in the behavior of female employees, versus differences in how employers interact with their female workers. While some of the difference may be due to a preference among women to avoid bargaining (e.g., the "nice girls don't ask" hypothesis of Babcock and Laschever, 2003), an alternative explanation is that employers and/or coworkers treat women differently when they bargain, imposing differential costs on women who negotiate (Bowles et al., 2007). It seems plausible that the treatment of women in the negotiation process would vary substantially from firm to firm, which may explain the emphasis in the U.S. on crafting legislation prohibiting unequal treatment *within a firm* by gender or race. However, our results suggest that even if the genders were equally effective at bargaining, women would still capture fewer rents by virtue of working at less profitable firms. This source of inequality is important even within industry and occupation cells, suggesting the pattern is not explained by early career choices. Additional research is necessary to determine how these differences emerge over the life cycle.

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Appendix

1. Data Appendix

a. Quadros de Pessoal

The Quadros de Pessoal (QP) dataset for 2002-2009 includes over 20 million observations on 4.5 million workers. Individuals are identified over time with a unique person identifier. Firms are identified by a unique firm id. To construct our analysis sample we drop the entire history for a person if: (1) the hiring date for any job is missing or inconsistent across observations (0.6% of observations dropped); (2) the individual is observed in two consecutive years at different firms, but the hiring date for the second job is the same as the hiring date for the first job (6.9% of observations dropped); the hourly wage in any year is too high or too low (0.3% dropped); the change in the log hourly wage from one year to the next is less than -1 or greater than 1 (1.6% dropped). After these deletions we retain only person-year observations in which the worker is between the ages of 19 to 65 (1.6% of observations dropped), with at least two years of potential labor market experience (i.e., $age - education - 6 \geq 2$) (0.7% dropped) and is employed as a wage-earner (droppping 9.3% of observations). Appendix Table A1 shows the characteristics of the male and female observations in the entire QP, and our analysis sample. Overall the samples are quite similar in terms of age, education, location, mean hourly wage, and mean monthly hours of work.

b. SABI

Bureau van Dijk's SABI data base has annual data for non-financial firms including: a firm tax identifier; balance sheet information (with sales and the value of intermediate inputs); total employment; the firm's name, address, industry, shareholder capital; and date of formation. Data are available from 2000 onward, but coverage expanded substantially in 2005, and information on employment is missing for many firms prior to 2006.

c. Matching QP and SABI

The following variables are common to QP and SABI and can be used to match observations for a given firm in a given year in the two data sets: (1) location – zip code and county (*concelho*) in SABI, parish (*freguesia*) and county in QP; (2) 5 digit industry; (3) year of firm creation; (4) shareholder capital; (5) annual sales. We do not use employment in our matching procedure, but we use it as a check variable.

In QP, total sales in a given year are reported for the previous calendar year. We therefore use sales in year $t-1$ to match observations in year t. In SABI, both sales and shareholder capital are reported in thousands of euro, whereas in QP they are reported in euros. We therefore round both variables in QP to the thousands. Sales and shareholder capital are treated as missing if the reported values are zero. The zip codes reported in SABI were converted to parishes, with the exception of a few codes that cross parish boundaries and a few that appear to be non-existent codes.

We use a multi-step matching procedure which uses exact matching at each stage, and sequentially relaxes the number of variables that have to match exactly. Firms that are matched at one step are removed from both data sets, leaving unmatched observations for the next step. The steps are as follows:

1. Exact matching based on 5 variables: location, industry, year of firm creation, sales and shareholder capital. We first attempt an exact match using sales and shareholder capital for 2009 (the other variables are time-invariant), then work backwards to 2005. We initially use parish and 5-digit industry to look for exact matches. We then repeat the process using county and 3-digit industry.

2. Exact matching based on 4 variables: location, industry, and any two of: year of firm creation, annual sales, or shareholder capital. As in step 1, we initially use parish and 5-digit industry to look for exact matches, then use county and 3-digit industry.

3. Exact matching based on 3 variables: location, industry, and any one of: year of firm creation, annual sales, or shareholder capital. As in step 1, we initially use parish and 5-digit

industry to look for exact matches, then use county and 3-digit industry. In this step, once a potential match was found, we compared data from QP and SABI to check the plausibility of the match. Specifically, we checked annual observations on sales and shareholder capital for 2005-2009. A match was validated only if the deviation between SABI and QP did not exceed 1% in any year for either sales or shareholder capital, or, in cases with a larger deviation in any one year, if the values in all other years were exactly the same in both data sets.

4. Exact matching based on 2 variables: location and any one of industry, year of firm creation, annual sales, or shareholder capital. As in step 3, potential matches were compared and only retained if the same criterion was met.

We matched a total of 301,417 firms between QP and SABI - representing about 80% of the firms that ever appear in SABI, and 53% of firms that appear at least once (with a worker in our analysis sample) in QP from 2002 to 2009. Of the matches, 52% were matched on all five variables, 31% were matched on four variables,12% were matched on three variables, and the remainder were matched on two varables. The match rate by firm size (based on average number of employees in QP) are as follows: 1-10 workers - 50.7%; 11-50 workers - 68.61%, 51-100 workers - 67.0%, 101-500 workers - 69.2%, over 500 workers - 61.0%.

Appendix Table A2 shows the match rates by major industry and by gender, calculated across person-year observation in our main QP analysis sample.

2. Model Fit Appendix

We conducted two analyses to evaluate the fit of the two-way fixed effects models presented in Table 3. In the first analysis, we first partitioned the estimated worker effects into 10 equally sized decile groups. We did the same for the estimated employer effects. We then calculated the mean residuals from equation (4) for 100 cells, defined by the decile of the person effect and the decile of the firm effect. The results are shown graphically in Appendix Figures A1 and A2, for males and females respectively. Note that apart from the cell made up of lowest-wage workers at lowest wage firms (the "1-1" cell), the absolute value of the mean is less than 0.01 in all cells for both gender groups.

In the second analysis we calculated mean wages for job movers, classifying each job by the quartile of the estimated gender-specific firm effect for the job. Appendix Figure A3 shows the profiles of wages for movers in the 2 years before and 2 years after a move. (For simplicity we limit attention to movers with at least 2 years of data before and after a move). In panel a., for example, we show the profiles of wages for men (four left panels) and women (four right panels) who originated at firms in quartile 1 and moved to other firms in quartiles 1 (first row), quartile 2 (second row), quartile 3 (third row) or quartile 4 (fourth row). In each graph we show the mean wage as well as the 25th, 50th and 75th percentiles of wages.

The graphs show four key facts about the wage profiles of movers: (1) wage trends prior to a move are very similar for different origin-destination groups; (2) wages rise significantly for workers who move from a lower quartile firm to a higher quartile firm; (3) wage fall significantly for workers who move from a higher quartile firm to a lower quartile firm; (4) wages are relatively flat for workers who change jobs but remain in the same quartile of firm effects.

Appendix Table A1: Descriptive Statistics for Overall QP and Analysis Sample

	Overall Population of Employees in QP		Analysis Sample	
	Males	Females	Males	Females
	(1)	(2)	(3)	(4)
Age:				
Mean Age	38.9	3702.0	38.1	36.9
Fraction ≤ 30 years old	0.28	0.32	0.30	0.33
Fraction ≥ 50 years old	0.21	0.16	0.19	0.14
Education:				
Mean Years Schooling	8.0	8.8	8.0	8.8
Fraction 6 years schooling	0.22	0.19	0.23	0.20
Fraction 9 years schooling	0.21	0.19	0.21	0.20
Fraction with High School	0.18	0.23	0.18	0.23
Fraction with University Degree	0.10	0.14	0.09	0.13
Mean Log Real Hourly Wage	1.61	1.42	1.59	1.41
(standard dev.)	(0.58)	(0.52)	(0.55)	(0.50)
Mean Monthly Hours	161.9	156.7	162.6	158.0
(standard dev.)	(25.9)	(31.8)	(24.7)	(30.1)
Fraction in Lisbon	0.35	0.36	0.35	0.35
Fraction in Oporto	0.13	0.13	0.13	0.13
Mean Firm Size (Number employees)	668	839	730	858
Fraction Female Workers at Firm	0.25	0.66	0.24	0.70
Number person-year obs.	11,651,615	9,011,089	9,070,492	7,226,310
Number of persons	2,550,576	2,040,863	2,119,687	1,747,492
Number of firms	431,991	391,982	349,692	336,239

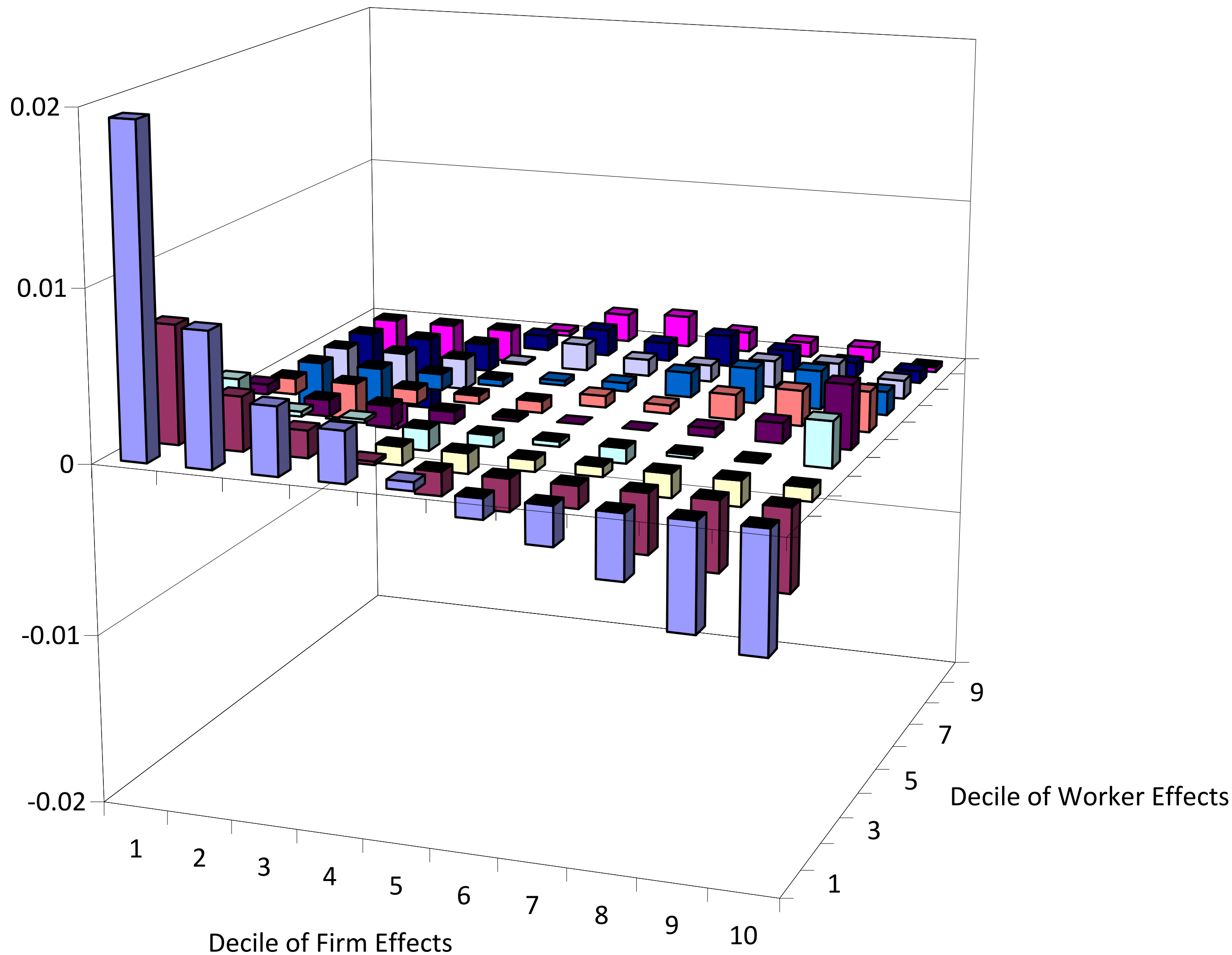
Notes: Overall sample in columns 1-2 includes all observations available in QP with consistent data for age, gender and education. Analysis sample in columns 3-4 excludes individuals with inconsistent employment histories. Person-year observations are also conditioned on being a paid worker in the year, age 19-65, with potential experience ≥2. Wages are measured in real (2009=100) Euros per hour. Lisbon refers to Greater Lisbon and Setubal, Oporto refers to Greater Oporto (NUTS-3 classifications).

Appendix Figure A2: Matching Rates of Observations in QP to Firm Identifier in SABI

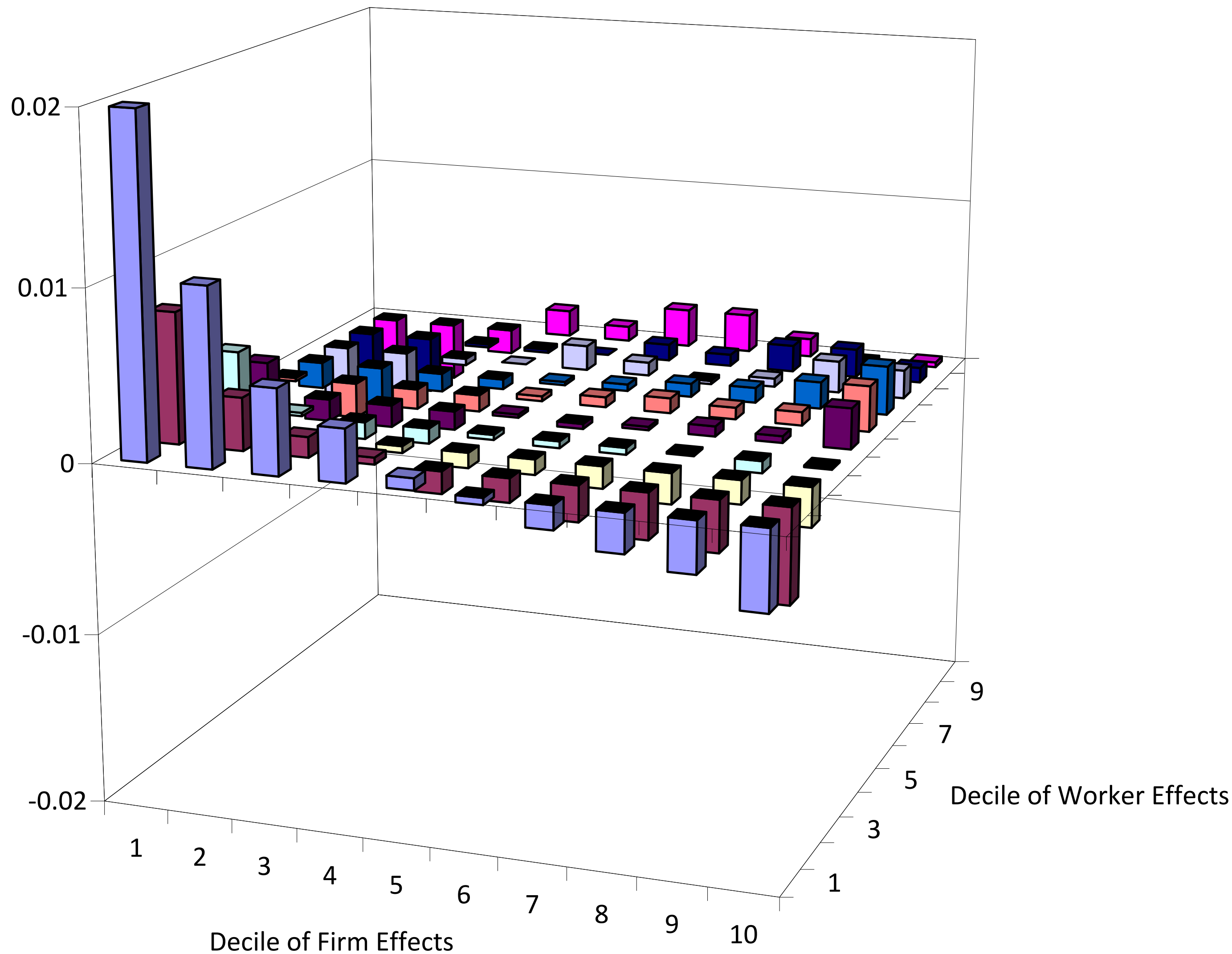
	Percent of All Observations in Industry	Percent of Observations Matched	Percent of Observations from Females	Percent Matched by Gender	
				Male	Female
	(1)	(2)	(3)	(4)	(5)
All Industries	100.0	70.9	44.3	73.3	67.8
Agriculture	1.7	52.0	40.6	50.9	53.7
Fishing	0.1	84.2	27.1	82.2	89.5
Mining	0.4	80.8	9.7	80.5	83.7
Food Products	3.5	75.2	49.1	74.9	75.6
Textiles	8.0	81.3	71.7	80.9	81.5
Wood Products	2.8	78.3	27.0	76.7	82.6
Paper	1.5	79.3	34.5	77.4	82.8
Chemicals	1.8	82.2	34.2	80.8	84.9
Other Mineral Products	2.0	81.4	29.3	81.0	82.4
Metal Fabrication	7.0	80.4	25.8	80.0	81.6
Utilities	0.8	86.5	17.7	87.3	82.7
Construction	12.4	69.5	8.3	69.0	74.1
Trade	19.5	79.4	46.6	79.8	78.9
Hotels	6.6	75.7	62.4	78.2	74.3
Transportation	5.9	71.9	22.9	72.9	68.5
Finance	2.8	27.5	44.4	27.6	27.3
Business Services	10.6	82.6	49.1	83.5	81.6
Education	2.1	42.8	76.2	44.1	42.5
Health	5.9	35.9	88.3	44.1	34.8
Recreation Services	1.0	64.2	44.2	66.4	61.5
Other	3.6	30.6	67.1	32.4	29.7

Note: All statistics are calculated across person-year observations in QP analysis sample for 2002-2009. "Matched" means that employer of person in given year can be matched to firm in SABI. Sample contains 9,070,492 person-year observations for males and 7,226,310 for females.

Appendix Figure A1: Mean Residuals for Males by Decile of Worker and Firm Effects

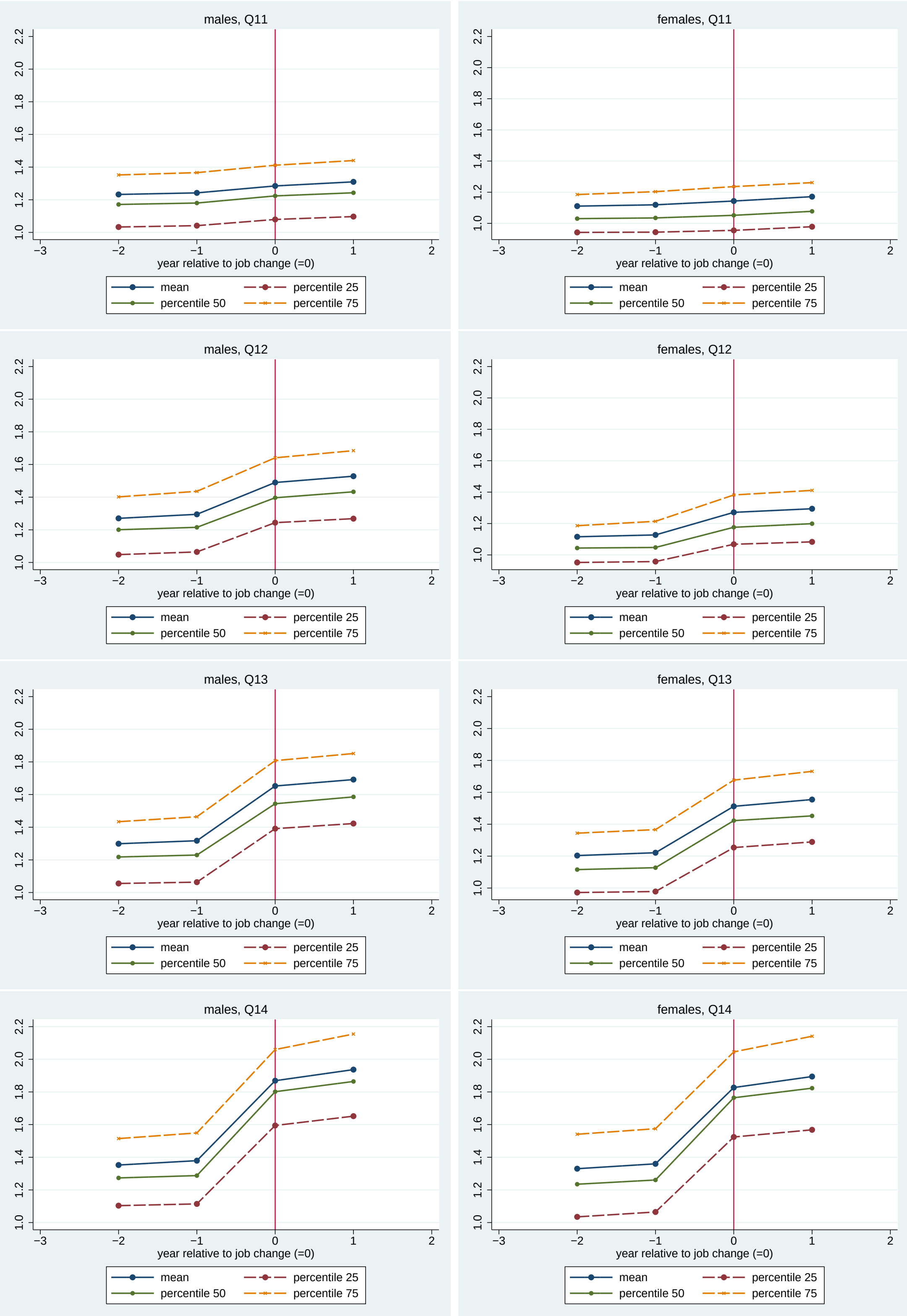


Appendix Figure A2: Mean Residuals for Females by Decile of Worker and Firm Effects

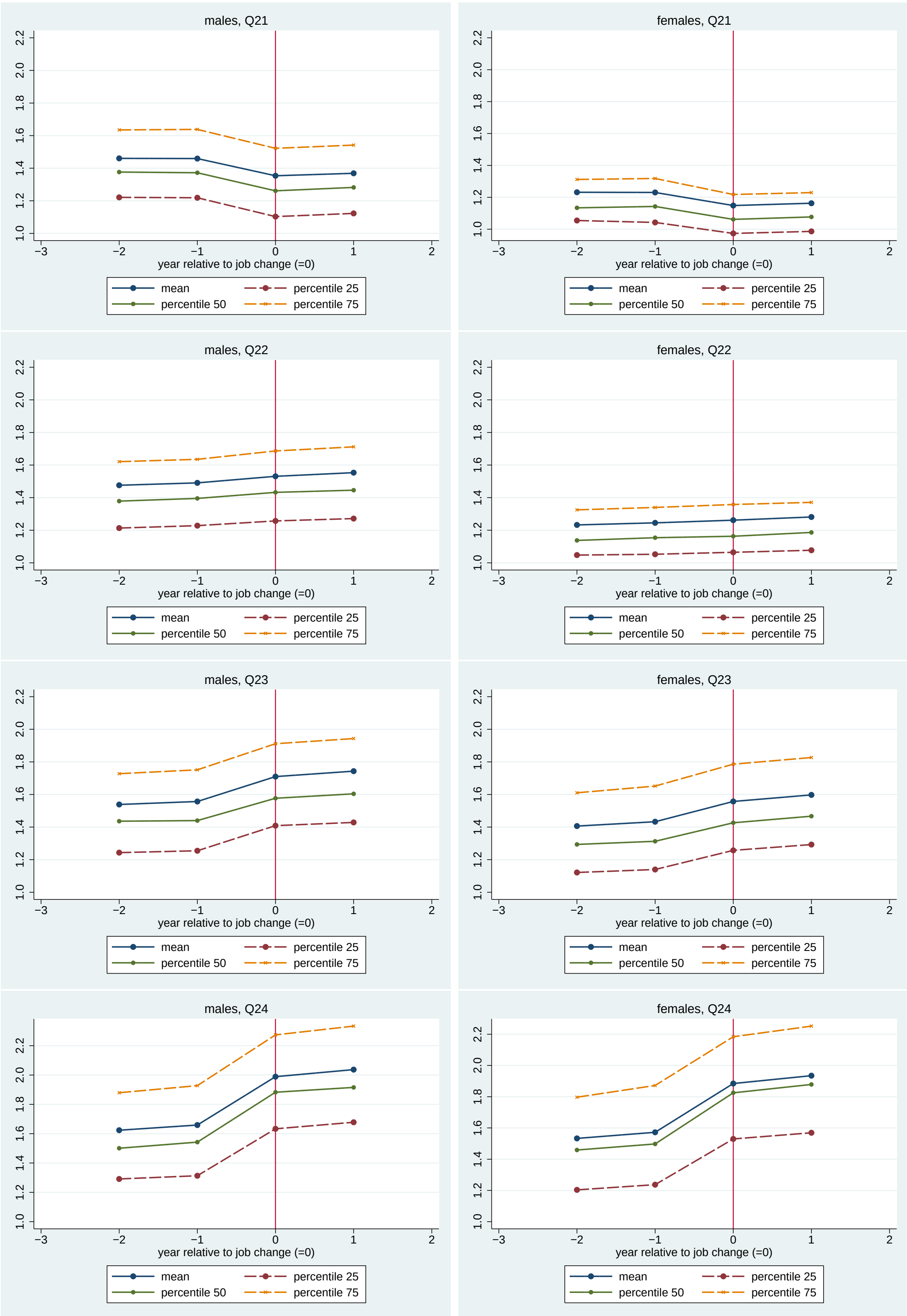


Appendix Figure A3: Wage Profiles of Job Movers Before and After a Move, by Gender and Origin/Destination Quartile of Firm Fixed Effects

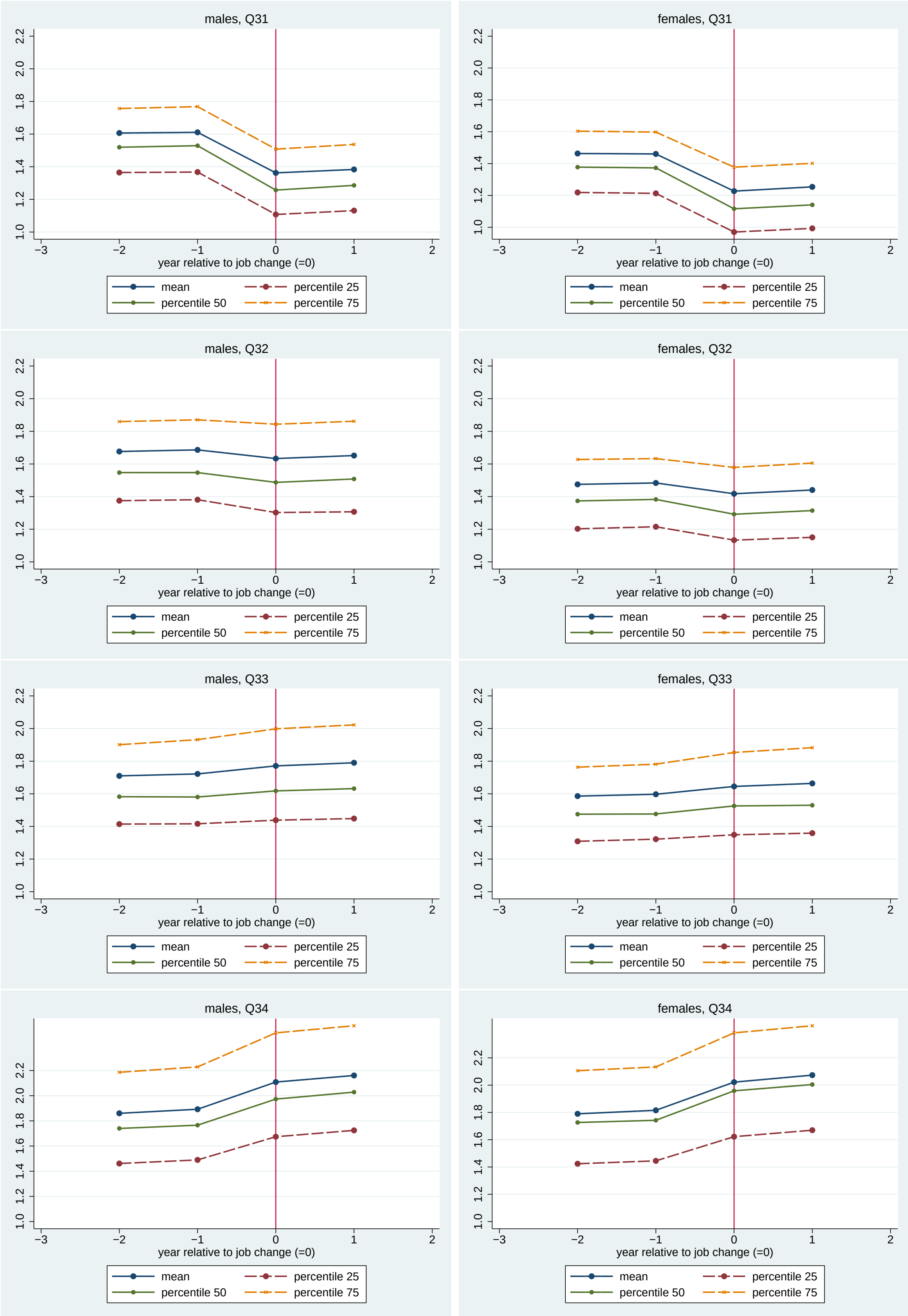
Panel a. Movers Originating in Quartile 1 Firms



Panel b. Movers Originating in Quartile 2 Firms



Panel c. Movers Originating in Quartile 3 Firms



Panel d. Movers Originating in Quartile 4 Firms

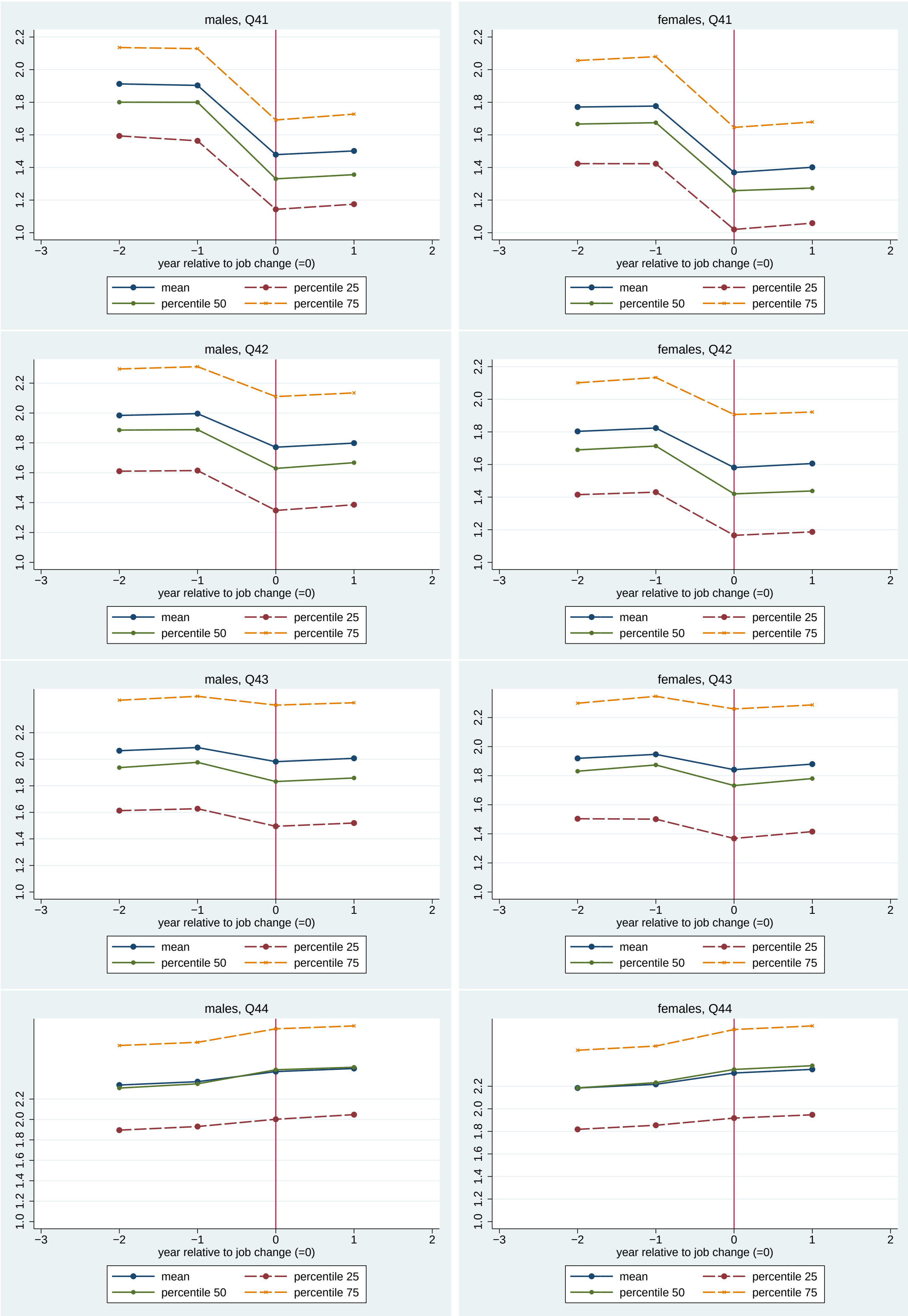


Figure 1: Trends in Real Hourly Wage of Men and Women

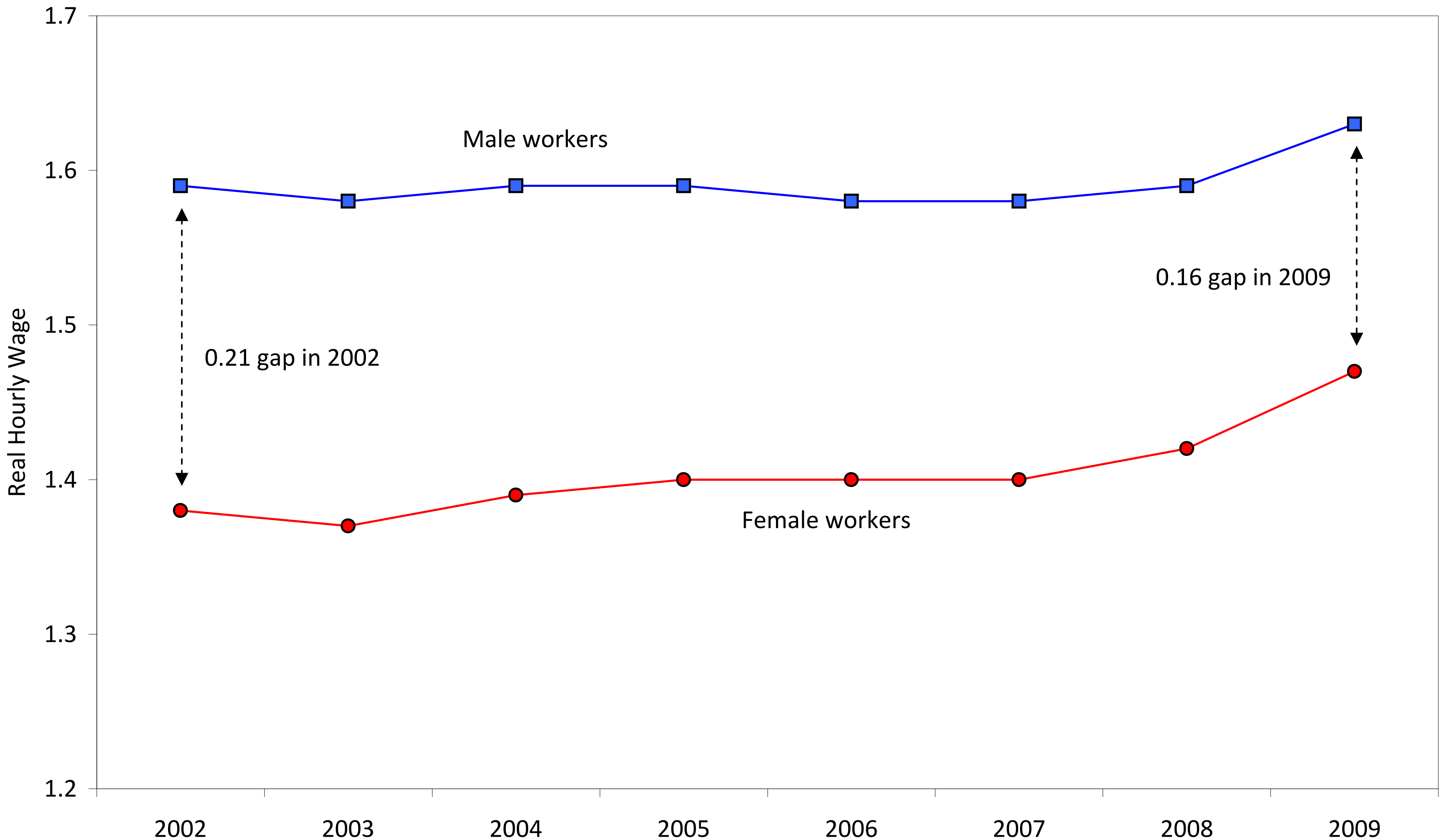


Figure 2a: Wage Profiles of Male and Female Job Movers Originating in Quartile 1

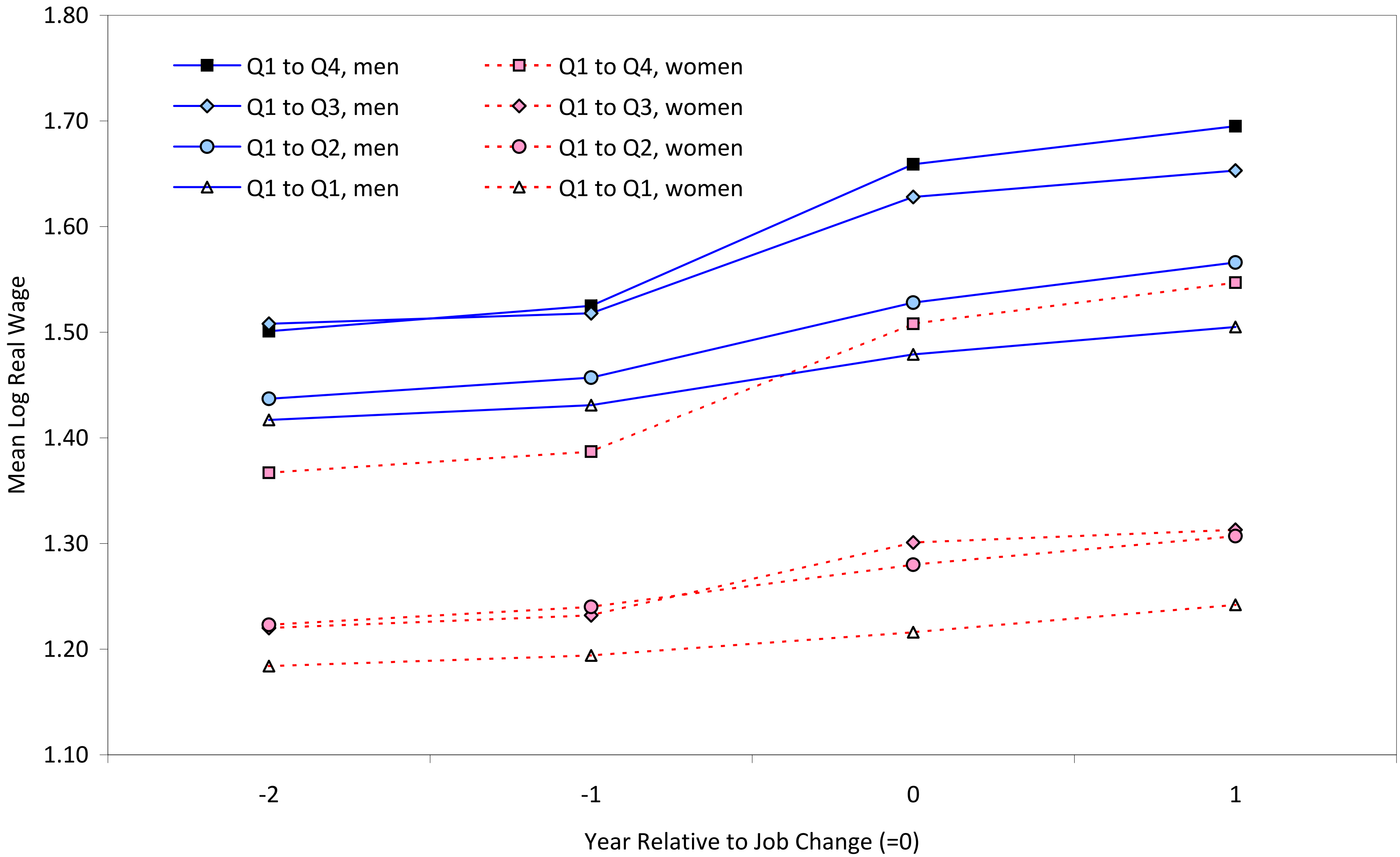


Figure 2b: Wage Profiles of Male and Female Job Movers Originating in Quartile 4

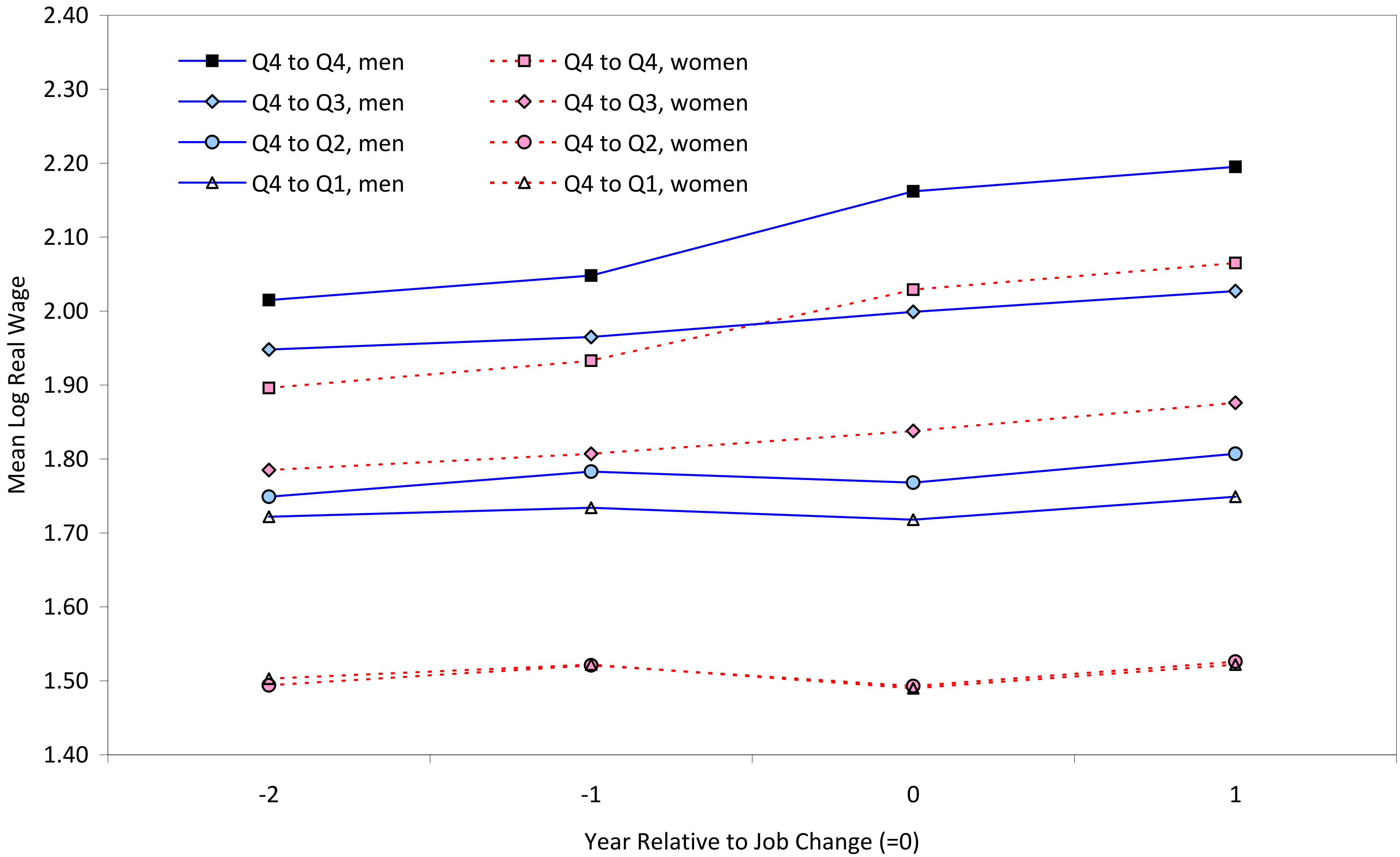


Figure 3: Wage Changes of Women and Men Between Same Job Quality Quartiles

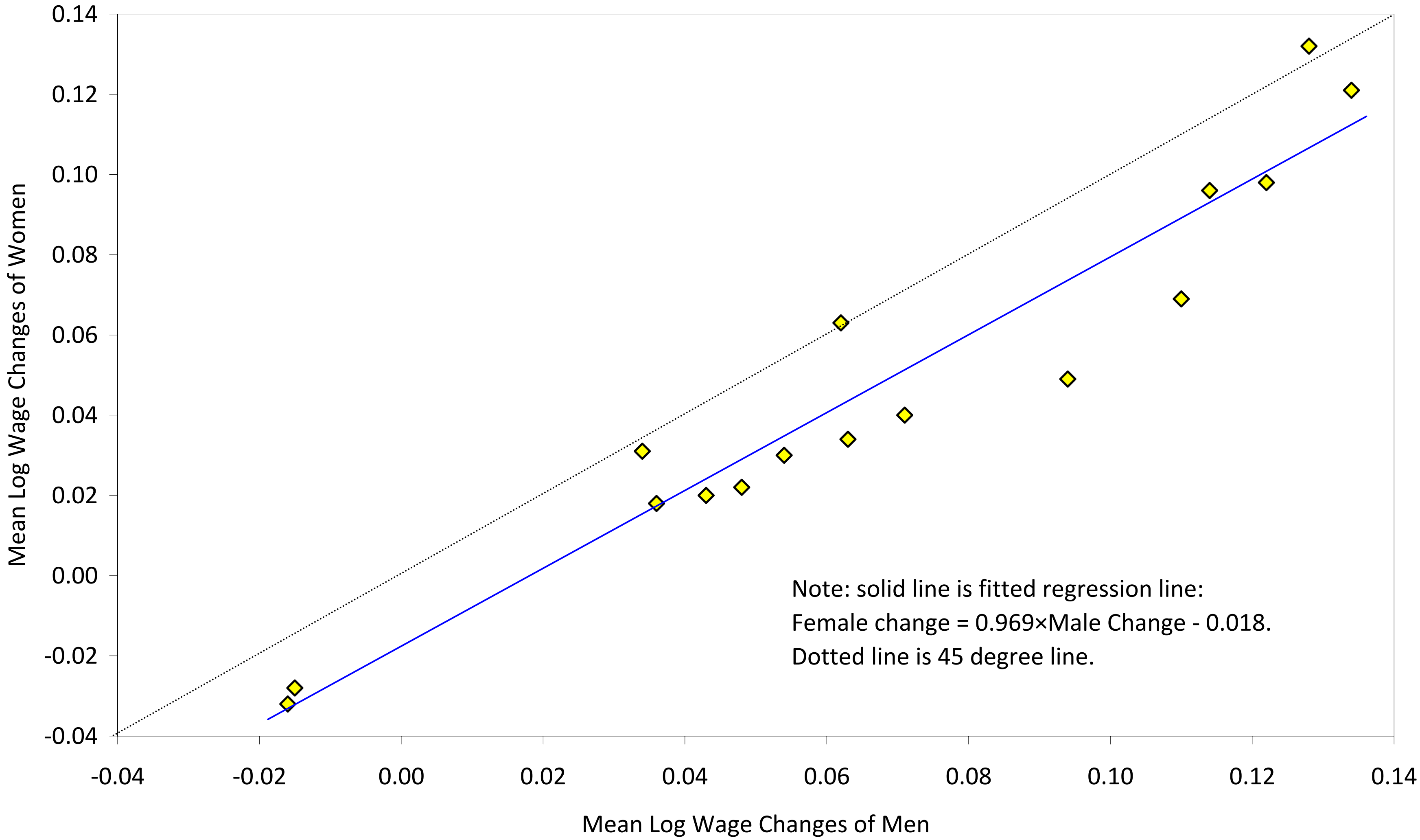


Figure 4: Relative Wage Changes of Women and Men Between Job Quality Quartiles

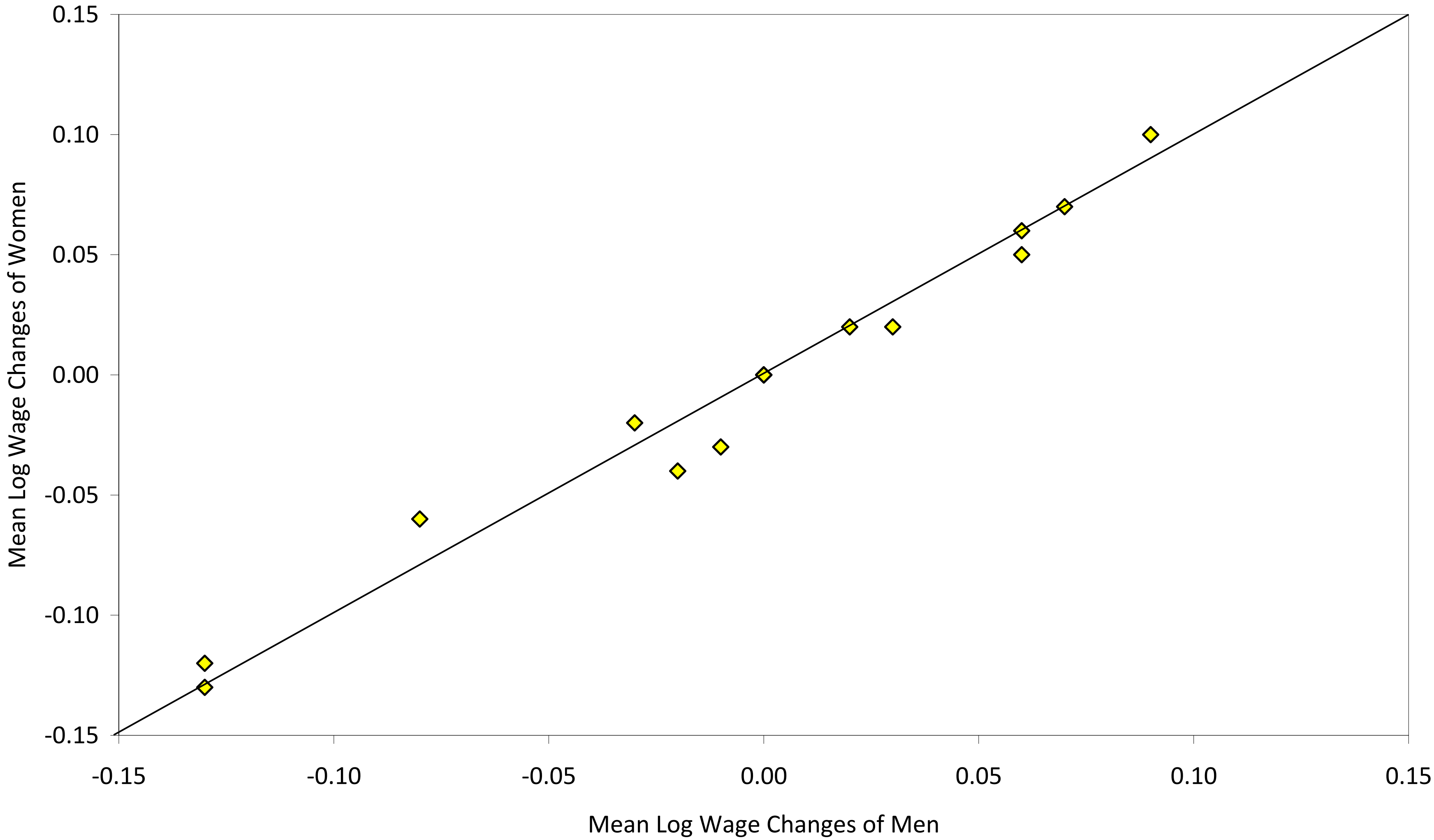


Figure 5: Relationship Between Mean Firm Fixed Effects for Females and Males
By Vingtile of Predicted Firm Quality

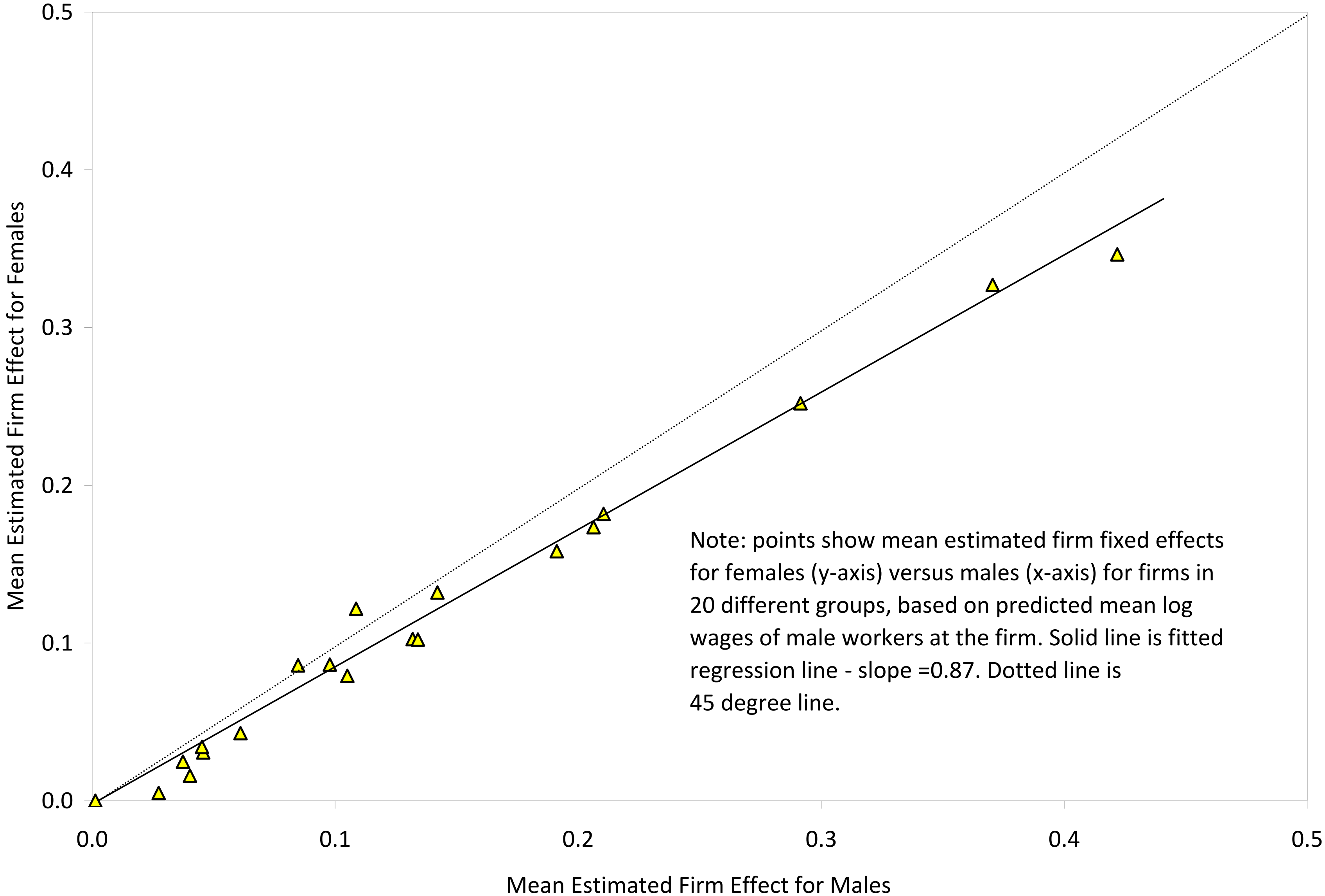


Figure 6a: Relation of Estimated Male and Female Firm Effects to Value Added per Worker

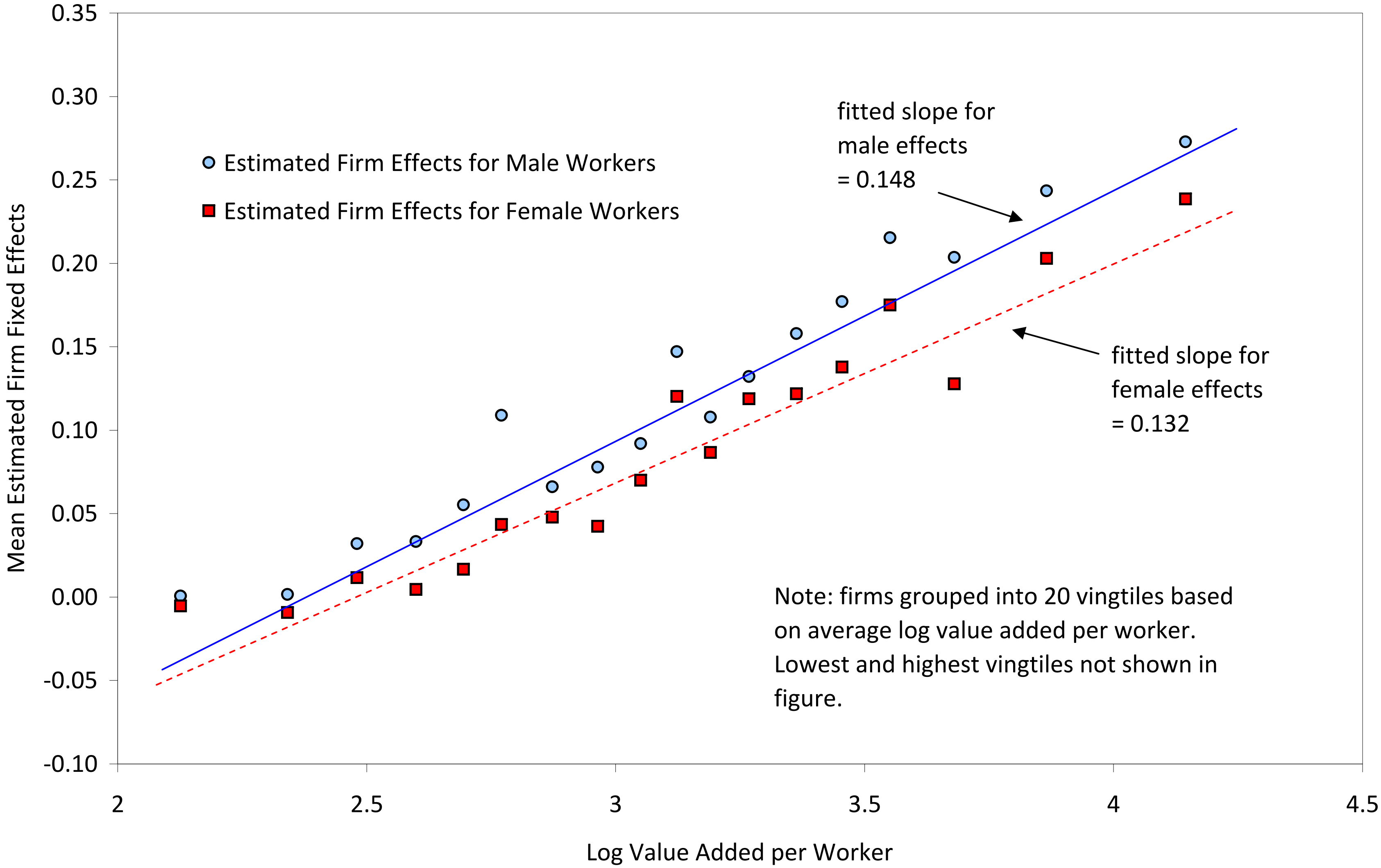


Figure 6b: Relation of Estimated Male and Female Worker Effects to Value Added per Worker

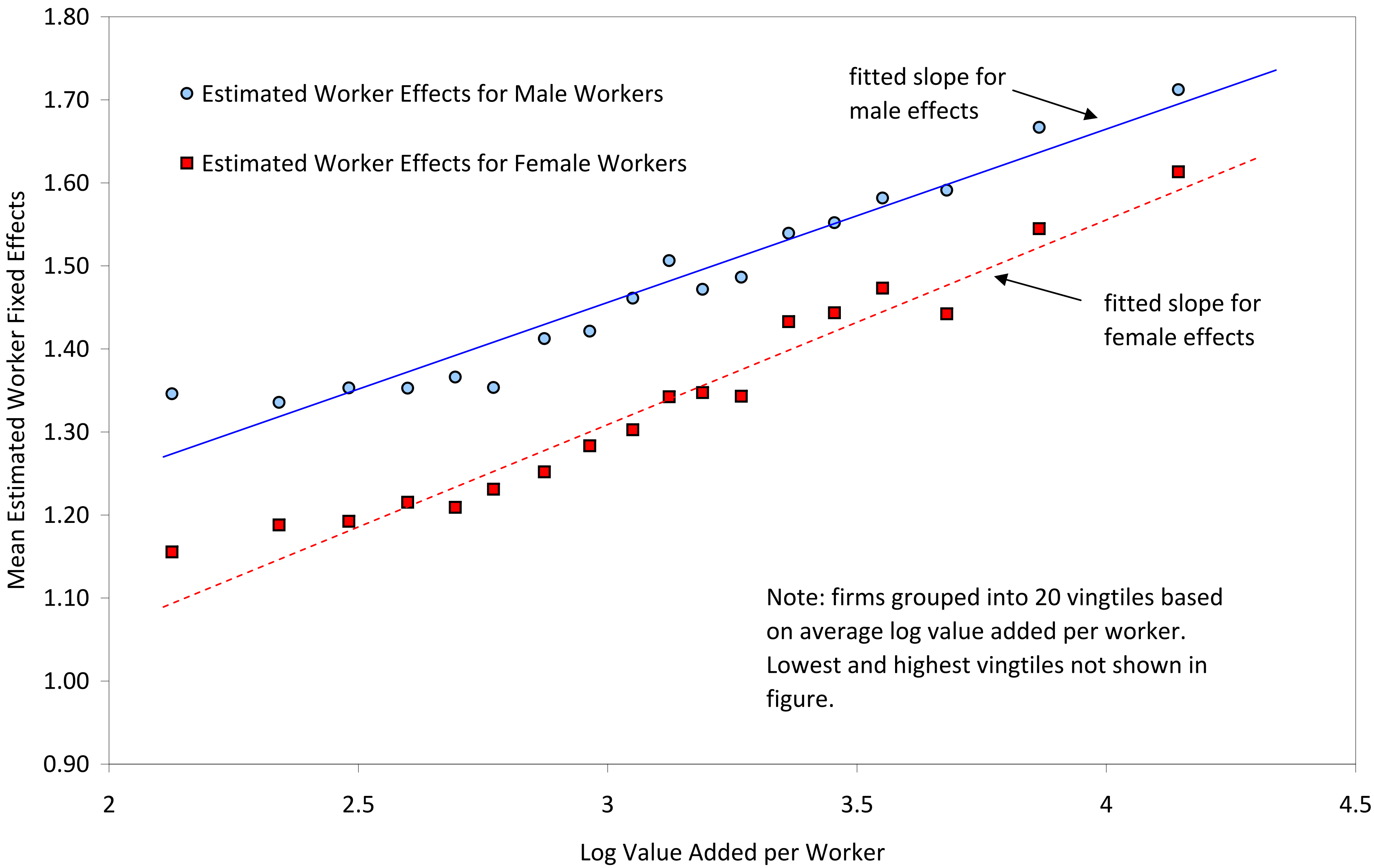


Figure 7: Changes in Wages of Job Stayers vs. Changes in Value Added per Worker

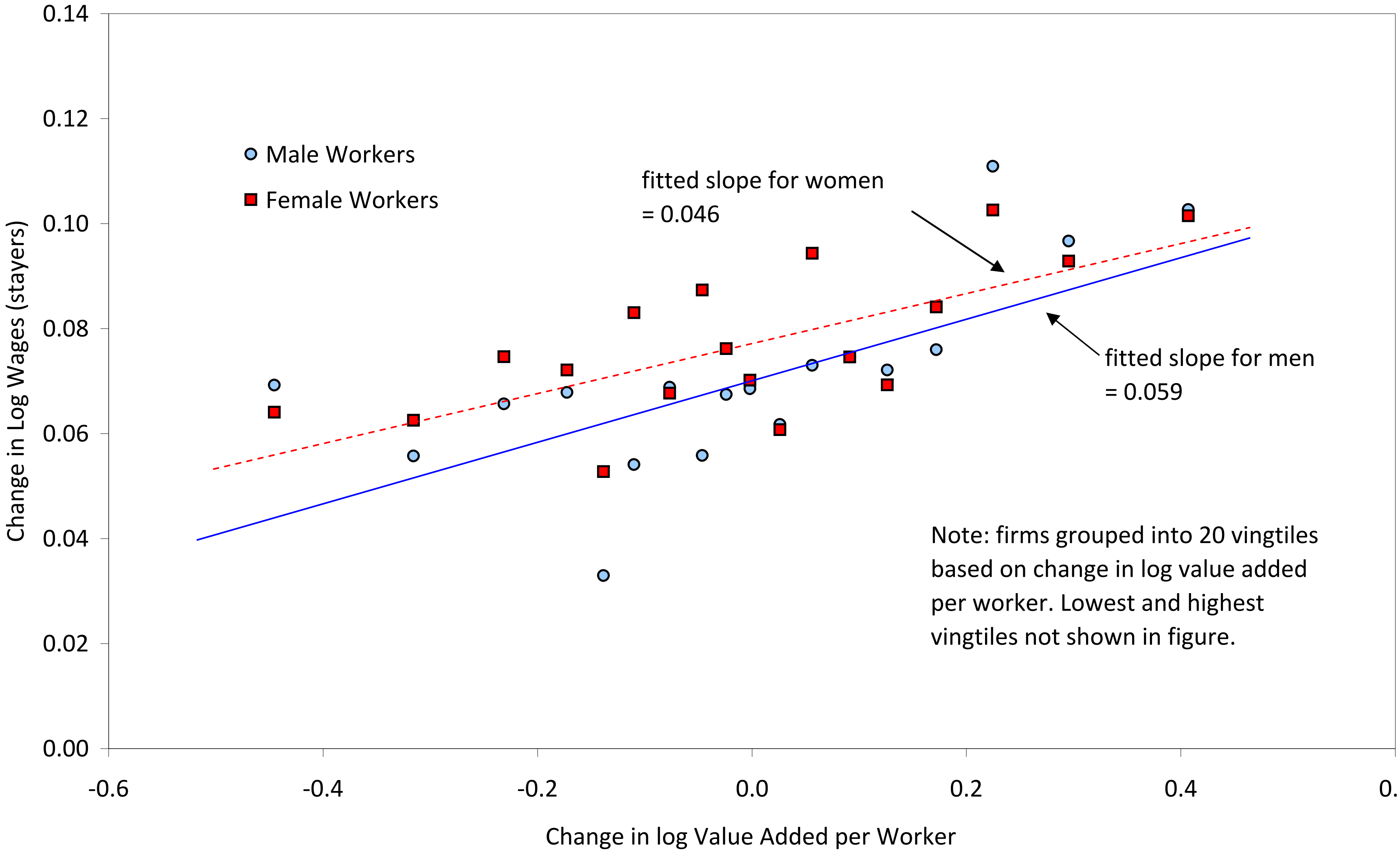


Table 1: Descriptive Statistics for Various Samples of Employees in QP, 2002-2009

	Overall Population of Employees in QP		Connected Set of Workers/Firms: All		Connected Set of Workers/Firms: Dual-Connected		Overall Population with Value Added Data	
	Males	Females	Males	Females	Males	Females	Males	Females
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age:								
Mean Age	38.1	36.9	38.0	36.5	38.0	36.4	38.1	36.7
Fraction ≤ 30 years old	0.30	0.33	0.30	0.34	0.30	0.34	0.30	0.33
Fraction ≥ 50 years old	0.19	0.14	0.18	0.13	0.19	0.13	0.19	0.14
Education:								
Mean Years Schooling	8.0	8.8	8.0	8.9	8.6	9.1	8.1	8.9
Fraction 6 years schooling	0.23	0.20	0.23	0.20	0.20	0.19	0.23	0.20
Fraction 9 years schooling	0.21	0.20	0.21	0.19	0.22	0.19	0.24	0.21
Fraction with High School	0.18	0.23	0.18	0.23	0.21	0.24	0.18	0.25
Fraction with University Degree	0.09	0.13	0.09	0.14	0.11	0.15	0.09	0.12
Mean Log Real Hourly Wage	1.59	1.41	1.62	1.43	1.71	1.48	1.57	1.38
(standard dev.)	(0.55)	(0.50)	(0.55)	(0.51)	(0.58)	(0.53)	(0.50)	(0.45)
Mean Monthly Hours	162.6	158.0	162.5	157.9	162.8	157.1	163.8	159.0
(standard dev.)	(24.7)	(30.1)	(24.8)	(29.9)	(24.0)	(30.5)	(24.5)	(30.8)
Fraction in Lisbon	0.35	0.35	0.36	0.37	0.42	0.40	0.34	0.37
Fraction in Oporto	0.13	0.13	0.13	0.13	0.13	0.13	0.12	0.14
Mean Firm Size (Number employees)	730	858	804	978	1,091	1,230	641	1,117
Fraction Female Workers at Firm	0.24	0.70	0.24	0.70	0.30	0.64	0.24	0.67
Mean Log Value Added per Worker							3.08	2.90
Number person-year obs.	9,070,492	7,226,310	8,225,752	6,334,039	6,012,521	5,012,736	3,337,923	2,454,616
Number of persons	2,119,687	1,747,492	1,889,366	1,505,517	1,450,288	1,247,503	1,210,220	923,857
Number of firms	349,692	336,239	216,459	185,086	84,720	84,720	159,508	147,649

Notes: Overall sample in columns 1-2 includes paid workers age 19-65 with potential experience ≥1. Sampe excludes individuals with inconsistent employment histories. Wages are measured in real (2009=100) Euros per hour. Value added is measured in thousands of real Euros per year. All statistics are calculated across person-year observations. See text for definitions of connected and dual connected sets.

Table 2: Mean Wages of Job Movers Before and After Job Change, By Origin and Destination Job Quality Quartile

	Male Job Changers				Female Job Changers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Q1 to Q1	Q1 to Q2	Q1 to Q3	Q1 to Q4	Q1 to Q1	Q1 to Q2	Q1 to Q3	Q1 to Q4
Mean log wage:								
2 years before move	1.42	1.44	1.51	1.50	1.18	1.22	1.22	1.37
1 year before move	1.43	1.46	1.52	1.53	1.19	1.24	1.23	1.39
year of move	1.48	1.53	1.63	1.66	1.22	1.28	1.30	1.51
1 year after move	1.51	1.57	1.65	1.70	1.24	1.31	1.31	1.55
Wage change at Job Change	0.05	0.07	0.11	0.13	0.02	0.04	0.07	0.12
Trend adjusted Wage Change	**0.00**	**0.02**	**0.06**	**0.09**	**0.00**	**0.02**	**0.05**	**0.10**
Number of obs.	12,409	3,261	2,447	1,066	10,360	2,938	1,931	784
Percent of Movers from Origin Quartile	64.7	17.0	12.8	5.6	64.7	18.3	12.1	4.9
	Q2 to Q1	Q2 to Q2	Q2 to Q3	Q2 to Q4	Q2 to Q1	Q2 to Q2	Q2 to Q3	Q2 to Q4
Mean log wage:								
2 years before move	1.47	1.53	1.55	1.58	1.29	1.34	1.37	1.39
1 year before move	1.50	1.55	1.56	1.60	1.30	1.35	1.39	1.41
1 year after move	1.53	1.61	1.66	1.72	1.32	1.39	1.44	1.51
2 years after move	1.56	1.64	1.69	1.77	1.35	1.41	1.47	1.54
Wage change at Job Change	0.04	0.06	0.09	0.12	0.02	0.03	0.05	0.10
Trend adjusted Wage Change	**-0.03**	**0.00**	**0.03**	**0.06**	**-0.02**	**0.00**	**0.02**	**0.06**
Number of obs.	3,068	9,863	3,357	1,674	2,032	7,779	2,179	1,333
Percent of Movers from Origin Quartile	17.1	54.9	18.7	9.3	15.3	58.4	16.4	10.0
	Q3 to Q1	Q3 to Q2	Q3 to Q3	Q3 to Q4	Q3 to Q1	Q3 to Q2	Q3 to Q3	Q3 to Q4
Mean log wage:								
2 years before move	1.69	1.59	1.82	1.84	1.34	1.37	1.59	1.68
1 year before move	1.70	1.61	1.83	1.85	1.35	1.38	1.61	1.70
1 year after move	1.74	1.66	1.89	1.98	1.37	1.41	1.67	1.83
2 years after move	1.77	1.69	1.92	2.02	1.38	1.45	1.70	1.87
Wage change at Job Change	0.04	0.05	0.06	0.13	0.02	0.03	0.06	0.13
Trend adjusted Wage Change	**-0.02**	**-0.01**	**0.00**	**0.07**	**-0.04**	**-0.03**	**0.00**	**0.07**
Number of obs.	1,808	3,010	13,746	4,296	1,391	2,274	9,763	3,422
Percent of Movers from Origin Quartile	7.9	13.2	60.1	18.8	8.3	13.5	57.9	20.3
	Q4 to Q1	Q4 to Q2	Q4 to Q3	Q4 to Q4	Q4 to Q1	Q4 to Q2	Q4 to Q3	Q4 to Q4
Mean log wage:								
2 years before move	1.72	1.75	1.95	2.02	1.50	1.49	1.79	1.90
1 year before move	1.73	1.78	1.97	2.05	1.52	1.52	1.81	1.93
1 year after move	1.72	1.77	2.00	2.16	1.49	1.49	1.84	2.03
2 years after move	1.75	1.81	2.03	2.20	1.52	1.53	1.88	2.07
Wage change at Job Change	-0.02	-0.01	0.03	0.11	-0.03	-0.03	0.03	0.10
Trend adjusted Wage Change	**-0.13**	**-0.13**	**-0.08**	**0.00**	**-0.13**	**-0.12**	**-0.06**	**0.00**
Number of obs.	615	1,261	3,532	6,435	487	959	2,062	3,894
Percent of Movers from Origin Quartile	5.2	10.6	29.8	54.3	6.6	13.0	27.9	52.6

Notes: see text. Sample includes male and female workers at firms in the "doubly connected" set (connected for both male and female workers), observed between 2002 and 2009 with 2+ years of wage data at the origin firm, 2+ years of data at the destination firm, and observed between consecutive years at origin and destination firms. Column headings (e.g. Q1 to Q2) refer to "job quality" quartile of origin and destination firms. Job quality is based on quartiles of predicted male wages, from regression on industry and location dummies and quadratic in firm size. Trend adjusted wage change is wage change relative to change for job movers from firms in origin quartile group who move to other firms in the same quartile.

Table 3: Summary of Estimated Two-way Fixed Effects Models for Male and Female Workers

	Male Workers (1)	Female Workers (2)
Standard deviation of log wages	0.554	0.513
Number of person-year observations	8,225,752	6,334,039
Summary of Parameter Estimates:		
Number person effects	1,889,366	1,505,517
Number firm effects	216,459	185,086
Std. dev. of person effects (across person-yr obs.)	0.420	0.400
Std. dev. of firm effects (across person-yr obs.)	0.247	0.213
Std. dev. of Xb (across person-yr obs.)	0.069	0.059
Correlation of person/firm effects	0.167	0.152
RMSE of AKM model	0.143	0.125
Adjusted R-squared of AKM model	0.934	0.940
Comparison job-match effects model:		
Number of job-match effects	2,689,648	2,087,590
RMSE of match-effects model	0.128	0.113
Adjusted R-squared of match-effects model	0.946	0.951
Std. deviation of job match effect	0.062	0.054
Inequality decomposition of two-way fixed effects model:		
Share of variance of log wages due to:		
person effects	57.6	61.0
firm effects	19.9	17.2
covariance of person and firm effects	11.4	9.9
Xb and associated covariances	6.2	7.5
residual	4.9	4.4

Notes: See text. Model includes dummies for individual workers and individual firms, year dummies interacted with education dummies, and quadratic and cubic terms in age interacted with education dummies (total of 44 parameters). Comparison job-match effects model includes dummies for each worker-firm job match as well as other covariates in basic model.

Table 4: Decompositions of the Gender Wage Gap

	Mean Female Log Wage (1)	Mean Male Log Wage (2)	Mean Gender Wage Gap (3)
Actual Data:			
1. Mean Log Wage (Workers in dual-connected set)	1.481	1.715	-0.234
Counterfactuals:			
2. Assuming **Male** Firm-specific Wage Premiums	1.516	1.715	-0.199
(percent of raw gender gap explained)			(14.9)
3. Assuming **Female** Firm-specific Wage Premiums	1.481	1.692	-0.211
(percent of raw gender gap explained)			(10.0)
4. Assuming **Male** Distribution Across Firms	1.528	1.715	-0.188
(percent of raw gender gap explained)			(19.9)
5. Assuming **Female** Distribution Across Firms	1.481	1.680	-0.199
(percent of raw gender gap explained)			(15.0)

Notes: counterfactuals based on estimated two-way fixed effects models described in Table 3.

Table 5: Estimated Relatioships Between Firm and Worker Effects and Mean Value Added Per Worker

	Estimated Male Effects		Estimated Female Effects	
	(1)	(2)	(3)	(4)
Panel A: Models for Estimated Gender-specific Firm Effects:				
1. Log value added per worker	0.145	0.130	0.115	0.099
(mean value at firm in all years)	(0.006)	(0.005)	(0.006)	(0.007)
2. Controls for Industry, Size, Location	no	yes	no	yes
3. R-squared	0.166	0.228	0.147	0.198
Panel B: Models for Estimated Worker Effects:				
1. Log value added per worker	0.219	0.200	0.227	0.186
(mean value at firm in all years)	(0.013)	(0.012)	(0.012)	(0.009)
2. Controls for Industry, Size, Location	no	yes	no	yes
3. R-squared	0.092	0.122	0.137	0.227

Notes: model estimated on person-year observations (n=2,888,701 for men, n=3,062,812) for men and women in dual-connected set whose current firm can be matched to financial data. Log of value added is mean value for firm in all years observed. Standard errors clustered by firm (47,477 clusters in all models). See text for description of sample.

Table 6: Descriptive Statistics for Job Stayers (2006-2009) at Firms with Complete Financial Data

	Male Workers		Female Workers	
	2006	2009	2006	2009
	(1)	(2)	(3)	(4)
Mean Age	38.25	41.25	37.24	40.24
Mean Education	8.03	8.51	8.51	8.59
Mean Firm Size (workers in QP)	624	723	1011	1413
Mean Fraction of Females at Firm	0.29	0.29	0.59	0.59
Mean Log Real Hourly Wage	1.63	1.70	1.40	1.48
(standard deviation)	(0.49)	(0.49)	(0.45)	(0.45)
Mean Log Value Added per Worker	3.20	3.19	3.00	2.98
(standard deviation)	(0.57)	(0.60)	(0.60)	(0.62)
Number of Observations	287,707		203,993	
Number of Firms	33,626		33,626	

Note: sample contains workers at dual connected firms with financial data for 2006-2009 who are employed at the firm continuously from 2006 to 2009.

Table 7: Models for the Change in Wage of Job Stayers (2006-2009) at Firms with Complete Financial Data

	Male Workers				Female Workers			
	OLS	OLS	First Stage Model	IV	OLS	OLS	First Stage Model	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Change in Log Value Added per Worker	0.029	0.028	--	0.056	0.019	0.018	--	0.050
(2006 to 2009)	(0.005)	(0.005)		(0.013)	(0.004)	(0.004)		(0.015)
Change in Log Value Added per Worker	--	--	0.326	--	--	--	0.350	--
(2007 to 2008)			(0.005)				(0.006)	
Controls for Age and Age-squared	no	yes	yes	yes	no	yes	yes	yes
RMSE	0.182	0.182	0.352	0.181	0.159	0.157	0.351	0.158
Number of Observations	287,707				203,993			
Number of Firms	33,626				33,626			

Note: sample contains workers at dual connected firms with financial data for 2006-2009 who are emplyed at the firm continuously from 2006 to 2009. Dependent variable is all columns except columns 3 and 7 is change in log real hourly wage from 2006 to 2009. Dependent variable in first stage models in columns 3 and 7 is change in log real value added from 2006 to 2009. In IV models (columns 4, 8) instrument for change in log value added is change from 2007 to 2008.

Table 8: Heterogeneity Across Subgroups in the Relative Effect of Value Added on Wages of Job Stayers

	Males		Females		Relative Rent Sharing Coefficient (col. 3 ÷ col. 1)
	Effect of Value Added on Wages	Percent of All Males in Subgroup	Effect of Value Added on Wages	Percent of All Females in Subgroup	
	(1)	(2)	(3)	(4)	(5)
1. Baseline Model (col. 3/6 of Table 7)	0.056	100	0.050	100	0.89
	(0.013)		(0.015)		(0.23)
2. By firm size (# in QP):					
a. 1-10 employees	0.014	11	0.034	12	2.48
	(0.015)		(0.015)		(2.29)
b. 11-99 employees	0.062	47	0.056	44	0.90
	(0.013)		(0.013)		(0.20)
c. 100 or more employees	0.054	42	0.046	43	0.86
	(0.031)		(0.034)		(0.55)
3. By share of female employees at firm:					
a. 30% or less	0.064	60	0.064	15	1.00
	(0.021)		(0.020)		(0.32)
b. 31-69%	0.043	34	0.036	45	0.83
	(0.015)		(0.013)		(0.31)
c. 70% or more	0.121	6	0.060	40	0.50
	(0.075)		(0.045)		(0.55)
4. By dispersion in male wages at firm in 2006 (missing if no male workers at firm in 2006):					
a. Higher dispersion	0.044	45	0.049	50	1.12
	(0.017)		(0.023)		(0.52)
b. Lower dispersion	0.070	52	0.061	38	0.88
	(0.020)		(0.027)		(0.32)
4. By industry:					
a. Manufacturing	0.060	38	0.031	41	0.51
	(0.020)		(0.022)		(0.28)
b. Trade	0.023	27	0.037	34	1.57
	(0.015)		(0.014)		(0.86)
b. Non-financial services	0.064	12	0.073	16	1.14
	(0.036)		(0.042)		(0.67)

Note: see notes to Table 7. Effect of value added is estimated from IV model fit to change in wages between 2006 and 2009, with controls for age and age-squared. Instrument is change in value added between 2007 and 2008. Standard errors clustered by firm in parentheses. Standard error of ratio estimated by delta method.