BARGAINING, SORTING, AND THE GENDER WAGE GAP: QUANTIFYING THE IMPACT OF FIRMS ON THE RELATIVE PAY OF WOMEN*

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There is growing evidence that firm-specific pay premiums are an important source of wage inequality. These premiums will contribute to the gender wage gap if women are less likely to work at high-paying firms or if women negotiate (or are offered) worse wage bargains with their employers than men. Using longitudinal data on the hourly wages of Portuguese workers matched with income statement information for firms, we show that the wages of both men and women contain firm-specific premiums that are strongly correlated with simple measures of the potential bargaining surplus at each firm. We then show how the impact of these firm-specific pay differentials on the gender wage gap can be decomposed into a combination of sorting and bargaining effects. We find that women are less likely to work at firms that pay higher premiums to either gender, with sorting effects being most important for low- and middle-skilled workers. We also find that women receive only 90% of the firm-specific pay premiums earned by men. Importantly, we find the same gender gap in the responses of wages to changes in potential surplus over time. Taken together, the combination of sorting and bargaining effects explain about one-fifth of the cross-sectional gender wage gap in Portugal. *JEL* Codes: J16, J31, J71.

I. Introduction

Despite rapid advances in the educational attainment and job experience of women, there is still a substantial gender wage gap in most countries (OECD 2015a). Though some analysts argue that the gap is primarily driven by male-female differences in productivity (e.g., Mulligan and Rubinstein 2008), a more

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expansive view, consistent with models of frictional labor markets (e.g., Manning 2011) is that equally productive men and women also face different job prospects and strike different wage bargains with their employers. Concern for such possibilities permeates the legal system in both the United States and the European Union, where laws require equal access to job openings for men and women and equal treatment of male and female employees within a firm.

Two long-established strands of research suggest that firm-specific pay policies may in fact be important for understanding the gender wage gap. One focuses on potential differences in the fractions of men and women employed at different firms (Blau 1977; Groshen 1991; Petersen and Morgan 1995), and in the rates that men and women move to higher-paying jobs (e.g., Loprest 1992; Hospido 2009; Del Bono and Vuri 2011). The other emphasizes the wage-setting power of firms and the possibility that women are offered (or negotiate) systematically lower wages at a given firm.[1] These studies point to two complementary channels for generating gender disparities: a sorting channel that arises if women are less likely to be employed at higher-wage firms, and a bargaining channel that arises if women obtain a smaller share of the surplus associated with their job.

In this article we provide the first comprehensive analysis of the impact of firm-specific pay premiums on the gender wage gap, using matched worker-firm data from Portugal merged with financial information for employers.[2] Building on a simple rent-sharing model, we develop an approach to measuring the sorting and bargaining channels via an Oaxaca-style decomposition (Oaxaca 1973; Fortin Lemieux, and Firpo 2011) of gender-specific firm wage effects. Like Abowd, Kramarz, and Margolis (1999; hereafter AKM), our model includes fixed effects for individual

1. Robinson's (1933) monopsonistic wage-setting model was motivated in part by trying to explain why a firm might pay lower wages to women than men. As pointed out by Barth and Dale-Olsen (2009), this framework has been largely ignored in the gender wage literature. Lang and Lehmann (2012) discuss models of employer wage setting in a racial discrimination context. Bertrand (2011) presents a review of work emphasizing the relative negotiating abilities of men and women.

2. 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 nonrandom sorting of men and women with different unobserved skill characteristics to more profitable firms. Barth and Dale-Olsen (2009) examine firm-specific gender wage differences in a monopsony framework.

workers and fixed effects for employers that measure the wage premium paid by each firm relative to some reference firm or group of firms. A key issue for assessing the contribution of the bargaining channel is the need to define the relevant reference group for each gender (Oaxaca and Ransom 1999). We use the mapping between measures of the potential surplus at each firm and the estimated wage premiums to define a normalization that yields a lower bound estimate of the differential bargaining power of women. We verify our results using an a priori assumption on the degree of rents available in the hotel and restaurant sector, a traditionally low-wage industry.

Since our analysis builds directly on AKM's assumption that different firms pay different wage premiums relative to the overall labor market, we begin our empirical analysis by providing some descriptive evidence on the presence of these premiums and on the plausibility of the exogenous mobility assumptions needed to measure them via ordinary least squares (OLS) methods. Corroborating earlier exercises by Card, Heining, and Kline (2013) with German data, and by Macis and Schivardi (2013) with Italian data, we find that these assumptions are approximately satisfied for both men and women in Portugal. Comparing the average wage gains and losses for men and women who move between matched sets of firms, we also show that women benefit less from firm-to-firm mobility than do men.

We then estimate separate AKM models for male and female workers in Portugal. We find that firm-specific pay premiums explain about 20% of wage variation among both men and women, while positive assortative matching (i.e., the positive covariation between worker and firm effects) explains another 10%. We also show that the pay premiums offered to men and women are highly correlated across firms. We use a simple decomposition method to assess the contribution of firm-specific wage setting to the overall gender wage gap, and to the wage gap among workers with different levels of age and education and in different occupations and industries. Overall we find that the underrepresentation of women at firms that offer higher wage premiums for both gender groups—the sorting effect—explains about 15% of the overall 23 log point gender gap in Portugal. Another 5% is attributable to the fact that women gain less than men from higher-wage firms—the bargaining effect. We find that sorting effects rise with age and are more important among less educated workers, whereas bargaining effects are larger for highly educated workers. Both components vary by occupation, with the largest contribution of sorting for traditional skilled and semiskilled blue-collar jobs and clerical jobs. To check whether the relative pay of women is distorted by the minimum wage, we also conduct a separate analysis of workers with at least a high school education.

We then narrow our focus to the component of the firm-specific wage premiums paid to men and women that is directly related to a simple proxy for the average bargaining surplus available at each firm. We find that women's wages are only 90% as responsive to observable measures of the surplus per worker as are men's, and we can easily reject the hypothesis of equal responsiveness, thus confirming that women gain a smaller share of firm-wide rents than do their male coworkers. We also confirm that women are underrepresented at firms with higher measured surplus levels. Bargaining and sorting based on the observable component of surplus account for about 80% of the overall impact of firm-specific pay premiums on the gender wage gap.

As a final step in our analysis, we examine the effects of changes in the average surplus per worker on the wages of men and women who remain with the firm over a multiyear period. This approach, which mirrors the design employed in the modern rent-sharing literature (e.g., Guiso, Pistaferri, and Schivardi 2005; Carlsson, Messina, and Skans 2014; Card, Devicienti, and Maida 2014), uses an entirely different component of wage variation than our analysis of firm-specific pay premiums. Reassuringly, we obtain a nearly identical 90% estimate of women's relative bargaining power, suggesting that a simple wage setting model with gender-specific bargaining parameters can successfully explain both the between-firm structure of relative wages for men and women and the variation over time at a given firm in male and female wages.

II. Firm-Specific Determinants of the Gender Wage Gap

In competitive labor market models, wages are determined by market-level supply and demand factors rather than by the wage-setting policies of particular firms.[3] This perspective is

3. Wages can vary across firms if there are market-based compensating differentials for firm-wide amenities or disamenities, such as long hours of work

central to Becker's (1957) model of employer-based discrimination, which asserts that the market-wide discriminatory wage premium depends on the preferences of the marginal employer of women.[4] Building on this framework, most studies of the gender wage gap focus on measured skill differences between men and women and attribute any unexplained component to a combination of discriminatory factors and unobserved skill gaps (see Altonji and Blank 1999; Blau and Kahn 2000, for reviews).[5]

Despite the market-level focus of most economic studies, legislation aimed at eliminating gender discrimination is primarily directed at firms. In the United States, for example, the Equal Pay Act requires that employers offer equal pay to men and women for "substantially equal" work, while Title VII of the Civil Rights Act prohibits firms from discriminating against women (and other protected groups) in decisions over hiring, layoffs, and promotions. In Portugal, articles 9, 13, 26, and 59 of the constitution ban discrimination and guarantee equality of access to jobs and the right to equal pay for equal work.

To the extent that firms have some control over the wages offered to a given worker, the average wages of women relative to men will be affected by two factors. The first is whether firms that tend to pay higher wages are more or less likely to hire women. The second is whether firms offer different average wage premiums for men and women relative to the "market" (or a reference employer).

The potential importance of the between-firm sorting channel to the gender wage gap was noted by Blau (1977), who used wage data for white-collar workers at different establishments in three cities and concluded that establishments with higher average wages tended to employ fewer women. Subsequent research, including Groshen (1991), Petersen and Morgan (1995), and Bayard et al. (2003), suggests that the differential sorting of women and men to higher and lower paying workplaces explains

(Bertrand, Goldin, and Katz 2010). We examine the correlation between firm-specific wage variation and average hours of work later in the article.

4. See Charles and Guryan (2008, 2011) for an application to the black-white wage gap and reviews of related work.

5. An interesting exception is audit-based studies of potential discrimination (e.g., Heckman and Siegelman 1993; Neumark, Bank, and Van Nort 1996; Bertrand and Mullainathan 2004), which focus on the hiring practices of individual employers.

some fraction of the gender wage gap.[6] A concern with these studies is that they do not control for unobserved characteristics of workers, thus confounding segregation by ability with segregation by gender. This concern is addressed by studies of interfirm mobility (Loprest 1992; Hospido 2009; Del Bono and Vuri 2011) which show that women are about as likely to move between firms as men, but experience smaller average wage gains with each move. Nevertheless, these studies cannot distinguish between the hypothesis that women are less likely to find jobs at higher-paying firms and the alternative that the wage gain for a given firm-to-firm transition is smaller for women than for men. Cardoso, Guimarães, and Portugal (forthcoming) also focus on differential sorting using an AKM-style model but imposing the assumption that the firm effects are the same for men and women.

The possibility that equally productive women and men are paid differently by firms with some wage-setting power was suggested by Robinson (1933) in her seminal analysis of imperfect labor markets and arises in wage posting models in which women's and men's turnover rates are differentially responsive to firm-specific wage premiums, or in search and matching models in which women and men have different relative bargaining power.[7] The relative bargaining power interpretation is also emphasized in the social psychology literature, which argues that women are less likely to initiate negotiations with their employers (Babcock and Laschever 2003; Bowles, Babcock, and Lai 2007) and are on average less successful negotiators.[8]

6. One piece of evidence suggesting that the exclusion of women is driven by employer preferences comes from studies of banking (Ashenfelter and Hannan 1986; Black and Strahan 2001) which find that deregulation led to a rise in the share of female employees in the industry. Neumark, Bank, and Van Nort (1996) also report that higher-wage restaurants are less likely to interview female applicants.

7. Manning (2011, section 3) shows that these two alternatives are observationally equivalent.

8. Stuhlmacher and Walters (1999) present a meta-analysis of lab-based studies of the effect of gender in bargaining and conclude that on average women obtain a smaller share of the surplus than men. Save-Soderbergh (2007) found that female college graduates who were asked to submit a salary demand at the start of their first job tended to ask for lower salaries and ended up receiving lower salaries than men.

III. Institutional Setting and Data Overview

Our analysis relies on an annual census of employees in Portugal that includes data on earnings and hours of work, as well as firm-specific information that allows us to link workers to the income statements of their employers. Although our focus on Portugal is driven by the richness of these data, three features suggest that our findings may be broadly generalizable to other settings. First, Portuguese women have relatively high labor force participation rates. Fifty-eight percent of adult women in the country were in the labor force in 2010 (ILO, 2011), comparable to the rates in the United States and Northern Europe. Second, the vast majority of women in Portugal (over 90% of those in private sector jobs) work full-time, reducing concerns that the gender wage gap is confounded 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 United States and United Kingdom, and is very close to the OECD-wide average.[9]

During our sample period roughly 90% of private sector jobs in Portugal were covered by sector-wide collective agreements negotiated by employer associations and trade unions (Addison, Portugal, and Vilares 2015). Bargaining is synchronized, and most wage clauses are renegotiated annually in January. Since these contracts set pay on a gender-neutral basis, they arguably exert some equalizing effect on the relative pay of women (Blau and Kahn 2003). On the other hand, firms have wide latitude in assigning employees to job categories, and most workers also earn substantial wage premiums over the base pay rates for their job category (Cardoso and Portugal 2005). The minimum wage is also relatively high in Portugal, potentially raising women's wages relative to men's—an issue we address in detail later. Nevertheless, Portugal has very high levels of overall wage inequality, suggesting that wage setting is relatively unconstrained by institutional forces.[10]

9. The gender gap in median full-time earnings was 16% in Portugal, 19% in the United States, and 16% on average across 26 OECD countries (OECD, 2012).

10. Martins and Pereira (2004) tabulate the 90/10 gap in hourly wages and in the returns to education for 16 countries, including the United States, the United Kingdom, and other European countries, finding that Portugal is highest in both measures of inequality.

III.A. Data Sources

Our main data source is Quadros de Pessoal (QP), a census of private sector employees conducted each October 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. Government employees and independent contractors are excluded from coverage, as are people who are unemployed or out of the labor force in the survey week.[11] Over our 2002–2009 sample period we have information on roughly 4 million workers who are observed between one and eight times, with firm and establishment identifiers for their jobs in the survey week. Since our financial data are firm-based, we aggregate establishments to the firm-level for the small fraction (4%) of multiplant firms.

The QP asks employers to report each employee's gender, education, occupation, regular monthly salary, regular wage supplements, and hours of work. Information is also collected on the industry, location, and founding date of the firm, as well as gross sales in the preceding calendar year. We construct hourly wages by dividing the sum of a worker's base salary plus any regular earnings supplements by his or her normal hours of work, yielding a "straight time" hourly wage.[12] The availability of hours information is a unique strength of the QP and allows us to address concerns that the gender wage gap is driven in part by differences in hours of work by men and women (e.g., Wood, Corcoran, and Courant 1993; Bertrand, Goldin, and Katz 2010).

We augment this information with financial data from the SABI (Sistema de Analisis de Balances Ibericos) database. Businesses in Portugal are required to file income (or profit and loss) statements and balance sheet information annually with the

11. Firm owners and employees on temporary leave are included in the data set but do not report wages, and so are excluded from our analysis.

12. All legal jobs in Portugal are covered by some form of contract, which specifies among other things the normal hours of work (Reis and Barbosa 2010). For employees at firms that have signed a sectoral or firm-specific collective bargaining contract, normal hours are part of the collective agreement. For others, normal hours are specified by their individual contract. Regular earnings supplements are payments such as meal allowances that are received regularly. Overtime work is allowed in Portugal, but until recent reforms was subject to a 50% pay premium for the first hour of overtime, and higher premiums for longer hours. Total annual hours of overtime are also limited by law.

Conservatoria do Registo Comercial.[13] These reports are publicly accessible and are collected by financial service firms and assembled into the SABI database by Bureau van Dijk. Information in SABI includes the firm's name, address, industry, founding date, and total employment, as well as income statement and balance sheet items. SABI data are available from 2000 onward, but coverage of the database was limited before 2006.

Since the QP does not include firm names or tax identifiers, we use a combination of variables that are reported in both QP and SABI to match the data sets. Specifically, we use location, industry, firm creation date, annual sales, and year-end shareholder equity as matching variables. Additional details regarding the matching process are provided in the Online Appendix. As described in Section A of the Online Appendix, we successfully match about 53% of firms that appear in our analysis sample between 2002 and 2009 to a firm with at least one year's information in SABI. Overall we have current-year employer financial data for about 66% of the person-year observations in our QP sample from 2006 to 2009.

III.B. Descriptive Overview

We begin with a brief overview of the differences between male and female employees in Portugal. We focus on individuals who are between 19 and 65 years of age, have more than 1 year of potential labor market experience, and worked as a paid employee in the QP reference week. Our primary analysis sample—described in columns (1) and (2) of Table I—contains annual wage observations from 2002 to 2009 for 2.1 million men and 1.7 million women.[14]

Comparisons between the two columns show that female workers in Portugal are slightly younger than their male counterparts but are more likely to have completed secondary or tertiary education. Despite the education gap, women earn about

13. Based on informal discussions with firm owners, we believe that the penalties for nonfiling are small, presumably accounting for missing data for many firms. Currently this information is collected as part of the Integrated System of Company Accounts (SCIE, Sistema de Contas Integradas das Empresas).

14. See Section A of the Online Appendix for details on the derivation of this sample, and Appendix Table A1 for comparisons with the overall population of 16–65-year-old workers in the QP. In the small number of cases where an individual is employed at two or more firms in the reference week, we assign them to their job with the most hours.

TABLE I

DESCRIPTIVE STATISTICS FOR VARIOUS SAMPLES OF EMPLOYEES IN QUADROS DE PESSOAL (QP), 2002–2009

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Connected Sets of Workers/Firms					
	Overall Analysis Sample		All		Dual-Connected		Analysis Sample with Value Added Data	
	Males	Females	Males	Females	Males	Females	Males	Females
Age:								
Mean age	38.1	36.9	38.0	36.5	38.0	36.4	37.9	36.3
Fraction $\leq$ 30 years old	0.30	0.33	0.30	0.34	0.30	0.34	0.31	0.35
Fraction $\geq$ 50 years old	0.19	0.14	0.18	0.13	0.19	0.13	0.18	0.13
Education:								
Mean years schooling	8.0	8.8	8.0	8.9	8.6	9.1	7.8	8.6
Fraction with high school	0.18	0.23	0.18	0.23	0.21	0.24	0.17	0.24
Fraction with degree	0.09	0.13	0.09	0.14	0.11	0.15	0.07	0.11
Mean log real hourly wage	1.59	1.41	1.62	1.43	1.71	1.48	1.54	1.36
(std. dev.)	(0.55)	(0.50)	(0.55)	(0.51)	(0.58)	(0.53)	(0.48)	(0.43)
Mean monthly hours	162.6	158.0	162.5	157.9	162.8	157.1	164.1	159.9
(std. dev.)	(24.7)	(30.1)	(24.8)	(29.9)	(24.0)	(30.5)	(23.6)	(29.4)
Fraction in Lisbon	0.35	0.35	0.36	0.37	0.42	0.40	0.32	0.34
Fraction in Oporto	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.14
Mean firm size (no. employees)	730	858	804	978	1,091	1,230	500	886
Fraction females at firm	0.24	0.70	0.24	0.70	0.30	0.64	0.24	0.67
Mean log VA/worker							3.05	2.88
Number person-year obs.	9,070,492	7,226,310	8,225,752	6,334,039	6,012,521	5,012,736	5,786,108	4,204,828
Number of persons	2,119,687	1,747,492	1,889,366	1,505,517	1,450,288	1,247,503	1,441,608	1,082,052
Number of firms	349,692	336,239	216,459	185,086	84,720	84,720	153,994	141,887

Notes. Overall analysis sample in columns (1)–(2) includes paid workers age 19–65 with potential experience $\geq$ 2, and consistent employment histories. See Section A of Online Appendix. Wages are measured in real (2009 = 100) euros per hour. Value added (VA) 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.

18% less per hour than men—very similar to the gender gap in median hourly wages in the United States in 2007 (EPI 2010).[15] Women also work slightly fewer hours per month than men, though the 3% difference is small by international standards.[16] The dispersion in monthly hours is larger for women than men while the dispersion in hourly wages is smaller for women. Thirty-five percent of both male and female employees work in the Lisbon area, another 13% work in the Porto area, and the remainder work in smaller cities and rural areas.

Comparing the characteristics of their workplaces, women work at slightly larger firms than men (858 employees versus 730), a feature that is also true in the United States and the United Kingdom.[17] More striking is the difference in the share of female employees at women's and men's workplaces—70% versus 24%. This gap indicates that there is significant gender segregation across firms.[18] Indeed, about 21% of men work at all-male firms, while 19% of women work at all-female firms.[19] The presence of single-gender firms poses a problem for assessing the role of firms in the gender wage gap, since we cannot observe the wages that would be offered to women at all-male firms or to men

15. The wage gap narrowed over our sample period, falling from 21% in 2002 to 16% in 2009—see Online Appendix Figure B1.

16. Data reported by the OECD (2012) for Portugal (based on labor force survey data that include government and independent contract workers excluded from QP) show 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 United States of 8% and 17%.

17. Papps (2012) and Mumford and Smith (2008) report roughly 10% larger workplace sizes for women than men in the United States and United Kingdom, respectively.

18. Hellerstein, Neumark, and McInerney (2008) report that in 2000, the average fractions of female coworkers for female and male workers at larger establishments in the United States were 61% and 41%, respectively. Mumford and Smith (2008, Online Appendix Table A2) report that in the United Kingdom in 2004 the average fraction of female employees at the workplace was 70% for women and 34% for men. These comparisons suggest that Portuguese firms may be more segregated by gender than those in the United States or United Kingdom, though we caution that estimates of segregation rates are potentially sensitive to the range of firm sizes included in the analysis.

19. About 20% of workers at single-gender firms are the only (paid) employee at their workplace. 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%). Mean log wages of workers at single-gender firms are relatively low: 1.28 for men and 1.19 for women.

at all-female firms. For most of our analysis we therefore limit attention to firms that hire at least one worker of each gender at some point in our sample period. Since the wage gap between men and women at single-gender firms is relatively small, eliminating employees at these firms leads to a slightly larger gender gap in the remaining subsample than in the labor market as a whole.

An important issue for an analysis of between-firm wage differentials is the rate of job mobility, since these differentials are identified by the wage changes of job movers. Online Appendix Table B1 shows that the distributions of the number of jobs held by men and women in our QP sample are very similar. Approximately 73% of men and 74% of women hold only one job during our sample period; 19% of both groups have two jobs; 6% have three jobs. The remaining 2% of men and 1% of women hold four to eight jobs. We also examined survival rates of new jobs that are observed starting during our sample period, and found that these are very similar. As shown in Online Appendix Figure B2, about 40% of new jobs last less than one year for both groups. We conclude that job mobility rates are very similar for women and men in Portugal.

IV. Modeling Framework

Here we present a very simple model that allows us to evaluate the effect of firm-specific pay premiums on the observed wages of women and men. Assume that we observe point-in-time wages for workers (indexed by $i \in \{1, \ldots, N\}$) in multiple periods (indexed by $t \in \{1, \ldots, T\}$). We denote worker i's gender by $G(i)$ which takes on values in the set $\{F, M\}$, and the identity of his or her employer in a given year by $J(i, t)$, which takes on values in the set $\{1, \ldots, J\}$. We refer to a particular gender as g and a particular firm as j.

We posit a wage-setting model in which the logarithm of the real wage earned by individual i in period t is given by:

$$w_{it} = a_{it} + \gamma^{G(i)} S_{iJ(i,t)t}. \tag{1}$$

Here, a_{it} represents the outside option available to worker i in period t (e.g., the wage in self employment), $S_{iJ(i,t)t} \geq 0$ is the match surplus between worker i and firm $J(i, t)$ in period t, and $\gamma^g \in [0, 1]$ is a gender-specific share of the surplus captured by a worker of gender $g \in \{F, M\}$. We are specifically interested in the question of whether women get a smaller share of the surplus associated with their job (i.e., $\gamma^F < \gamma^M$).

We assume that $S_{iJ(i,t)t}$ can be decomposed into three components:

$$S_{iJ(i,t)t} = \overline{S}_{J(i,t)} + \phi_{J(i,t)t} + m_{iJ(i,t)}. \tag{2}$$

The first term, $\overline{S}_{J(i,t)}$, captures time-invariant factors like market power or brand recognition that raise the average surplus for all employees at the firm. The second component, $\phi_{J(i,t)t}$, represents time-varying factors that raise or lower the average surplus for all employees. The third component, $m_{iJ(i,t)}$, captures a person-specific component of surplus for worker i at his or her current employer, attributable to idiosyncratic skills or characteristics that are particularly valuable at this job.

We assume that the outside option a_{it} can be decomposed into a permanent component α_i (due, for example, to ability or general skills), a time-varying component associated with an observed set of characteristics X_{it} (e.g., labor market experience and changing returns to education), and a transitory component ε_{it}:

$$a_{it} = \alpha_i + X_{it}'\beta^{G(i)} + \varepsilon_{it}, \tag{3}$$

where β^g is a gender-specific vector of coefficients.

Equations (1) through (3) imply the wage of worker i in period t can be written:

$$w_{it} = \alpha_i + \psi_{J(i,t)}^{G(i)} + X_{it}'\beta^{G(i)} + r_{it}, \tag{4}$$

where $\psi_{J(i,t)}^{G(i)} \equiv \gamma^{G(i)}\overline{S}_{J(i,t)}$ and $r_{it} \equiv \gamma^{G(i)}\left(\phi_{J(i,t)t} + m_{iJ(i,t)}\right) + \varepsilon_{it}$ is a composite error. Equation (4) is consistent with an additive "two-way" worker-firm effects model of the type considered by AKM and many subsequent authors, with person effects, *gender-specific* firm effects, and gender-specific returns to the covariates X_{it}. We use this model as the basis for our main analysis, though as explained below, we also explore the possibility that the share of surplus received by workers varies between occupations—specifically, between "typically female" occupations and "typically male" occupations.

IV.A. *Exogeneity*

We estimate models based on equation (4) by OLS, yielding estimated gender-specific effects for each firm. For these estimates to be unbiased, the following orthogonality conditions must hold:

$$E\Big[(r_{it}-\bar{r}_i)\Big(D^j_{it}-\overline{D}^j_i\Big)|G(i)\Big]=0\ \forall j\in\{1,\ldots,J\}, \tag{5}$$

where $D^j_{it}\equiv 1[J(i,t)=j]$ is an indicator for employment at firm j in period t and bars over variables represent time averages. To gain some insight into the restrictions implied by equation (5), it is useful to consider the special case where $T=2$. With two periods, fixed effects estimation is equivalent to first differences estimation and equation (5) reduces to:

$$E\Big[(r_{i2}-r_{i1})\Big(D^j_{i2}-D^j_{i1}\Big)|G(i)\Big]=0\ \forall j\in\{1,\ldots,J\}. \tag{6}$$

Using the fact that $(D^j_{i2}-D^j_{i1})$ takes on values of $+1$ for workers who move to firm j in period 2, –1 for those who leave firm j in period 1, and 0 for all others, we can write:

$$\begin{aligned}E\Big[(r_{i2}-r_{i1})\Big(D^j_{i2}-D^j_{i1}\Big)|G(i)\Big]=&\,E\Big[r_{i2}-r_{i1}|D^j_{i2}=1,D^j_{i1}=0,G(i)\Big]\\&\times P\Big(D^j_{i2}=1,D^j_{i1}=0|G(i)\Big)\\&-E\Big[r_{i2}-r_{i1}|D^j_{i2}=0,D^j_{i1}=1,G(i)\Big]\\&\times P\Big(D^j_{i2}=0,D^j_{i1}=1|G(i)\Big).\end{aligned}$$

The term $E[r_{i2}-r_{i1}|D^j_{i2}=1,D^j_{i1}=0,G(i)]$ is the mean change in the unobserved wage determinants for *joiners* of firm j, while the term $E[r_{i2}-r_{i1}|D^j_{i2}=0,D^j_{i1}=1,G(i)]$ is the corresponding change for *leavers* of this firm. Hypothetically, it is possible that these two terms are roughly comparable in magnitude since the decision to leave one firm is a decision to join another. In such a case, the mean bias associated with joiners and leavers would cancel whenever the number of firm joiners and leavers is equal, as would occur when the firm's employment is in steady state. However, while joining and leaving firms may yield similar average biases, the joiner and leaver bias associated with any particular firm may be quite different, which would lead to a violation of equation (6).

Since

$$r_{i2}-r_{i1}=\gamma^{G(i)}\big[\phi_{J(i,2)2}-\phi_{J(i,1)1}+m_{iJ(i,2)}-m_{iJ(i,1)}\big]+\varepsilon_{i2}-\varepsilon_{i1},$$

there are three channels through which the changes may be related to firm-specific mobility. The first is a connection between firm-wide shocks ϕ_{jt} and mobility rates. For example, workers may be more likely to leave firms that are experiencing negative shocks and join firms that are experiencing positive shocks. If this is true, then we would expect to see a systematic "Ashenfelter dip" in the wages of leavers just prior to their exit and unusual wage growth for recent joiners. We look for such patterns below and find no evidence that they are present in the data.

A second potential channel arises if mobility is related to the idiosyncratic match effects (m_{ij}). Many search and matching models assume that workers search over jobs that differ by a match effect in pay. An implication is that the wage gains of movers will overstate the gains for a typical worker. For example, suppose that firm A offers a 10% larger average wage premium than firm B. If mobility is independent of the match effects, then movers from firm B to firm A will experience a 10% average wage gain, while movers from firm A to firm B will experience a 10% average wage loss. If instead mobility is based in part on comparative advantage, then the expected wage losses associated with moving from A to B will tend to be offset by an improvement in match effects. In the limit, if all firm transitions are voluntary and selection is based solely on the match components, *all* moves will lead to wage gains, as in the dynamic matching model of Eeckhout and Kirchner (2011). In the following analysis, we examine workers moving in opposite directions between groups of high- and low-wage firms, and find that their wage changes exhibit the approximate symmetry (i.e., equal magnitude and opposite sign) predicted by an additive model with exogenous mobility. This symmetry is inconsistent with selective mobility based on the match component of wages.

A third channel arises if the direction of firm-to-firm mobility is correlated with the transitory wage shock ε_{it}. For example, a worker who is performing well and receiving promotions may be more likely to move to a higher wage firm, whereas workers who are stalled in their job may be more likely to move down the job ladder to a lower-paying firm. Systematic mobility of this form implies that people moving to higher-wage firms will have different trends prior to moving than those who move to lower-wage firms. Again, in our analysis we find no evidence for any of these predictions.

What drives firm-to-firm mobility if it is not related to the elements in r_{it}? The most straightforward explanation is that worker-firm matching is based on a combination of the permanent component of worker ability (the α_i component in equations (3) and (4)) and the average wage premiums offered by firms. Skilled workers, for example, are more likely to engage in on-the-job search (Pissarides and Wadsworth 1994; Hall and Krueger 2012) suggesting that they will be more likely to move to high-wage firms over time. Skilled workers also may have networks of friends and family members that are more likely to work at high-wage firms, leading to network-based sorting (as in Kramarz and Skans 2014). These forms of sorting create no bias for our estimation strategy because we condition on time-invariant worker and firm characteristics. Finally, sorting based on nonwage dimensions such as the location of the firm or its recruiting effort creates no bias provided that these factors are uncorrelated with the time varying error component in equation (4).

IV.B. Normalization

As explained by Abowd, Creecy, and Kramarz (2002) the firm effects in a two-way fixed effects model such as equation (4) are only identified within a "connected set" of firms linked by worker mobility. In our analysis below, we limit attention to workers and firms in the largest connected set for each gender. Even within these sets we still require a linear restriction to normalize the firm effects, since the wage premium for any given firm is only identified relative to a reference firm or set of firms.

According to our model the true firm effects for each gender are nonnegative, and will be zero at firms that offer no surplus above an employee's outside option. We therefore normalize the firm effects by setting the average wage premium for a set of "low-surplus" firms to 0. More precisely, letting $\overline{S}_j^o$ denote an observed measure of average surplus per worker at firm j, we assume that:

$$E\left[\psi^g_{J(i,t)} | \overline{S}^o_{J(i,t)} \leq \tau\right] = 0, g \in \{F, M\}, \tag{7}$$

where τ is a threshold level such that firms with observed surplus per worker below τ pay zero rents on average. If equation (7) is correct, then imposing this condition will yield a set of normalized firm effects that coincide with the true firm effects (apart from sampling errors). Otherwise, the normalized effects for each gender will be equal to the true firm effects, minus the average value of the firm effects for that gender group at firms with $\overline{S}_j^o < \tau$.

As discussed later, we use mean log value added per worker for all years the firm is observed in the SABI data set as our primary measure of surplus per worker. Value added is reported for most firms and is constructed as the sum of wage payments, nonwage labor costs, depreciation, interest costs, taxes, and profits (i.e., the sum of payments to labor and capital, plus taxes).[20] Under standard assumptions, value added will be equal to revenues minus the costs of all intermediate inputs. We also repeat our analysis using mean log sales per worker as an alternative measure of surplus and obtain very similar results.

Although the normalized effects could, in principle, be estimated in a single step, we opt instead for a two-step approach. We first estimate the gender-specific firm effects via unrestricted OLS, arbitrarily setting the effects for a particular large firm to 0. We then renormalize the effects by subtracting off the average value of the gender-specific firm effects at low surplus firms. We explain how we estimate the threshold τ in Section VI.C.

As a check on this procedure we normalize the firm effects by assuming that firms in the hotel and restaurant industry pay zero surplus on average. This assumption is motivated by the extensive literature on industry wage differences (e.g., Dickens and Katz 1987; Krueger and Summers 1988) which suggests that these differentials are, at least in part, driven by rents. We observe that firms in the hotel and restaurant sector have the smallest wage premiums on average, so we simply assume that rents are on average zero in this sector.

IV.C. Decomposing the Effect of Firm-Level Pay Premiums

Equation (4) provides a simple framework for measuring the impact of firm-level pay premiums on the gender wage gap. Using *male* and *female* as shorthand for the respective conditioning events that $G(i) = M$ and $G(i) = F$, we can denote the average pay premium received by men as $E[\psi^M_{J(i,t)}|male]$ and the average premium received by women as $E[\psi^F_{J(i,t)}|female]$. As in the traditional Oaxaca wage decomposition (see, e.g., Oaxaca 1973; Fortin,

20. This is the standard national accounts definition (see, e.g., Strassner and Moyer 2002).

Lemieux, and Firpo 2011), we can decompose the difference in pay premiums into a combination of bargaining power and sorting effects in either of two ways:

$$E[\psi^M_{J(i,t)}|male] - E[\psi^F_{J(i,t)}|female] = E[\psi^M_{J(i,t)} - \psi^F_{J(i,t)}|male] \quad (8)$$
$$+E[\psi^F_{J(i,t)}|male] - E[\psi^F_{J(i,t)}|female]$$
$$= +E[\psi^M_{J(i,t)} - \psi^F_{J(i,t)}|female]$$
$$+E[\psi^M_{J(i,t)}|male] - E[\psi^M_{J(i,t)}|female]. \quad (9)$$

The first term in equation (8) is the average bargaining power effect, calculated by comparing ψ^M_j and ψ^F_j across the distribution of jobs held by men. The second line of equation (8) gives the average sorting effect, calculated by comparing the average value of ψ^F_j across the jobs held by men versus women. In the alternative decomposition (equation (9)) the bargaining power effect is calculated using the distribution of jobs held by women, and the sorting effect is calculated by comparing the average value of the male pay premiums across jobs held by men versus women.

It is worth emphasizing that the estimated sorting effects in equations (8) and (9) are invariant to the particular normalization chosen for the firm effects. In contrast, the estimated bargaining effects depend on the normalization: subtracting different constants from the male and female effects will obviously lead to different values for the first line of either equation (8) or (9). Provided that the rents received by female workers at low-surplus firms are no larger than the rents received by male workers at these firms, however, our choice of normalization will yield a lower bound estimate of the bargaining effect, and the overall decomposition will lead to a lower bound estimate of the effect of firm-specific pay premiums on the gender wage gap.

IV.D. Relating the Estimated Firm Effects to Measures of the Bargaining Surplus

An alternative approach to measuring the sorting and bargaining components of the gender wage gap is to look directly at how the estimated wage premiums offered by a given firm vary with the measured surplus per worker at the firm. Specifically, building on our normalization approach, we assume that:

$$E\left[\overline{S}_{J(i,t)}|\overline{S}^o_{J(i,t)}\right] = \kappa \max\left\{0, \overline{S}^o_{J(i,t)} - \tau\right\}. \quad (10)$$

In other words, actual average surplus per worker is linearly related to the deviation of the observed surplus measure from the threshold level τ for firms with $\overline{S}_j^o > \tau$, and is 0 otherwise. For simplicity we refer to the quantity max $\{0, \overline{S}_j^o - \tau\}$ as firm j's "net surplus" NS_j. Given a value for τ (which we estimate in a prior step, as explained in Section VI.C) we can write:

$$\psi^g_{J(i,t)} = \pi^g NS_{J(i,t)} + \nu^g_{J(i,t)}, \tag{11}$$

where $\pi^g \equiv \gamma^g \kappa$ and $E[\nu^g_{J(i,t)}|NS_{J(i,t)}, G(i)] = 0$. Notice that $\frac{\pi^F}{\pi^M} = \frac{\gamma^F}{\gamma^M}$. By taking the ratio of the estimated gender specific slopes after estimating equation (11) for male and female workers we obtain a direct estimate of the bargaining power ratio $\frac{\gamma^F}{\gamma^M}$.

Using this setup, we can decompose the difference in the average value of the first term of equation (11) for male relative to female workers as:

$$E[\pi^M NS_{J(i,t)}|male] - E[\pi^F NS_{J(i,t)}|female]$$

$$= (\pi^M - \pi^F)E[NS_{J(i,t)}|male] + \pi^F \begin{pmatrix} E[NS_{J(i,t)}|male] \\ -E[NS_{J(i,t)}|female] \end{pmatrix} \tag{12}$$

$$= (\pi^M - \pi^F)E[NS_{J(i,t)}|female] + \pi^M \begin{pmatrix} E[NS_{J(i,t)}|male] \\ -E[NS_{J(i,t)}|female] \end{pmatrix}. \tag{13}$$

Focusing only on the part of the firm surplus that is explained by our observed measure of net surplus, the contribution of the bargaining channel to the male-female wage gap is simply the difference in coefficients $(\pi^M - \pi^F)$, weighted by the measured net surplus at men's jobs (equation (12)) or women's jobs (equation (13)). The corresponding contribution of the sorting channel is the difference in average net surplus at men's jobs and women's jobs, weighted by either π^F (equation (12)) or π^M (equation (13)).

IV.E. Within-Firm Changes in Wages over Time

Although our main focus is on gender differences in between-firm wage differentials, our model also implies that the wages of male and female employees who are observed working at the same firm over time will respond differently to changes in firm surplus. Define $S_{jt} \equiv \overline{S}_j + \phi_{jt}$ as the actual surplus per worker in period t, and S^o_{jt} as the observed surplus measure for firm j in year t. We assume that these are related by:

$$S_{jt} = \lambda \max\left\{0, S^o_{jt} - \tau\right\} + \varsigma_{jt}$$

$$\equiv \lambda NS_{jt} + \varsigma_{jt}, \tag{14}$$

where the error ς_{jt} has mean 0 when we condition on the firm's observed net surplus and the characteristics of workers observed working at the firm continuously between an initial period $t=1$ and a later period $t=T$ (i.e., "stayers"). Using equation (4) we can therefore write:

$$E\left[w_{iT} - w_{i1} | NS_{J(i,1)1}, NS_{J(i,1)T}, X_{i1}, X_{iT}, G(i), stayer\right]$$

$$= (X_{iT} - X_{i1})'\beta^{G(i)} + \theta^{G(i)}[NS_{J(i,1)T} - NS_{J(i,1)1}], \tag{15}$$

where $\theta^g = \gamma^g \lambda$ and *stayer* is shorthand for the conditioning event that worker i is continuously employed at the same firm throughout the sample period. Estimating this equation by OLS separately by gender yields estimates of the slope parameters θ^M and θ^F which can be used to form another estimate of the relative bargaining power ratio $\frac{\gamma^F}{\gamma^M}$, based on the differential reactions of male and female wages to changes in surplus.

To actually estimate the relative bargaining power ratio (and its sampling error) we rely on the insight from our model that:

$$\frac{E\left[w_{iT} - w_{i1} - (X_{iT} - X_{i1})'\beta^F | female, stayer, J(i,1) = j\right]}{E\left[w_{iT} - w_{i1} - (X_{iT} - X_{i1})'\beta^M | male, stayer, J(i,1) = j\right]} = \frac{\gamma^F}{\gamma^M}.$$

That is, the covariate-adjusted average wage changes of male and female stayers at the same firm are deterministically related by the gender bargaining power ratio. Given the small size of most firms in our sample, we estimate this relationship using a two-step instrumental variables (IV) procedure. For each gender, we regress the change in wages on covariates and firm dummies to obtain adjusted average firm wage changes by gender. We then regress the adjusted average change in female wages at each firm on the corresponding average male change using the change in measured surplus as an instrument and weighting by the total number of stayers at each firm. Similarity of this estimate, based on within-firm changes in wages and measured surplus, with the estimate from equation (11) based on between-firm variation in wages and surplus, provides support for the simple rent-sharing model specified by equations (1)–(3).

V. Descriptive Evidence on Firm-Specific Pay Premiums

Although the two-way effects model specified in equation (4) has been widely used over the past decade, the additive structure of the model and the restrictive assumptions needed for OLS estimation have been strongly criticized by some authors (e.g., Lopes de Melo 2009; Eeckhout and Kirchner 2011). Following Card, Heining, and Kline (2013), we present some descriptive evidence on the patterns of wage changes for people who move between jobs with higher- and lower-paid coworkers. We document five basic facts that are all consistent with equation (4) and the exogenous mobility condition (5). First, men and women who move between jobs with higher- and lower-paid coworkers experience systematic wage gains and losses, suggesting that there are significant firm-specific pay premiums for both genders. Second, there is no indication that movers to firms with higher- or lower-paid coworkers experience differential wage trends prior to their move. Third, wage changes for people who move between firms with similarly paid coworkers experience little or no excess wage growth relative to job stayers. Fourth, the gains and losses from moving between jobs with higher-paid and lower-paid coworkers are approximately symmetric, suggesting that the firm-specific pay premiums are additively separable (in logarithms) from other pay components and that mobility patterns are not driven by comparative advantage in wages. Fifth, women gain less than men from moving to jobs with more highly paid coworkers, as predicted by a rent-sharing model in which women get a smaller share of the rents than men.

We begin by selecting men and women from the overall analysis sample described in columns (1) and (2) of Table I who are employed at firms with at least one worker of each gender at some point in our sample period. We construct mean log co-worker wages for each person in each year (i.e., the leave-out mean log wage at their firm including both male and female coworkers), and assign each person in each year the quartile of their mean coworker wages. (We do not adjust wages for time effects or any worker characteristics). For job changers who are observed for at least two years at their origin firm and two years at their destination firm, we then classify the move based on their coworker wage quartile in the last year at the old job and their coworker wage quartile in the first year at their new job. Finally, we construct average wages in the years before and after the move for each of the 16 groups of male and female job changers.

Figures I and II plot the wage profiles before and after the job change for men and women who moved from jobs in the lowest (1st) quartile of coworker wages, and for those who moved from jobs in the highest (4th) quartile. The figures show that men and women who move from jobs with highly paid co-workers to jobs with poorly-paid co-workers experience large average wage losses, while those who move in the opposite direction experience large wage gains.[21] Moving within a quartile group, by comparison, is associated with relatively small wage changes. Moreover, although the levels of wages on the old job differ between people from the same origin quartile who move to different destination quartiles, the trends prior to moving are very similar across groups. Likewise, the trends after moving are similar across groups. These observations imply that interfirm mobility is correlated with the permanent component of individual wages (i.e., the α_i component of equation (4)) but not with the transitory error components (i.e., ϕ_{jt} or ε_{it}).

Online Appendix Table B2 summarizes all 16 groups of men and women, including information on the numbers of observations in each origin/destination group, the fractions of each origin group that move to each of the four possible destination groups, and the average wage change experienced by each group from two years before to two years after the move. The table also reports an average regression-adjusted wage change for job changers, using the coefficients from a model of wage changes fit to the sample of job stayers who remain on the same job over a given four-year interval. The average adjusted wage changes for job changers who stay in the same coworker wage quartile are all relatively small—for example, 0.5% for male movers from

21. The QP does not collect information that allows us to distinguish the reasons for job changes, though we suspect that many transitions to higher-quartile firms are voluntary moves, while many of the transitions to lower-quartile firms arise from layoffs and firing events. As documented in Online Appendix Table B2, moves up are more common than moves down.

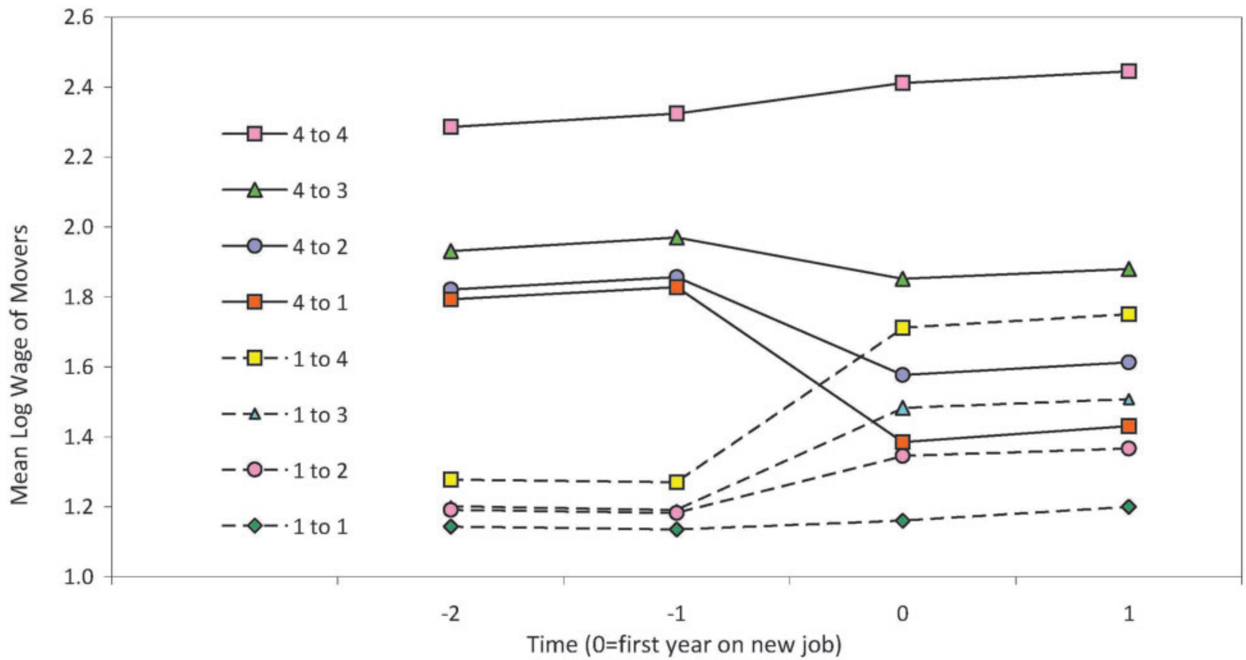

FIGURE I

Mean Log Wages of Male Job Changers, Classified by Quartile of Mean Co-Worker Wage at Origin and Destination Firm

Figure shows mean wages of male workers at mixed-gender firms who changed jobs in 2004–2007 and held the preceding job for two or more years, and the new job for two or more years. Each job is classified into quartiles based on mean log wage of coworkers of both genders in the last year of the old job (for origin firm) and in the first year on the new job (for the destination firm). See text for additional details.

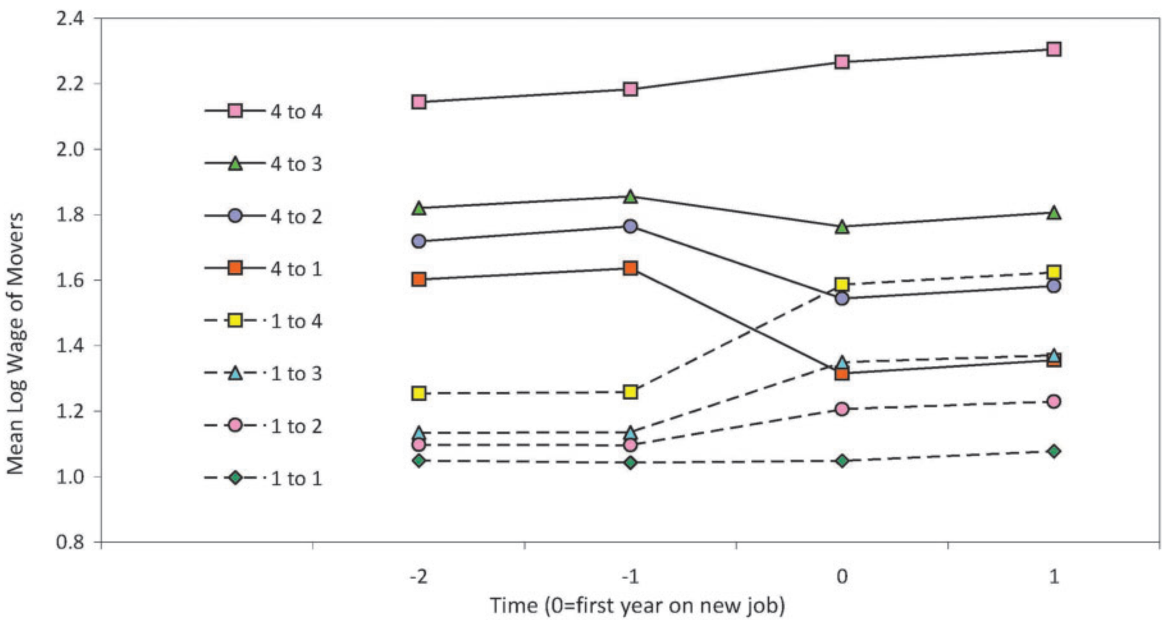

FIGURE II

Mean Wages of Female Job Changers, Classified by Quartile of Mean Co-Worker Wage at Origin and Destination Firm

Figure shows mean wages of female workers at mixed gender firms who changed jobs in 2004–2007 and held the preceding job for two or more years, and the new job for two or more years. Each job is classified into quartiles based on mean log wage of coworkers of both genders in the last year of the old job (for origin firm) and in the first year on the new job (for the destination firm). See text for additional details.

quartile 1 jobs to other quartile 1 jobs, and −1.2% for female movers from quartile 2 jobs to other quartile 2 jobs, suggesting that mobility itself has little effect on wage growth. The only exception is for movers among firms in quartile 4, who experience relatively modest wage gains (6.1% for men and 7.0% for women) relative to stayers.

Movers between quartiles, on the other hand, experience relatively large wage gains or losses, even controlling for experience. Moreover, although not precisely symmetric, the mean wage changes for people who move in opposite directions between quartile groups (e.g., from quartile 1 to quartile 2, versus from quartile 2 to quartile 1) are of similar magnitude and uniformly of opposite sign. This is illustrated in Online Appendix Figures B3 and B4, where we graph the mean adjusted wage changes for downward movers (e.g., from quartile 4 to quartile 3 firms) against the adjusted wage changes for symmetric upward movers (e.g., from quartile 3 to quartile 4). The wage changes of matched upward and downward movers lie very close to a line with slope −1, consistent with the symmetry implications of an AKM model with exogenous mobility, though for both men and women we can formally reject the hypothesis of symmetry.[22]

Comparisons between Figures I and II point to another important fact, which is that the wage changes for female movers in a given origin-destination group tend to be smaller in absolute value than the corresponding changes for men. This is illustrated graphically in Figure III, where we plot the adjusted wage changes for each of the 16 origin-destination quartiles for women against the corresponding adjusted changes for men. The points lie very tightly clustered around a line with a slope significantly less than 1, confirming that women gain less from moving to jobs with more highly paid coworkers and lose less from

22. The null hypothesis of symmetry is equivalent to the restriction that the sum of each upward and downward change in a quartile-to-quartile pair is zero. To account for the first stage regression adjustment of wage changes, we used a block bootstrap procedure to compute the standard error of the sum of each transition pair allowing for two-way clustering on worker and firm. This was accomplished by running three bootstraps: one resampling workers, one firms, and one worker-firm matches. The three asymptotic variances were then combined according to equation (2.11) of Cameron, Gelbach, and Miller (2011). We then used the estimated covariance matrix of the quartile-to-quartile sums to compute a Wald test of the hypothesis that the six sums were jointly zero. The test statistics were 17.6 for men and 87.9 for women, both of which possess an asymptotic $\chi^2(6)$ distribution (1% critical value is 16.8).

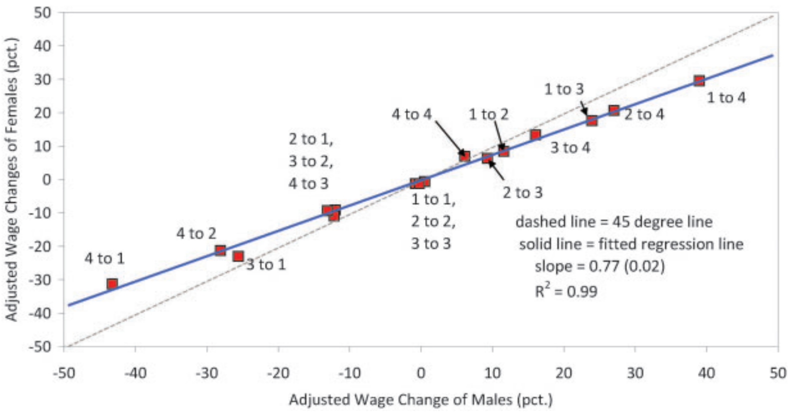

FIGURE III

Comparison of Adjusted Wage Changes of Male and Female Job Movers by Quartile of Coworker Wages at Origin and Destination Firms

Points represent regression adjusted mean log wage changes of male and female job movers in different origin/destination quartiles of mean coworker wages. For example "4 to 1" point shows mean wage changes for men and women who move from 4th quartile of coworker wages to 1st quartile. Fitted line is estimated by OLS to 16 points.

moving in the opposite direction. Equation (4) implies that the expected wage change for men who move from firm j to firm k is $\psi_k^M - \psi_j^M$, and the expected change for women making the same transition is $\psi_k^F - \psi_j^F = (\frac{\gamma^F}{\gamma^M})(\psi_k^M - \psi_j^M)$. The slope of the line in Figure III (0.77) can therefore be interpreted as an estimate of the relative bargaining power ratio.[23]

To summarize, our descriptive analysis suggests that firm-specific wage premiums are an important feature of the wage structure, and that firm mobility is (at least over the horizon we study) related to time-invariant person components of wages but not to time-varying or match components of wages. Furthermore, moves between matched groups of firms affect the wages of men proportionally more than the wages of women—a pattern we interpret as strong qualitative evidence that men have greater average bargaining power than women.

23. This estimate should be regarded as suggestive rather than definitive, since women and men are not equally distributed across the firms in each quartile group. We present estimates based on firm-specific comparisons below.

VI. Estimation of Worker-Firm Models

VI.A. Estimation Sample

We turn now to a more systematic analysis of the firm-specific pay premiums for men and women. Building on equation (4), we fit models that include person effects, gender-specific firm effects, and a set of time-varying observable covariates with gender-specific coefficients. For simplicity, we restrict our analysis to the largest connected set of firms for each gender. The estimation samples are described in columns (3) and (4) of Table I. 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 are very similar to those in our overall analysis sample, and in particular have only slightly higher average wages. After estimating the AKM models separately using these samples, we then narrow our focus to workers who are employed at firms that are in the connected sets for both men and women. This dual-connected sample of men and women—described in columns (5) and (6) of Table I—includes just over two thirds of the person-year observations from columns (1) and (2). Individuals in the dual-connected set have higher education than in the workforce as a whole, and also have somewhat higher average wages. The gender wage gap is also larger in this sample than in our overall sample (23% versus 18%), reflecting the omission of the single-gender firms, which as noted earlier have a relatively small gender gap.

VI.B. Estimation Results

Columns (1) and (2) of Table II summarize the parameter estimates and fit of our models for men and women in the largest connected sets of workers of each gender.[24] The models include fixed effects for workers and firms as well as year dummies, fully interacted with four education dummies (for 6, 9, 12, and 16 years of education), and quadratic and cubic terms in age interacted with education dummies.[25]

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

25. For each education group, we omit the 2002 year effect and recenter the quadratic and cubic terms around age 40. Since year and age are perfectly collinear when we include person effects, we exclude the linear age term. This normalization yields interpretable estimates of the year and person effects if the age profile is flat at age 40, which appears to be approximately true in Portugal (see Card and Cardoso, 2012).

TABLE II

SUMMARY OF ESTIMATED TWO-WAY FIXED EFFECTS MODELS FOR MALE AND FEMALE WORKERS

	(1) All Males	(2) All Females
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 model	0.143	0.125
Adjusted R-squared of model	0.934	0.940
Correlation of estimated male/female firm effects	0.590	
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
Standard 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. Models include 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 models include dummies for each worker-firm job match as well as other covariates in basic model. Samples include only observations in largest connected set.

We show the standard deviations of the estimated person and firm effects and the covariate indexes $\left(X_{it}'\hat{\beta}^g\right)$ for each observation, as well as the correlation of the person and firm effects, the residual standard deviation of the model, the adjusted R^2 statistics, and the (worker-year weighted) correlation of the estimated male and female firm effects $\left(\psi_{J(i,t)}^M, \psi_{J(i,t)}^F\right)$. For both males and females, 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 worker characteristics that are equally rewarded at all firms. The correlations between the estimated person and firm effects are both positive, implying that more highly skilled men and women are disproportionately employed at firms that pay higher wages to all their workers. Such positive assortative matching has been found in many recent studies of wage determination.[26] Our estimates of the male and female firm effects are also strongly positively correlated ($\rho = 0.59$), indicating that firms that pay higher wage premiums to men tend to pay more to women as well. Since the firm effects contain sampling errors and are estimated on disjoint samples, their sample correlation is a downward-biased estimate of the true correlation between the gender-specific wage premiums.

The middle panel of Table II shows fit statistics for a generalized model that includes dummies for each worker-firm match. This model, which relaxes the additive structure of equation (4), provides only a slight improvement in fit, with about a 1 percentage point rise in the adjusted R^2 statistics. By comparing the residual standard error of the generalized model to the corresponding standard error for the AKM model we can construct an estimate of the standard deviation of the permanent job match effects (the $m_{iJ(i,t)}$) that are absorbed in the job match model but included in the residual of an AKM model. 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. Evidently, the part of the wage premium that is shared by all workers at a given firm is much larger, on average, than the worker-specific match component.

We have also conducted a series of additional specification checks of the fit of our basic models. In one check, we examine the mean residuals from equation (4) for subgroups of observations classified by the decile of the estimated person effect and the decile of the estimated firm effect. As shown in Online Appendix Figures B5 and B6, we find that the mean residuals are very small in all 100 cells for both genders, supporting our

26. See, for example, Card, Heining, and Kline (2013) for West Germany, Maré and Hyslop (2006) for New Zealand, Skans, Edin, and Holmlund (2008) for Sweden, and Bagger, Sorensen, and Vejelin (2013) for Denmark. The sampling errors in the estimated person and firm effects from a model such as equation (4) are in general negatively correlated (see, e.g., Maré and Hyslop 2006; Andrews, Schank, and Upward 2008), implying that the correlations between the estimated effects are downward-biased estimates of the degree of assortative matching.

conclusion that the additive structure of equation (4) provides a good approximation to the wage-setting process. In a second check, we examined the mean residuals for workers who transition between groups of firms, classified by the quartile of the (gender-specific) estimated firm effects. We find that the mean residuals are small in magnitude for all groups of movers.

The bottom rows of Table II present the main components of a simple decomposition of the variance of wages across workers implied by the fitted version of equation (4):

$$Var(w_{it}) = Var(\hat{\alpha}_i) + Var\left(\hat{\psi}^{G(i)}_{J(i,t)}\right) + 2Cov\left(\hat{\alpha}_i, \hat{\psi}^{G(i)}_{J(i,t)}\right)$$

$$(16) \qquad + Var\left(X'_{it}\hat{\beta}^{G(i)}\right) + 2Cov\left(\hat{\alpha}_i + \hat{\psi}^{G(i)}_{J(i,t)}, X'_{it}\hat{\beta}^{G(i)}\right) + Var(\hat{r}_{it}).$$

Among both male and female workers, person effects account 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%. 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 (<5%), reflecting the high R^2 coefficients for the underlying models.

VI.C. *Normalizing the Estimated Firm Effects*

The next step in our analysis is to renormalize the estimated firm effects from the models in Table II. Following the approach outlined in Section IV.B., we identify a threshold level for our measure of the size of the surplus available at a firm—value added per worker—such that firms below that threshold are "zero surplus" firms. Figure IV shows the relationship between average log value added per worker and the estimated firm effects for men and women (which were normalized for purposes of estimation by setting the effects to zero for the largest firm in the sample). We group firms into percentile bins of value added and plot the average male and female firm effects in each bin against mean log value added per worker for firms in the bin.

A striking feature of this figure is the piecewise linear nature of the relationship between the estimated firm effects and value added. Firms in the bottom 15 or so percentiles pay very similar average wages, while at higher percentiles the wage premiums for men and women are linearly increasing in log value added per worker, suggesting a constant elasticity relationship between

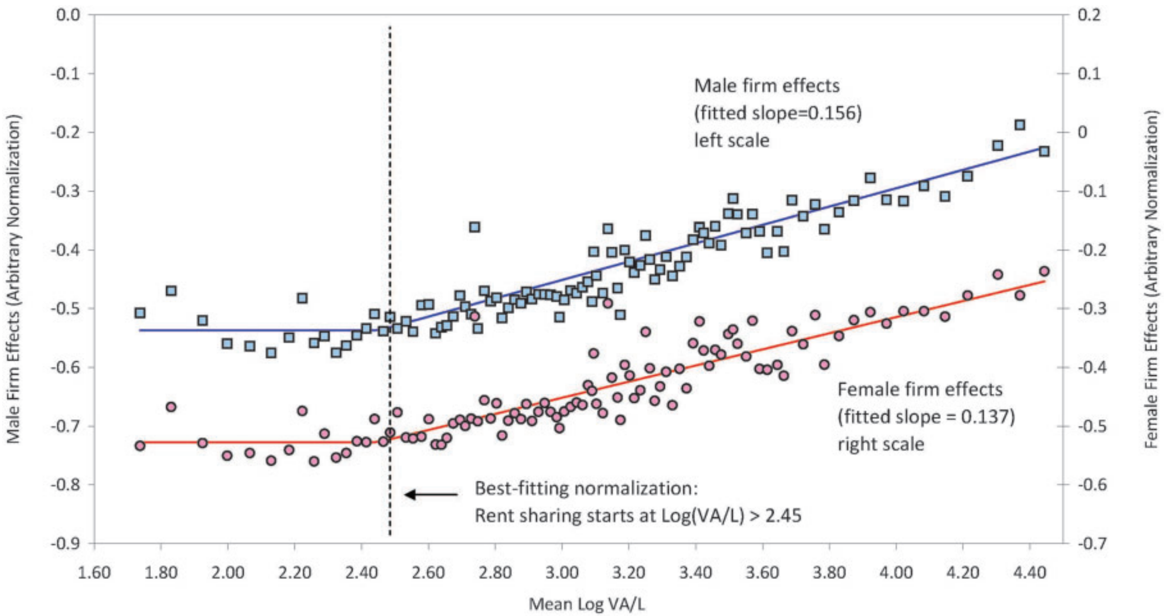

FIGURE IV

Firm Fixed Effects versus Log Value Added/Worker

Points shown represent mean estimated firm-specific wage premiums from AKM models for men and women, averaged across firms with value-added data available in 100 percentile bins of mean log value added per worker. See text for explanation of arbitrary normalization of the firm effects.

wages and value added above a kink point. To identify the kink point more formally, we fit a series of bivariate regression models of the form:

$$\hat{\psi}^M_{J(i,t)} = \pi^M_0 + \pi^M \max\left\{0, \overline{S}^o_{J(i,t)} - \tau\right\} + \nu^M_{J(i,t)}$$

$$\hat{\psi}^F_{J(i,t)} = \pi^F_0 + \pi^F \max\left\{0, \overline{S}^o_{J(i,t)} - \tau\right\} + \nu^F_{J(i,t)}, \tag{17}$$

where (as before) $\overline{S}^o_j$ is the average of log value added per worker at firm j and τ is a threshold beyond which the firm begins to share rents. We estimated these equations using firm-level data for all firms in the dual-connected sample that can be matched to the financial data set.[27] We then selected the value of τ that minimized the mean squared error of the system of two equations. This procedure selects a value of $\hat{\tau} = 2.45$, which visually matches the pattern in the figure. The estimated values of the coefficients π^M and π^F are 0.156 and 0.137, respectively.[28] We show the fitted relationships in Figure IV.

The implied set of "no surplus" firms (i.e., those with $\overline{S}^o_j < \hat{\tau}$) account for 9% of all person-years at dual-connected firms with financial information. As documented in Online Appendix Table B3, these firms are relatively small, have relatively low sales per worker, tend to employ more women than men, and are disproportionately concentrated in the hotel and restaurant sector. Given the estimate of $\hat{\tau}$, we then normalized the estimated firm effects for both genders to have employment-weighted averages of zero across all firms with $\overline{S}^o_j < \hat{\tau}$.[29] To check the sensitivity of our normalization procedures, we used a nonparametric bootstrap procedure to estimate the sampling error of $\hat{\tau}$, which yielded an estimated standard error of 0.09. We then recalculated the normalizing constants using the upper and lower bounds of the 95% confidence interval for τ. We obtained normalizing constants that are quite close to the baseline

27. We fit these equations to firm-level data using the 47,477 dual-connected firms with matched financial data, weighting each firm by the total number of person-years of employment at the firm in our data set. These firms account for 63% of the person-year observations at dual-connected firms.

28. Online Appendix Figure B7 shows the adjusted R^2 from the bivariate system for a range of values of τ and the associated estimates of the coefficients (π^M, π^F).

29. This is essentially the same as subtracting the estimated values of the constants ($\hat{\pi}^M_0$ and $\hat{\pi}^F_0$) in equation (17) from $\hat{\psi}^M_j$ and $\hat{\psi}^F_j$, respectively.

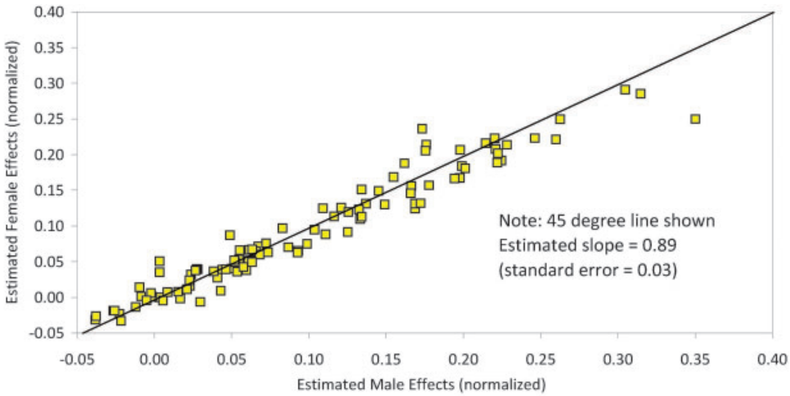

FIGURE V

Estimated Firm Effects for Female and Male Workers: Firm Groups Based on Mean Log Value Added per Worker

Figure shows bin scatter plot of estimated firm effects for female workers against estimated firm effects for male workers. Firm-level data are grouped into 100 percentile bins based on mean log value added per worker at the firm. Estimated slope is estimated across percentile bins by OLS.

constants for $\hat{\tau} = 2.45$, suggesting that our procedure is relatively insensitive to uncertainty about the location of $\hat{\tau}$. As described below, we also confirm this insensitivity using a normalization that assumes the mean wage premiums paid by firms in the hotel and restaurant sector are zero, and by replicating the procedure from equation (17) using sales per worker instead of value added per worker as the indicator of surplus.

Figure V graphs the normalized firm effects for women against the corresponding effects for men, using the same 100 groups as in Figure IV. As noted in Table II, there is a strong relationship between the average premiums paid to male workers in each group and the average premiums paid to female workers—the employment-weighted correlation of $\hat{\psi}_j^F$ and $\hat{\psi}_j^M$ is 0.59, and the corresponding regression of $\hat{\psi}_j^F$ on $\hat{\psi}_j^M$ has a slope of 0.56. Given the presence of sampling errors in the estimated firm effects, however, this is a downward-biased estimate of the rent-sharing ratio $\frac{\gamma^F}{\gamma^M}$. Grouping firms into cells based on their average value added per worker averages out the sampling errors and yields a relatively precisely estimated slope coefficient of 0.89.

VII. Firm-Specific Pay Premiums and the Gender Wage Gap

VII.A. Basic Decompositions

Next we use the normalized firm effects for men and women to quantify the impact of firm-specific pay premiums on the gender wage gap, using the framework of equations (8) and (9). The top row of Table III shows the terms involved in these alternative decompositions for all workers in the dual-connected sample. As shown in column (1), the gender wage gap for this sample is 0.234. Columns (2) and (3) show the mean values of the estimated firm wage premiums among men and women, respectively. These can be interpreted as estimates of the average rents received by men and women relative to jobs at no-surplus firms. The difference in column (4) (0.049) is the overall contribution of firm-specific pay premiums to the gender wage gap and accounts for 21% of the overall gender wage gap.

The part of this total that is attributable to the sorting channel can be calculated by evaluating the difference in the average of the male wage premiums weighted by the shares of men versus women at each firm, or by calculating a corresponding difference in the average of the female wage premiums. The first of these two estimates is shown in column (5), and amounts to 0.035 (or 15% of the overall gender wage gap), while the second is shown in column (6), and amounts to 0.047 (or 20% of the gap). Likewise, the contribution of the bargaining channel can be calculated either by taking the average difference in the estimated male and female wage premiums, weighted by the fraction of men at each firm (column (7)), or by taking the average difference in the two premiums, weighted by the fraction of women at each firm (column (8)). The first method yields a very small estimate of the bargaining effect (0.003 or 1.2% of the wage gap) and the second yields a somewhat larger estimate (0.015 or 6.3% of the gender gap).[30]

To interpret the magnitude of the bargaining effect, note that our estimate of the average rents received by male workers in

30. In the wage decomposition literature (e.g., Jann, 2008) the sorting effect is often called an "endowment" effect, since it evaluates the differences in the shares of men and women at different firms, using the "returns" to each firm calculated for either men or women. The bargaining effect is often called a "coefficient" effect, since it evaluates the differences in the estimated "returns" to working at a given firm for men versus women using the "endowments" of men or women.

TABLE III

CONTRIBUTION OF FIRM-SPECIFIC PAY PREMIUMS TO THE GENDER WAGE GAP AT DUAL-CONNECTED FIRMS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Means of Firm Premiums			Decompositions of Contribution of Firm Component			
					Sorting		Bargaining	
	Gender Wage Gap	Male Premium among Men	Female Premium among Women	Total Contribution of Firm Components	Using Male Effects	Using Female Effects	Using Male Distribution	Using Female Distribution
All	0.234	0.148	0.099	0.049	0.035	0.047	0.003	0.015
				(21.2)	(14.9)	(19.9)	(1.2)	(6.3)
By age group:								
Up to age 30	0.099	0.114	0.087	0.028	0.019	0.029	−0.001	0.009
				(28.1)	(18.9)	(29.3)	(−1.2)	(9.3)
Ages 31–40	0.228	0.156	0.111	0.045	0.029	0.040	0.004	0.016
				(19.7)	(12.6)	(17.8)	(1.9)	(7.0)
Over age 40	0.336	0.169	0.099	0.069	0.050	0.064	0.005	0.019
				(20.6)	(15.0)	(19.1)	(1.5)	(5.6)
By education group:								
< High school	0.286	0.115	0.055	0.059	0.045	0.061	−0.002	0.015
				(20.8)	(15.6)	(21.4)	(−0.6)	(5.2)
High school	0.262	0.198	0.137	0.061	0.051	0.051	0.010	0.010
				(23.3)	(19.6)	(19.5)	(3.8)	(3.7)
University	0.291	0.259	0.213	0.047	0.025	0.029	0.018	0.022
				(16.1)	(8.7)	(9.9)	(6.2)	(7.4)

Notes. Sample includes male and female workers in dual connected set (Table I, columns (5)–(6)). Entry in column (1) is the difference in mean log wages of males and females, estimated over all workers in the subset of the dual-connected set indicated by the row heading. Estimated firm effects are from models described in columns (1) and (2) of Table II. Entry in column (4) is the total contribution of firm-specific wage premiums to the gender wage gap reported in column (1). Entries in columns (5)–(8) are the contributions of sorting effect and bargaining effect to gender wage gap, calculated using method described in the text. Entries in parentheses represent the percent of the overall male-female wage gap (in column (1)) that is explained by the source described in column heading.

Portugal is modest (14.8%). If women and men had the same distribution across firms, but women earned only 90% of the wage premiums received by men (i.e., $\frac{\gamma^F}{\gamma^M} = 0.9$), then we would obtain an estimate of the bargaining effect equal to 1.5%. This is about equal to the estimate in column (8) based on the female distribution of workers across firms. The estimate of the bargaining effect based on the male distribution is smaller, implying that men are relatively concentrated at firms where the gap $\hat{\psi}_j^M - \hat{\psi}_j^F$ is small.

The lower rows of Table III present a parallel set of decompositions for different age and education subgroups. Comparing across age groups (rows 2–4) the entries in column (1) show that the male-female wage gap in Portugal widens dramatically with age. Firm-specific pay differentials contribute to this pattern, with most of the increase attributable to a rise in the sorting effect with age. A higher-resolution summary is provided in Figure VI, which shows the overall gender gap (plotted with triangles) and the components of our decomposition for 2–4-year age bins. Our estimate of the average rents received by men (plotted with squares) shows that these rise with age until the mid-thirties, and then are relatively stable until the mid-fifties, when they begin to fall off. The age profile of average rents for women (plotted with circles) is flatter and peaks earlier. Thus our estimate of the total contribution of firm wage premiums to the gender wage gap (plotted with diamonds) rises until the mid-fifties, peaking at around 7.5 percentage points. As shown by the dotted lines at the bottom of the figure, the sorting component explains between 75% and 95% of the overall contribution.

Comparisons across education groups in the bottom rows of Table III show that the gender wage gap is roughly constant across education groups, but the average pay premiums received by both men and women are increasing with years of schooling, confirming that there is positive assortative matching between higher-skilled workers and higher-paying firms. As shown in column (4), the net effect of firm-specific pay premiums on the gender wage gap is about the same for workers with less than high school or high school education, but is somewhat smaller for university-educated workers, reflecting a much smaller sorting effect for these workers, coupled with a larger bargaining effect.

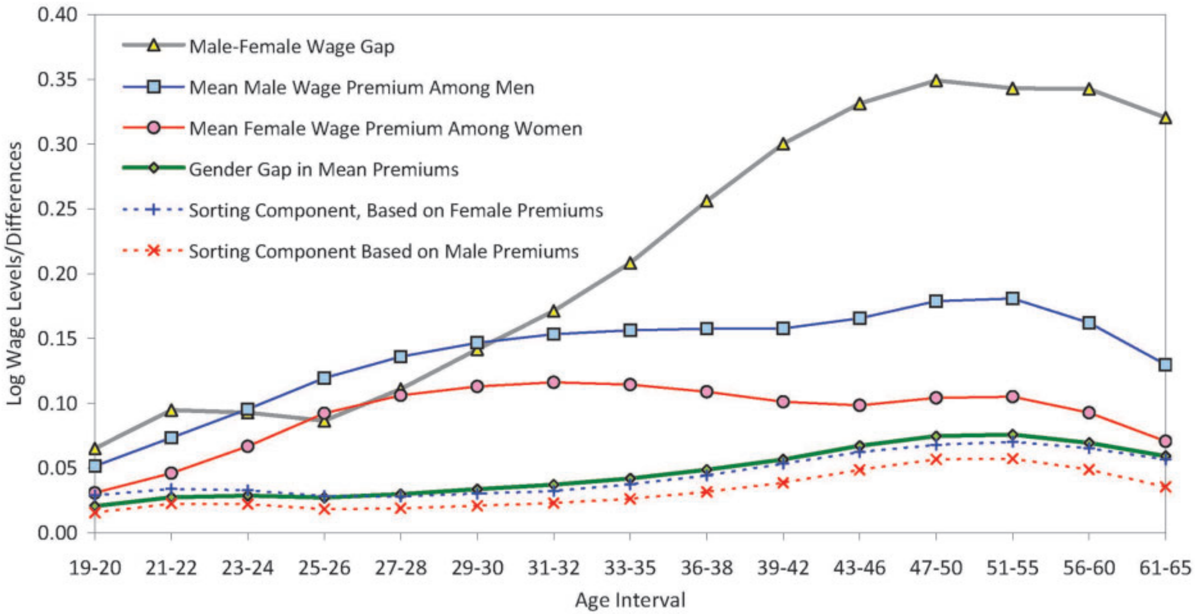

FIGURE VI

Evolution of Gender Wage Gap and Its Components over the Life Cycle

Figure shows unadjusted male-female wage gap, means of firm-specific wage premiums earned by men and women, and the difference in mean premiums, which is the total contribution of firm-specific wage components to the gender wage gap. Dashed lines show the effect of differential sorting of males and females to specific firms, evaluated using male and female firm-specific wage premiums.

VII.B. Decompositions by Occupation and Industry

Men and women tend to work in different occupations (see, e.g., Manning and Swaffield 2008 and Goldin 2014 for recent analyses; Cardoso, Guimarães, and Portugal forthcoming for a discussion in the Portuguese context). This raises the question of whether some of the differences identified in Table III are actually due to occupation rather than gender. We investigate this issue in Table IV, assigning each worker to his or her modal occupation. Notice first that the gender wage gap varies widely across occupations, from around 15% for professionals, technicians, clerks, and service workers to 40% for craft occupations. The average size of the firm-specific wage premiums received by male and female workers also varies substantially, with a net contribution to the gender wage gap that ranges from 1% for managers and service workers to 6% or more for technicians, clerks, and craft workers. For most occupations the sorting effect is larger than the bargaining effect, though for managers and professionals—the two groups with the highest fraction of university-educated workers—the bargaining effect is relatively large, consistent with the patterns in Table III.

A potential concern with these comparisons is that some of the differential bargaining power we measure for women may actually be due to differential rent sharing across occupations. To investigate this possibility we fit separate AKM models for male and female workers who work in "mainly male" or "mainly female" occupations, allowing unrestricted firm effects for each gender and occupation group.[31] We then investigated whether there is a systematic difference in bargaining power between men and women who work in a given occupation group. The results are summarized in Online Appendix Table B4, and show that while between-firm sorting is an important component of the gender gap for workers in both occupation groups, the bargaining effect is concentrated among workers in traditionally male occupations.

We have also examined the contributions of the sorting and bargaining channels to the gender wage gap for workers in

31. More detail is provided in Section B of the Online Appendix. We classify individuals as having "mainly female" or "mainly male" occupations depending on whether the average share of females in the occupations they are observed holding in our sample is above or below the median for all occupations. Using this approach, 85% of women and 27% of men have mainly female occupations; 15% of women and 73% of men have mainly male occupations.

TABLE IV

CONTRIBUTION OF FIRM-LEVEL PAY COMPONENTS TO GENDER WAGE GAP, BY MODAL OCCUPATION

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Means of Firm Premiums:			Decompositions of Contribution of Firm Component:			
					Sorting		Bargaining	
	Gender Wage Gap	Male Premium among Men	Female Premium among Women	Total Contribution of Firm Components	Using Male Effects	Using Female Effects	Using Male Distribution	Using Female Distribution
All	0.234	0.148	0.099	0.049	0.035	0.047	0.003	0.015
				(21.2)	(14.9)	(19.9)	(1.2)	(6.3)
By modal occupation:								
Managers	0.238	0.219	0.210	0.010	−0.005	−0.003	0.013	0.014
(69% male)				(4.0)	(−2.1)	(−1.4)	(5.4)	(6.1)
Professionals	0.153	0.253	0.221	0.032	0.007	0.016	0.016	0.025
(51% male)				(20.9)	(4.3)	(10.3)	(10.6)	(16.6)
Technicians	0.157	0.238	0.179	0.059	0.040	0.039	0.020	0.019
(65% male)				(37.5)	(25.4)	(24.5)	(13.0)	(12.1)
Clerks	0.128	0.217	0.161	0.056	0.044	0.047	0.009	0.012
(41% male)				(43.8)	(34.4)	(36.6)	(7.1)	(9.4)
Services	0.145	0.055	0.046	0.008	0.009	0.020	−0.012	0.000
(32% male)				(5.8)	(6.0)	(13.9)	(−8.0)	(0.2)
Craft	0.389	0.088	0.016	0.072	0.041	0.078	−0.006	0.031
(68% male)				(18.6)	(10.5)	(20.1)	(−1.5)	(8.1)
Operatives	0.303	0.150	0.104	0.045	0.032	0.036	0.009	0.013
(75% male)				(15.0)	(10.6)	(11.9)	(3.1)	(4.4)
Elementary	0.196	0.109	0.055	0.054	0.033	0.066	−0.012	0.022
(47% male)				(27.6)	(16.6)	(33.7)	(−6.1)	(11.0)

Notes. see notes to Table III. Workers are classified into their most common occupation during years they are observed. Farm and fishing workers are included with operatives.

different major industries. The results, summarized in Online Appendix Table B5, show that in most industries women are underrepresented at firms that pay higher wage premiums for men and women, with particularly large sorting effects in the chemical, nonmetallic mineral, business services, and utility industries. Likewise, the wage premiums paid to women are smaller than the premiums for men in most sectors, with relatively large bargaining effects in the food products, paper and publishing, and chemical industries. An interesting exception to these patterns is construction, which has the smallest fraction of female workers among the major industries (11%). Females in construction are better-educated than males, earn higher average wages, and tend to be sorted to firms that pay higher wages to both men and women (i.e., a sorting effect of the "wrong sign").

VII.C. An Alternative Normalization

Our estimates of the relative bargaining effect rely on a normalization that allows us to estimate the average rents earned by men and women. To check the robustness of our findings, we considered an alternative normalization based on the assumption that firms in the hotel and restaurant industry pay zero rents to workers on average. Firms in this sector pay the lowest average wage premiums of all major industries (see Online Appendix Table B5). Job turnover rates are also high, suggesting that workers are able to find a job in the industry relatively easily. We therefore normalized the estimated wage premiums for men and women such that the weighted average of both premiums is 0 in the sector (weighting by the total number of workers at each firm). Online Appendix Table B6 reproduces the decompositions in Table III using this alternative normalization assumption. The estimated sorting effects are invariant to normalization and are therefore the same as in Table III. The estimated bargaining effects, however, are uniformly larger, reflecting the fact that the mean of the estimated wage premiums for male workers is about the same under the baseline and alternative normalizations, but the mean of the estimated premiums for female workers falls by about 2 percentage points under the alternative normalization. As a result the estimated bargaining power effects are all increased by 0.021 relative to those presented in Table III.[32]

32. Inspection of expressions (8) and (9) shows that if the estimated female premiums are all adjusted downward relative to the male premiums, then the

Under this alternative normalization, firm-specific wage premiums explain about 30% of the gender wage gap for workers as a whole, with 15–20% explained by sorting effects and 10–15% explained by relative bargaining effects. This alternative normalization therefore suggests that our baseline procedure leads to a conservative estimate of the bargaining power effect.

VII.D. Compensating Differentials for Hours?

A recent literature (e.g., Bertrand, Goldin, and Katz 2010; Goldin 2014) suggests that part of the gender wage gap is due to compensating differentials for long hours of work. If some firms offer packages of high wages and long hours that are relatively unattractive to female workers, we would attribute the resulting pattern of wage and employment outcomes to the sorting channel. To assess the potential role of hours differences in accounting for differences in firm-specific pay premiums, we calculated two alternative measures of average hours worked by the male and female employees at each firm—one based on regular contractual hours, the other based on total hours, including regular hours and overtime. We then regressed the estimated wage premiums for each gender on the alternative measures of mean hours. We fit OLS models with and without controls for major industry, and IV models in which we used hours of the other gender group as an instrument for each group's hours to address the "division bias" problem (Borjas 1980) that arises because wages are constructed by dividing monthly earnings by monthly hours.[33] The results, summarized in Online Appendix Table B7, show no evidence that differences in mean hours (excluding or including overtime) are significant determinants of the wage premiums offered by different firms to either men or women, once we account for systematic differences across industries.

Of course there may be other features of the hours "packages" offered by different firms—for example, requirements for flexibility in responding to seasonal demand shocks—that we cannot measure, since we only see hours in the survey reference

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estimated bargaining effect for any subgroup of workers is raised by the difference in the adjustment factors.

33. This procedure will not eliminate all the bias if the measurement errors in male and female hours at the same firm are correlated, for example, by misreporting practices that lead to over- or understatement of the hours of all workers at a firm.

week of the QP. Thus we cannot rule out that part of the variation in the firm-specific wage premiums offered to men and women is driven by differences in hours-related requirements or other unobserved features of the job.

VII.E. Confounding Effects of the Minimum Wage?

During our sample period, the minimum wage in Portugal was typically set at 50% of the median wage of full-time workers (OECD 2015b). This is higher than in the United States or the United Kingdom and potentially high enough to compress the gender wage gap (see DiNardo, Fortin, and Lemieux 1996). Indeed, among workers in our dual-connected sample, 7% of men and 18% of women have a wage in at least one year that is within 5% of the minimum wage. Upward pressure from the minimum wage might constrain some firms that would otherwise offer lower wages to female workers, pushing up ψ_j^F relative to ψ_j^M and leading us to conclude that the relative bargaining effect is small. To examine this possibility, we reestimated our AKM models using workers who are at least 25 years old with a high school education or more. Among this subgroup (who represent 24% of all men and 31% of all women in our analysis sample), only 2% of the men and 3% of the women ever have a wage within 5% of the minimum wage. We then repeated the normalization exercise described in Section VI.C. and formed a new set of decompositions. The results are presented in Section C of the Online Appendix.

Eliminating younger and less educated workers leads to a reduction in the fraction of the remaining workers in the connected sets for each gender group, since many of the links between firms are formed through mobility of these groups. The selective loss of firms that mainly hire less-educated workers leads to an attenuation of the between-firm sorting effect on the gender wage gap (see Online Appendix Table C2). The estimated impact of the bargaining channel, however, is larger in this subsample than in the workforce as a whole, and comparable to the estimate we obtain for university educated workers in our main analysis—around 2 percentage points. Interestingly, the estimated bargaining power effect for university-educated workers is similar whether we use firm effects estimated for all workers (as in our main analysis) or the firm effects estimated only for higher-educated workers. This suggests that the firm effects for women in our main analysis are not significantly attenuated by inclusion of younger and less educated workers whose wages are constrained by the national minimum wage.[34]

VIII. Firm Wage Premiums and Measured Surplus

VIII.A. Estimates of Rent-Sharing Models

Here we turn to the question of how the estimated wage premiums for different firms are related to measures of the potential surplus at the firm. As a starting point, we note that the simple correlation coefficients between mean log value added per worker and the firm effects are 0.42 for men and 0.38 for women, while the corresponding correlations with mean log sales per worker are 0.38 and 0.36. Given the presence of sampling errors in the estimated wage premiums, and potential noise in measures of value added and sales (particularly for the small firms that compose the bulk of our sample), these correlations are reassuringly strong.

Table V presents estimates of the rent-sharing coefficients π^M and π^F based on equation (11), using three alternative measures of surplus. The models in row 1 use the net surplus measure from our baseline normalization procedure, which is based on mean log value added per worker. The estimation sample includes all firms in the dual-connected set that can be linked to SABI and have at least one year of value added data. By construction, the estimates are identical to the estimates obtained from equation (17) at the optimized value for $\hat{\tau}$, and yield elasticities of 0.16 for men and 0.14 for women.[35] To estimate the

34. To test the attenuation hypothesis directly, we regressed the estimated firm effects for women from our main analysis on the estimated effects obtained using only older and more educated workers, instrumenting the right-hand-side variable with the estimated firm effect for men from our main analysis. The model was estimated at the firm level, weighting by the number of person-year observations for females at the firm. The estimated coefficient is 1.02 (standard error 0.01), suggesting that there is minimal attenuation of the firm effects from our main analysis.

35. These coefficients are slightly bigger than the IV estimates of the elasticity of wages with respect to value added per worker reported by Card, Devicienti and Maida (2014, hereafter CDM) based on matched worker-firm data for Italy (e.g., 0.09 in Table A4 of CDM). Using the average ratio of quasi-rents to value added reported by CDM, the coefficients in Table V imply elasticities of wages with respect to quasi rents per worker of about 0.07—comparable to the estimates reported

TABLE V

Estimated Relationship between Gender-Specific Firm Effects and Measures of Surplus per Worker

	(1)	(2)	(3)	(4)
		Regressions of Firm Effects on Measure of Surplus:		
	Number of Firms	Male Firm Effects	Female Firm Effects	Ratio: Column (3) / Column (2)
Surplus measure:				
1. Excess mean log value added per worker	47,477	0.156	0.137	0.879
		(0.006)	(0.006)	(0.031)
2. Mean log sales per worker	75,163	0.072	0.064	0.897
		(0.005)	(0.004)	(0.036)
3. Excess mean log sales per worker	75,163	0.092	0.081	0.883
		(0.006)	(0.006)	(0.038)

Notes. Columns (2)–(3) report coefficients of surplus measure indicated in row heading in regression models in which the dependent variables are the estimated firm effects for males or females. All specifications include a constant and are estimated at the firm level, weighting by the total number of male and female workers at the firm. Ratios in column (4) are estimated by instrumental variables, treating the firm effect in female wages as the dependent variable, the firm effect in male wages as the endogenous explanatory variable, and the surplus measure as the instrument. Standard errors, clustered by firm, are in parentheses.

sampling error for the ratio, note that $\frac{\hat{\pi}^F}{\hat{\pi}^M}$ is the two-stage least squares estimate of the parameter δ_1 from a simple model of the form:

$$\hat{\psi}^F_{J(i,t)} = \delta_0 + \delta_1 \hat{\psi}^M_{J(i,t)} + e_{J(i,t)},$$

using net surplus as an instrumental variable for $\hat{\psi}^M_j$. We therefore use the conventional standard error of the two-stage least squares estimator as our estimated standard error for the ratio. The estimated ratio is 0.88 with a standard error of 0.03 (column (4)). We can therefore rule out the null hypothesis of

by Arai and Heyman (2009) for Swedish workers, Martins (2009) for Portuguese workers, Guertzgen (2009) for German workers, and Guiso, Pistaferri, and Schivardi (2005) for Italian workers, but only about one-quarter as large as elasticities estimated by Abowd and Lemieux (1993) and Van Reenen (1996) using firm-level data without controls for worker quality. See CDM for a more detailed summary of the recent rent-sharing literature.

equal rent sharing ($\pi^F = \pi^M$) in favor of the alternative that women receive a smaller share of the component of firm-wide rents that is directly related to excess value added.

In row 2 we check the robustness of this conclusion using mean log sales per worker as an alternative proxy for the potential surplus at each firm. Since sales (for the previous calendar year) are reported in QP, we are able to expand the sample to include all firms in the dual-connected set with reported sales in at least one year. Sales are also measured independently of labor costs at the firm, so a finding that our conclusions are robust to using sales per worker provides a check that there is not a measurement-related problem in using value added per worker.

As expected, given that sales per worker are significantly noisier than value added per worker, the estimated rent-sharing elasticities in row 2 are smaller in magnitude than the elasticities in row 1.[36] Nevertheless their ratio is virtually the same and is again significantly different from 1. The models in row 3 use a third indicator, which is derived from an alternative normalization procedure in which we define net surplus using the excess of sales per worker over a minimum threshold level (see the next section). This choice leads to a slight increase in the rent-sharing coefficients relative to the specifications in row 2, but again their ratio is nearly invariant.

To probe the robustness of the results in Table V we reestimated the models including controls for industry (20 dummies) and location (dummies for firms located in Lisbon or Porto) and a quadratic in firm size (based on average total employment in all years). Estimates from these models are presented in Online Appendix Table B8. In brief, the addition of controls leads to a slight attenuation (on the order of 10–15%) in the estimated rent sharing coefficients, with a slightly bigger attenuation of the coefficients for women than men. These models therefore reinforce our conclusion that women get a smaller share of rents than men, though in all cases the estimated ratios are within a standard error of the ratios in Table V.

36. A regression of mean log value added per worker on mean log sales per worker has a coefficient of 0.39 across firms with valid data for (trimmed) value added per worker. Interestingly, the reverse regression of mean log sales per worker on mean log value added per worker has a coefficient of 1.08, suggesting that log sales per worker is (approximately) equal to log value added per worker plus noise.

In summary, we find that the estimated firm-specific wage premiums for men and women are highly correlated with measures of the surplus per worker at the firm. Importantly, the estimated correlations are uniformly smaller for women than men, providing strong support for the view that women get a smaller share of the surplus than men.

VIII.B. Decompositions of the Gender Wage Gap Using Observed Measures of Surplus

The component of the firm-specific wage premium received by male workers that is directly attributed to observable surplus is $\hat{\pi}^M E[NS_{J(i,t)}|male]$, while the corresponding component for female workers is $\hat{\pi}^F E[NS_{J(i,t)}|female]$. Their difference gives the contribution of the observable component of surplus to the gender wage gap and can be decomposed into bargaining and sorting channels using equations (12) and (13). In Online Appendix Table B9 we present estimates of the terms in these two alternative decompositions, using the overall sample of dual-connected workers. We find that the component of the firm effects that is explained by our observed measure of surplus accounts for about 80% of the total rent premiums received by both men and women, and 80% of the impact of firm-specific premiums on the gender wage gap. Applying the decompositions in equations (12) and (13), we find that differential sorting of men to high-surplus firms accounts for about two-thirds of the total effect of our surplus indicator, while the lower bargaining power of women accounts for about one-third, or 1–1.5 log points of the overall gender gap. This evidence on the differential sharing of observed surpluses reinforces our conclusion that the lower relative bargaining power of women contributes to the gender wage gap, particularly for subgroups of workers who are most likely to work at high-surplus firms (e.g., higher-educated workers).

VIII.C. Sales per Worker as an Alternative Measure of Surplus

In our main analysis we use data on value added per worker to choose the normalization for the estimated firm-specific wage premiums, and to measure the relative bargaining power of male and female workers. To check the robustness of our procedures, we redid our analysis using sales per worker as an alternative indicator of surplus. As discussed already, an advantage of sales is that it is reported in QP for most firms (89% of the 84,720 firms in our dual-connected sample).

Section D of the Online Appendix summarizes the results from this alternative approach. Online Appendix Figure D1 graphs the (unnormalized) firm effects for men and women against mean log sales per worker. As we found using mean log value added per worker as a measure of the surplus, there is a clear visual kink in the relationship between the estimated firm effects and mean log sales per worker. Using the procedure described in Section VI.C, we identified the kink point τ_s. We then define excess mean log sales per worker as max $\{0, \overline{S}_j - \overline{L}_j - \hat{\tau}_s\}$ where $(\overline{S}_j - \overline{L}_j)$ represents mean log sales per worker, calculated using annual sales data in QP for all years in which it is available for a given firm. Online Appendix Table D1 presents a series of decompositions parallel to those in Table III but using the normalization that mean firm premiums for workers at firms with $\overline{S}_j - \overline{L}_j \leq \hat{\tau}_s$ equal 0. Reassuringly, this alternative normalization yields estimates of the bargaining effect that are essentially identical to the estimates from our baseline procedure. Finally, as reported in row 3 of Table V, use of excess mean log sales per worker as a measure of surplus yields estimates of the rent-sharing coefficients for male and female workers that are about 60% as large as the estimates based on excess mean log value added per worker (consistent with the fact that sales per worker is substantially noisier than value added per worker) but have the same ratio as we found using value added per worker. Overall, we conclude that our results are highly robust to the use of either value added per worker or sales per worker as an indicator of the surplus available at each firm.

IX. Within-Firm Changes in Profitability and Wages

As a final step in our analysis, we use observations from the last four years of our analysis sample to measure the effects of changes in the measured surplus at each firm on the wages of male and female job stayers. Our base sample includes information on some 280,000 men and 200,000 women who were employed continuously at a firm that can be linked to value-added information for 2006 and 2009 in SABI. These workers have similar age, education, and wages as men and women in our overall analysis sample (see Online Appendix Table B10). Moreover, the

TABLE VI

EFFECTS OF CHANGES IN MEASURED SURPLUS PER WORKER ON THE CHANGE IN WAGES OF STAYERS

Surplus measure and sample:	(1) Number of Firms	(2) Estimated Rent Sharing Coefficients: Male Stayers	(3) Estimated Rent Sharing Coefficients: Female Stayers	(4) Ratio: Column (3) / Column (2)
1. Excess log value added per worker (Winsorized at +/- 0.50). Sample = stayers at firms with value added data, 2006–9	33,104	0.049 (0.007)	0.045 (0.008)	0.911 (0.086)
2. Excess log value added per worker (not Winsorized). Sample = stayers at firms with value added data, 2006–9	33,104	0.035 (0.006)	0.031 (0.006)	0.894 (0.091)
3. Excess log sales per worker (Winsorized at +/- 0.50). Sample = stayers at firms with sales data, 2005–8	44,266	0.021 (0.006)	0.018 (0.005)	0.876 (0.182)

Notes. Dependent variables are average change in wages of male or female workers at a firm (regression-adjusted for quadratic in age). Table entries are coefficients of the measured change in surplus per worker, as defined in row heading. Ratios in column (4) are estimated by instrumental variables, treating average change in wages of female stayers as the dependent variable, average change in wages of male stayers as the endogenous explanatory variable, and the change in surplus measure as the instrument. Standard errors, clustered by firm, are in parentheses.

wage gap between male and female stayers is 22 log points in both 2006 and 2009—about the same as in our dual-connected set.

Table VI presents a series of models based on equation (15) that show the relationship between the change in excess log value added at a firm and the wage changes of male and female stayers. We estimate these models in two steps, first regressing individual wage changes on a quadratic in age (separately by gender) and firm indicators, then in a second stage regressing the estimated firm-specific coefficients on the change in excess log value added at the firm, weighted by the number of workers at the firm in the gender group. Given the large variability in measured value added, our main specifications Winsorize the change in excess value added at +/- 0.50. As shown in row 1, the resulting estimates of the rent-sharing coefficients for male and female stayers are 0.049 and 0.045, respectively. Their ratio, shown in column (4), is 0.91 (with a standard error of 0.09). The point estimate of the relative bargaining power of women is therefore quite consistent with estimates based on between firm comparisons (in Table V), though we cannot rule out a value of 1 for the ratio. Row 2 shows the same specification, estimated without Winsorizing the change in excess value added. Although the rent-sharing coefficients become about 30% smaller—as would be expected if some of the very large changes are due to measurement error—their ratio remains very close to 0.9.

Row 3 uses a larger sample of stayers, observed over the period from 2005 to 2008, for which we have data on sales from the QP. (The earlier sample period reflects the fact that the QP data report sales from the previous calendar year, so the latest sales data are for 2008.) Using excess log sales per worker (defined in Section VIII.C) as our measure of surplus, we again estimate significant rent-sharing coefficients for men and women, with a ratio of 0.88, though the standard error is relatively large.

Compared to the rent-sharing coefficients from the between-firm analysis Table V, the estimates for stayers are only about 30% as large (e.g., 0.049 for men in row 1 of Table VI versus 0.156 for men in row 1 of Table V). There are several plausible explanations for the discrepancy. First, we suspect that our measures of surplus are relatively noisy, and that taking the difference over a four-year period leads to a decrease in the signal-to-noise ratio relative to the average over the same period. Second, contrary to equation (1), it may be that wages are less responsive to transitory fluctuations in rents than to permanent differences. Guiso, Pistaferri, and Schivardi (2005), for example, analyze the relationship between wages and firm value added using Social Security earnings record for Italian workers and find smaller impacts of short-run changes in value added than for long-run changes. A third possibility is that the rent-sharing coefficients are attenuated because we are focusing on a selected sample of job stayers. To evaluate this possibility we constructed a simple grouped data control function (Gronau, 1974) for selection bias for the male and female stayers at each firm, based on the fractions of workers employed at the firm in 2006 who stayed to 2009, and reestimated the models including this control.[37] The results,

37. The control function is $\frac{\phi(\Phi^{-1}(p))}{p}$, where p is the fraction of stayers among the (gender-specific) set who were at the firm in the base period, ϕ is the normal density function, and Φ is the normal cumulative density function. This is an appropriate control function if the individual probability of staying is determined by a latent

presented in Online Appendix Table B11, give no indication of selectivity bias.

Overall, the estimates in Table VI, while limited by the relatively short sample period over which we can observe job stayers, are supportive of the hypothesis that female workers gain less than their male coworkers when their employer becomes more profitable. Indeed, our estimates of the ratio $\frac{\gamma^F}{\gamma^M}$ are centered around 0.9—very similar to the ratios in Table V. We have estimated a variety of additional models for other subgroups of male and female stayers, including workers in larger and smaller firms, workers in firms with larger and smaller fractions of female employees, and workers in firms with higher and lower within-firm wage inequality. Unfortunately, as suggested by the standard errors for the estimated ratios in Table VI, 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 0.9—the average ratio across firms.

X. Conclusions

A growing body of research argues that firm-specific wage premiums are a pervasive and economically important feature of labor market earnings. These premiums will contribute to the gender wage gap if women tend to work at firms that offer smaller premiums, or if female employees tend to earn smaller premiums than their male colleagues when employed at the same firms. Our analysis of Portuguese data finds that female employees receive about 90% of the wage premiums that men earn at equivalent firms. Moreover, women are disproportionately likely to work at low-surplus firms paying small premiums to both genders. We conclude that the sorting and bargaining channels together explain about 20% of the gender wage gap in Portugal, with roughly two-thirds of this 20% explained by sorting and one-third by the shortfall in relative bargaining power.

Our approach to combining worker-firm fixed effects models with Oaxaca-style decompositions into sorting and bargaining components is potentially applicable to other classic wage gaps,

index with a firm-specific component and a normally distributed error, and individual wage changes have a normally distributed error.

including the black/white wage gap, the immigrant/native wage gap, the experience profile of wages, and wage gaps based on various measures of intelligence—explanations for which have traditionally relied on a market price perspective. The proliferation of rich employer-employee data sets offers the opportunity to determine the extent to which these heavily studied sources of wage inequality are in fact mediated by heterogeneity across firms.

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Supplementary Material

An Online Appendix for this article can be found at QJE online (qje.oxfordjournals.org).

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