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Unequal pay or unequal employment? A cross-country analysis of gender gaps*

Claudia Olivetti
Boston University

Barbara Petrongolo
London School of Economics
CEP, CEPR and IZA

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Abstract

Gender wage and employment gaps are negatively correlated across countries. We argue that non-random selection of women into work explains an important part of such correlation and thus of the observed variation in wage gaps. The idea is that, if women who are employed tend to have relatively high-wage characteristics, low female employment rates may become consistent with low gender wage gaps simply because low-wage women would not feature in the observed wage distribution. We explore this idea across the US and EU countries estimating gender gaps in potential wages. We recover information on wages for those not in work in a given year using alternative imputation techniques. Imputation is based on (i) wage observations from other waves in the sample, (ii) observable characteristics of the nonemployed and (iii) a statistical repeated-sampling model. We then estimate median wage gaps on the resulting imputed wage distributions, thus simply requiring assumptions on the position of the imputed wage observations with respect to the median, but not on their level. We obtain higher median wage gaps on imputed rather than actual wage distributions for most countries in the sample. However, this difference is small in the US, the UK and most central and northern EU countries, and becomes sizeable in Ireland, France and southern EU, all countries in which gender employment gaps are high. In particular, correction for employment selection explains more than a half of the observed correlation between wage and employment gaps.

Keywords: median gender gaps, sample selection, wage imputation.
JEL classification: E24, J16, J31

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1 Introduction

There is substantial international variation in gender pay gaps, from 25-30 log points in the US and the UK, to 10-20 log points in a number of central and northern European countries, down to an average of 10 log points in southern Europe. International differences in overall wage dispersion are typically found to play a role in explaining the variation in gender pay gaps (Blau and Kahn 1996, 2003). The idea is that a given level of dissimilarities between the characteristics of working men and women translates into a higher gender wage gap the higher the overall level of wage inequality. However, OECD (2002, chart 2.7) shows that, while differences in the wage structure do explain an important portion of the international variation in gender wage gaps, the inequality-adjusted wage gap in southern Europe remains lower than in the rest of Europe and in the US.

In this paper we argue that, besides differences in wage inequality and therefore in the returns associated to characteristics of working men and women, a significant portion of the international variation in gender wage gaps may be explained by differences in characteristics themselves, whether observed or unobserved. This idea is supported by the striking international variation in employment gaps, ranging from 10 percentage points in the US, UK and Scandinavian countries, to 15-25 points in northern and central Europe, up to 30-40 points in southern Europe and Ireland. If selection into employment is non-random, it makes sense to worry about the way in which selection may affect the resulting gender wage gap. In particular, if women who are employed tend to have relatively high-wage characteristics, low female employment rates may become consistent with low gender wage gaps simply because low-wage women would not feature in the observed wage distribution. This idea could thus be well suited to explain the negative correlation between gender wage and employment gaps that we observe in the data (see Figure 1).

Different patterns of employment selection across countries may in turn stem from a number of factors. First, there may be international differences in labor supply behavior and in particular in the role of household composition and/or social norms in affecting participation. Second, labor demand mechanisms, including social attitudes towards female employment and their potential effects on employer choices, may be at work, affecting both the arrival rate and the level of wage offers of the two genders. Finally, institutional differences in labor markets regarding unionization and minimum wages may truncate the wage distribution at different points in different countries, affecting both the composition of employment and the observed wage distribution. In this paper we will be agnostic as regards the separate role of these factors in shaping gender gaps, and aim at recovering alternative measures of selection-corrected gender wage gaps.

Although there exist substantial literatures on gender wage gaps on one hand, and gender employment, unemployment and participation gaps on the other hand,[1] to our knowledge the variation in both quantities and prices in the labor market has not been simultaneously exploited

[1] See Altonji and Blank (1999) for an overall survey on both employment and gender gaps for the US, Blau and Kahn (2003) for international comparisons of gender wage gaps and Azmat, Güell and Manning (2006) for international comparisons of unemployment gaps.

to understand important differences in gender gaps across countries. In this paper we claim that the international variation in gender employment gaps can indeed shed some light on well-known cross-country differences in gender wage gaps. We will explore this view by estimating selection-corrected wage gaps.

In our empirical analysis we aim at recovering the counterfactual wage distribution that would prevail in the absence of non-random selection into work - or at least some of its characteristics. In order to do this, we recover information on wages for those not in work in a given year using alternative imputation techniques. Our approach is closely related to that of Johnson, Kitamura and Neal (2000) and Neal (2004), and simply requires assumptions on the position of the imputed wage observations with respect to the median. Importantly, it does not require assumptions on the actual level of missing wages, as typically required in the matching approach, nor it requires arbitrary exclusion restrictions often invoked in two-stage Heckman sample selection correction models.

We then estimate raw median wage gaps on the sample of employed workers (our base sample) and on a sample enlarged with wage imputation for the nonemployed, in which selection issues are alleviated. The impact of selection into work on estimated wage gaps is assessed by comparing estimates obtained under alternative sample inclusion rules. The attractive feature of median regressions is that, if missing wage observations fall completely on one or the other side of the median regression line, the results are only affected by the position of wage observations with respect to the median, and not by specific values of imputed wages. One can therefore make assumptions motivated by economic theory on whether an individual who is not in work should have a wage observation below or above median wages for their gender.

Imputation can be performed in several ways. First, we use panel data and, for all those not in work in some base year, we search backward and forward to recover hourly wage observations from the nearest wave in the sample. This is equivalent to assuming that an individual's position with respect to the base-year median can be recovered by the ranking of her wage from the nearest wave in the base-year distribution. As such position is determined using levels of wages in other waves in the sample, we are in practice allowing for selection on unobservables.

While imputation based on this procedure arguably uses the minimum set of potentially arbitrary assumptions, it has the disadvantage of not providing any wage information on individuals who never worked during the sample period. In order to recover wage observations also for those never observed in work, we make assumptions on their position with respect to the median, based on their observable characteristics, specifically unemployment status, education, experience and spouse income. In this case we are allowing for selection on observable characteristics only. Having done this, earlier or later wage observations for those with imputed wages in the base year can shed light on the goodness of our imputation methods.

Finally, we extend the framework of Johnson et al. (2000) and Neal (2004) by using probability models for assigning individuals on either side of the median of the wage distribution. We first estimate the probability of each individual belonging above or below their gender-specific median using a simple human capital specification. Individuals are then assigned above- or below-median wages according to such predicted probabilities, using repeated imputation techniques (Rubin, 1987). More specifically, the missing wage values are replaced by (a small number of) simulated versions, thus obtaining independent simulated datasets. The estimated wage gaps on each of the simulated complete datasets are combined to produce estimates and confidence intervals that incorporate missing-data uncertainty. This method has the advantage of using all available information on the characteristics of the nonemployed and of taking into account uncertainty about the reason for missing wage information.

In our study we use panel data sets that are as comparable as possible across countries, namely the Panel Study of Income Dynamics (PSID) for the US and the European Community Household Panel Survey (ECHPS) for Europe. We consider the period 1994-2001, the longest time span for which data are available for all countries. Our estimates deliver higher median wage gaps on imputed rather than actual wage distributions for most countries in the sample, and across alternative imputation methods. This implies, as one would have expected, that women tend on average to be more positively selected into work than men. However, the difference between actual and potential wage gaps is small in the US, the UK and most central and northern European countries, and becomes sizeable in Ireland, France and southern Europe, i.e. countries in which the gender employment gap is highest. In other words, correcting for selection into employment explains more than half of the observed negative correlation between gender wage and employment gaps. In particular, in Spain, Italy, Portugal and Greece the median wage gap on the imputed wage distribution reaches closely comparable levels to those of the US and of other central and northern European countries.

Our results thus show that, while the raw wage gap is much higher in Anglo Saxon countries than in Ireland and southern Europe, the reason is probably not to be found in more equal pay treatment for women in the latter group of countries, but mainly in a different process of selection into employment. Female participation rates in catholic countries and Greece are low and concentrated among high-wage women. Having corrected for lower participation rates, the wage gap there widens to similar levels to those of other European countries and the US.

We also estimate wage gaps adjusted for characteristics on both actual and imputed wage distributions. Adjusted wage gaps are somewhat affected by correction for selection in Ireland, France and southern Europe, although the increase in the estimated wage gap implied by imputation is much smaller than that observed on raw wage gaps. The interpretation is that selection indeed seems to take place along a small number of observable characteristics. Conditional on such characteristics, the employed and nonemployed population look much more similar in terms of potential wage offers.

The paper is organized as follows. Section 2 briefly discusses the related literature. Section 3 describes the data sets used and presents descriptive evidence on gender gaps. Section 4 describes our imputation and estimation methodologies. Section 5 estimates median gender wage gaps on actual and imputed wage distributions, to illustrate how alternative sample selection rules affect the estimated gaps. Section 6 discusses decompositions of international differences in wage gaps. Conclusions are brought together in Section 7.

2 Related work

The importance of selectivity biases in making wage comparisons has long been recognized since seminal work by Gronau (1974) and Heckman (1974). The current literature contains a number of country-level studies that estimate selection-corrected wage gaps across genders or ethnic groups, based on a variety of correction methodologies. Among studies that are more closely related to our paper, Neal (2004) estimates the gap in potential earnings between black and white women in the US by fitting median regressions on imputed wage distributions, using alternative methods of wage imputation for women non employed in 1990. He finds that "the black-white gap in log-potential wages among young adult women in 1990 was at least 60 percent larger than the gap implied by reported earnings and hours worked", thus revealing that black women are more strongly selected into work according to high-wage characteristics. Using both wage imputation and matching techniques, Chandra (2003) finds that the wage gap between black and white US males was also understated, due to selective withdrawal of black men from the labor force during the 1970s and 1980s.[2]

Turning to gender wage gaps, Blau and Kahn (2004) study changes in the US gender wage gap between 1979 and 1998 and find that sample selection implies that the 1980s gains in women's relative wage offers were overstated, and that selection may also explain part of the slowdown in convergence between male and female wages in the 1990s. Their approach is based on wage imputation for those not in work, along the lines of Neal (2004). Mulligan and Rubinstein (2004) also argue that the narrowing of the gender wage gap in the US during 1964-2002 may be a direct impact of progressive selection into employment of high-wage women, in turn attracted by widening within-gender wage dispersion. This idea follows the implications of the Roy's (1951) model, as applied to the choice between market and non-market work in the presence of rising dispersion in the returns to market work. Correction for selection into work is implemented here using a two-stage Heckman (1979) selection model. The authors show that while in the 1970s the gender selection bias was negative, i.e. nonemployed women had higher earnings potential than working women, it switched sign in the mid 1980s.[3]

Related work on European countries includes Blundell, Gosling, Ichimura and Meghir (2004), Albrecht, van Vuuren and Vroman (2003) and Beblo, Beninger, Heinze and Laisney (2003). Blundell

[2] See also Blau and Beller (1992) and Juhn (2003) for earlier use of matching techniques in the study of selection-corrected race gaps.

[3] Earlier studies that discuss the importance of changing characteristics of the female workforce in explaining the dynamics of the gender wage gap in the US include O'Neil (1985), Smith and Ward (1989) and Goldin (1990).

et al. examine changes in the distribution of wages in the UK during 1978-2000. They allow for the impact of non-random selection into work by using bounds to the latent wage distribution according to the procedure proposed by Manski (1994). Bounds are first constructed based on the worst case scenario and then progressively tightened using restrictions motivated by economic theory. Features of the resulting wage distribution are then analyzed, including overall wage inequality, returns to education, and gender wage gaps. Albrecht et al. estimate gender wage gaps in the Netherlands having corrected for selection of women into market work according to the Buchinsky's (1998) semi-parametric method for quantile regressions. They find evidence of strong positive selection into full-time employment. Finally, Beblo et al. show selection corrected wage gaps for Germany using both the Heckman (1979) and the Lewbel (2002) two-stage selection models. They find that correction for selection has an ambiguous impact on gender wage gaps in Germany, depending on the method used.

Interestingly, most of the studies cited find that correction for selection has important consequences for our assessment of gender wage gaps. At the same time, none of these studies use data from southern European countries, where employment rates of women are lowest, and thus the selection issue should be most relevant. In this paper we use data for the US and for a representative group of European countries to investigate how non-random selection into work may have affected the gender wage gap.

3 Data

3.1 The PSID

Our analysis for the US is based on the Michigan Panel Study of Income Dynamics (PSID). This is a longitudinal survey of a representative sample of US individuals and their households. It has been ongoing since 1968. The data were collected annually through 1997 and every other year after 1997. In order to ensure consistency with European data, we use five waves from the PSID, from 1994 to 2001. We restrict our analysis to individuals aged 16-64, having excluded the self-employed, full-time students, and individuals in the armed forces.[4]

The wage concept that we use throughout the analysis is the gross hourly wage. This is given by annual labor income divided by annual hours worked in the calendar year before the interview date. Employed workers are defined as those with positive hours worked in the previous year.

The characteristics that we exploit for wage imputation for the nonemployed are human capital variables, spouse income and nonemployment status, i.e. unemployed versus out of the labor force. Human capital is proxied by education and work experience controls. Ethnic origin is not included

[4] The exclusion of self-employed individuals may require some justification, in so far the incidence of self employment varies importantly across genders and countries, as well as the associated earnings gap. However, the available definition of income for the self employed is not comparable to the one we are using for the employees and the number of observations for the self employed is very limited for European countries. Both these factors prevent us from including the self-employed in our analysis.

here as information on ethnicity is not available for the European sample. We consider three broad educational categories: less than high school, high school completed, and college completed. They include individuals who have completed less than twelve years of schooling, between twelve and fifteen years of schooling, and at least sixteen years of schooling, respectively. This categorization of the years of schooling variable is chosen for consistency with the definition of education in the ECHPS, which does not provide information on completed years of schooling, but only on recognized qualifications.

Information on work experience refers to years of actual labor market experience (either full- or part-time) since the age of 18. When individuals first join the PSID panel as a head or a wife (or cohabitor), they are asked how many years they worked since age 18, and how many of these years involved full-time work. These two questions are also asked retrospectively in 1974 and 1985, irrespective of the year in which they had joined the sample. The answers to these questions form the base from which we calculate actual work experience, following the procedure of Blau and Kahn (2004). Given the initial values of work experience, we update work experience for the years of interest using the longitudinal work history file from the PSID. For example, in order to construct the years of actual experience in 1994 for an individual who was in the survey in 1985, we add to the number of years of experience reported in 1985 the number of years between 1985 and 1994 during which they worked a positive number of hours.[5] This procedure allows one to construct the full work experience in each year until 1997. As the survey became biannual after 1997, there is no information on the number of hours worked by individuals between 1997 and 1998 and between 1999 and 2000. We fill missing work experience information for 1998 following again Blau and Kahn (2004). In particular, we use the 1999 sample to estimate logit models for positive hours in the previous year and in the year preceding the 1997 survey, separately for males and females. The explanatory variables are race, schooling, experience, a marital status indicator and variables for the number of children aged 0-2, 3-5, 6-10, and 11-15, who are living in the household at the time of the interview. Work experience in the missing year is obtained as the average of the predicted values in the 1999 logit and the 1997 logit. We repeat the same steps for filling missing work experience information in 2000.

Spouse income is constructed as the sum of total labor and business income in unincorporated enterprises both for spouses and cohabitors of respondents. Finally, the reason for nonemployment, i.e. unemployment versus inactivity, is given by self-reported information on employment status.

When estimating adjusted wage gaps, we control for human capital and job characteristics. In particular, our wage equation includes controls for education, work experience, industry and occupation. We consider 12 occupational categories, based on the 3-digits occupation codes from the 1970 Census of the Population, and 12 industries. We also include 51 state dummies. The results obtained on this specification were not sensitive to the inclusion of controls for ethnic origin.

[5] The measure of actual experience used here includes both full-time and part-time work experience, as this is better comparable to the measure of experience available from the ECHPS.

3.2 The ECHPS

Data for European countries are drawn from the European Community Household Panel Survey. This is an unbalanced household-based panel survey, containing annual information on a few thousands households per country during the period 1994-2001.[6] The ECHPS has the advantage that it asks a consistent set of questions across the 15 members states of the pre-enlargement EU. The Employment section of the survey contains information on the jobs held by members of selected households, including wages and hours of work. The household section allows to obtain information on the family composition of respondents. We exclude Sweden and Luxembourg from our country set, as wage information is unavailable for Sweden in all waves, and unavailable for Luxembourg after 1996.

As for the US, we restrict our analysis of wages to individuals aged 16-64 as of the survey date, and exclude the self-employed, those in full-time education and the military. The definition of variables used replicates quite closely that used for the US.

Hourly wages are computed as gross weekly wages divided by weekly usual working hours. The EU education categories are: less than upper secondary high school, upper secondary school completed, and higher education. These correspond to ISCED 0-2, 3, and 5-7, respectively. Unfortunately, no information on actual experience is available in the ECHPS, and we use a measure of potential work experience, computed as the current age of an individual, minus the age at which she started her working life. Spouse income is computed as the sum of labor and non-labor annual income for spouses or cohabitors of respondents. Finally, unemployment status is determined using self-reported information on the main activity status.

When estimating adjusted wage gaps, our wage equation specification is as close as possible to that estimated for the US, subject to slight data differences. Besides differences in the definition for work experience, the occupational and industrial classification of individuals is slightly different from the one used for the PSID. In particular, we consider 18 industries and 9 broad occupational groups; although this is not the finest occupational disaggregation available in the ECHPS, it is the one that allows the best match with the occupational classification available in the PSID. We finally control for region of residence at the NUT1 level, meaning 11 regions for the UK, 1 for Finland and Denmark, 15 for Germany, 1 for the Netherlands, 3 for Belgium and Austria, 2 for Ireland, 8 for France, 12 for Italy, 7 for Spain, 2 for Portugal and 4 for Greece.

All descriptive statistics for both the US and the EU samples are reported in Table A1.

[6] The initial sample sizes are as follows. Austria: 3,380; Belgium: 3490; Denmark: 3,482; Finland: 4,139; France: 7,344; Germany: 11,175; Greece: 5,523; Ireland: 4,048; Italy: 7,115; Luxembourg: 1,011; Netherlands: 5,187; Portugal: 4,881; Spain: 7,206; Sweden: 5,891; U.K.: 10,905. These figures are the number of household included in the first wave for each country, which corresponds to 1995 for Austria, 1996 for Finland, 1997 for Sweden, and 1994 for all other countries.

3.3 Descriptive evidence on gender gaps

Table 1 reports raw gender gaps in log gross hourly wages and employment rates for all countries in our sample. At the risk of some oversimplification, one can classify countries in three broad categories according to their levels of gender wage gaps. In the US and the UK men's hourly wages are 25 to 30 log points higher than women's hourly wages. Next, in northern and central Europe the gender wage gap in hourly wages is between 10 and 20 log points, from a minimum of 11 log points in Denmark, to a maximum of 24 log points in the Netherlands. Finally, in southern European countries the gender wage gap is on average 10 log points, from 6.3 in Italy to 13.4 in Spain. Such gaps in hourly wages display a roughly negative correlation with gaps in employment to population rates. Employment gaps range from 10 percentage points in the US, the UK and Scandinavia,[7] to 15-25 points in northern and central Europe, up to 30-40 points in southern Europe and Ireland. The relationship between wage and employment gaps is represented in Figure 1. The coefficient of correlation between them is -0.497 and is significant at the 7% level.

Such negative correlation between wage and employment gaps may reveal significant sample selection effects in observed wage distributions. If the probability of an individual being at work is positively affected by the level of her potential wage offers, and this mechanism is stronger for women than for men, then high gender employment gaps become consistent with relatively low gender wage gaps simply because low wage women are relatively less likely than men to feature in observed wage distributions.

Table 1 also reports wage and employment gaps across three schooling levels. Employment gaps everywhere decline with educational levels, if anything more strongly in southern Europe than elsewhere. On the other hand, the relationship between gender wage gaps and education varies across countries. While the wage gap is either flat or rises slightly with education in most countries, it falls sharply with education in Ireland and southern Europe. In particular, if one looks at the low-education group, the wage gap in southern Europe is closely comparable to that of other countries - while being much lower for the high-education group. However, the fact that the low-education group has the lowest weight in employment makes the overall wage gap substantially lower in southern Europe.

Interestingly, in southern Europe countries, the overall wage gap tends to be smaller than each of the education-specific gaps, and thus lower than their weighted average. One can think of this difference in terms of an omitted variable bias. The overall gap is simply the coefficient on the male dummy in a wage equation that only controls for gender. The weighted average of the three education-specific gaps would be the coefficient on the male dummy in a wage equation that controls for both gender and education. Education would thus be an omitted variable in the first regression, and the induced bias has the sign of the correlation between education and the male dummy, given that the correlation between education and the error term is positive. While the overall correlation

[7]Similarly as in other Scandinavian countries, the employment gap in Sweden over the same sample period is 5.2 percentage points.

between education and the male dummy tends to be positive in all countries, such correlation becomes negative and fairly strong among the employed in southern Europe, lowering the overall wage gap below each of the education-specific wage gaps. The fact that, conditional on being employed, southern European women tend to be more educated than men may be itself interpreted as a signal of selection into employment based on high-wage characteristics.

In Table 1A we report similar gaps for the population aged 25-54, as international differences in schooling and/or retirement systems may have affected relevant gaps for the 16-64 sample. However, when comparing the figures of Table 1 and 2, we do not find evidence of important discrepancies between the gender gaps computed for those aged 16-64 and those aged 25-54. The rest of our analysis therefore uses the population sample aged 16-64.

4 Methodology

We are interested in measuring the gender wage gap:

$$D = E\left(w|X, \text{male}\right) - E\left(w|X, \text{female}\right), \tag{1}$$

where D denotes the gender gap in mean log wages, w denotes log wages and X is a vector of observable characteristics. Average wages for each gender are given by:

$$E\left(w|X,g\right) = E\left(w|X,g,I=1\right)\Pr(I=1|X,g) + E\left(w|X,g,I=0\right)\left[1-\Pr(I=1|X,g)\right], \tag{2}$$

where I is an indicator function that equals 1 if an individual is employed and zero otherwise and g =male, female. Wage gaps estimated on observed wage distributions are based on the $E\left(w|X,g,I=1\right)$ term alone. If there are systematic differences between $E\left(w|X,g,I=1\right)$ and $E\left(w|X,g,I=0\right)$, cross-country variation in $\Pr(I=1|X,g)$ may translate into misleading inferences concerning the international variation in potential wage offers. This problem typically affects estimates of female wage equations; even more so when one is interested in cross-country comparisons of gender wage gaps, given the cross-country variation in $\Pr(I=1|X,\text{male}) - \Pr(I=1|X,\text{female})$, measuring the gender employment gap. Our goal is to retrieve gender gaps in potential (offer) wages, as illustrated in (1), where $E\left(w|X,g\right)$ is given by (2). For this purpose, the data provide information on both $E\left(w|X,g,I=1\right)$ and $\Pr(I=1|X,g)$, but clearly not on $E\left(w|X,g,I=0\right)$, as wages are only observed for those who are in work.

A number of approaches can be used to correct for non-random sample selection in wage equations and/or recover the distribution in potential wages. The seminal approach suggested by Heckman (1974, 1979) consists in allowing for selection on unobservables, i.e. on variables that do not feature in the wage equation but that are observed in the data.[8] Heckman's two-stage parametric

[8] In this framework, wages of employed and nonemployed would be recovered as

$$\begin{aligned} E\left(w|X,g,I=1\right) &= X\beta + E\left(\varepsilon_1|\varepsilon_0 > -V\gamma\right) \\ E\left(w|X,g,I=0\right) &= X\beta + E\left(\varepsilon_1|\varepsilon_0 < -V\gamma\right), \end{aligned}$$

specifications have been used extensively in the literature in order to correct for selectivity bias in female wage equations. More recently, these have been criticized for lack of robustness and distributional assumptions (see Manski, 1989). Approaches that circumvent most of the criticism include semi-parametric selection correction models that appeared in the literature since the early 1980s (see Vella, 1998, for an extensive survey of both parametric and non-parametric sample selection models). Two-stage nonparametric methods allow in principle to approximate the bias term by a series expansion of propensity scores from the selection equation, with the qualification that the term of order zero in the polynomial is not separately identified from the constant term in the wage equation, unless some additional information is available (see Buchinski, 1998). Usually, the constant term in the wage regression is identified from a subset of workers for which the probability of work is close to one, but in our case this route is not feasible since for no type of women the probability of working is close to one in all countries.

Selection on observed characteristics is instead exploited in the matching approach, which consists in imputing wages for the non-employed by assigning them the observed wages of the employed with matching characteristics (see Blau and Beller, 1992, and Juhn, 1992, 2003).

The approach of this paper is also based on some form of wage imputation for the non-employed, but it simply requires assumptions on the position of the imputed wage observations with respect to the median of the wage distribution, and not on their level, as in Johnson et al. (2000) and Neal (2004).[9] We then estimate median wage gaps on the resulting imputed wage distributions, i.e. on the enlarged wage distribution that is obtained implementing alternative wage imputation methods for the nonemployed. The attractive feature of median regressions is that, if missing wage observations fall completely on one or the other side of the median regression line, the results are only affected by the position of wage observations with respect to the median, and not by specific values of imputed wages, as it would be in the matching approach. One can therefore make assumptions motivated by economic theory on whether an individual who is not in work should have a wage observation below or above median wages, conditional on characteristics. When estimating raw gender wage gaps, the only characteristic included is a gender dummy. Thus one should make assumptions on whether a nonemployed individual should earn above- or below-median wages for their gender.

More formally, let's consider the linear wage equation

$$w_i = X_i\beta + \varepsilon_i, \tag{3}$$

where w_i denotes (log) wage offers, X_i denotes characteristics, now also including gender, with associated coefficients β, and ε_i is an error term such that $Med\,(\varepsilon_i|X_i) = 0$. Let's denote by $\hat{\beta}$ the hypothetical LAD estimator based on true wage offers. However, wage offers w_i are only observed for the employed, and missing for non-employed. If missing wage offers fall completely below

respectively, where V is the set of covariates used in the selection equation, with associated parameters γ, and ε_1 and ε_0 are the error terms in the wage and the selection equation, respectively.

[9] See also Chandra (2003) for a non-parametric application to racial wage gaps among US men.

the median regression line, i.e. $w_i < X_i\widehat{\beta}$ for the non-employed ($I_i = 0$), one can then define a transformed dependent variable y_i that is equal to w_i for $I_i = 1$ and to some arbitrarily low imputed value $\widetilde{w}_i$ for $I_i = 0$, and the following result holds:

$$\hat{\beta}_{imputed} \equiv \arg\min_{\beta} \sum_{i=1}^{N} |y_i - X_i'\beta| = \hat{\beta} \equiv \arg\min_{\beta} \sum_{i=1}^{N} |w_i - X_i'\beta|. \tag{4}$$

Condition (4) states that the LAD estimator is not affected by imputation (see Johnson et al. 2000 for details). Clearly, (4) also holds when missing wage offers fall completely above the median regression line, i.e. $w_i < X_i\widehat{\beta}$, and y_i is set equal to some arbitrarily high imputed value $\widetilde{w}_i$ for the non-employed. More in general, the LAD estimator is also not affected by imputation when missing wage offers fall on both sides of the median, provided that observations on either side are imputed correctly, and that the median does not fall within either of the imputed sets. For example, suppose that the potential wages of the non-employed could be classified in two groups, A and B, such that $w_i > X_i\widehat{\beta}$ for $i \in A$ and $w_i < X_i\widehat{\beta}$ for $i \in B$, i.e. the predicted median does not belong to A or B. If y_i is set equal to some arbitrarily high value for all $i \in A$ and equal to some arbitrarily low value for all $i \in B$, LAD inference is still valid.

It should be noted, however, that in order to use median regressions to evaluate gender wage gaps in (1) one should assume that the mean and the median of the (log) wage distribution coincide, in other words that the (log) wage distribution is symmetric. This is clearly true for the log-normal distribution, which is typically assumed in Mincerian wage equations. In what follows we therefore assume that the distribution of offer wages is log-normal.[10]

Having said this, imputation can be performed in several ways, which we describe below.

Imputation on unobservables. We first exploit the panel nature of our data sets and, for all those not in work in some base year, we recover hourly wage observations from the nearest wave in the sample. The underlying identifying assumption is that an individual's position with respect to the base-year median, conditional on X, can be recovered looking at the level of her wage in the nearest wave. As the position with respect to the median is determined using levels of wages in other waves in the sample, we are allowing for selection on unobservables.

This procedure of imputation makes sense when an individual's position in the latent wage distribution stays on the same side of the median across adjacent waves in the panel. In other words, as we estimate median wage gaps, we do not need an assumption of stable rank throughout the whole wage distribution, but only with respect to the median. It may be interesting to interpret our identifying assumption in the context of the framework developed by Di Nardo, Fortin and

[10] If one does not impose symmetry of the (log) wage distribution, the equivalent of (2) would be

$$\begin{aligned} Med\,(w|X,g) &= F^{-1}(1/2) \\ &= F^{-1}\left\{F\left[Med\,(w|X,g,I=1)\right]\Pr(I=1|X,g) + F\left[Med\,(w|X,g,I=1)\right]\left[1-\Pr(I=1|X,g)\right]\right\}. \end{aligned}$$

Lemieux (1996) in order to estimate counterfactual densities of wages. In doing this, they assume that the structure of wages, conditional on a set of individual characteristics, does not depend on the distribution of characteristics themselves, i.e. it would be the same both in the actual and the counterfactual states of the world. If our objective were to recover the counterfactual density of wages that would be observed if all individuals were in work, we would need to assume that the distribution of wage offers, conditional on X, were the same whether one is employed or nonemployed. However, as we aim at recovering just the median of such counterfactual density of wages, conditional on X, we need a much weaker identifying assumption, namely that the cumulative density of wages up to the median be the same in the actual and counterfactual states of the world. In other words, we require individuals to remain on the same side of the median of the potential wage distribution for their X characteristics when switching employment status.

While imputation based on this procedure arguably exploits the minimum set of potentially arbitrary assumptions, it has the disadvantage of not providing any wage information on individuals who never worked during the sample period. It is therefore important to understand in which direction this problem may distort, if at all, the resulting median wage gaps. If women are on average less attached to the labor market than men, and if individuals who are less attached have on average lower wage characteristics than the fully attached, then the difference between the median gender wage gap on the imputed and the actual wage distribution tends to be higher the higher the proportion of imputed wage observations in total non-employment in the base year. Consider for example a country with very persistent employment status: those who do not work in the base year and are therefore less attached are less likely to work at all in the whole sample period. In this case low wage observations for the less attached are less likely to be recovered, and the estimated wage gap is likely to be lower. Proportions of imputed wage observations over the total non-employed population in 1999 (our base year) are reported in Table A2: the differential between male and female proportions tends to be higher in Germany, Austria, France and southern Europe than elsewhere. Under reasonable assumptions we should therefore expect the difference between the median wage gap on the imputed and the actual wage distribution to be biased downward relatively more in this set of countries. This in turn means that we are being relatively more conservative in assessing the effect of non-random employment selection in these countries than elsewhere.

Even so, it would of course be preferable to recover wage observations also for those never observed in work during the whole sample period. To do this, we rely on the observed characteristics of the nonemployed.

Imputation on observables. We perform imputation based on observable characteristics in two ways. First, we can recover wage observations for the non-employed by making assumptions about whether they place above or below the median wage offer, conditional on X, based on a small number of characteristics. Let's summarize these characteristics in a vector Z: in our specifications, Z will include, in turn, employment status (unemployed versus out of the labor force), education and work experience, and spouse income. Of course Z cannot include any of the variables in the X-vector (trivially, one cannot use human capital variables to impute missing wage observations in the estimation of human-capital corrected wage gaps). While this condition is easy to meet when estimating raw wage gaps, i.e. when the X-vector only contains a gender dummy, it becomes hard to satisfy when estimating gender gaps adjusted for characteristics. We will come back to this in Section 6.

This imputation method for placing individuals with respect to the median follows a sort of educated guess, based on their observable characteristics. However, we again use wage information from other waves in the panel to assess the goodness of such guess.

We also use probability models for imputation of missing wage observations, based on Rubin's (1987) two-step methodology for repeated imputation inference.[11] In the first step a statistical model is chosen for wage imputation, which should be closely related to the nature of the missing-data problem. In the second step one obtains (a small number of) repeated and independent imputed samples. The final estimate for the statistic of interest is obtained by averaging the estimates across all rounds of imputation. The associated variances take into account variation both within and between imputations (see the Appendix for details).

In the first step we use multivariate analysis in order to estimate the probability of an individual's belonging above or below the median of the wage distribution, conditional on X. Assume for simplicity that X only contains a gender dummy. On the sub-sample of employed workers we build an indicator function M_i that is equal to one for individuals whose wage is higher than the median of the observed wage distribution for their gender and zero otherwise. We then estimate for each gender a probit model for M_i, with explanatory variables Z_i that are available for both the employed and the non-employed sub-samples, typically human capital controls. Using the probit estimates we obtain predicted probabilities of having a latent wage above the median given gender, $\hat{P}_i = \Phi(\widehat{\gamma} Z_i) = \Pr(M_i = 1|X_i)$, for the nonemployed subset, where Φ is the c.d.f. of the standardized normal distribution and $\widehat{\gamma}$ is the estimated vector of parameters from the probit model. This imputation procedure is grounded in economic theory, as we would expect that individuals with a relatively high level of educational attainment or work experience would be more likely to feature in the upper half of the wage distribution. The predicted probabilities $\hat{P}_i$ are then used in the second step as sampling weights for the nonemployed. That is, in each of the independent imputed samples, employed individuals feature with their observed wage, and nonemployed individuals feature with a wage above median with probability $\hat{P}_i$ and a wage below median with probability $1 - \hat{P}_i$.

The repeated imputation procedure effectively uses all the information available for individuals who are not observed in work at the time of survey. We compare this methodology to what may

[11] See Rubin (1987) for an extended analysis of this technique and Rubin (1996) for a survey of more recent developments. The repeated imputation technique was developed by Rubin as a general solution to the statistical problem of missing data in large surveys, being mostly due to non-reponses. Imputations can be created under Bayesian rules, and repeated imputation methods can be interpreted as an approximate Bayesian inference for the statistics of interest, based on observed data. In this paper, we abstract from Bayesian considerations and apply the methodology in our non-Bayesian framework.

be defined as simple imputation. That is, having estimated predicted probabilities $\hat{P}_i$ of belonging above the median for those not in work, we assign them wages above the median if $\hat{P}_i > 0.5$ and below otherwise. This simple imputation procedure tends to overestimate the median gender wage gap on the imputed sample if there is a relatively large mass of non-employed women with $\hat{P}_i < 0.5$ but very close to 0.5.

As discussed in Rubin (1987), one of the advantages of repeated imputation is that it reflects uncertainty about the reason for missing information. While simple imputation techniques such as regression or matching methods assign a value to the missing wage observation in a deterministic way (given characteristics), repeated imputation is based on a probabilistic model, i.e. on repeated random draws under our chosen model for non-employment. Hence, unlike simple imputation, inference based on repeated imputation takes into account the additional variability underlying the presence of missing values.

Similarly as when making imputation based on wage information from adjacent waves, we need to assume some form of separability between the structure of wages and individual employment status. In particular we need to assume that, conditional on our vector of attributes, individuals stay on the same side of the median whether they are employed or nonemployed.

In both simple and repeated imputation, we initially estimate a probit model for the probability of belonging above or below the median of the *observed* wage distribution. However, due precisely to the selection problem, such median may be quite different from that of the potential wage distribution, i.e. the median that would be observed if everyone were employed. This could introduce important biases in our estimates on the imputed sample. In order to attenuate this problem we also perform repeated and simple imputation on an expanded sample, augmented with wage observations from adjacent waves. This allows us to get a better estimate of the "true" median in the first step of our procedure, thus generating more appropriate estimates of the median wage gap on the final, imputed sample. Note that in this case we are combining imputation on both observables and unobservables.

It is worthwhile to discuss here the main differences between alternative imputation methods, also in light of the interpretation of the results presented in the next section. Our imputation methods differ in terms of the underlying identifying assumptions and of resulting imputed samples. The first method, where missing wages are imputed using wage information form adjacent waves, implicitly assumes that an individual's position with respect to the median is proxied by their wage in the nearest wave in the panel. In other words, if the position of individuals in the wage distribution changes over time, any movements that happen within either side of the median do not invalidate this method. With this procedure one can recover at best individuals who worked at least once during the eight-year sample period. We thus want to emphasize that this is a fairly conservative imputation procedure, in which we impute wages for individuals who are relatively weakly attached to the labor market, but not for those who are completely unattached and thus never observed in work. While this may affect our estimates (and we will discuss how in the next section), this procedure has the advantage of restricting imputation to a relatively "realistic" set of potential workers.

In the second and third imputation methods, we assume instead that an individual's position with respect to the median can be proxied by a small number of observable characteristics. In the second method, we take educated guesses regarding the position in the wage distribution of someone with given characteristics. This procedure is more accurate the more conservative the criteria used for imputation. For example, assigning individuals with college education above the median and individuals with no qualifications below the median is more conservative but probably more accurate than assigning all those with higher than average years of schooling above the median and all the rest below the median. With this method, our imputed sample is typically larger than the one obtained with the first method, although still substantially smaller than the existing population. Finally, with the third method, we estimate the probability of belonging above the median for the whole range of our vector of characteristics, thus recovering predicted probabilities and imputed wages for the whole existing population - except of course those with missing information on characteristics.

Different imputed samples will have an impact on our estimated median wage gaps. In so far women are more likely to be non-employed than men, and non-employed individuals are more likely to receive lower wage offers than employed ones, the larger the imputed sample with respect to the actual sample of employed workers, the larger the estimated correction for selection.

Having said this, it is important to stress that with all three imputation methods used there is nothing that would tell a priori which way correction for selection is going to affect the results. This is ultimately determined by the wages that the nonemployed earned when they were previously (or later) employed, and by their observable characteristics, depending on methods.

With these clarifications in mind, we move next to the description of our results.

5 Results on raw wage gaps

5.1 Imputation based on unobservables

Our first set of results refers to imputation based on unobservable characteristics. In other words. an individual's position with respect to the median of the wage distribution is proxied by the position of their wage obtained from the nearest available wave.

The results are reported in Table 2. Column 1 reports raw (unadjusted) wage gaps for individuals with hourly wage observations in 1999, which is our base year. These replicate very closely the wage gaps reported in Table 1, with the only difference that mean wage gaps for the whole sample period are reported in Table 1, while median wage gaps for 1999 are reported here. As in Table 1, the US and the UK stand out as the countries with the highest wage gaps, followed by central and northern Europe, and finally Scandinavia and Southern Europe.

In column 2 missing wage observations in 1999 are replaced with the real value of the nearest wage observation in a 2-year window, while in column 3 they are replaced with the real value of the nearest wage observation in the whole sample period, meaning a maximum window of [-5, +2] years. Comparing figures in columns 1-3, one can see that the median wage gap remains substantially unaffected or affected very little in the US, the UK, and a number of European countries down to Austria, and increases substantially in Ireland, France and southern Europe, this latter group including countries with the highest gender employment gap. While sample selection seems to be fairly neutral in a large number of countries in our sample, or, in other words, selection in market work does not seem to vary systematically with wage characteristics of individuals, it is mostly high-wage individuals who work in catholic countries, and this seems to give a downward-biased estimate of the gender wage gap when one does not account for non-random sample selection. Note finally that in Scandinavian countries and the Netherlands the wage gap in potential wages decreases slightly, if anything providing evidence of an underlying selection mechanism of the opposite sign.

Arulampalan, Booth and Bryan (2004) find evidence of glass ceilings, defined as a difference of at least 2 points between the 90th percentile (adjusted) wage gap and the 75th or the 50th percentile gap, in most European countries, and evidence of sticky floors, defined as a difference of at least 2 points between the 10th percentile (adjusted) wage gap and the 25th or 50th percentile gap, only in Germany, France, Italy and Spain (but report no evidence for Portugal or Greece). Sticky floors for low-educated women in Spain are also documented by De La Rica, Dolado and Llorens (2005). Similarly, our descriptive evidence of section 3.3 shows a strongly decreasing wage gap in levels of education in southern Europe. High wage gaps at the bottom of the wage distribution in some southern European countries may discourage employment participation of low-wage women relatively more than in other countries. This would be consistent with a sizeable impact of employment selection at the bottom of the wage distribution in these countries. Our selection-corrected estimates for the gender wage gap precisely go in this direction.

For each sample inclusion rule in column 1-3 one can compute the adjusted employment rate for each gender, i.e. the proportion of the adult population that is either working or has an imputed wage. These proportions are reported in columns 1-3 of Table 5. When moving from column 1 to 3, the fraction of women included increases substantially in southern Europe, and only slightly less in countries like Germany or the UK, where the estimated wage sample is not greatly affected by the sample inclusion rules. Moreover, the fraction of men included in the sample also increases across imputation rules. It is thus not simply the lower female employment rate in several countries that determines our findings, it is also the fact that in some countries selection into work seems to be less correlated to wage characteristics than in others.

As one would expect from our cross-country results, controlling for selection removes most of the observed negative correlation between wage and employment gaps. At the bottom of each column in Table 2 we compute the coefficient of correlation between the wage gap in the same column and the adjusted employment gap, as obtained from the relevant column of Table 5. The correlation coefficient between unadjusted median wage gaps and employment gaps is -0.455, and is significantly different from zero at the 10% level. Using the adjusted estimates from column 3, this falls to -0.181, and is not significantly different from zero at standard levels. The importance of sample selection can also be grasped graphically by looking at Figure 2, which shows the relationship between median wage and employment gaps, either unadjusted (estimates from column 1) or selection-adjusted (estimates from column 3). While a downward-sloping pattern can be detected in Panel A, Panel B rather shows a random scatter-plot.

The estimates of columns 2 and 3 do not control for aggregate wage growth over time. If aggregate wage growth was homogeneous across genders and countries, then estimated wage gaps based on wage observations for other waves in the panel would not be not affected. But if there is a gender differential in wage growth, and if such differential varies across countries, then simply using earlier (later) wage observations would deliver a higher (lower) median wage gap in countries where women have relatively lower wage growth with respect to men.[12] We thus estimate real wage growth by regressing log real hourly wages for each country and gender on a linear trend.[13] The resulting coefficients are reported in Table A3. These are then used to adjust real wage observations outside the base year and re-estimate median wage gaps. The resulting median wage gaps on the imputed wage distribution are reported in column 4 and 5. Despite some differences in real wage growth rates across genders and countries, adjusting estimated median wage gaps does not produce any appreciable change in the results reported in columns 2 and 3, which do not control for real wage growth.

Note that in Table 2 we are (at best) recovering on average 24% of the non-employed females in the four southern European countries, as opposed to approximately 46% in the rest of countries (see Table A2). For men, the respective proportions are 54% and 60%. Such differences happen because (non)employment status tends to be more persistent in southern Europe than elsewhere, and much more so for women than for men. As briefly noted in Section 3, given that we recover relatively fewer less-attached women in southern Europe, we are being relatively more conservative in assessing the effect of non-random employment selection in southern Europe than elsewhere.

For this reason it is important to try to recover wage observations also for those never observed in work in any wave of the sample period, as explained in the next section.

5.2 Imputation based on observables

In Table 3 we estimate median wage gaps on imputed wage distributions, making assumptions on whether individuals who were nonemployed in 1999 had potential wage offers above or below the median for their gender. Column 1 reports for reference the median wage gap on the base sample,

[12] Note however that, even if real wage growth were homogeneous across genders, imputation based on wage observations from adjacent waves would not be affected only if the proportion of men and women in the sample remained unchanged after imputation.

[13] Of course, for our estimated rates of wage growth to be unbiased, this procedure requires that participation into employment be unaffected by wage growth, which may not be the case.

which is the same as the one reported in column 1 of Table 2. In column 2 we assume that all those not in work would have wage offers below the median for their gender.[14] This is an extreme assumption, and should only be taken as a benchmark. This assumption is clearly violated in cases like Italy, Spain and Greece, in which more than a half of the female sample is not in work in 1999, as by definition all missing observations cannot fall below the median. For this reason we do not report estimated gaps for these three countries. However, also for other countries there are reasons to believe that not all nonemployed individuals would have wage offers below their gender mean. Of course, we cannot know exactly what wages these individuals would have received if they had worked in 1999. But we can form an idea of the goodness of this assumption looking again at wage observations (if any) for these individuals in all other waves of the panel. This allows us to compute what proportion of imputed observations had at some point in time wages that were indeed below their gender median. Such proportions are also presented separately for each gender in column 2. They are fairly high for men, but sensibly lower for women, which makes the estimates based on this extreme imputation case a benchmark rather than a plausible measure for the gender wage gap. Having said this, estimated median wage gaps increase substantially for most countries, except Denmark and Finland.

We next make imputations based on observed characteristics of nonemployed individuals. In column 3 we impute wage below the median to all those who are unemployed (as opposed to non participants) in 1999. With respect to the base sample, the implied median wage gap stays roughly unchanged everywhere down to Austria, and increases substantially in Ireland, France and southern Europe. Also, the proportion of "correctly" imputed observations, computed as for the previous imputation case, is now much higher. Those who do not work because they are unemployed are thus relatively more likely to be over-represented in the lower half of the wage distribution.

In column 4 we assume that all those with less than upper secondary education and less than 10 years of potential labor market experience have wage observations below the median for their gender. Those with at least higher education and at least 10 years of labor market experience are instead placed above the median. In the four southern European countries the gender wage gap increases substantially: with respect to the imputation rule of column 3, it doubles in Italy and Greece and it increases by 10 log points in Spain and Portugal. This finding underscores the importance of selection with respect to human capital in southern Europe. For this set of countries, except Greece, the proportions of correctly imputed observations for men and women also generally increases relative to column 3. Interestingly, this is not the case for the US, the UK, Finland, Denmark and Germany, where the proportions of correctly imputed observations under this imputation rule is lower than in the previous case.

The next imputation method is implicitly based on the assumption of assortative mating and consists in assigning wages below the median to those whose partner has total income in the bottom

[14] In the practice, whenever we assign someone a wage below the median we pick $\widetilde{w}_i = -5$, this value being lower than the minimum observed (log) wage for all countries, and thus lower than the median. Similarly, whenever we assign someone a wage above the median we pick $\widetilde{w}_i = 20$.

quartile of the gender-specific distribution of total income. The results are broadly similar to those in column 3: the wage gap is mostly affected in Ireland and southern Europe. It would be natural to perform a similar exercise at the top of the distribution, by assigning a wage above the median to those whose partner has total income in the top quartile. However, in this case the proportion of correctly imputed observations was too low to rely on the assumption used for imputation.

We also make imputation based on observable characteristics to recover wage observations only for those who never worked, using first use wage observations available from other waves, and then imputing the remaining missing ones using education and experience information as done in column 4. The results show again a much higher gender gap in Ireland, France, and southern Europe, and not much of a change elsewhere with respect to the base sample of column 1.

Similarly as with the previous imputation method, we report in columns 4-8 of Table 5 the proportion of men and women included in our imputed samples. As expected, we are now able to recover wage information for a higher fraction of the adult population.[15] The correlations between median wage gaps on the imputed wage distribution and the corresponding adjusted employment gaps, reported in the bottom row of Table 3, are once again not significantly different from zero at standard significance levels. The notable exception is column 4, where the correlation between the two series becomes positive, large, and statistically significant. This is due to the fact that, under this imputation rule, the estimated gender wage gap in southern Europe increases disproportionately relative to other countries, while the employment gap on the imputed sample is much less affected.[16]

We finally use a probabilistic model for assigning to individuals wages above or below their gender median, using both simple and repeated imputation techniques. As mentioned above, this involves a two-step procedure, using once more data for 1999 as our base year. In the first step we estimate a probit model for the probability that an individual with a non-missing wage falls above their gender median, given a set of characteristics. We consider two alternative specifications for the probit regressions: a simple human capital specification that controls for education (two dummies for upper secondary and higher education), experience and its square, and a more general specification that also controls for marital status, the number of children of different ages (between 0 and 2, 3 and 5, 6 and 10, and 11 and 15 years old), and the position of the spouse in their gender specific distribution of total income (three dummies corresponding to the three highest quartiles). Since the results of the exercise do not vary in any meaningful way across specifications, we only report findings for the human capital specification. The estimated coefficients for the first stage probit regression conform to standard economic theory. Individuals with higher levels of educational attainment and/or of labor market experience are more likely to feature in the top half of the wage

[15]In column 4 such proportions are generally not equal to 100% because we did not impute wages to those who are employed but have missing information on hourly wages.

[16]We have also computed the correlations between median wage gaps on the imputed wage distribution and the employment gaps on the base sample. For all imputation rules, the resulting correlation were positive and statistically significant. In our tables we are thus reporting the more conservative values.

distribution.[17]

In the second step we use the estimated coefficients from the probit regression to compute the predicted probability that a missing wage observation falls above the gender median. We consider two alternative mechanisms to impute wages. According to the first mechanism, which we define as simple imputation, we impute a value of the wage above (below) the median if the predicted probability is greater (smaller) than 0.5. This implies that a missing-wage observation is assigned a value below median even if it would only marginally feature in the bottom part of the wage distribution.

In order to circumvent this problem, our second imputation mechanism is based on the repeated imputation methodology discussed in Section 4. For its implementation we construct 20 independent imputed samples. In each imputed sample, the employed feature with their observed wage, and for each nonemployed individual we "draw" her position with respect to the median using the predicted probability obtained from the probit model. In the practice we draw independent random numbers from a uniform distribution with support [0,1] and assign a nonemployed worker a wage above (below) the median if the random draw is lower (higher) than their predicted probability. For each of the 20 samples we estimate the median gender wage gap and obtain the corresponding bootstrapped standard error.[18] For each country and specification, the estimated median wage gap is then obtained by averaging the estimates across the 20 rounds of imputation. The standard errors are adjusted to take into account both between and within-imputation variation (see the Appendix for details).

The results for this exercise are summarized in Table 5. Column 1 reports the median wage gap for the base sample, which is the same as the one reported in column 1 of Table 3. Column 2 reports the estimated median wage gap using simple imputation. In Column 3 we use simple imputation to recover wage observations only for those who never worked in the sample period. That is, we first use wage observations available from other waves to impute missing wages and then impute the remaining missing ones as done in Column 2. Note that this procedure changes the reference median wage: by expanding the wage sample we are in practice able to compute a median wage that is closer to the latent median, i.e. the median that one would observe if everybody were in work. Columns 4 and 5 report results based on repeated imputation, having computed the reference median as in columns 3 and 4, respectively.

For all countries, and in particular for Ireland, France and Southern Europe, wage imputation generates larger estimates of the median gender wage gap than in the base sample of column 1. The estimates are of the same order of magnitude than the ones obtained when we assign a wage below median to all missing wage observations or to all the unemployed individuals with missing wages (see columns 2 and 3 in Table 3). When we use simple imputation for the base sample (column 2) we cannot report estimated gaps for Italy, Spain and Greece, as in these countries more than half

[17] The results are available upon request from the authors.

[18] We use the STATA command `bsqreg` where we set the number of replications to 200.

of the female sample would be assigned a wage below median, similarly to what we had in column 2 of Table 3.

We first compare the median wage gap obtained with simple imputation on the base sample (column 2) with that obtained with simple imputation on the sample expanded with wage observations from other waves (column 3). In the latter case it is now possible to obtain estimated gaps for Italy, Spain and Greece. This is due to the difference between the reference median wage in the two columns, and highlights the importance of estimating the median wage on a distribution that is as close as possible to the latent one. For all countries except the UK and Austria the estimated median wage gap in column 3 is lower than in column 2. This decline is largest for Belgium, France, and Southern Europe. The use of the expanded sample seems to allow us to get a better estimate of the "true" median in the first step of our procedure, thus generating more appropriate estimates of the median wage gap on the final, imputed sample. The same discussion applies to the results obtained using repeated imputation (comparing entries in column 4 and column 5).

Second, we compare the results obtained with simple and repeated imputation. Repeated imputation generates a lower estimate of the median gender gap than simple imputation for almost all countries. However, this tendency is stronger for Ireland, France and Southern Europe (see columns 2 and 4). Simple imputation tends to overestimate the gender wage gap when there is a relatively heavy mass of women with a predicted probability of featuring below the median that is slightly lower than 0.5, and this turns out to be the case for countries with high gender employment gaps. Moreover, with repeated imputation we can obtain estimates of the wage gap for Italy and Spain, since we now assign less than 50% of the female sample below the median. This is still not the case for Greece.

Repeated imputation on the expanded sample should provide the most accurate estimate of the median wage gap across countries. Comparing column 1 and column 5 we find that the median wage gap on the imputed wage distribution increases slightly for the US and the UK, decreases slightly in Scandinavia and the Netherlands, and stays roughly unchanged in most other central European countries. However, estimated gender wage gaps on imputed distributions more than double in Ireland, France and southern Europe. Specifically, the median wage gap in Spain, Portugal and Italy is well above 20 log points, and reaches levels that are comparable to those observed for the US and the UK both in the base and in the imputed samples. For Greece, we obtain an even larger estimated gender wage gap (0.5). All the figures in this column are very close to those reported in column 4 of Table 3. This is not surprising, as the vector of Z variables used for making imputation is roughly the same, and the only difference between the two sets of results consists in the type of imputation method (educated guesses in Table 3 versus a statistical model for imputation in Table 4).

Cross-country correlations between wage and employment gaps are reported in the bottom row of Table 4. The underlying employment rates are now very close to 1 (see column 9 in Table 5), as the only observations that remain out of the sample are the employed with genuinely missing wages or those with missing information on human capital characteristics. While the correlation on the base sample is negative and significant, it becomes not significantly different from zero in all samples obtained under simple and repeated imputation.

To broadly summarize our evidence, one could note that whether one corrects for selection on unobservables (Table 2) or on observables (Table 3 and 4), our results are qualitatively consistent in identifying a clear role of sample selection in Ireland, France and southern Europe.[19] Quantitatively, the correction for sample selection is smallest when wage imputation is performed using wage observation from other waves in the panel, and increases when it is instead performed using observed characteristics of the nonemployed. As argued above, this is mainly due to different sizes of the imputed samples. While only individuals with some degree of labor market attachment feature in the imputed wage distribution in the first case, the use of observed characteristics may in principle allow wage imputation for the whole population, thus including individuals with no labor market attachment at all.

The fact that controlling for unobservables does not greatly change the picture obtained when controlling for a small number of observables alone (education, experience and spouse income) implies that most of the selection role can indeed be captured by a set of observable individual characteristics, and possibly some unobservables closely correlated to them.

6 Results on adjusted wage gaps

Our discussion so far referred to unadjusted wage gaps. In other words, our X vector only contained a gender dummy in all estimated specifications. The results obtained were specifically targeted at explaining the main stylized fact highlighted at the beginning of this paper, namely the cross-country correlation between raw wage and employment gaps.

In this section we move on to the estimation of gender wage gaps adjusted for observable characteristics. Similarly, as above, we compare the unexplained gender gap in earnings across different imputation rules. Comparisons of adjusted rather than raw wage gaps across sample inclusion or imputation rules is a further test of whether selection mostly happens along observed or unobserved worker characteristics.

While similar imputation methods could in principle be used in estimating raw and adjusted wage gaps, in practice one needs stronger assumptions in order to establish whether a missing

[19] We have performed a number of robustness tests and more disaggregate analyses on the results reported in Tables 2 to 4. First, we have restricted the estimates to individuals aged 25-54 in 1999. The results were very similar to those obtained on the larger sample. Second, for the imputation rules reported in Table 2 and 3, we have repeated our estimates separately for three education groups (less than upper secondary education, upper secondary education, and higher education), and we found that most of the selection occurs across rather than within groups, as median wage gaps disaggregated by education are much less affected by sample inclusion rules than in the aggregate. Finally, we have repeated our estimates separately for three demographic groups: single individuals without kids in the household, married or cohabiting without kids, and married or cohabiting with kids. We found evidence of a strong selection effect in Ireland, France and southern Europe among those who are married or cohabiting, especially when they have kids, and much less evidence of selection among single individuals without kids.

wage observation should be placed above or below the median, as all our imputation rules are conditional on the vector of covariates included in the wage equation. For example, imagine that the X vector contains not only a gender dummy but also human capital variables. When missing wage observations are imputed using wage information from other waves in the panel, one needs to assume that an individual's position in the latent wage distribution stays on the same side of the median across adjacent waves in the panel, *within* cells defined by gender and human capital levels. When observable characteristics are used for imputation, one should be assuming that someone with characteristics Z should earn a wage, say, below the median, again conditional on their gender and human capital levels. Hence, all variables in X should be excluded from the Z-vector, which of course limits the choice of observable characteristics that can be used for imputation. These caveats and limitations should be borne in mind when interpreting our estimates of adjusted wage gaps.

We estimate two main specifications across imputation rules: the first one controls human capital variables and state or region of residence, and the second one also includes job controls. For the first case, the median wage gap is estimated on the base sample and three alternative imputed samples, namely (i) a sample enlarged with (the real value of) wage observations from all other waves in the panel; (ii) a sample enlarged with wage imputation based on unemployment versus inactivity status and (iii) a sample enlarged with wage imputation based on spouse income.[20] For the second case, one would need job controls for the nonemployed, which restricts our choice of imputation methods to the inclusion of wage observations (and thus job controls) from all other waves in the panel.

The results are reported in Table 6, and the proportions of the adult population included in each sample are reported in Table 7. Column 1 of Table 6 reports the median wage gap on the base 1999 sample, having controlled for education (1 dummy for secondary education completed and one for college education), experience and its square, and state or region of residence.[21] Only in eight countries out of fourteen is the wage gap adjusted for characteristics lower than the raw wage gap of column 1, Table 2, and even in those cases the difference between the two is not very large, except perhaps in the US and the Netherlands. Beyond these two cases, the raw wage gaps found in Table 2 seem thus largely unexplained by observable characteristics. In particular, in a number of countries, and especially in southern Europe, the adjusted wage gap is even larger than the raw wage gap, meaning that employed women have higher wage characteristics than men, again consistently with some degree of selection with respect to a few observables. In column 2 the working sample is expanded using available wage observations from other waves in the panel. Similarly as in Table 2, estimated wage gaps are not greatly affected in the US and in all European

[20] We do not report estimates for those employed at least once in a window of [-2,+2] years, as they do not provide additional information from those based on individuals employed at least once in the sample period, nor we report estimates corrected for real wage growth, as they do not differ much from those of sample (i).

[21] The simplest human capital specification excluding state or region of residence gave very similar results to those reported here.

countries down to Austria. They are indeed affected in Ireland, France and southern Europe, although the increase in the estimated wage gap implied by imputation is much smaller than that observed on raw wage gaps of Table 2. In particular, using the estimates of columns 1 and 2 of Table 6, correction for selection raises the median wage gap in Ireland, France and southern EU by an average 14%. The same calculation on the corresponding columns 1 and 3 of Table 2 gives an average increase of 60%. The interpretation is that selection indeed seems to take place along a small number of observable characteristics. Conditional on such characteristics, the employed and nonemployed population look more similar in terms of potential wage offers. Similar considerations are valid looking at columns 3 and 4 of Table 6, where wage imputation is based on unemployment status or spouse income in the bottom quartile, although these results seem to be less reliable, as the proportion of correctly imputed observations is fairly low in some cases.

The estimates presented in the last two columns of the Table also control for job characteristics like occupation and industry. The increase in the median wage gap implied by wage imputation is now tiny, except for the US.[22] In most cases virtually all the difference between the wages of the employed and potential wage offers of the nonemployed can be explained by differences in human capital and in the potential jobs that the nonemployed would occupy when employed.

In order to estimate the adjusted wage gaps of Table 6 we assumed that the returns to human capital and other characteristics are the same across genders. We have also relaxed this assumption by estimating separate wage equations for men and women on the base sample and on that enlarged with wage observations from other waves in the panel, and then applying the well-known Oaxaca (1973) decomposition of the resulting wage gaps into gender differences in characteristics and gender differences in the returns to characteristics.[23] The component represented by differences in returns should correspond to the unexplained gender wage gaps of Table 6. The results obtained on the Oaxaca decomposition are in line with those of Table 6 (and thus not reported here). On the base sample, the contribution of characteristics was actually negative in southern Europe when one included human capital, region and job controls in the estimated wage equations, meaning that differences between male and female coefficients explain more than 100% of the observed wage gap. This is again a consequence of very low female employment rates in these countries, in the presence of selective participation into employment. On the enlarged sample, the characteristics' component become bigger in Ireland, France and southern Europe, confirming once more the importance of selection along observable characteristics.[24]

[22]It would be large in Belgium, but either estimates are not significantly different from zero, probably due to small sample size for this country.

[23]The decomposition is: $\overline{w}^M - \overline{w}^F = \left(\overline{X}^M - \overline{X}^F\right)\widehat{\beta}^M + \overline{X}^F\left(\widehat{\beta}^M - \widehat{\beta}^F\right)$, where upper bars denote sample averages, hats denote OLS estimates and superscripts denote gender.

[24]Another method that has been used recently for understanding the international variation in the gender pay gap is the one proposed by Juhn, Murphy and Pierce (1991) to study the slowdown in the convergence of black and white wages in the US, and first adapted to the study of cross-country differences in the gender wage gap by Blau and Kahn (1996). This method consists in decomposing international differences in wage gaps into differences in characteristics, and differences in (male) returns to these characteristics. While the former representes the contribution of employment selection to the variation in the gender wage gap, the latter represents the contribution of the wage structure. We

7 Conclusions

Gender wage gaps in the US and the UK are much higher than in other European countries, and especially so with respect to Ireland, France and southern Europe. Although at first glance this fact may suggest evidence of a more equal pay treatment across genders in the latter group of countries, appearances can be deceptive.

In this paper we note that gender wage gaps across countries are negatively correlated with gender employment gaps, and illustrate the importance of non random selection into work in understanding the observed international variation in gender wage gaps. To do this, we perform wage imputation for those not in work, by simply making assumptions on the position of the imputed wage observations with respect to the median. We then estimate median wage gaps on imputed wage distributions, and assess the impact of selection into work by comparing estimated wage gaps on the base sample with those obtained on a sample enlarged with wage imputation. Imputation is performed according to different methodologies based on observable or unobservable characteristics of missing wage observations. With all imputation methods there is nothing that would tell a priori which way correction for selection is going to affect the results, as this is ultimately determined by the wages that the nonemployed earned when they were previously (or later) employed, or by their observable characteristics.

We find higher median wage gaps on imputed rather than actual wage distributions for most countries in the sample, meaning that, as one would have expected, women tend on average to be more positively selected into work than men. The only notable exceptions are Scandinavian countries and the Netherlands where the wage gap in potential wages decreases slightly, if anything providing evidence of an underlying selection mechanism of the opposite sign. In all other countries the selection-corrected gender wage gap is higher than the uncorrected one. However, this difference is small in the US, the UK and most central and northern European countries, and it is sizeable in Ireland, France and southern Europe, i.e. countries in which the gender employment gap is highest. Our (most conservative) estimates suggest that correction for employment selection explains about 60% of the observed negative correlation between wage and employment gaps. In particular, in Italy, Spain, Portugal and Greece the median wage gap on the imputed wage distribution ranges between 20 and 30 log points across specifications. These are closely comparable levels to those of the US and of other central and northern European countries.

Our analysis identifies a clear direction for future work. As we argue in this paper, gender employment gaps are important in understanding cross-country differences in gender wage gaps. Hence, one should ultimately assess the importance of demand and supply factor in explaining variation in these gaps. As emphasized in recent work by Fernández and Fogli (2005) and by Fortin (2005a and 2005b) 'soft variables' such as cultural beliefs about gender roles and family

also estimated this decomposition on our data, and found that the contribution of characteristics relative to that of the wage structure was much stronger in southern Europe than elsewhere. This effect was attenuated on the imputed wage distribution, in line with evidence on the importance of employment selection presented in this paper.

values and individual attitudes towards greed, ambition and altruism are important determinants of women's employment decisions as well as of gender wage differentials. We believe that cross-countries differences in these 'fuzzy' variables, as well as differences in labor market and financial institutions, might contribute to explain the cross-country patterns of women's selection into the labor force discussed in this paper and hence the international variation in gender pay gaps.

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Appendix. Rubin's (1987) repeated imputation methodology

We are interested in estimating the median $\hat{\beta}$ of the distribution of (log) wages w. However, part of the wages are observed w_{obs} and part of the wages are missing w_{mis}. If wages where available for everyone in the sample we would have $\hat{\beta} = \hat{\beta}(w_{obs}, w_{mis})$, our statistic of interest. In the absence of w_{mis} suppose that we have a series of $m > 1$ repeated imputations of the missing wages, $w_{mis}^1, ..., w_{mis}^m$. From this expanded data set we can calculate the imputed-data estimates of the median of the wage distribution $\hat{\beta}^\ell = \hat{\beta}\left(w_{obs}, w_{mis}^\ell\right)$ as well as their estimated variances $U^\ell = U\left(w_{obs}, w_{mis}^\ell\right)$ for each round of imputation $\ell = 1, .., m$. The overall estimate of β is simply the average of the m estimates so obtained, that is: $\bar{\beta} = \frac{1}{m}\sum_{\ell=1}^m \hat{\beta}^\ell$. The estimated variance for $\bar{\beta}$ is given by $T = (1 + \frac{1}{m})B + \bar{U}$ where $B = \frac{\sum_{\ell=1}^m (\hat{\beta}^\ell - \bar{\beta})^2}{(m-1)}$ is the between-imputation variance and $\bar{U} = \frac{1}{m}\sum_{\ell=1}^m U^\ell$ is the within-imputation variance. Test and confidence interval for the statistics are based on a Student's t-approximation $(\bar{\beta} - \beta)/\sqrt{T}$ with degrees of freedom given by the formula: $(m-1)\left[1 + \frac{\bar{U}}{(1+\frac{1}{m})B}\right]^2$. As discussed in Rubin (1987) with a 50% missing observations, an estimate based on 5 repeated imputation has a standard deviation that is only about 5% wider than one based on an infinite number of repeated imputations. Since in some of our countries we have more than 50% missing observations we use $m = 20$ in our repeated imputation methodology.[25] Note that this methodology requires that $\left(\hat{\beta} - \beta\right)/\sqrt{U}$ follows a standard Normal distribution. That is, $\hat{\beta}$ is a consistent estimator of β with a limiting Normal distribution. The LAD estimation property that we discussed above ensure that this is the case.

[25] This choice is quite conservative. Schafer (1999) suggests that there is little benefit to choose m bigger than 10.

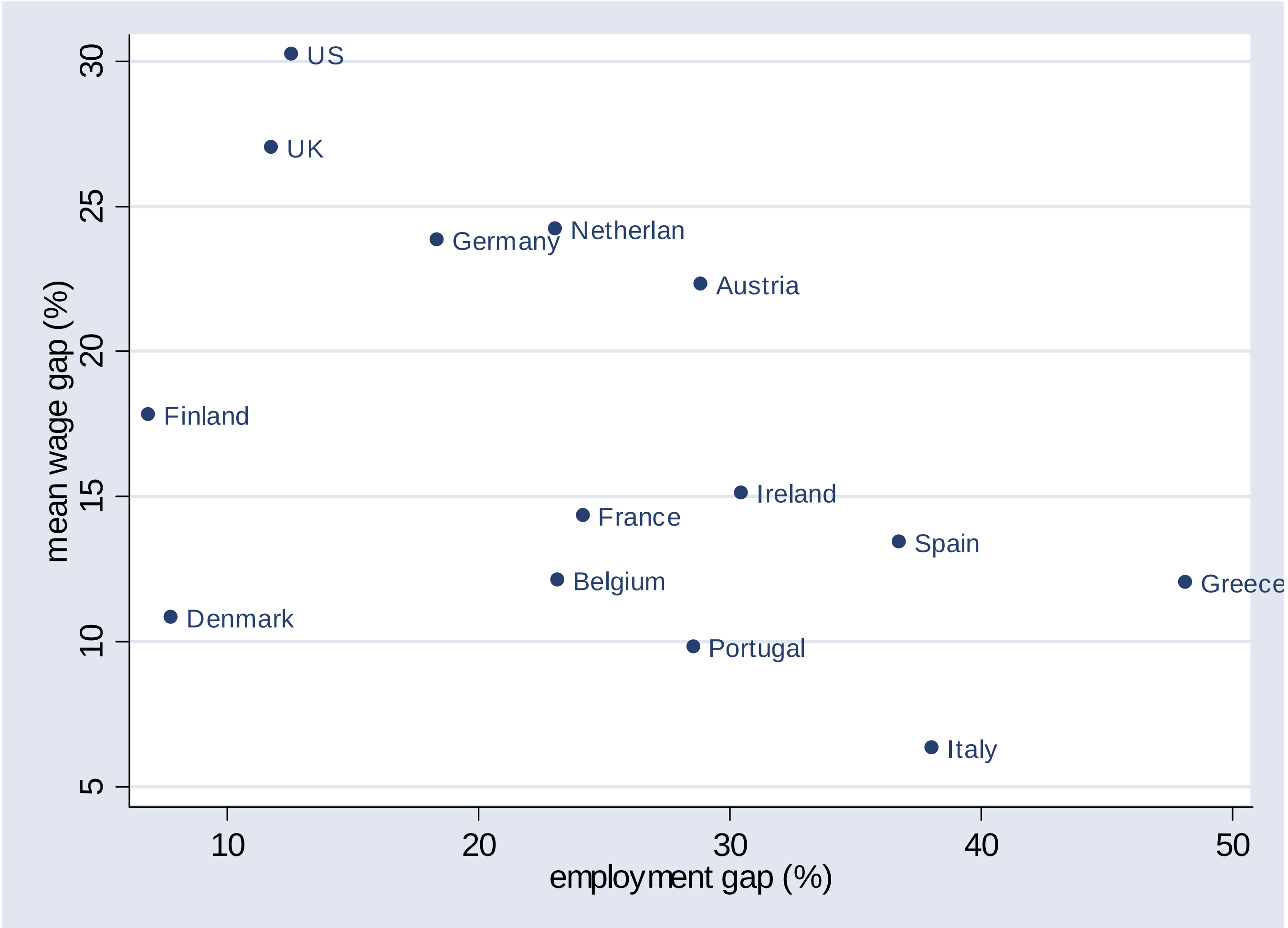


Figure 1: Gender gaps in mean (log) hourly wages and in employment, 1994-2001

Table 1
Raw (mean) wage and employment gaps, 1994-2001
Aged 16-64

	Wage gaps by highest qualification				Employment gaps by highest qualification			
Country	Total	Low	Medium	High	Total	Low	Medium	High
US	30.2	29.6	31.0	39.4	12.6	22.1	13.8	9.2
UK	27.0	24.5	22.2	25.0	11.8	12.2	10.2	8.5
Finland	17.8	17.7	17.5	27.8	6.9	5.8	8.7	8.1
Denmark	10.8	8.0	10.1	16.8	7.8	17.5	6.7	3.0
Germany	23.8	15.5	21.4	25.3	18.4	23.2	17.5	8.5
Netherlands	24.2	23.7	23.5	27.7	23.1	23.2	26.0	12.5
Belgium	12.1	20.1	14.3	15.4	23.2	38.7	26.8	6.7
Austria	22.3	10.4	23.5	26.3	28.9	39.6	24.3	10.5
Ireland	15.1	29.4	15.9	10.4	30.5	36.6	29.8	13.6
France	14.3	17.8	15.7	17.9	24.2	32.3	21.5	11.6
Italy	6.3	15.9	5.6	9.5	38.1	49.8	24.7	14.1
Spain	13.4	24.2	21.2	15.0	36.8	43.8	29.0	16.9
Portugal	9.8	22.7	15.8	8.0	28.6	34.7	9.0	2.0
Greece	12.0	20.9	18.2	12.6	48.2	58.8	42.4	22.1

Notes
1. The sample includes individuals aged 16-64, excluding the self-employed, the military and those in full-time education.
2. Definitions. Low qualification: less than upper secondary education. Medium qualification: upper secondary education. High qualification: higher education.
3. Source: PSID (1994-2001) and ECHPS (1994-2001).

Table 1A
Raw (mean) wage and employment gaps, 1994-2001
Aged 25-54

	Wage gaps by highest qualification				Employment gaps by highest qualification			
Country	Total	Low	Medium	High	Total	Low	Medium	High
US	31.7	30.9	30.6	35.9	13.4	27.31	14.22	10.16
UK	30.5	30.4	26.8	24.0	13.5	13.8	12.2	9.5
Finland	18.4	19.7	17.6	27.0	7.5	4.4	10.1	8.8
Denmark	11.2	12.1	9.6	15.6	7.1	17.4	6.6	2.9
Germany	24.0	28.3	20.3	23.9	18.5	25.1	17.7	9.4
Netherlands	23.9	24.0	22.6	27.0	24.5	24.6	28.1	13.8
Belgium	10.9	20.0	13.7	13.4	20.8	36.3	26.1	6.4
Austria	22.5	25.8	20.9	25.1	26.8	35.7	24.1	11.5
Ireland	17.9	35.2	19.5	5.1	28.9	32.9	31.2	13.2
France	14.2	19.1	15.7	16.9	22.6	29.9	21.7	11.3
Italy	5.7	16.5	5.0	7.1	37.9	51.1	26.4	13.9
Spain	11.6	23.1	21.1	12.4	37.9	46.9	32.5	17.3
Portugal	11.8	26.4	15.4	6.1	26.5	33.0	9.2	2.2
Greece	9.6	21.6	15.3	7.2	46.5	58.6	44.6	20.6

Notes
1. The sample includes individuals aged 25-54, excluding the self-employed, the military and those in full-time education.
2. Definitions. Low qualification: less than upper secondary education. Medium qualification: upper secondary education. High qualification: higher education.
3. Source: PSID (1994-2001) and ECHPS (1994-2001).

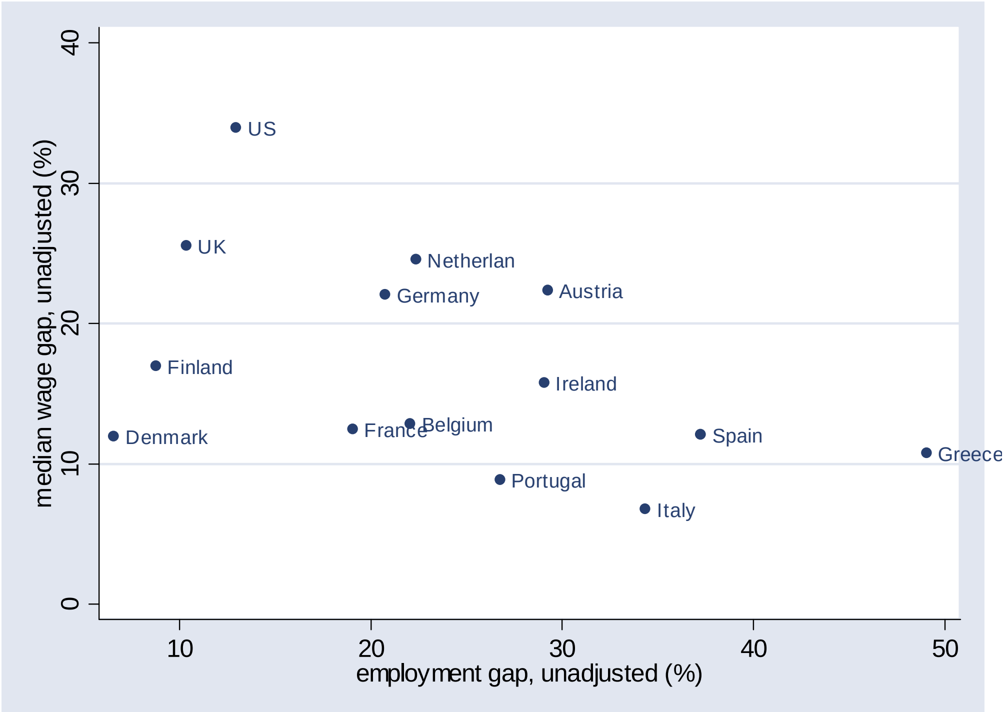
Table 2
Raw (median) wage gaps, 1999, under alternative sample inclusion rules
Wage imputation based on wage observations from adjacent waves

	1	2	3	4	5
US	0.339	0.359	0.371	0.361	0.374
UK	0.255	0.252	0.259	0.271	0.276
Finland	0.169	0.149	0.149	0.158	0.158
Denmark	0.119	0.095	0.095	0.086	0.086
Germany	0.220	0.236	0.232	0.247	0.244
Netherlands	0.245	0.215	0.220	0.218	0.225
Belgium	0.128	0.106	0.115	0.105	0.115
Austria	0.223	0.239	0.238	0.235	0.235
Ireland	0.157	0.256	0.260	0.272	0.279
France	0.124	0.144	0.158	0.152	0.168
Italy	0.067	0.060	0.073	0.070	0.081
Spain	0.120	0.170	0.184	0.161	0.171
Portugal	0.088	0.175	0.180	0.183	0.200
Greece	0.107	0.194	0.212	0.197	0.196
Correlation	-0.455*	-0.227	-0.181	-0.232	-0.231

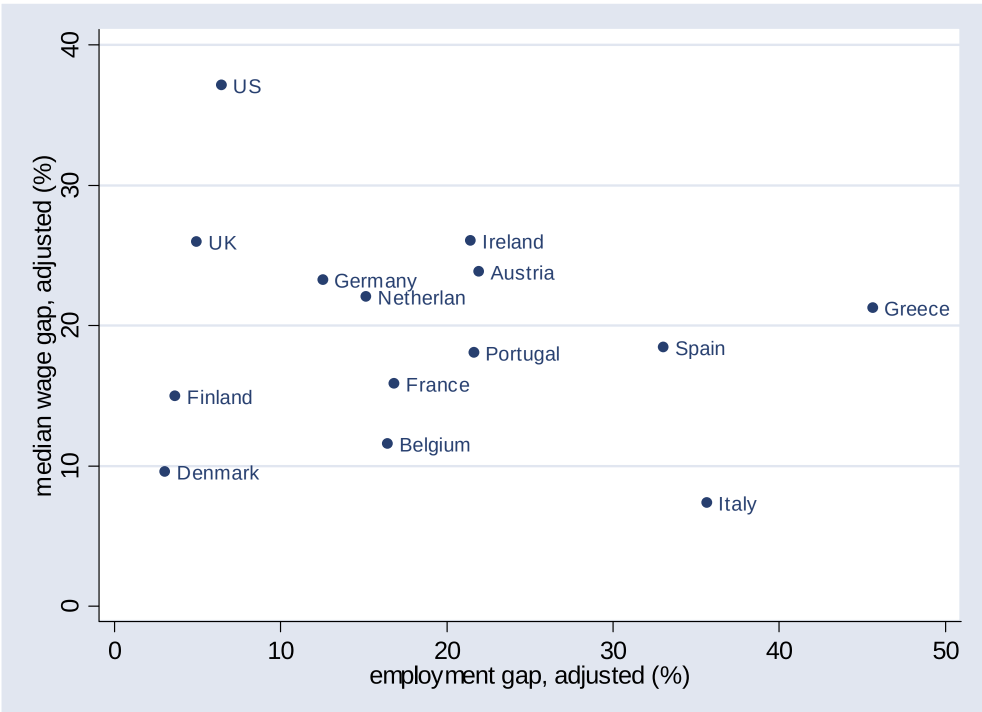
Notes. All wage gaps are significant at the 1% level. Figures in the last row display the cross-country correlation between the gender wage gap and the corresponding gender employment gap after imputation (*denotes significance at the 10% level). Sample: aged 16-64, excluding the self-employed, the military and those in full-time education. Source: PSID and ECHPS.

Sample inclusion rules by columns:

1. Employed at time of survey in 1999
2. Wage imputed from other waves when nonemployed (-2,+2 window)
3. Wage imputed from other waves when nonemployed (-5,+2 window)
4. Wage imputed from other waves when nonemployed (-5,+2 window), adjusted for real wage growth by gender and country.
5. Wage imputed from other waves when nonemployed (-5,+2 window), adjusted for real wage growth by gender and country.



Panel A: Unadjusted gender gaps. Correlation: 0.455*



Panel B: Selection adjusted gender gaps. Correlation: 0.181.

Figure 2: Gender gaps in median hourly wages and employment, 1999.

Table 3
Raw (median) wage gaps, 1999, under alternative imputation rules
Wage imputation based on observables – Educated guesses

	1	2			3			4			5			6
	Wage gap	Wage gap	Goodness imputation		Wage gap	Goodness imputation		Wage gap	Goodness imputation		Wage gap	Goodness imputation		Wage gap
			M	F		M	F		M	F		M	F	
US	0.339	0.455	0.81	0.71	0.340	1.00	0.90	0.350	0.70	0.78	0.355	0.63	0.86	0.372
UK	0.255	0.354	0.77	0.59	0.221	0.80	0.78	0.226	0.60	0.50	0.248	0.78	0.76	0.258
Finland	0.169	0.163	0.78	0.71	0.120	0.78	0.81	0.127	0.67	0.48	0.147	0.88	0.78	0.149
Denmark	0.119	0.105	0.67	0.75	0.078	0.73	0.75	0.083	0.88	0.63	0.100	0.88	0.63	0.095
Germany	0.220	0.403	0.72	0.47	0.239	0.74	0.67	0.225	0.65	0.66	0.241	0.67	0.77	0.232
Netherlands	0.245	0.422	0.45	0.43	0.257	0.65	0.59	0.311	0.78	0.62	0.216	0.45	0.73	0.296
Belgium	0.128	0.267	0.72	0.66	0.143	0.79	0.75	0.100	0.80	0.58	0.111	0.70	0.94	0.135
Austria	0.223	0.438	0.71	0.48	0.222	0.71	0.74	0.220	1.00	0.81	0.250	0.73	0.75	0.239
Ireland	0.157	0.718	0.82	0.18	0.217	0.86	0.71	0.248	0.90	0.78	0.267	0.70	0.91	0.267
France	0.124	0.442	0.76	0.38	0.140	0.81	0.81	0.161	0.86	0.87	0.123	0.75	0.90	0.186
Italy	0.067	-	0.69	-	0.115	0.73	0.66	0.268	0.91	0.71	0.141	0.70	0.87	0.241
Spain	0.120	-	0.59	-	0.205	0.74	0.60	0.297	0.86	0.73	0.159	0.52	0.90	0.302
Portugal	0.088	0.377	0.59	0.43	0.182	0.59	0.63	0.283	0.84	0.67	0.187	0.63	0.55	0.265
Greece	0.107	-	0.75	-	0.240	0.75	0.66	0.491	0.79	0.55	0.281	0.73	0.61	0.408
Correlation	-0.455*	-0.001			0.074			0.461*			0.131			

Notes. All wage gaps are significant at the 1% level. In specification 2 no results are reported for Italy, Spain and Greece as more than 50% of women in the sample are nonemployed. Figures in the last row display the cross-country correlation between the gender wage gap and the corresponding gender employment gap after imputation (*denotes significance at the 10% level). Sample: aged 16-64, excluding the self-employed, the military and those in full-time education. Source: PSID and ECHPS.

Sample inclusion rules by columns:

1. Employed at time of survey in 1999;
2. Impute wage<median when nonemployed;
3. Impute wage<median when unemployed;
4. Impute wage<median when nonemployed & education<upper secondary educ. & experience<10 years; Impute wage>median when nonemployed & education >=higher educ. & experience>=10 years;
5. Impute wage<median when nonemployed & spouse income in bottom quartile;
6. Wage imputed from other waves when nonemployed (-5,+2 window) and (4).

Table 4
Raw (median) wage gaps in sample, 1999, under alternative imputation rules
Wage imputation based on observables – Probabilistic model

	Base sample	Simple imputation		Repeated imputation	
	1	2	3	4	5
US	0.339	0.404	0.396	0.370	0.385
UK	0.255	0.251	0.285	0.257	0.276
Finland	0.169	0.160	0.152	0.149	0.152
Denmark	0.119	0.098	0.093	0.099	0.094
Germany	0.220	0.248	0.250	0.232	0.234
Netherlands	0.245	0.326	0.304	0.258	0.254
Belgium	0.128	0.227	0.192	0.159	0.161
Austria	0.223	0.192	0.251	0.214	0.243
Ireland	0.157	0.386	0.368	0.363	0.325
France	0.124	0.335	0.222	0.210	0.196
Italy	0.067	-	0.383	0.388	0.256
Spain	0.120	-	0.500	0.422	0.323
Portugal	0.088	0.37	0.292	0.270	0.248
Greece	0.107	-	0.758	-	0.512
Correlation	-0.455*	-0.263	-0.132	-0.101	-0.048

Notes. All wage gaps are significant at the 1% level. In specification 2 no results are reported for Italy, Spain and Greece as more than 50% of women in the sample have a predicted probability of having below-median wages higher that 0.5. Figures in the last row display the cross-country correlation between the gender wage gap and the corresponding gender employment gap after imputation (*denotes significance at the 10% level). Sample: aged 16-64, excluding the self-employed, the military and those in fulltime education. Source: PSID and ECHPS.

Sample inclusion rules by columns:

1. Employed at time of survey in 1999;
2. Impute wage >(<) median if nonemployed and $\hat{P}_i > (<) 0.5$. $\hat{P}_i$ is the predicted probability of having a wage above the base sample median, conditional on gender, as estimated from a probit model including two education dummies, experience and its square for each gender.
3. Impute wage >(<) median if nonemployed and $\hat{P}_i > (<) 0.5$. $\hat{P}_i$ as above, having enlarged the base sample with wage observation from other waves in the panel.
4. Impute wage >(<) median with probability $\hat{P}_i$ $(1-\hat{P}_i)$ if nonemployed. Repeated imputation with 20 repeated samples. $\hat{P}_i$ is the predicted probability of having a wage above the base sample median, as estimated from a probit model including a gender dummy, two education dummies, experience and its square.
5. Impute wage >(<) median with probability $\hat{P}_i$ $(1-\hat{P}_i)$ if nonemployed. Repeated imputation with 20 repeated samples. $\hat{P}_i$ as above, having enlarged the base sample with wage observation from adjacent waves.

Table 5
Percentage of adult population in samples for Tables 2 to 4:

	No. obs. in 1999		1 (%)		2 (%)		3 (%)		4 (%)		5(%)		6(%)		7(%)		8 (%)		9 (%)	
	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F
US	3386	4301	94.8	81.8	97.4	90.0	97.7	91.2	100.0	100.0	95.3	82.6	96.2	88.0	96.1	85.8	97.8	92.5	99.6	99.3
UK	2694	3293	84.6	74.2	90.8	84.1	91.9	86.9	96.7	97.1	89.5	76.4	89.0	83.2	87.6	77.0	94.6	91.6	98.9	98.7
Finland	1886	2154	89.2	80.4	94.4	90.6	95.0	91.3	99.0	98.5	98.3	90.8	90.6	84.4	90.1	81.4	95.9	93.4	99.7	99.3
Denmark	1282	1338	93.1	86.5	98.8	95.1	99.0	95.9	98.0	98.1	97.0	92.6	94.1	89.6	93.8	87.5	99.3	96.9	99.8	99.5
Germany	3743	4034	88.2	67.4	95.8	81.0	97.7	85.1	98.5	94.0	96.8	75.0	89.9	70.7	90.4	68.7	98.0	86.6	99.4	96.6
Netherlands	2990	3476	87.1	64.7	91.5	75.2	93.2	78.0	99.7	99.2	90.2	75.1	92.6	85.2	92.0	69.2	97.6	93.6	99.6	99.5
Belgium	1364	1634	88.0	65.9	92.2	73.3	93.2	76.7	98.8	98.3	94.9	76.9	90.0	74.5	91.6	71.8	94.4	83.5	99.0	98.2
Austria	1756	1881	94.6	65.3	98.1	73.9	98.4	76.4	99.7	97.9	99.0	68.8	95.4	67.7	95.4	67.9	98.7	77.7	99.8	94.2
Ireland	1586	1979	84.2	55.1	89.7	66.3	90.6	69.1	99.6	99.1	92.6	58.6	87.8	63.0	87.8	60.7	92.9	75.1	99.9	99.1
France	3067	3557	71.2	52.1	90.8	71.3	92.5	75.6	86.2	90.8	79.0	62.5	75.8	64.6	73.4	53.6	94.4	83.2	97.8	97.4
Italy	3952	4903	74.7	40.3	86.7	49.5	87.9	52.2	94.9	97.2	91.2	52.8	80.8	63.8	77.3	49.2	92.1	73.8	98.3	96.9
Spain	3648	4289	78.0	40.7	88.1	53.7	90.0	56.9	99.6	99.6	90.5	51.8	83.1	59.6	83.0	42.1	92.7	71.9	99.6	99.4
Portugal	2916	3294	88.4	61.6	94.0	70.6	95.0	73.3	99.3	98.8	93.9	68.7	92.3	80.4	90.4	66.2	97.4	88.4	99.3	98.8
Greece	1812	2746	81.8	32.7	90.6	43.0	91.4	45.7	99.8	99.3	93.7	43.2	85.4	55.8	83.9	41.3	93.1	66.1	99.1	98.4

Notes. Figures in columns 1-9 represent the proportions of males and females included in the sample across imputation rules of Tables 2 and 3. Sample: aged 16-64, excluding the self-employed, the military and those in full-time education. Source: PSID and ECHPS.

Sample inclusion rules by column:

1. Employed at time of survey in 1999;
2. Wage imputed from other waves when nonemployed (-2,+2 window);
3. Wage imputed from other waves when nonemployed (-5,+2 window);
4. Impute wage<median when nonemployed;
5. Impute wage<median when unemployed;
6. Impute wage<median when nonemployed & education<upper secondary educ. & experience<10 years; Impute wage>median when nonemployed & education>=higher educ. & experience>=10;
7. Impute wage<median when nonemployed & spouse income in bottom quartile;
8. (3) and (6);
9. (3) and wage imputed using probabilistic model (see notes to Table 4).

Table 6
Adjusted (median) wage gaps, 1999, under alternative imputation rules

Regressors included:	Human capital and state (region) dummies								Human capital, state dummies and job controls	
	1	2	3			4			5	6
	Wage gap	Wage gap	Wage gap	Goodness imputation M	Goodness imputation F	Wage gap	Goodness imputation M	Goodness imputation F	Wage gap	Wage gap
US	0.283	0.308	0.284	0.79	0.79	0.283	0.50	1	0.152	0.221
UK	0.229	0.235	0.210	0.74	0.72	0.227	0.74	0.65	0.173	0.172
Finland	0.222	0.220	0.216	0.64	0.69	0.220	0.88	0.67	0.125	0.134
Denmark	0.112	0.110	0.126	0.70	0.71	0.114	0.75	0.75	0.094	0.095
Germany	0.181	0.181	0.193	0.60	0.51	0.220	0.51	0.55	0.155	0.158
Netherlands	0.170	0.184	0.213	0.62	0.53	0.178	0.33	0.65	0.154	0.157
Belgium	0.121	0.130	0.171	0.59	0.58	0.149	0.55	0.81	0.018	0.065
Austria	0.208	0.219	0.207	0.72	0.63	0.228	0.70	0.67	0.173	0.175
Ireland	0.225	0.259	0.213	0.73	0.62	0.272	0.74	0.55	0.129	0.142
France	0.170	0.197	0.222	0.68	0.57	0.194	0.62	0.54	0.096	0.122
Italy	0.095	0.098	0.154	0.29	0.36	0.167	0.38	0.55	0.116	0.119
Spain	0.139	0.171	0.207	0.58	0.51	0.176	0.43	0.67	0.143	0.157
Portugal	0.175	0.183	0.197	0.51	0.65	0.196	0.57	0.46	0.140	0.146
Greece	0.098	0.122	0.175	0.51	0.46	0.214	0.33	0.28	0.076	0.090

Notes All wage gaps are significant at the 1% level except for Belgium in columns 5 and 6, where they are not significant at standard levels. Regressors included in columns 1-4 are: two education dummies, experience and its square, state dummies for the US, region dummies for the EU. Regressors included in columns 5-6 are: all those of columns 1-4 plus 12 occupation and 12 industry dummies for the US, 9 occupation and 18 industry dummies for the EU. Sample: aged 16-64, excluding the self-employed, the military and those in full-time education. Source: PSID and ECHPS.

Sample inclusion rules by columns:
1. Employed at time of survey in 1999;
2. Wage imputed from other waves when nonemployed (-5,+2 window)
3. Impute wage<median when nonemployed and & individual is unemployed;
4. Impute wage<median when nonemployed & spouse income in bottom quartile;
5. Employed at time of survey in 1999;
6. Wage imputed from other waves when nonemployed (-5,+2 window).

Table 7
Percentage of adult population in sample for Table 6

	No. obs. in 1999		1 (%)		2 (%)		3 (%)		4 (%)		5(%)		6(%)	
	M	F	M	F	M	F	M	F	M	F	M	F	M	F
US	3386	4301	91.9	80.0	94.7	89.1	92.4	80.7	92.0	80.1	56.1	43.2	87.1	74.2
UK	2694	3293	86.9	79.2	90.2	85.8	95.9	89.5	87.9	80.8	48.3	42.2	49.9	45.7
Finland	1886	2154	91.6	84.9	94.1	88.3	95.2	90.9	92.2	86.4	55.5	52.8	57.4	55.1
Denmark	1282	1338	77.2	60.3	82.0	70.5	84.5	66.9	79.1	63.3	62.6	44.9	66.1	50.6
Germany	3743	4034	83.2	62.8	86.8	72.1	86.2	73.0	88.0	68.8	79.2	50.2	81.8	52.9
Netherlands	2990	3476	83.9	62.9	86.4	69.6	90.3	73.3	87.2	68.6	12.0	10.4	13.6	13.8
Belgium	1364	1634	89.2	60.4	91.6	67.9	93.2	63.6	90.0	65.2	88.2	57.6	90.2	61.5
Austria	1756	1881	71.4	46.5	74.8	55.9	78.7	49.7	74.7	51.7	70.4	43.4	71.6	46.4
Ireland	1586	1979	55.5	39.4	66.4	52.5	62.5	49.1	57.5	43.6	52.4	37.1	60.8	44.4
France	3067	3557	69.2	37.6	75.5	44.4	83.9	49.0	71.7	45.7	64.8	35.3	69.0	38.9
Italy	3952	4903	75.5	39.4	81.4	48.7	87.7	50.4	80.4	44.1	73.5	37.4	77.5	40.5
Spain	3648	4289	84.1	58.9	86.8	65.1	89.2	65.7	86.1	63.3	82.5	57.0	84.8	60.4
Portugal	2916	3294	76.8	31.1	80.3	38.1	87.1	40.4	78.8	39.5	73.2	29.9	75.1	32.9
Greece	1812	2746	80.8	69.9	83.9	77.8	85.3	71.8	83.7	73.1	75.4	61.2	77.7	65.4

Notes. Figures in columns 1-6 represent the proportions of males and females included in the sample across imputation rules of Table 6. Sample: aged 16-64, excluding the self-employed, the military and those in full-time education. Source: PSID and ECHPS.

Sample inclusion rules by column:

1. Employed at time of survey in 1999;
2. Wage imputed from other waves when nonemployed (-5,+2 window)
3. Impute wage<median when unemployed;
4. Impute wage<median when nonemployed & spouse income in bottom quartile;
5. Employed at time of survey in 1999;
6. Wage imputed from other waves when nonemployed (-5,+2 window).

Table A1: Descriptive statistics of samples used

	US						**UK**						**Finland**					
	Males			Females			Males			Females			Males			Females		
	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std
Employed	3386	0.949	0.220	4301	0.819	0.385	2694	0.878	0.327	3293	0.771	0.420	1886	0.902	0.298	2154	0.818	0.386
Unemployed	3386	0.014	0.064	4301	0.017	0.085	2694	0.049	0.216	3293	0.021	0.144	1886	0.091	0.288	2154	0.104	0.305
Inactive	3386	0.047	0.212	4301	0.174	0.379	2694	0.073	0.260	3293	0.208	0.406	1886	0.007	0.083	2154	0.078	0.267
Log(hourly wage)	3213	2.760	0.703	3521	2.440	0.660	2278	3.493	0.512	2445	3.238	0.507	1682	5.645	0.477	1731	5.476	0.397
Age	3386	39.702	10.430	4301	39.050	10.439	2694	37.944	12.168	3293	38.112	11.935	1886	39.510	11.450	2154	40.388	11.302
Educ 1	3253	0.166	0.372	4058	0.170	0.376	2694	0.290	0.454	3293	0.331	0.471	1886	0.206	0.405	2154	0.199	0.399
Educ 2	3253	0.576	0.494	4058	0.593	0.491	2694	0.075	0.264	3293	0.106	0.307	1886	0.479	0.500	2154	0.380	0.485
Educ 3	3253	0.258	0.437	4058	0.237	0.425	2694	0.634	0.482	3293	0.563	0.496	1886	0.315	0.465	2154	0.421	0.494
Experience	3279	20.995	18.295	4196	15.493	16.108	2694	20.115	14.004	3293	21.826	14.030	1886	21.190	12.604	2154	21.704	12.131
Married	3386	0.771	0.421	4301	0.652	0.476	2693	0.701	0.458	3292	0.723	0.448	1886	0.753	0.431	2154	0.799	0.401
No. Kids 0-2	3386	0.162	0.423	4301	0.182	0.452	2694	0.109	0.338	3293	0.127	0.367	1886	0.137	0.399	2154	0.143	0.404
No. Kids 3-5	3386	0.175	0.423	4301	0.205	0.468	2694	0.112	0.349	3293	0.135	0.380	1886	0.135	0.375	2154	0.143	0.387
No. Kids 6-10	3386	0.305	0.614	4301	0.344	0.641	2694	0.189	0.495	3293	0.232	0.533	1886	0.238	0.559	2154	0.267	0.585
No. Kids 11-15	3386	0.307	0.626	4301	0.349	0.654	2694	0.187	0.492	3293	0.219	0.524	1886	0.221	0.519	2154	0.244	0.533
Spouse 1st quartile	3386	0.208	0.406	4301	0.166	0.373	2601	0.099	0.298	2971	0.071	0.257	1836	0.064	0.245	2064	0.065	0.247
Spouse 2nd quartile	3386	0.200	0.400	4301	0.156	0.363	2601	0.109	0.311	2971	0.120	0.325	1836	0.143	0.350	2064	0.137	0.344
Spouse 3rd quartile	3386	0.200	0.400	4301	0.156	0.363	2601	0.220	0.414	2971	0.247	0.432	1836	0.261	0.439	2064	0.260	0.439
Spouse 4th quartile	3386	0.153	0.360	4301	0.154	0.361	2601	0.263	0.441	2971	0.254	0.436	1836	0.278	0.448	2064	0.328	0.470

Table A1 (continued): Descriptive statistics on samples used

	Denmark						Germany						Netherlands					
	Males			Females			Males			Females			Males			Females		
	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std
Employed	1282	0.950	0.218	1338	0.884	0.320	3743	0.897	0.303	4034	0.733	0.442	2990	0.874	0.332	3476	0.655	0.476
Unemployed	1281	0.039	0.194	1338	0.061	0.239	3732	0.085	0.280	3987	0.076	0.265	2971	0.031	0.174	3413	0.106	0.308
Inactive	1282	0.010	0.100	1338	0.055	0.229	3743	0.017	0.130	4034	0.191	0.393	2990	0.095	0.293	3476	0.240	0.427
Log(hourly wage)	1194	6.308	0.425	1158	6.190	0.351	3303	4.497	0.608	2720	4.277	0.573	2604	4.886	0.497	2250	4.641	0.520
Age	1282	39.869	11.362	1338	39.851	11.270	3743	38.990	11.765	4034	38.969	11.640	2990	42.010	11.256	3476	41.658	11.254
Educ 1	1282	0.170	0.376	1338	0.173	0.378	3743	0.213	0.410	4034	0.249	0.433	2990	0.886	0.318	3476	0.818	0.386
Educ 2	1282	0.537	0.499	1338	0.531	0.499	3743	0.566	0.496	4034	0.590	0.492	2990	0.040	0.196	3476	0.067	0.251
Educ 3	1282	0.293	0.455	1338	0.297	0.457	3743	0.220	0.414	4034	0.161	0.367	2990	0.074	0.261	3476	0.115	0.319
Experience	1282	22.259	12.340	1338	21.880	12.330	3743	23.262	13.530	4034	23.093	13.263	2990	24.538	14.245	3476	24.975	17.309
Married	1280	0.777	0.416	1335	0.801	0.399	3743	0.737	0.440	4034	0.782	0.413	2990	0.813	0.390	3476	0.806	0.396
No. Kids 0-2	1282	0.148	0.395	1338	0.158	0.404	3743	0.084	0.289	4034	0.091	0.302	2990	0.100	0.324	3476	0.098	0.320
No. Kids 3-5	1282	0.141	0.385	1338	0.153	0.394	3743	0.111	0.342	4034	0.117	0.351	2990	0.130	0.374	3476	0.127	0.369
No. Kids 6-10	1282	0.218	0.509	1338	0.251	0.534	3743	0.190	0.472	4034	0.204	0.489	2990	0.234	0.557	3476	0.239	0.563
No. Kids 11-15	1282	0.197	0.489	1338	0.231	0.516	3743	0.203	0.485	4034	0.217	0.494	2990	0.238	0.557	3476	0.250	0.569
Spouse 1st quartile	1245	0.076	0.266	1274	0.057	0.233	3584	0.159	0.366	3830	0.075	0.264	2827	0.227	0.419	3151	0.101	0.301
Spouse 2nd quartile	1245	0.129	0.336	1274	0.174	0.379	3584	0.067	0.250	3830	0.143	0.350	2827	0.080	0.271	3151	0.105	0.306
Spouse 3rd quartile	1245	0.261	0.439	1274	0.265	0.442	3584	0.256	0.437	3830	0.293	0.455	2827	0.252	0.434	3151	0.264	0.441
Spouse 4th quartile	1245	0.304	0.460	1274	0.295	0.456	3584	0.243	0.429	3830	0.259	0.438	2827	0.245	0.430	3151	0.315	0.465

Table A1 (continued): Descriptive statistics on samples used

	Belgium						Austria						Ireland					
	Males			Females			Males			Females			Males			Females		
	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std
Employed	1364	0.892	0.310	1634	0.674	0.469	1756	0.949	0.219	1881	0.674	0.469	1586	0.846	0.362	1979	0.559	0.497
Unemployed	1363	0.068	0.252	1632	0.111	0.314	1756	0.044	0.205	1878	0.035	0.183	1586	0.084	0.277	1978	0.035	0.185
Inactive	1364	0.039	0.193	1634	0.214	0.410	1756	0.007	0.082	1881	0.291	0.454	1586	0.071	0.256	1979	0.405	0.491
Log(hourly wage)	1201	7.649	0.410	1076	7.521	0.399	1662	6.343	0.494	1229	6.120	0.493	1335	3.462	0.584	1090	3.304	0.547
Age	1364	40.695	10.083	1634	40.110	10.343	1756	36.695	11.829	1881	38.969	12.405	1586	37.176	12.745	1979	40.007	13.081
Educ 1	1364	0.268	0.443	1634	0.277	0.447	1756	0.233	0.423	1881	0.350	0.477	1586	0.412	0.492	1979	0.424	0.494
Educ 2	1364	0.359	0.480	1634	0.342	0.475	1756	0.701	0.458	1881	0.577	0.494	1586	0.390	0.488	1979	0.397	0.489
Educ 3	1364	0.374	0.484	1634	0.381	0.486	1756	0.065	0.247	1881	0.073	0.260	1586	0.197	0.398	1979	0.179	0.384
Experience	1364	21.975	12.630	1634	22.022	14.288	1756	21.478	12.045	1881	24.590	14.983	1586	20.327	14.009	1979	23.178	14.739
Married	1359	0.796	0.403	1632	0.770	0.421	1756	0.630	0.483	1880	0.710	0.454	1586	0.551	0.498	1979	0.654	0.476
No. Kids 0-2	1364	0.116	0.334	1634	0.119	0.341	1756	0.087	0.307	1881	0.114	0.358	1586	0.083	0.292	1979	0.116	0.343
No. Kids 3-5	1364	0.133	0.369	1634	0.138	0.379	1756	0.104	0.332	1881	0.113	0.344	1586	0.099	0.329	1979	0.132	0.377
No. Kids 6-10	1364	0.303	0.632	1634	0.302	0.615	1756	0.191	0.476	1881	0.214	0.500	1586	0.247	0.574	1979	0.290	0.605
No. Kids 11-15	1364	0.260	0.555	1634	0.267	0.568	1756	0.206	0.505	1881	0.221	0.516	1586	0.284	0.612	1979	0.317	0.636
Spouse 1st quartile	1328	0.172	0.378	1564	0.083	0.276	1714	0.131	0.337	1834	0.093	0.290	1558	0.177	0.382	1940	0.080	0.272
Spouse 2nd quartile	1328	0.032	0.175	1564	0.104	0.306	1714	0.092	0.289	1834	0.129	0.335	1558	0.033	0.178	1940	0.101	0.301
Spouse 3rd quartile	1328	0.227	0.419	1564	0.279	0.449	1714	0.202	0.402	1834	0.221	0.415	1558	0.158	0.365	1940	0.190	0.393
Spouse 4th quartile	1328	0.361	0.480	1564	0.293	0.455	1714	0.197	0.398	1834	0.260	0.439	1558	0.175	0.380	1940	0.275	0.447

Table A1 (continued): Descriptive statistics on samples used

	France						**Italy**						**Spain**					
	Males			Females			Males			Females			Males			Females		
	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std
Employed	3067	0.850	0.357	3557	0.613	0.487	3952	0.798	0.401	4903	0.430	0.495	3648	0.784	0.411	4289	0.411	0.492
Unemployed	3063	0.079	0.269	3554	0.104	0.305	3949	0.165	0.371	4902	0.126	0.332	3648	0.125	0.331	4289	0.111	0.314
Inactive	3067	0.072	0.258	3557	0.283	0.450	3952	0.037	0.189	4903	0.444	0.497	3648	0.090	0.287	4289	0.478	0.500
Log(hourly wage)	2183	5.653	0.519	1853	5.529	0.519	2953	4.190	0.407	1975	4.123	0.418	2846	8.412	0.511	1746	8.293	0.548
Age	3067	38.898	10.731	3557	40.091	11.206	3952	37.430	11.258	4903	39.657	11.874	3648	38.210	12.100	4289	40.304	12.651
Educ 1	3067	0.646	0.478	3557	0.616	0.487	3952	0.487	0.500	4903	0.527	0.499	3648	0.561	0.496	4289	0.604	0.489
Educ 2	3067	0.096	0.294	3557	0.117	0.321	3952	0.413	0.492	4903	0.387	0.487	3648	0.192	0.394	4289	0.166	0.372
Educ 3	3067	0.259	0.438	3557	0.267	0.443	3952	0.101	0.301	4903	0.086	0.280	3648	0.247	0.431	4289	0.230	0.421
Experience	3067	25.273	16.773	3557	26.998	17.215	3952	20.472	13.258	4903	26.170	16.875	3648	21.718	14.152	4289	24.426	16.610
Married	2950	0.745	0.436	3447	0.771	0.420	3952	0.606	0.489	4903	0.717	0.450	3648	0.616	0.486	4289	0.696	0.460
No. Kids 0-2	3067	0.133	0.371	3557	0.137	0.378	3952	0.100	0.318	4903	0.107	0.329	3648	0.084	0.289	4289	0.089	0.300
No. Kids 3-5	3067	0.123	0.353	3557	0.120	0.347	3952	0.083	0.287	4903	0.092	0.305	3648	0.078	0.284	4289	0.082	0.288
No. Kids 6-10	3067	0.231	0.519	3557	0.244	0.528	3952	0.156	0.426	4903	0.162	0.429	3648	0.159	0.412	4289	0.169	0.425
No. Kids 11-15	3067	0.225	0.513	3557	0.249	0.536	3952	0.143	0.395	4903	0.159	0.420	3648	0.173	0.444	4289	0.194	0.465
Spouse 1st quartile	2832	0.178	0.383	3283	0.071	0.257	3868	0.276	0.447	4794	0.121	0.326	3622	0.297	0.457	4214	0.064	0.245
Spouse 2nd quartile	2832	0.037	0.189	3283	0.113	0.317	3868	0.000	0.000	4794	0.082	0.274	3622	0.003	0.057	4214	0.103	0.304
Spouse 3rd quartile	2832	0.245	0.430	3283	0.271	0.444	3868	0.088	0.283	4794	0.241	0.428	3622	0.089	0.285	4214	0.234	0.423
Spouse 4th quartile	2832	0.275	0.447	3283	0.305	0.460	3868	0.233	0.423	4794	0.267	0.442	3622	0.224	0.417	4214	0.290	0.454

Table A1 (continued): Descriptive statistics on samples used

	Portugal						Greece					
	Males			Females			Males			Females		
	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std	Obs	Mean	Std
Employed	2916	0.891	0.312	3294	0.628	0.484	1812	0.821	0.384	2746	0.334	0.472
Unemployed	2896	0.055	0.228	3276	0.071	0.258	1812	0.118	0.323	2746	0.105	0.306
Inactive	2916	0.052	0.223	3294	0.298	0.458	1812	0.061	0.240	2746	0.562	0.496
Log(hourly wage)	2578	7.904	0.545	2028	7.815	0.671	1483	8.881	0.516	897	8.775	0.534
Age	2916	36.907	12.524	3294	39.330	12.976	1812	37.414	11.606	2746	40.043	12.919
Educ 1	2916	0.804	0.397	3294	0.765	0.424	1812	0.386	0.487	2746	0.500	0.500
Educ 2	2916	0.126	0.332	3294	0.124	0.329	1812	0.393	0.489	2746	0.354	0.478
Educ 3	2916	0.070	0.255	3294	0.111	0.315	1812	0.221	0.415	2746	0.146	0.354
Experience	2916	21.095	14.189	3294	22.828	16.507	1812	19.094	12.085	2746	24.410	16.965
Married	2916	0.641	0.480	3294	0.723	0.447	1812	0.597	0.491	2746	0.737	0.440
No. Kids 0-2	2916	0.095	0.309	3294	0.104	0.320	1812	0.098	0.333	2746	0.107	0.351
No. Kids 3-5	2916	0.084	0.291	3294	0.094	0.306	1812	0.086	0.288	2746	0.091	0.303
No. Kids 6-10	2916	0.143	0.414	3294	0.163	0.430	1812	0.176	0.467	2746	0.180	0.472
No. Kids 11-15	2916	0.169	0.442	3294	0.199	0.475	1812	0.184	0.463	2746	0.189	0.477
Spouse 1st quartile	2858	0.207	0.405	3205	0.084	0.277	1801	0.250	0.433	2721	0.104	0.306
Spouse 2nd quartile	2858	0.000	0.019	3205	0.141	0.348	1801	0.000	0.000	2721	0.112	0.315
Spouse 3rd quartile	2858	0.193	0.395	3205	0.246	0.431	1801	0.094	0.292	2721	0.251	0.433
Spouse 4th quartile	2858	0.234	0.423	3205	0.245	0.430	1801	0.250	0.433	2721	0.268	0.443

Notes. The descriptive statistics refer to the base 1999 samples, the self-employed, the military and those in full-time education. Source: PSID and ECHPS.

Description of variables:
Employed, unemployed and inactive are self-defined.
Educ1=1 if Less than grade 12 (US); =1 if Less than upper secondary education (EU).
Educ2=1 if Grade 12 completed (US); =1 if Upper secondary education completed (EU)
Educ3=1 if Grade 16 completed (US); =1 if Higher education (EU)
Experience: Actual full-time or part-time experience in years (US); Current age – age started first job (EU)
Married=1 if living in a couple

Table A2:
Proportions of imputed wage observations in total nonemployment

	Male	Female
USA	0.549	0.517
UK	0.478	0.493
Finland	0.534	0.558
Denmark	0.852	0.694
Germany	0.802	0.541
Netherlands	0.477	0.378
Belgium	0.429	0.319
Austria	0.702	0.319
Ireland	0.406	0.312
France	0.740	0.490
Italy	0.523	0.199
Spain	0.545	0.273
Portugal	0.571	0.305
Greece	0.526	0.193

Notes. Figures report the proportion of individuals who were not employed in 1999 but were employed in at least another year in the sample period, over the total number of nonemployed individuals in 1999. Sample: aged 16-64, excluding the self-employed, the military and those in full-time education. Source: PSID and ECHPS.

Table A3:
Aggregate real wage growth

	Males				Females			
	Coef.	(s.e.)	No. obs.	R^2	Coef.	(s.e.)	No. obs.	R^2
USA	0.021^{***}	0.002	20317	0	0.023^{***}	0.002	22376	0.01
UK	0.025^{***}	0.002	23963	0.01	0.034^{***}	0.001	24907	0.02
Finland	0.014^{***}	0.003	9648	0	0.018^{***}	0.002	9933	0.01
Denmark	0.022^{***}	0.002	10762	0.01	0.018^{***}	0.002	10016	0.01
Germany	0.003^{*}	0.001	35106	0	0.003^{*}	0.001	27904	0
Netherlands	0	0.002	20796	0	0.002	0.002	17563	0
Belgium	0.012^{***}	0.002	9994	0	0.013^{***}	0.002	8569	0
Austria	0.012^{***}	0.002	12225	0	0.010^{***}	0.003	8963	0
Ireland	0.027^{***}	0.002	11861	0.01	0.035^{***}	0.003	9276	0.02
France	0.008^{***}	0.002	20166	0	0.013^{***}	0.002	16927	0
Italy	0.004^{***}	0.001	25341	0	0.008^{***}	0.001	16578	0
Spain	0.013^{***}	0.001	24119	0	0.009^{***}	0.002	14246	0
Portugal	0.030^{***}	0.002	20232	0.01	0.037^{***}	0.002	15280	0.02
Greece	0.021^{***}	0.002	13121	0.01	0.022^{***}	0.002	8110	0.01

Notes. Results from regressions of log gross hourly wages by country and gender on a linear time trend. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively. Sample: aged 16-64, excluding the self-employed, the military and those in full-time education. Source: PSID and ECHPS.