Essays on Labor Markets in Developing Countries

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ABSTRACT

Chapter 1 examines the log wage gap between male and female wage and salary earners in urban Turkey. Correcting for selection is crucial in analyzing this gender gap considering that Turkey has the lowest female labor force participation in the OECD. Using quantile regression techniques and accounting for the selection of women into work, I find that the impact of selection is initially positive and large, decreases towards the end of the wage distribution and becomes negative. After

correcting for selection, I find a sticky floor phenomenon; that is, the gender log wage gap is positive and very high at the bottom of the wage distribution and decreases toward zero at the upper end. I find that most of the gender gap is due to differences in observed characteristics.

Chapter 2, which is coauthored with Rita Almeida, examines how stringent employment protection affects firm size by looking at the enforcement of advance notice regulations. We exploit a large micro dataset across 70 countries and explore the within country variation in the enforcement of labor laws in countries with very different advance notice procedures. The findings show that firms facing stricter enforcement of advance notice procedures tend to be smaller and have a more educated workforce. This suggests that strict employment protection rules reduce average employment and disproportionally affect the low skilled. There is robust evidence that effects are stronger for more labor intensive firms in manufacturing, especially those operating in low technology sectors. Sensitivity analysis indicates that these results do not hold in countries where the rule of law is weak.

Chapter 3 contributes to the literature by providing insights on the impact of exporting at different points of the productivity distribution. In particular, using plant-level data on India, I test the “self-selection” and “learning by exporting” hypotheses with respect to labor productivity and wages. I find that exporting firms are more productive and pay higher wages than non-exporting firms not only on average but also throughout the distribution. While the export premium on

productivity is slightly increasing, the impact on wages is roughly constant throughout the distribution. Although I find no evidence of self-selection, I do find some evidence suggesting post-entry improvements in productivity and wages.

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Table of Contents

Introduction

Growing inequality between rich and poor countries has attracted much attention of researchers and policymakers on labor markets in developing countries. Among the most popular policy recommendations are job creation, expanding employment as well as increasing openness and integrating into global economy. The following collection of essays empirically examines three different but equally important subjects on these issues in developing country labor markets. These three essays apply different econometric methods using different datasets. The first chapter studies the extent of and the underlying factors contributing to the gender wage gap in urban Turkey while the second one analyzes the impact of firing regulations on firm size and workforce composition in a sample of firms across seventy countries. The third chapter looks at the relationship between exporting and labor productivity and wages in a panel of Indian firms.

Chapter one uses data from the Turkish Household Budget Survey to identify the magnitude of and underlying reasons for the gender wage gap. Accounting for the selection of women into work uncovers important patterns of the female labor force participation in the Turkish labor market. Using counterfactual analysis and taking into account selection of women into work, I find that differences in the labor market characteristics between men and women give rise to the most of the gender wage gap.

Chapter two, which is coauthored with Rita Almeida, examines how stringent employment protection affects firm size and workforce composition by taking account of the variability of enforcement of regulations in developing countries. I focus on the rigidities introduced by advance notice procedures. I expect that the effects of the same labor regulations will differ across countries depending on the strength of enforcement. I assume that firms facing a stricter enforcement of the labor code will be constrained to a larger extent in their employment decisions. Using data from the World Bank Enterprise Surveys, I present suggestive evidence that strict employment protection rules reduce average employment and disproportionately affect the employment of lower skilled workers. This effect is stronger for manufacturing firms that operate in low-tech industries that are mostly labor-intensive.

Finally, the last chapter investigates the direction of observed positive relationship between exporting and labor productivity and wages using data on Indian firms from the Enterprise Surveys. Exporting firms are on average more productive and pay higher wages to their workers. There are two potential explanations for this: firms with superior characteristics become exporters or exporting makes firms better by exposing them to international markets. While both of these hypotheses could simultaneously account for the superior performance of exporters, empirical results provide weak evidence only for the latter.

Chapter 1: The Gender Wage Gap and Sample Selection in the Turkish Labor Market

1. Introduction

The wage gap between men and women has been a widely investigated subject among labor economists in the last decades. Although there is an extensive literature on gender wage differentials in developed countries, such studies have been quite rare for Turkey as for most developing countries.

This paper examines the gender wage gap in Turkey in 2004. This is particularly interesting when one considers that Turkey, compared to other OECD countries, has the lowest female labor force participation rate which has remained quite steady contrary to what other OECD countries have been experiencing.[1] Moreover, the Turkish labor market is among those in which the female-male wage ratios are relatively high (Anker, 1990).[2] In this respect, the low participation of

[1] See Jaumotte (2003)

[2] Using 1990 data, Anker (1997) estimates average female-male (weekly) wage ratios several countries. He finds the ratios in the world to be roughly 70-75 percent based on daily and weekly reference periods. Turkey, with 84.5, has one of the ratios together with Scandinavian countries, some other OECD and developing countries.

females with a relatively low gender wage gap in the Turkish labor market fits well with evidence from the US and several European countries that a negative correlation between the gender wage gap and the employment gap exists across countries (Petrongolo and Olivetti, 2008). To the extent that low participation rates imply that only women with high-wage characteristics are observed working, the observed gender wage gap would be smaller in countries where women participate less.

The Turkish labor market is characterized by a steadily decreasing labor force participation rate from 67 percent in 1978 to 47 percent in 2004 (Dereli, 2006).[3] Since 2001, the female labor market participation rate has been around 25-26 percent while it has been around 72-73 percent for males. Participation rates are even lower in urban areas due to steady migration from rural areas.[4] Participation rates are 19.9 and 70.8 percent in 2006 for urban women and men, respectively. Figure 1 exhibits the labor force participation rates of urban men and women aged 15 and above.[5] The labor force participation rate shows a declining pattern for both men and women.

[3] These figures do not differentiate between genders.

[4] This is because migrant women, who worked as unpaid family workers on farms prior to migrating to the cities, can no longer find a job given their low skills (Ilkkaracan, 1998).

[5] Note that the sample I use in this study is restricted to men and women of ages between 25 and 55. I generated Figure 1 based on a query on the website of SIS (www.tuik.gov.tr) which does not allow a modifications on the age group. This was because data prior to 2004 is not released to public electronically.

The labor force participation rate also differs substantially by educational attainment. Table 1 presents the labor force participation rates of men and women across different education groups. Although the participation rate of women with university education is significantly higher than that of women with high school and primary school education, men participate more at each educational category than women.

Selection into the labor force should be taken into consideration when studying the gender log wage gap in the Turkish labor market for comparability with the results from countries in which selection is not an issue. Beyond that, even in countries with similar labor force participation rates, differences in patterns of labor force participation may lead to different measured and potential wage gaps.[6] Therefore, I follow the approach developed by Albrecht, Van Vuuren and Vroman (2009) and look at wage inequality at different points of the wage distribution taking into account the selection of women into the labor force. After the selection effect is accounted for, I decompose the wage gaps into two components, using the Machado-Mata (MM hereafter) technique: differences due to differences in characteristics and differences due to differences in the returns to those characteristics. In another developing country context, Badel and Peña (2007) applied this method to analyze

[6] Neal (2004) also highlights the importance of selection correction in analyzing wage differentials between black and white women in the US since differences in patterns of labor force participation of black and white women lead to very different results despite identical participation rates.

the gender log wage gap in the Colombian labor market, where they find a U-shaped wage gap after controlling for selection of women.

To the best of my knowledge, the gender log wage gap in the Turkish labor market has been studied in only a few other papers, which use earlier data. Using data from the 1994 Household Expenditure Survey, Tansel (2005) finds that the wages of men and women are almost at par in the public sector while the average log wage differential is 0.22 and 0.27 in the state-owned enterprises and formal private sector jobs, respectively. Tansel decomposes the gender log wage gap by employment in public, private and state-owned enterprises in the Oaxaca-Blinder fashion accounting for selection into a particular sector. Tansel distinguishes also between formal and informal private sector jobs. The wage differentials by sector are then decomposed into four components: pure rent (i.e., the part that is not explained by observables), differences due to differences in endowments, in market returns to endowments and in selection. Tansel finds that around 44 percent of the wage differential in the covered private sector is unexplained, i.e., due to pure rent from being in the private sector as well as due to differences in market returns suggesting a substantial degree of discrimination against women. While the Oaxaca decomposition suggests that 64 percent of the earnings gap is not explained, educational attainment is found to be in favor of women.

Dayioglu and Tunali (2004) analyze the evolution of the gender log wage gap in urban Turkey between 1988 and 1994. Using micro data from the 1988 and 1994

Household Labor Force Surveys, they find that the raw gender log wage gap widened from 2 percent in 1988 to 15 percent in 1994. They use the Oaxaca-Blinder method to decompose the gender wage gap into parts due to differences in returns and due to differences in endowments and correct for selection of both genders into work. Their results suggest that differences in returns to observable and unobservable traits account for a log wage gap of around 12-18 percent in 1988 to 22-33 percent in 1994. This implies that differences in returns account for more than the gender wage gap while differences in endowments (including workplace characteristics) act in favor of the women. Using a modification of Heckman's two-step methodology, the authors identify a significant selection effect for both genders. Further, by grouping individuals on the basis of education and years of (potential) experience, they report that the evolution is different in the government and non-government sectors such that while in the former women in all education-experience groups are adversely affected by the widening wage gap during the period and a glass ceiling phenomenon was observed, in the latter, wage gaps were higher and women with college degree were observed having wages on par with men. In an earlier study, Dayioglu and Kasnakoglu (1997) report a raw (monthly) wage gap estimate of 51 percent using 1987 data from Household Income and Expenditure Survey and support the view that the gender gap is more pronounced in the self-employed category than for wage earners.

Using matched employer-employee data from 1995, Ilkkaracan and Selim (2007) find that the gender log wage gap remains even after controlling for workplace characteristics in addition to standard Mincerian wage equation covariates. Ilkkaracan and Selim report a raw gender gap of 34 percent. By decomposing the wage gap using the Oaxaca-Blinder method, they conclude that the inclusion of workplace characteristics variables explains 78 percent of this gap through lower endowments of women. Tenure and experience, heavier concentration of men in workplaces covered by collective bargaining and finally industrial and occupational gender segregation are found to be the leading factors in explaining the gender gap.

The magnitude of the gender log wage gap varies greatly depending on the study and the data. The magnitude of the gender gap in all of the above mentioned papers is remarkably larger than the raw gap identified in my study, most likely due to the economic crisis of 1994 which may have resulted in wage cuts for women. Another source of this discrepancy may be that the dataset used in Ilkkaracan and Selim (2007) is a sample from formal sector manufacturing firms with at least 10 workers. One would suspect that given that their sample is restricted to formal sector firms, the measured wage gap would understate the wage gap in the entire labor market. Restricting the 2004 sample used in my study to formal manufacturing firms with more than 10 workers yields a gender gap at the mean of 20 percent while the raw gender gap found using the entire sample is 7 percent. This, again, can be

explained by two factors. First, the difference in survey years and the fact that the male unemployment rate did not fall while the female unemployment rate fell from 9.3 percent in 1993 to 8 percent following the economic recession in 1994, lends support to the possibility that women may have suffered wage cuts due to the economic crisis.[7] Second, as stated in Ilkkaracan and Selim, the discrepancy might also be due to the variation of the gender wage gap across sectors and the fact that public employment, which has been more egalitarian in pay schemes, is less prevalent in the manufacturing sector than in other sectors.

Since women select into the labor market in a nonrandom way, the gender log wage gaps should be corrected to account for selection. As in Albrecht, Van Vuuren and Vroman (2009), I correct for selection bias in the case of quantile regressions using the technique in Buchinsky (1998). The selection effect is positive and large in magnitude at the lower end of the distribution and then decreases and turns negative. Ignoring the effect of selection would yield misleading results for women at different points of the wage distribution. In particular, the measured gender log wage gap underestimates the true one for women with relatively low wages if selection of women into the labor force is not taken into consideration. The selection effect is decomposed into two parts: due to observables and unobservables.

[7]The wage cuts were most likely implemented as a means of recovering from the economic crisis which is also reflected in the decline in unit labor costs from 78.4 to 49.2. Base year of real output is 2000. (Source OECD)

I find that sample selection based on observable characteristics works to the advantage of working women, simply because working women more are higher educated than non-working women. The part of the selection effect that is due to unobservables is negative and increases in magnitude throughout the distribution. This rather rare finding in the literature suggests that the unobservable factors that are affecting participation are negatively correlated with the unobservables determining the wage.

Finally, I construct a counterfactual to the selection-corrected wages to simulate the distribution of wages for women that we would expect to observe if all women worked and had the male distribution of observed characteristics. After adjusting for selection and gender differences in the distribution of observed characteristics, the gender log wage gap disappears at the lower tail of the distribution and then turns negative. This implies that women would earn at par with men at the bottom of the wage distribution, after controlling for differences in the distribution of characteristics, and would receive substantially higher wages than men in the rest of the distribution.

2. Data and Descriptive Statistics

The data used are drawn from the Turkish Household Budget Survey, 2004, collected by the State Institute of Statistics. The sample is a cross section of 35,388 individuals from 8,640 households in the urban and rural areas. Only the urban

subsample of the data is used since employment in rural areas is predominantly in agriculture, mostly in the form of unpaid family workers and self employment is also prevalent in the agricultural sector The dataset includes rich information including household composition, household consumption expenditures and housing type as well as labor market variables such as labor force status, sector of employment, occupation and earnings. Two stage stratified clustered sampling was used to collect the data.

The focus of the analysis is on individuals who are either wage or salary earners or casual workers, who are between ages 25 and 55, and who reside in an urban area. The urban areas are defined as those with population greater than 20,000. Also excluded from the analysis were 491 individuals who work fewer than 40 hours per week and 13 observations whose weekly hours were top-coded.[8] This leaves 2,742 men and 549 women with a complete set of covariates. I also use information on 4,401 women who are not working and who are between 25 and 55 years old to explain the participation decision of females.

The dataset provides information on the monthly earnings of individuals in the month of the survey. I obtain the hourly wages by dividing the monthly income by the hours worked (assuming the individual works 4 weeks in a month).

[8] The legal definition of full time work requires 40 and 45 hours per week in the public and private sectors, respectively.

Information on non-labor income, which includes real estate and interest income, is also provided in the dataset.

Table 2 provides the descriptive statistics for the key variables for men, working women and nonworking women. Women earn on average slightly less than men. Men are older than working women, however nonworking women are on average the oldest. Men are more likely to be married than both working and nonworking women. In terms of schooling attainment, nonworking women are the least educated; 73 percent of nonworking women have either no diploma or finished only elementary school. On average working women are more highly educated than men. The proportion of working women with at least a college degree is substantially higher than that of men. Twenty-four percent of working women have at least a college degree, while this is 12 percent for men. Higher education of working women relative to men is also observed by Tansel (2005) and in the form of increased educational attainment of women between 1988 – 1994 by Dayioglu and Tunali (2004).

Figure 2 shows the 95 percent confidence interval of the gender wage gap along the wage distribution for the final sample used in this study. The gender wage gap is obtained by taking the difference of their respective distributions at each percentile. The log wage gap displays the pattern of a so called “sticky floor”: it is around 30 percent in the lower end of the distribution, decreases to around 10 percent in the center of the distribution and finally decreases towards zero. While glass

ceilings are more commonly observed in European countries (Albrecht et al. (2003) and (2009), Arulampalam, Booth and Bryan (2004)), studies for developing countries differ in their findings about the pattern of the gender gap across the distribution. Chi and Li (2007) identify glass ceilings in China. Ganguli and Terrell (2005) point to the existence of glass ceilings in Ukraine, especially in the public sector. De la Rica, Dolado and Llorens (2008) examine the gender wage gap in Spain by different education groups. They find evidence of glass ceilings for the tertiary educated group and of sticky floors for the group with primary and secondary education group. While these studies are informative about the magnitude of and the factors contributing to the gender gap in their respective countries, they are not useful benchmarks for cross-country comparisons of the gender wage gaps since not at all of them take into consideration differing patterns of female labor force participation.

To explain the incidence of work, information on home ownership, being household head, log non-labor income and log other household income and two dichotomous variables for whether there are 2 children younger than 18 in the household and whether there are at least 3 children younger than 18 in the household are used in the selection equation. Homeownership is a dummy variable that equals one if the person lives in a house owned by a member of that household. Non-labor income is income not related to labor market activities such as interest income, real estate income and pensions. Other household income is obtained by subtracting total

individual income (including non-labor income) from total household income. In terms of non-labor income, nonworking women fare slightly better. It is striking that very few individuals report a nonzero amount of non-labor income. Only 55 percent of men, 52 percent of working women and 14 percent of nonworking women declare positive non-labor income. Among the women who report positive amounts of non-labor income, those who do not work have the lower levels of non-labor income than working women on average. Working and nonworking women report almost the same levels of other household income. As for household composition, working women are the least likely to come from households with young children. In summary, working women are the youngest and most educated and least likely to be a member of a household with young children.

In the selection analysis, I estimate the probability of being employed as a function of age, age squared, marital status and education levels, the logarithm of other household income, logarithm of non-labor income, dummy variables for home ownership, household composition, and for being the head of household.

3. Methodology

3.1 The Machado Mata Method

The Machado-Mata (2005) decomposition technique can be viewed as a generalization of the Oaxaca-Blinder decomposition (Oaxaca 1973). By making use of counterfactual analysis, the MM method decomposes the observed gap between

the male and female wage distributions into two components: one due to differences in observed characteristics and the other due to differences in the returns to those characteristics.

Consider the two groups, men and women. The basic assumption of the MM method is that for each group, the θ^{th} quantile of the conditional (log) wage distribution, $Q_\theta(W|X)$, is characterized by the regression quantiles $\beta(\theta)$: $Q_\theta(W|X)=x\beta(\theta)$ where $\beta(\theta)$ is a vector of quantile regression coefficients and $\theta \in (0,1)$. For a given $\theta \in (0,1)$, $\beta(\theta)$ can be estimated as in Koenker and Bassett, (1978). This assumption can be used to simulate the counterfactual distribution of log wages that we would expect to observe if women were endowed with the labor market characteristics of men but were paid like women. Comparing this counterfactual distribution with the observed male wage distribution would then give the component of the log wage gap that is due to differences in the returns to labor market characteristics. By the same argument, the part of the observed log wage gap that can be attributed to differences in characteristics can be found by generating the counterfactual wage distribution that would arise if women retained their own characteristics but received men's returns to those characteristics.

To see this let j=F,M denote female and male groups. Consider the counterfactual random variable W^{FM} such that $Q_\theta(W^{FM} | XF=x^F)= x^F \beta^M(\theta)$ and $\theta \in (0,1)$. The MM method generates a sample from the unconditional distribution of W^{FM} by first drawing θ from the standard uniform distribution and then computing

$\hat{\beta}^{M}(\theta)$. By sampling X^{F} from the empirical distribution of X for the F population $\widehat{W}^{FM} = x^{F}\hat{\beta}^{M}(\theta)$. By repeating this process N times, a sample of size N can be obtained. This random variable has the interpretation of the wage that a female worker would earn were she to receive men's returns to her characteristics. It must be noted that the sample generated in this way is not a true sample from the stochastic variable W^{FM} since they are based on estimates rather than the true population parameters. Albrecht, Van Vuuren and Vroman (2009) (AVV hereafter) show that the sample quantiles of the counterfactual distribution generated by this procedure yields consistent and asymptotically normal estimates of the quantiles of the counterfactual distribution that they are designed to simulate.

3.2 Selection Correction

Since women select into the labor force in a nonrandom way, selection of women into the labor force must be accounted for in analyzing the gender gap. To the extent that women who have the highest earnings potential are those who are indeed working, the observed gender log wage gap underestimates the true gap. AVV (2009) extend the Machado Mata decomposition to allow for a sample selection correction. They start by defining W^{F} as a counterfactual random variable that denotes the log wage that a randomly selected woman would earn were she to engage in market work and X^{F} the characteristics of that randomly selected woman that determine her market wage. The quantiles of W^{F} conditional on vector x^{F} are

given by: $Q_\theta(W^F \mid X^F=x^F)= x^F \beta(\theta), \quad \theta \in (0,1)$ where $\beta(\theta)$ is the true parameter correcting for selection. They follow estimation method given by Buchinsky (1998) and estimate $Q_\theta(W^F \mid X^F=x^F)=x^F \beta(\theta)+h\theta(z^F\gamma)$, where z is a vector of characteristics that affect the probability that a woman works.[9] The vector z may include the variables that influence the wage but for identification z must contain at least one variable that is not included in X and that is not correlated with the log wage. The novelty here is the term $h_\theta(z^F\gamma)$, which corrects for selection at the θ^{th} quantile and may be considered equivalent to the Inverse Mill's ratio in the Heckman (1979) method. However, it should be noted that $h_\theta(z^F\gamma)$ is quantile specific and does not make any distributional assumptions. As suggested by Buchinsky (1998) and AVV (2009) a power series approximation of $h_\theta(z^F\gamma)$: $\hat{h}_\theta(zF\gamma)=\delta_0(\theta)+\delta_1(\theta)\lambda(z^F\gamma)+\delta_2(\theta)\lambda^2(z^F\gamma)+\ldots$ where $\lambda(.)$ is the Inverse Mill's Ratio, although any function of the single index, as well as the index, itself could be used. In their first step AVV (2009) estimate the γ's by using the single-index method as suggested by Ichimura (1993). The single-index model minimizes the squared distance between the binary variable D that equals one if a woman is observed working and $P(D=1 \mid Z=z^F)\equiv G(z^F\gamma)$ by a kernel regression. Having computed γ, the next step is to estimate the $\hat{\beta}(\theta)$ using the Buchinsky technique. By sampling x^F from

[9]Note that this method is not free of criticisms. Melly and Huber (2008) suggest that the Buchinsky (1998) method is vulnerable to the violation of the assumption of independence of the error terms and the covariates in the wage equation.

the empirical distribution they compute $\hat{w}^F = x^F\hat{\beta}(\theta)$ By repeating the above steps M times a sample from the counterfactual distribution of w^F of size M can be generated.

4. Findings

4.1 Single Index Estimation and Quantile Regressions

I start the analysis by estimating the quantile wage regressions for men and women separately. I regress log hourly wage on age, age squared and the education variables, with less than secondary school as the base category. Table 3 reports the quantile regressions for men and women without selectivity correction. The return to age is quite large for both men and women relative to what is found in other studies in the literature. The return to education is higher for women than for men for all levels of educational attainment except at the first decile. Returns to high school and college education are not strictly increasing along the wage distribution which is contrary to the findings of Machado and Mata, (2001) and Martins and Pereira, (2004).

For a sound comparison of the male and female quantile regression coefficients we need to account for the selection of women into work. Table 4 displays the estimation results for the incidence of work for the women in our dataset. I report both the single index estimation results using the technique of

Ichimura (1993) and probit estimates.[10] In addition to the control variables in the wage regression, a dummy variable for homeownership, being the household head, and being a member of a household with two or with three or more children younger than 18 years old as well as logarithm of non-labor income and the logarithm of other household income are used. All variables except the log of non-labor income have the expected signs. The Hausman test rejects the null hypothesis of normally distributed errors.[11] This further justifies the use of semiparametric methods in the analysis.

Table 5 reports the quantile wage regressions adjusted for selection using the Buchinsky method. The results are presented using a linear selection correction function of the following form: $h\theta(zF\gamma)= \delta 0(\theta)+\delta 1(\theta)\lambda(zF\gamma)$. A couple of patterns emerge. First, the return to age switches sign and becomes insignificant when selection into work is controlled for. Second the returns to all levels of schooling

[10] Note that the constant and the coefficient of one of the continuous variables can not be identified in a single index model. Therefore, I normalize by setting the constant and the coefficient of the age variable equal to their probit counterparts.

[11] The Hausman test is done using the estimates from the Probit and Single Index estimations. Under the null hypothesis of normally distributed errors, the test statistic is $(d_{single}-d_{probit})'(V_{single} -V_{probit})^{-1} (d_{single}-d_{probit}) \sim \chi(df)$ and i={single index, probit} where d_i and V_i are the coefficients and covariance matrix and df is the dimension of d_i. The result of the Hausman test indicates that we reject the null hypothesis of normally distributed errors.

decrease at all quantiles. Finally, the coefficient of the inverse Mill's ratio is negative and significant in the first half of the distribution which points to a negative correlation between the unobservable factors determining selection into work and wages.

4.2 Decomposition Results without Selection Correction

Figure 2 shows the raw gender gap obtained by the quantile regression of log wage on a gender dummy variable and a constant at each percentile of the distribution, without controlling for any additional variables. The wage gap starts at around 30 percent, quickly falls down to around 17-20, and stagnates around 12-13 percent through the middle and decreases to zero at the end of the distribution.

This raw gap is decomposed into a component that is due to differences in the characteristics and one due to differences in the returns to these characteristics. Figure 3 displays the difference between the actual male log wage distribution and the counterfactual log wage distribution that would arise if women had men's labor market characteristics but retained their own returns to those characteristics. These labor market characteristics are age, age squared and education. By doing this exercise, I am able to find the log wage gap that is due to differences in returns. Comparison of Figures 2 and 3 reveals that differences due to differences in returns can only explain the gender wage gap at the very end of the distribution. This is equivalent to saying that at the upper tail of their respective wage distribution, were

women to have men's distribution of endowments and receive their own returns, they would have earned even wages at par with men.

Another useful exercise is to estimate the part of the wage gap that is due to differences in characteristics. Figure 4 shows the part of the raw gap that remains after controlling for the differences in the returns to observable labor market characteristics. In this case the gap is not significantly different from zero for the most of the distribution, except for between 25th and 60th percentiles. Controlling for differences in returns reverses the gender gap between the 25th and 60th percentiles of the log wage distribution, which implies that were women to receive the male returns to their observed characteristics, they would have outearned men by about 10 percent at the middle of the wage distribution. This is not surprising given that the share of working women with a college degree or more is twice that of men.

4.3 Decomposition Results with Selection Correction

Low participation rates and low attachment of women to the labor force imply that selection of women into the labor force should be taken into account when comparing the wages of men and women. To do that we need to simulate the distribution of wages had all women worked as in AVV (2009). This is done by generating the counterfactual wage distribution of women by using the selectivity adjusted coefficients and the labor market characteristics of all women as opposed to

only working women. Comparing this distribution with the observed male distribution gives the gender wage gap corrected for selection.

4.4 Decomposition of the Selection Term

The selection effect is calculated as the difference between the observed female wage distribution and the distribution that would result if all women worked, i.e., corrected for selection. As shown in Figure 5, the selection effect is initially strongly positive and significant, becomes insignificant between and then turns negative toward the top of the distribution. It is around 30 percent in the first decile which implies that the raw gap underestimates the true one at the bottom of the distribution. That is, at the low end of the wage distribution, women who are observed working are the ones who have higher earnings potential. The selection effect is not significantly different than zero for the middle of the distribution and significantly negative for the top portion of the wage distribution. The negative selection effect at the top of the wage distribution implies that at the top of the wage distribution women who are working are actually the ones with lower potential wages. This negative selection effect is rather rare in the literature. Using Dutch data, AVV (2009) identify a positive and significant selection effect that ranges between 8 to 15 percent while Badel and Peña (2008) find a positive and slightly increasing selection effect ranging between 15 and 30 percent for Colombia.

Figure 6 depicts the gender log wage gap after selection is adjusted for. This is done by comparing the difference between the male log wage distribution and the counterfactual wage distribution for women that would be observed if all women were to work. Comparison of Figure 6 with the raw gap in Figure 1 suggests that the gap with selection correction is larger than the raw gap until the 3rd decile and is not significantly different than zero until the 65th percentile. It is important to note that the gender gap with selection correction becomes negative after the 90th percentile which suggests that had all women worked, women would have earned higher wages than men at the top their respective wage distributions.

Using the Machado-Mata technique, I estimate the counterfactual wage distribution that would have prevailed if working women's returns to labor market characteristics are adjusted for selection. That is, in generating the selection-corrected wage distribution for women, if a sample is taken from the set of covariates for the working women (instead of all women), one would obtain the counterfactual distribution which is based on the assumption that nonworking women have the same distribution of characteristics as women who work. The difference between the above counterfactual distribution and the one that results if all women were to work gives the portion of the selection effect that is due to observable characteristics between working and nonworking women. Figure 7 shows that the part of the selection effect that is due to observables is strongly positive and varies between 20

and 50 percent at different points of the distribution. This is simply due to substantial differences in education between working and nonworking women.

The part of the selection effect that is due to unobservables is obtained as the difference between the observed wage distribution for women and the counterfactual distribution that would arise if we sampled from the covariates of the working women when correcting for selection. This part can be considered to arise from the differences in the returns to characteristics induced by taking into consideration nonrandom selection of women into work. Figure 8 shows that the part of the selection effect due to unobservables is negative and grows in magnitude throughout the distribution. This calls for further scrutiny since the sign and the magnitude of the component of the selection effect due to unobservables are not in line with either Albrecht, Van Vuuren and Vroman (2009) or Badel and Peña (2008). This rather rare finding can best be explained by the conjecture that Turkish women work only when they have to.

Finally, I generate the counterfactual wage distribution that would arise if all women worked, had men's distribution of characteristics and received their own returns to these characteristics (corrected for selection). Figure 9 displays the difference between the observed male log wage distribution and the resulting counterfactual log wage distribution. Hence, comparing Figure 9 and Figure 6 we can see that accounting for the difference in characteristics fully explains the gender gap until the 20th percentile and reverses it after then. That is, if all women worked

and had the men's distribution of characteristics they would receive higher wages (up to 80 percent more at the upper end of the distribution) than men. This finding is the opposite of that found by Dayioglu and Tunali (2004) and Tansel (2005) that most of the gender wage gap arises from differences in returns to characteristics.

5. Conclusion

This paper analyzes the gender wage gap over the wage distribution by taking into account the selection of women into work, which is very relevant given the very low female labor force participation rate in Turkey.

Analysis of the log wage gap over the distribution reveals interesting patterns that can not be observed by standard methods which look at the gap only at the mean and therefore can not be informative about different points of the wage distribution. I find that the raw wage gap underestimates the true wage gap for the majority of the wage distribution and substantially overestimates it at the very top portion where women are estimated to earn higher wages than men. This finding suggests that at the upper tail of the wage distribution it is not necessarily the women with high-wage potential who are observed working and hence further underscores the appropriateness of the use of quantile regression analysis.

It is remarkable that the main component of the gender gap is differences in returns if sample selection is ignored whereas differences in characteristics explain

most of the gap when selection is controlled for. This is simply because of the large differences in education between working women and those who do not work.

Decomposing the selection effect indicates that while women select positively into work based on observable characteristics, their unobservable characteristics that determine selection into work are negatively correlated with wages. Given the very low participation rates, one possible interpretation would be that women only work out of necessity. After controlling for selection of women into work, I find that differences in characteristics are the main component of the wage gap for the majority of the distribution.

Finally, this paper provides a snap-shot of the gender log wage gap in Turkish labor market which has not been analyzed until now. The stark contrast between the findings of this paper and past studies can be attributed to the use of different datasets and methodologies. Therefore, extension of these results by using the new waves of the Turkish Household Budget Survey will provide not only a better understanding of the evolution of the gender wage gap over time but also policy implications for gender equity.

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Appendix

Table 1: Labor Force Participation by Gender and Education (Urban)

Men

Year/Education	Illiterate	Literate/No Diploma	Elementary School	Secondary School or Equivalent	High School	College or More
2006	36,4	50,1	75,9	82,5	65,9	83,3
2005	39,6	49,2	76,6	82,3	66,5	84,2
2004	38,5	45,0	76,5	78,0	67,0	85,0
2003	41,5	34,6	75,5	69,9	61,3	82,4
2002	39,6	36,9	76,7	67,9	62,1	83,5
2001	41,6	42,2	77,5	65,9	64,7	83,1
2000	44,7	45,3	78,4	62,1	64,7	82,1

Women

Year/Education	Illiterate	Literate/No Diploma	Elementary School	Secondary School or Equivalent	High School	College or More
2006	5,6	10,1	13,3	20,4	27,6	69,8
2005	6,3	9,6	12,8	20,4	26,5	69,9
2004	5,7	8,7	12,1	18,6	25,9	71,1
2003	5,5	8,5	11,8	17,9	24,6	69,3
2002	5,8	7,9	12,7	16,6	27,9	70,6
2001	5,4	7,8	11,5	14,0	26,1	70,2
2000	5,1	7,9	10,4	13,6	27,5	69,6

Source : SIS

Table 2: Summary Statistics

	Men	Working women	Nonworking Women
Variable	**Mean**	**Mean**	**Mean**
Log hourly wage	14.86	14.77	-
Age	37.35	35.69	38.78
Married	0.91	0.64	0.88
Education (No diploma)	0.03	0.05	0.19
Education (Elementary school)	0.39	0.28	0.54
Education (Secondary school)	0.12	0.08	0.07
Education (High School or equivalent)	0.34	0.38	0.17
Education (College or more)	0.12	0.21	0.02
Household with 2 child.<18 years old	0.26	0.18	0.19
Household with 3 or more child.<18 years old	0.14	0.04	0.13
Household head	0.86	0.11	0.06
Homeownership	0.56	0.59	0.66
Log Nonlabor Income	12.55	10.05	2.88
Log Other household Income	21.72	22.92	23.09
Obs	2742	549	4401

Table 3: Quantile Regressions for Women - Not corrected for selection

Percentile	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Age	0.130**	0.114*	0.073	0.089**	0.071**	0.056+	0.061**	0.051	0.030
	[0.057]	[0.034]	[0.048]	[0.036]	[0.033]	[0.030]	[0.028]	[0.037]	[0.036]
Age squared	-0.002**	-0.001*	-0.001	-0.001**	-0.001+	-0.001	-0.001+	-0.001	0.000
	[0.001]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]
Education									
Secondary school	0.503*	0.357*	0.367**	0.324*	0.240**	0.122	0.139	0.227**	0.394*
	[0.163]	[0.101]	[0.143]	[0.108]	[0.099]	[0.092]	[0.085]	[0.111]	[0.115]
High School or equivalent	0.862*	0.750*	0.666*	0.769*	0.791*	0.775*	0.757*	0.815*	0.844*
	[0.112]	[0.063]	[0.089]	[0.066]	[0.061]	[0.058]	[0.055]	[0.073]	[0.076]
College or more	1.354*	1.257*	1.170*	1.208*	1.189*	1.106*	1.052*	1.163*	1.446*
	[0.126]	[0.073]	[0.104]	[0.078]	[0.071]	[0.067]	[0.063]	[0.084]	[0.085]
Constant	11.119*	11.448*	12.500*	12.270*	12.718*	13.109*	13.187*	13.473*	14.132*
	[1.041]	[0.617]	[0.875]	[0.651]	[0.598]	[0.551]	[0.509]	[0.679]	[0.663]

Quantile Regressions for Men - Not corrected for selection

Percentile	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Age	0.173*	0.157*	0.156*	0.148*	0.144*	0.130*	0.122*	0.114*	0.114*
	[0.021]	[0.015]	[0.018]	[0.014]	[0.014]	[0.014]	[0.014]	[0.015]	[0.023]
Age squared	-0.002*	-0.002*	-0.002*	-0.002*	-0.002*	-0.001*	-0.001*	-0.001*	-0.001*
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Education									
Secondary school	0.210*	0.149*	0.107**	0.137*	0.125*	0.104*	0.120*	0.130*	0.045
	[0.055]	[0.039]	[0.048]	[0.038]	[0.038]	[0.038]	[0.037]	[0.042]	[0.061]
High School or equivalent	0.495*	0.483*	0.501*	0.519*	0.515*	0.496*	0.466*	0.443*	0.367*
	[0.038]	[0.027]	[0.034]	[0.027]	[0.027]	[0.027]	[0.027]	[0.030]	[0.043]
College or more	1.080*	1.006*	0.947*	0.954*	0.885*	0.844*	0.823*	0.844*	0.843*
	[0.055]	[0.039]	[0.049]	[0.039]	[0.038]	[0.039]	[0.038]	[0.042]	[0.062]
Constant	10.583*	11.073*	11.277*	11.515*	11.669*	12.051*	12.288*	12.561*	12.767*
	[0.395]	[0.280]	[0.344]	[0.268]	[0.264]	[0.265]	[0.259]	[0.288]	[0.425]

Standard errors in brackets. + significant at 10%; ** significant at 5%; * significant at 1%

Table 4: Estimating the Probability of Work

	Probit		Single Index	
	Coefficients	Std. Err.	Coefficients	Std. Err.
Age	0.27	0.04	0.27	-
Age squared	0.00	0.00	0.00	0.00
Married	-0.49	0.08	-0.11	0.03
Secondary school	0.39	0.10	0.60	0.07
High School or equivalent	0.73	0.07	0.20	0.03
College or more	1.49	0.11	0.37	0.07
Household with 2 child.<18 years old	-0.54	0.08	-0.03	0.03
Household with 3 or more child.<18 years old	-0.18	0.11	-0.13	0.04
Household head	-0.12	0.06	-0.02	0.03
Homeownership	-0.66	0.12	-1.08	0.09
Log Nonlabor Income	0.04	0.00	0.02	0.00
Log Other household Income	-0.37	0.04	-0.07	0.02
Constant	2.89	1.18	2.89	-
Hausman test	625.1			

Table 5: Quantile Regressions for Women - Corrected for selection

Percentile	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Age	-0.028	-0.051	-0.057	-0.027	-0.060	-0.005	0.051	0.019	0.076
	[0.080]	[0.072]	[0.070]	[0.068]	[0.054]	[0.057]	[0.051]	[0.065]	[0.091]
Age squared	0.000	0.000	0.000	0.000	0.000	0.000	-0.001	0.000	-0.001
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Education									
Secondary school	0.318	0.221	0.124	0.162	0.115	0.075	0.133	0.191	0.392**
	[0.182]	[0.156]	[0.151]	[0.139]	[0.110]	[0.114]	[0.101]	[0.131]	[0.167]
High School or equivalent	0.760*	0.622*	0.599*	0.654*	0.688*	0.716*	0.757	0.791	0.846
	[0.111]	[0.096]	[0.092]	[0.085]	[0.067]	[0.070]	[0.063]	[0.082]	[0.102]
College or more	1.117*	1.067*	1.008*	1.047*	1.029*	1.037*	1.048*	1.135*	1.445*
	[0.135]	[0.111]	[0.107]	[0.102]	[0.083]	[0.088]	[0.079]	[0.104]	[0.131]
Inverse Mills Ratio	-0.69*	-0.677*	-0.714*	-0.554**	-0.619**	-0.314	-0.039	-0.109	0.168
	[0.279]	[0.288]	[0.280]	[0.268]	[0.220]	[0.233]	[0.209]	[0.266]	[0.367]
Constant	13.951*	14.272*	14.558*	13.897*	14.566*	13.093*	11.811*	12.559*	11.271*
	[1.872]	[1.842]	[1.791]	[1.751]	[1.423]	[1.494]	[1.348]	[1.728]	[2.433]

Standard errors in brackets. + significant at 10%; ** significant at 5%; * significant at 1%

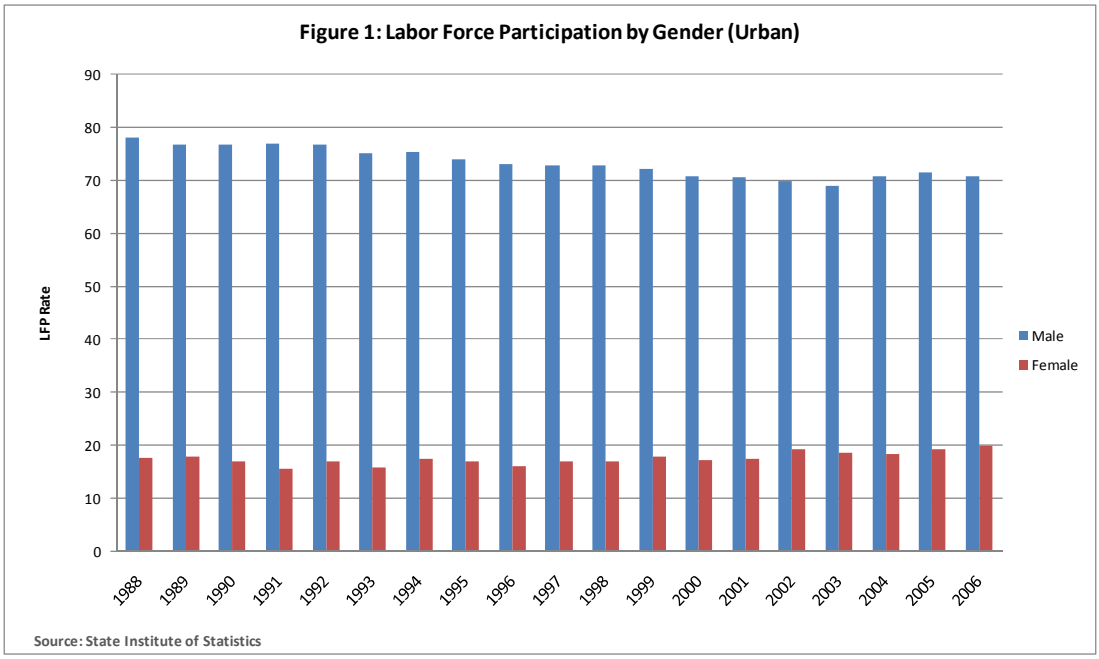


Figure 1: Labor Force Participation by Gender (Urban)
LFP Rate
90
80
70
60
50
40
30
20
10
0
1988
1989
1990
1991
1992
1993
1994
1995
1996
1997
1998
1999
2000
2001
2002
2003
2004
2005
2006
Male
Female
Source: State Institute of Statistics

Figure 2: Raw Gender Wage Gaps

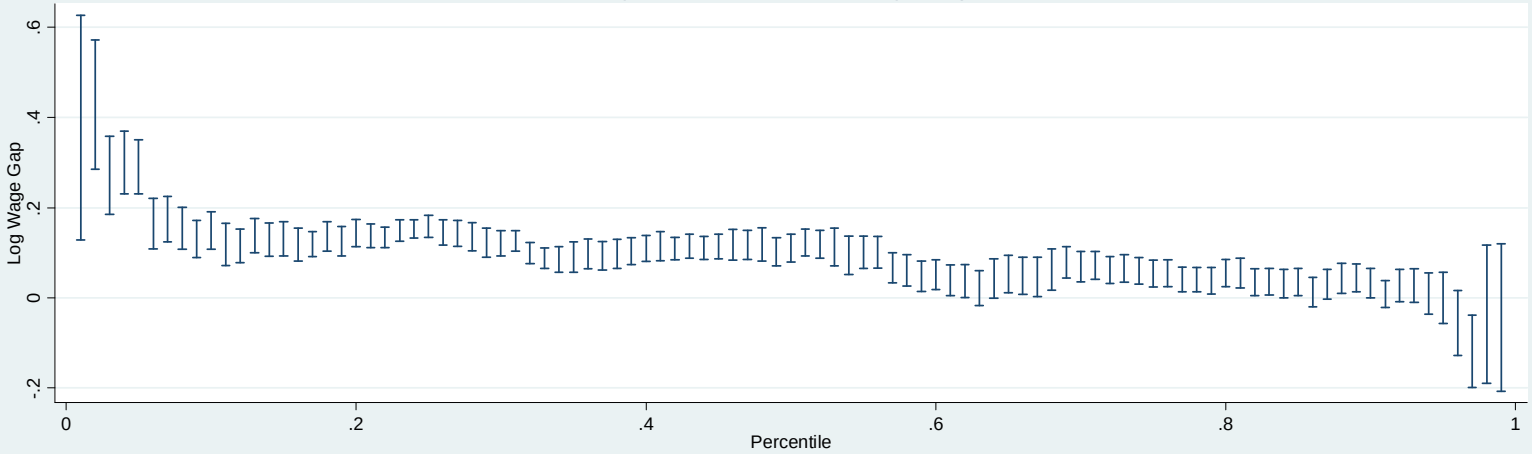


Figure 3: Log Wage Gaps if Women Had Men's Characteristics and Received Their Own Returns

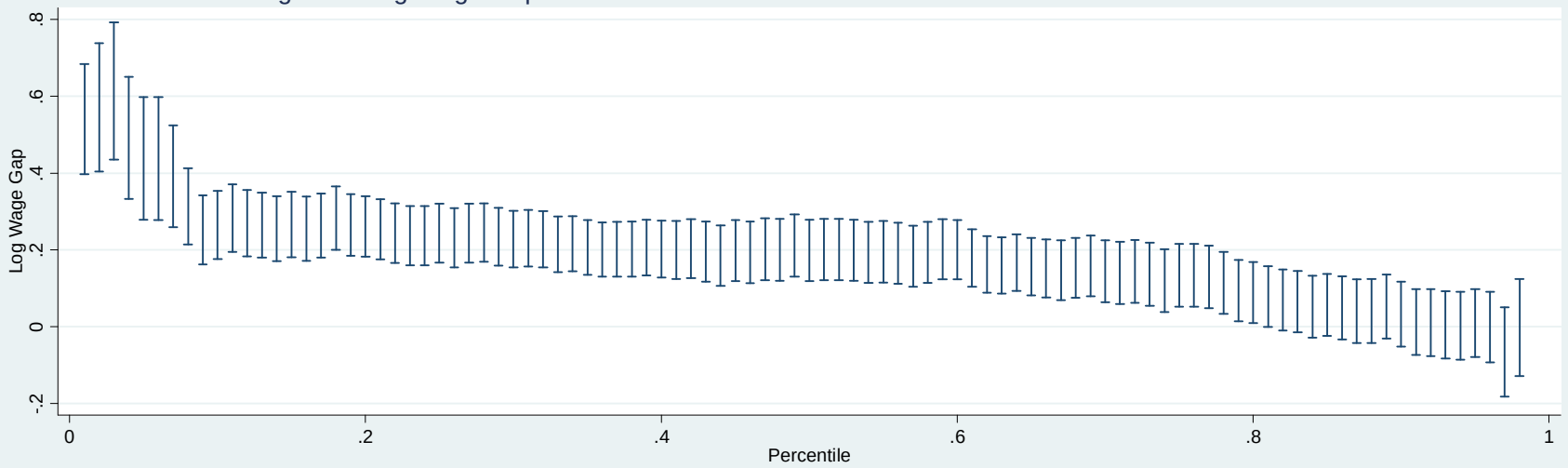


Figure 4: Log Wage Gaps if Women Retained Their Own Characteristics and Received Men's Returns

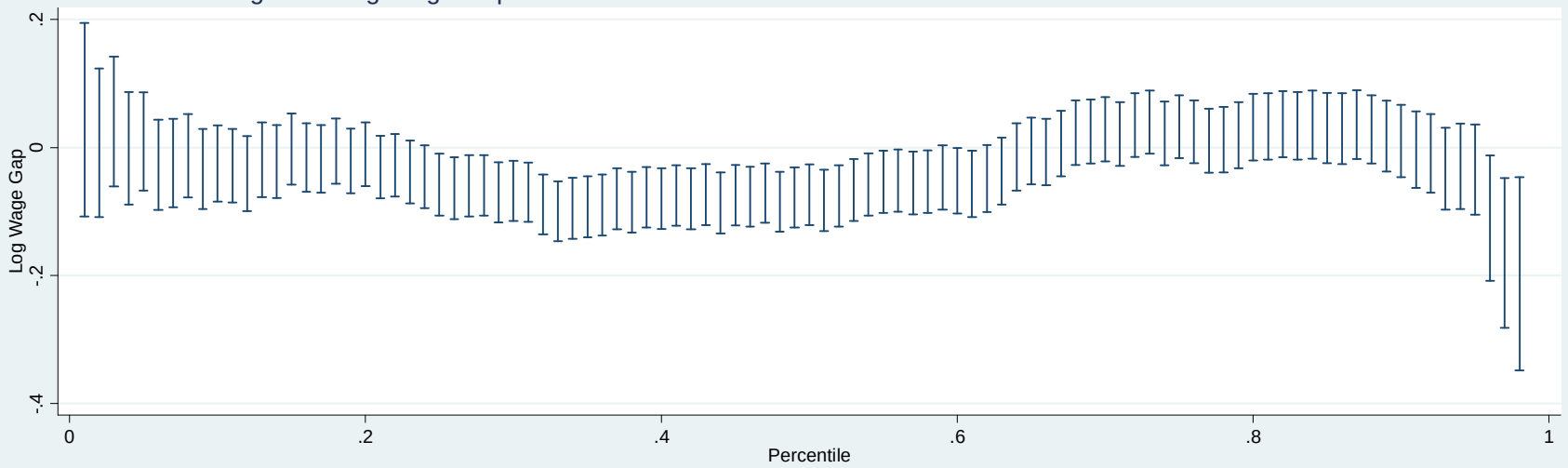


Figure 5: Log Wage Gap Between Women's Wages Before and After Selection

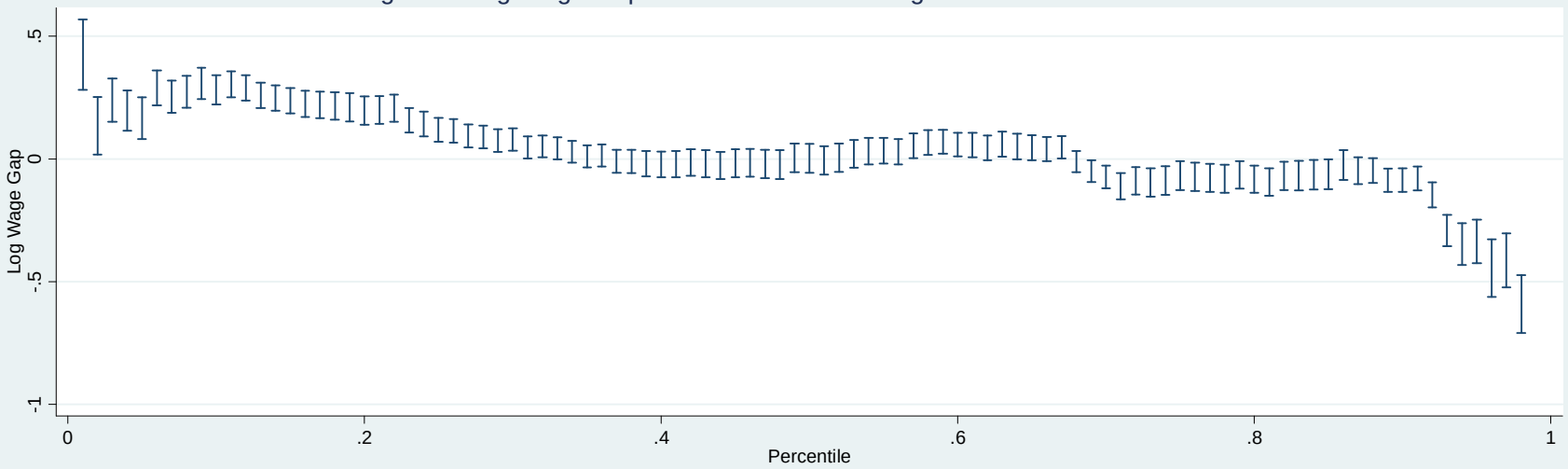


Figure 6: Log Wage Gap Between Men and Women Corrected for Selection

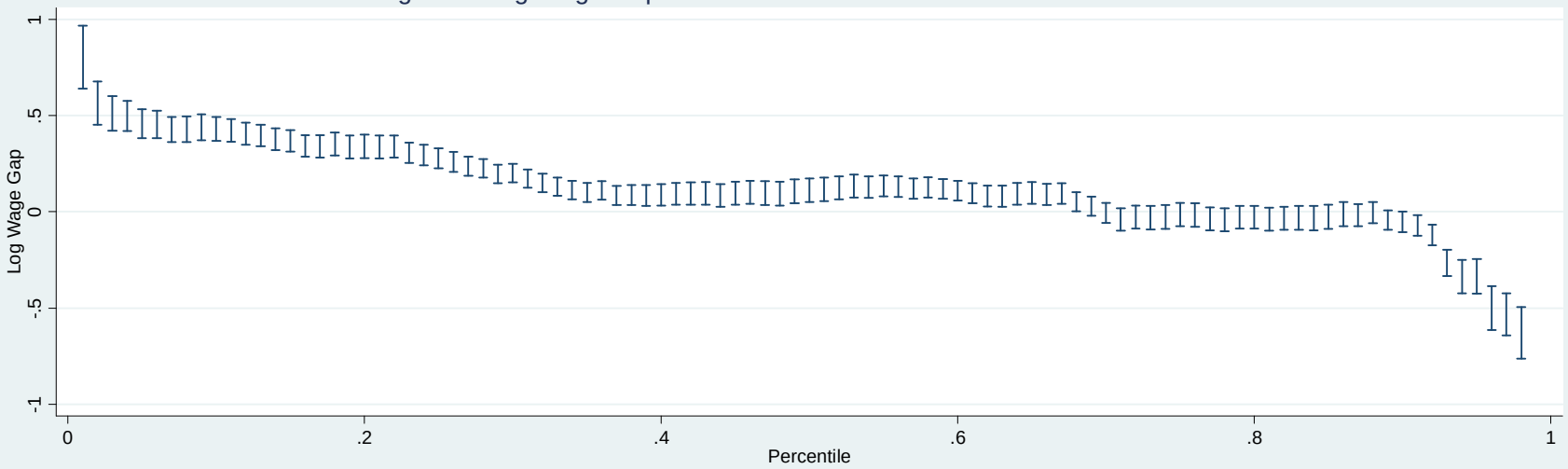


Figure 7: Sample Selection Based on Observables

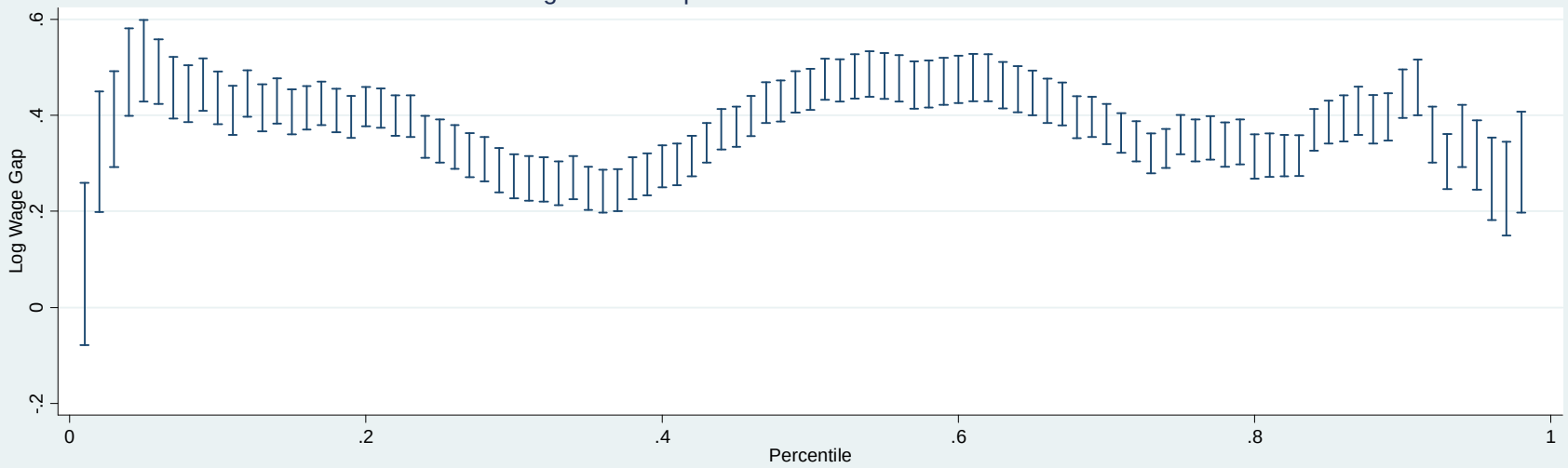


Figure 8: Sample Selection Based on Unobservables

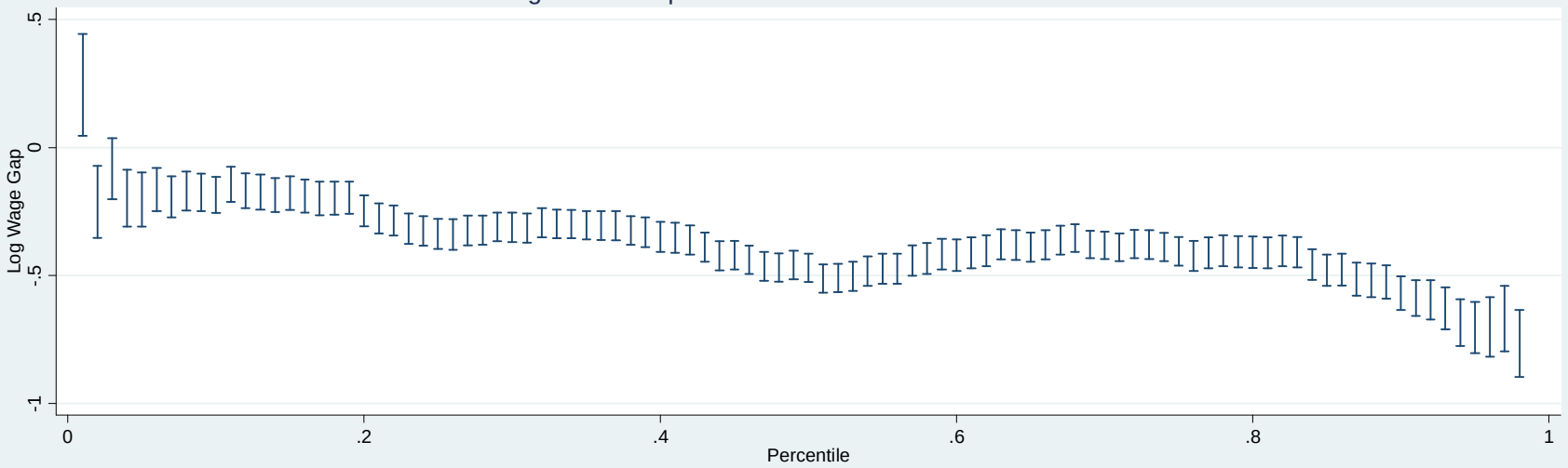
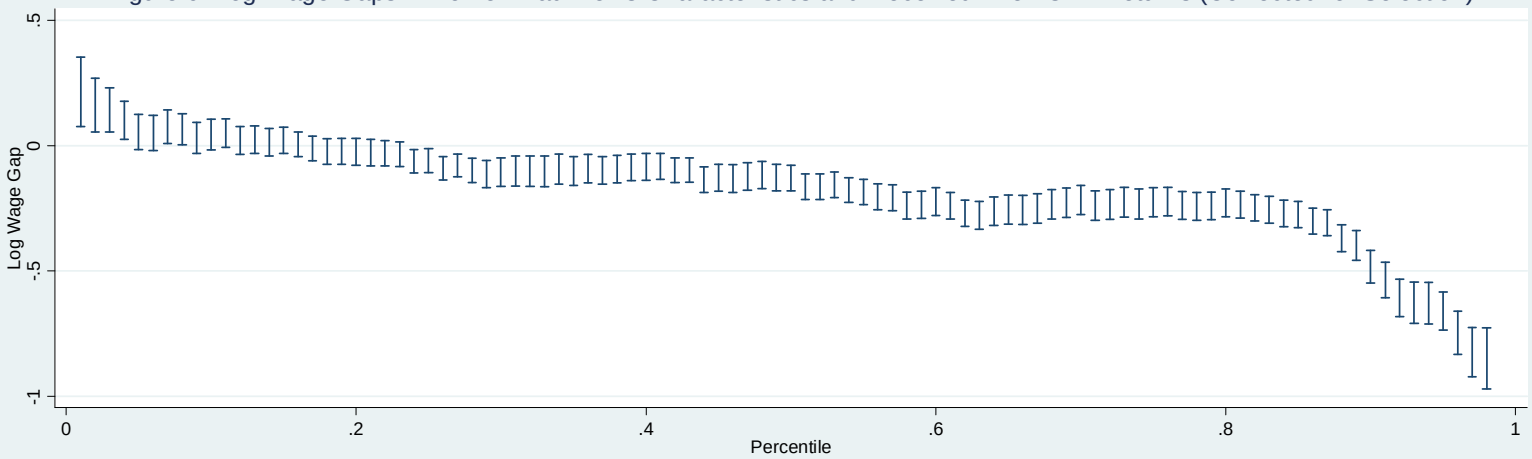


Figure 9: Log Wage Gaps if Women Had Men's Characteristics and Received Their Own Returns (Corrected for Selection)



Chapter 2: Employment Protection and Firm Size: Micro Evidence from Developing Countries

1. Introduction

Job security regulations (firing procedures and firing costs) are usually put in place to protect job stability and benefit those workers ("insiders") that already have a job. They discourage firms from adjusting their workforce in the presence of an adverse economic shock. However, job security regulations are also likely to alter the hiring decisions of firms. In expansionary times, firms hire fewer workers because they take into account that these workers may have to be laid off in the future, which in turn will be more costly. Therefore, while they unambiguously decrease both hiring and firing rates at the firm level, reducing worker and job flows, the net impact on aggregate employment is undetermined.[1] The net effect depends on

[1] Albrecht, Navarro and Vroman (2009) model a labor market with unemployment generated by search. By extending the Mortenson-Pissarides model with endogenous job destruction to allow for an informal sector and continuous worker types, they show that an increase in a severance tax increases employment duration in the formal sector and decreases labor productivity. The reason is that

whether the negative effect on layoffs is more than offset by the reduction in hiring rates. It is possible that overly strict labor laws hamper job creation and job reallocation, eventually hurting labor productivity. This paper studies the effect of strict firing regulations on firm size and workforce composition in developing countries. We explore micro data for 70 developing countries and cross country variation in the enforcement of firing regulations. We focus on the advance notice requirements of EPL instead of severance payments.

As explained in Boeri, Helppie and Macis (2008), " EPL is a multidimensional institution, including severance payments, advance notice, and other procedural requirements established by law or collective agreements that limit the ability of firms to lay off their employees. In spite of the multidimensionality of EPL, its various dimensions can be conceptually divided into two components. The first component can be seen as a monetary transfer from the employer to the worker (e.g., severance payments); while the second can be modeled as a tax in that it corresponds to a payment to a third party, outside the employment relationship (e.g., litigation and other procedural costs). This distinction is important because the two

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severance taxes make job destruction costly and hence relatively unproductive matches are sustained. By making the labor market more stagnant it also reduces the rate at which workers find formal-sector jobs. Kugler and Pica (2008), Autor, Kerr and Kugler (2007) and Kugler (1999) document evidence that firing costs decrease the rate of entry into and exit out of unemployment and therefore slow down job flows.

components have different predicted effects on the employment choices of firms and hence on labor market outcomes."

In the context of developed countries, earlier research tried to quantify the impact of EPL by exploring the cross country time series variation in labor regulation (Lazear, 1990, Nickel, et al., 1999). These studies typically relied on the cross-country variation in *de jure* labor market regulation. However, identification based on the cross-country time series variation in *de jure* regulation has well-known shortcomings such as omitted variables, measurement error and the difficulty of determining the direction of causality (Kugler, 2007, Micco and Pages, 2006). In developing countries there is a large gap between the written law and its effective implementation. Labor markets are usually characterized by weak enforcement of the law and a large informal sector. For this reason, it is important to take into account the degree of enforcement of the law while trying to identify the effect of regulations on firm outcomes.[2] In this paper, we compare firm size and workforce composition for firms facing, all else constant, different degrees of enforcement of labor regulations. Exploring variation in enforcement in developing countries is

[2] Although much of the literature on the effects of labor regulations has not taken into consideration enforcement, some exceptions include Boeri and Jimeno, (2005), Caballero, Cowan, Engel and Micco, (2004).

conceptually closer to comparing countries with different degrees of *de facto* regulations (Almeida and Carneiro, 2005, 2006, 2007 and 2008).[3]

Related to our paper is also the empirical literature exploring the within country variation in the exposure of firms to different types of labor regulations.[4] In some countries (like Italy or Spain) firms smaller than a given size threshold are not required to comply with all labor regulations. This exemption provides discontinuity in the effects of regulations within countries and, under certain conditions, the comparison between these two groups of firms can be informative of the causal effects of the regulations on labor market outcomes. For example, Abidoye, Orazem and Vodopivec (2008), Schivardi and Torrini (2005) and Boeri and Jimeno (2003) document that workers who are employed at firms that are below the threshold scale and hence exempt from severance payments are likelier to be laid off while the same does not hold for temporary workers A reduction in job security provisions is also

[3] Almeida and Carneiro (2008) look at how enforcement of labor regulation affects firm size and other firm characteristics exploring Enterprise Surveys in Brazil. They proxy enforcement of regulation with the number of labor inspections at the city level. Their findings show that stricter enforcement of labor law constrain firm size and lead to reduced use of informal labor.

[4] A related strand of the literature explores within country time series variation in labor regulations in developing countries (including job protection laws). For example, Besley and Burgess (2004) and Ahsan and Pages (2007) explore time variation at the state-level in India and find that stricter pro-worker labor regulation has a negative impact on state aggregate employment. (Industry level data).

found to decrease the likelihood of employment for female and unskilled workers relative to male and skilled ones (Montenegro and Pages, 2003).

This paper investigates the link between firing regulations and firm size and skill composition at the firm level. We explore a large firm level dataset across several developing countries and compare firm size for firms facing different degrees of enforcement of firing regulations. Our main sources of data are the *Enterprise Surveys*, collected by the World Bank, which collect firm-level data across more than 70 developing countries. This data is particularly useful to analyze this topic because it contains detailed information on labor force characteristics and on enforcement of different types of regulations (including labor regulations).

Our empirical approach is close to a difference-in-difference approach, as employed by Rajan and Zingales (1998).[5] We compare firms with similar observable characteristics (e.g., age and ownership type), within the same country and operating in the same sector, and differing in the degree of enforcement of labor regulations

[5] Rajan and Zingales (1998) ask whether industries that require higher levels of external financing develop relatively faster in countries with more developed financial markets. They develop a difference-in difference approach using the interaction of a country's level of financial development and an industry's degree of dependence of external financing as their variable of interest. They use the level of external financing in the US as a benchmark of an industry's external financing dependency. They find robust evidence that industries with relatively higher external financing needs grow disproportionately faster in countries with better developed financial markets.

they face. We also perform several checks to analyze the robustness of our results. To account for the unobserved country characteristics (e.g., the level of development in the country) that are likely to be correlated with *de facto* regulations and firm size we control for country fixed effects. By controlling for firm level characteristics (e.g., age, ownership, location) we account for the differences across firms that could be correlated with firm size and the strictness of enforcement. We also control for the degree of enforcement of labor law faced by each firm. This allows us to control for unobservable firm characteristics, simultaneously correlated with the enforcement of the law and with firm size, in countries with different degrees of the law.

Our prior is that firing regulations (and their costs) will be more binding when firms face stricter enforcement of laws. We follow Almeida and Carneiro (2007) and measure enforcement of labor regulations with the number of visits done by labor inspections in each firm, after controlling for total number of inspections.

Our findings show that firms that are subject to stricter enforcement of labor regulations (90^{th} percentile of enforcement) in a country with rigid employment protection laws (at the 90^{th} percentile of the difficulty of firing index, e.g., in Portugal) are 18 percent smaller than firms subject to looser enforcement in a country with less rigid regulations (10^{th} percentile of the difficulty of firing index, e.g., in Bulgaria). This reduction in firm size tends to be larger in manufacturing and, especially in low technology sectors, with higher labor intensity. We also find that

enforcement has a positive and significant impact on the share of skilled workforce. It is interesting and reassuring that our results do not hold in countries which are characterized by weak rule of law.

Empirical evidence has also shown that strict employment protection affects employment composition (Montenegro and Pages, (2003)). Employment protection reduces the employment rates of the youth and the unskilled at the benefit of older and skilled workers. In addition, job security is also found to shift the distribution of employment against the most disadvantaged, namely women and the youth (Heckman and Pages, 2004).

The differential impact of EPL on different types of workers may operate through several channels. First, when dismissing a worker is costly, firms may switch to more labor-saving technology by investing more in capital and hence increase the productivity of each remaining worker (capital deepening). Firms may also employ more high-skilled workers who are better able to work with the capital intensive equipment which further impacts the composition of the workforce (skill deepening) (Autor, Kerr and Kugler, 2007). Second, due to the anticipation of long tenure on the job (since the firm will be more reluctant to dismiss the worker in the presence of firing costs, the job duration is longer), firms may choose to hire outstanding employees and further invest in training programs to increase worker productivity (Almeida and Aterido, 2008, Wasmer, 2006). Third, according to the insider-outsider literature, since their jobs are secured workers who are already

employed may be less willing to accept wage cuts that are necessary to bring down unemployment. This will create two types of workers: insiders, who are employed workers and outsiders, who are unemployed or who hold temporary jobs that are not covered by job security. Finally, if low skilled workers have more elastic supply of labor than skilled, then a drop in labor demand would cause a greater fall in low skilled employment than high skilled employment.

Our findings bear important policy implications in the sense that stricter EPL constrain firm size. This will lead to a lower employment rate unless smaller firm size is not compensated by increased entry of firms. Besides, to the extent that economic growth is driven by growth of existing firms (rather than by the creation of new firms), the effect of strict firing regulations on firm size matters for economic growth, (Rajan and Zingales, 1998). Furthermore, if high levels of labor regulations and extensive informality are associated with lower levels of economic growth (as documented in Loayza, Oviedo and Serven 2006) and if the quality of institutional environment acts as a balancing factor in neutralizing the negative impact of regulations on growth, unduly restrictive regulations in developing countries will have further implications on growth. In addition to its impact on the level of employment, we document that the impact of EPL is not neutral on different types of workers. This in turn affects the income distribution and hence inequality.

Taken together the findings of this paper should not be interpreted as suggesting eliminating regulations altogether, since a high quality institutional

environment is indispensable for enhancing growth and development. However, in the case of developing countries which are characterized by high levels of informality, a possible recommendation would be to promote more flexible labor laws rather than lifting the enforcement of regulations. [6]

The paper is organized as follows. Section 2 describes the datasets we use and gives summary statistics. Section 3 explains the empirical methodology. Section 4 reports the main findings followed by the sensitivity analyses we perform to ensure the robustness of our results. Section 5 concludes.

2. Data and Descriptive Statistics

The main dataset that we use is the Enterprise Survey, collected by the World Bank, across more than 70 developing countries. [7] The surveys were conducted between 2002 and 2005 and the samples were designed to be representative of the

[6] The average of share of informal employment in total employment estimated by Schneider is 35 percent. We do not have information on the number of informal workers employed by the firms in our sample.

[7] The Enterprise Surveys are now available for more than 78 developing countries. We use data for only 70 countries due to the lack of information on the main variables of interest. The information collected in these surveys is usually based on a 1.5-2 hours interview with the firm manager. This dataset has been used in other studies . (e.g., Svensson, 2003, Almeida and Carneiro, 2005, 2006, Pierre and Scarpetta, 2004, and Aterido, Hallward-Dreimeier and Pages, 2007, Almeida and Aterido, 2008)

population of firms according to their industry and location within each country. In almost all countries the sampling frame restricted the attention to formal sector firms so that our findings refer mostly to the formal sector and to registered employment.

Although in most countries only one wave of data is available, the information available in the survey has several advantages for analyzing this topic. First, it is based on a common questionnaire across a large set of countries, which yields comparable information on several firm-level variables. Among other things, the survey collects detailed information on labor market variables, like firm size and workforce composition and other detailed firm characteristics (e.g., firm ownership structure, age, exports and imports). Our final sample covers more than 30,000 firms distributed across a wide set of manufacturing industries - auto and auto components, beverages, chemicals, electronics, food, garments, leather, metals and machinery, non-metallic and plastic materials, paper, textiles, wood and furniture – covering Africa, Asia, Eastern Europe, and Latin America.

Second, the survey collects detailed information on the enforcement of different pieces of regulation, including labor market regulations. In particular, we take advantage of the richness of the data set to construct alternative measures of enforcement of regulation: number of inspections by labor authorities (and others), perceived stringency of regulation, perceived consistency and coherence in the

application of the law and efficiency of the government and enforcement of property rights in the country.[8]

We also explore the Doing Business data set, also collected by the World Bank. This dataset includes detailed information on several regulatory features of the economies, including regulations of entry, labor and product markets. Of special interest to us is the information related to the rigidity in firing and firing costs. In particular, the dataset includes information on difficulty and expense of dismissing a redundant worker (Difficulty of Firing), and the cost of a redundant worker, expressed in weeks of wages (Firing Costs) and the non-wage labor cost (captured as a percentage of wages). The difficulty of firing index has different components: (i) whether redundancy is disallowed as a basis for terminating workers; (ii) whether the employer needs to notify a third party (e.g., government agency) to terminate a redundant worker; (iii) whether the employer needs to notify a third party to

[8] In particular, firms were asked: How would you evaluate the overall efficiency of government in delivering services (e.g. public utilities, public transportation, security, education and health etc)

(1) Very inefficient (2) Inefficient (3) Somewhat inefficient, (4) Somewhat efficient (5) Efficient (6) Very efficient.

Firms were also asked: "In general government officials' interpretations of regulations affecting my establishment are consistent and predictable." To what extent do you agree with this statement?

About property rights, firms were asked "I am confident that the judicial system will enforce my contractual and property rights".

terminate a group of 25 redundant workers; (iv) whether the employer needs approval from a third party to terminate a redundant worker; (v) whether the employer needs approval from a third party to terminate a group of redundant workers; (vi) whether the law requires the employer to reconsider reassignment or retraining options before redundancy termination; (vii) whether priority rules apply for redundancies; and (viii) whether priority rules apply for reemployment. The index is calculated as one point for each of the above requirements and is scaled up to 100. Therefore, higher values of the index indicate more rigid firing regulations. In our sample, the top 5 least restrictive countries in terms of firing regulations are Brazil, Guatemala, Nicaragua and Honduras which score zero in the rigidity of firing index. This implies that none of the above requirements are in place for those countries. In contrast, Georgia and Cameroon with a score of 70 out of 80 are the two countries that rank the highest on the difficulty of firing index (See Figure 3).

We also use GDP per capita (in 2000 dollars) of the countries in our sample to proxy for the level of development. To control for the level of development of the governments and corruption levels we use three governance indicators. The three governance indicators are measured in units ranging from about -2.5 to 2.5 with higher values corresponding to better governance outcomes. Rule of Law is the extent to which agents have confidence in and abide by the rules of the society, and in particular the quality of contract enforcement, the police, and the courts as well as the likelihood of crime and violence; Control of Corruption is the extent to which

public power is exercised for private gain. Including both petty and grand forms of corruption, as well as "capture' of the state by elites and private interests; Regulatory Quality is the ability of the government to formulate and implement sound policies and regulations that permit and promote private sector development.

To control for the entry regulations in product markets, we include the number of procedures required to start a business from the Doing Business dataset.

Our main outcomes of interest are total firm employment (firm size) and the share of skilled workers. We also use age export status, dummies for government ownership, share of foreign ownership and the city where the firm is located. Table A1 defines the variables used in our analysis. Our final sample has 30351 firms in 70 countries, although the number of observations with a complete set of variables varies with specifications. Appendix A2 provides more details about the geographical composition of our sample.

Table 1 presents the descriptive statistics for our main variables. The average firm in our sample employs 212 workers 13 percent of whom are temporary. About 62 percent of the average firm's workforce is comprised of professionals and skilled workers. Eighty nine percent of the firms in our sample are less than 50 percent foreign-owned and 8 percent are owned by the state.

Following Almeida and Carneiro (2007), we proxy enforcement by the number of visits by labor officials. Figures 1 and 2 display the average of the days of

labor inspections by firm size across regions, income levels and sector of activity. As expected, enforcement increases with firm size across regions, income levels and sectors since larger firms are more visible to the inspectors. Figure 3 plots the link between rigidities in firing and the strictness of enforcement at the country level. The difficulty of firing and the strength of enforcement of labor regulations do not seem to be correlated.

3. Empirical Approach

We examine whether the stringency of employment protection (captured by the rigidity in firing) in developing countries affects total firm size and workforce composition (share of skilled workers) by exploring one possible channel for such a relationship: that a stricter enforcement of employment protection laws increases the cost to firms of adjusting labor. The simple reduced form model we use relates the degree of enforcement of labor regulations faced by each firm with the outcomes of interest. Let Y_{jc} denote the main outcomes of interest in firm j in country c (e.g., total employment in the firm and the skill composition of the labor force):

$$Y_{jc} = \beta R_c * E_{jc} + \chi E_{jc} + \delta Z_{jc} + \eta_c + \mu_s + \varepsilon_{jc}$$

where R is a measure of the de jure labor regulation in country c, E is a measure of the enforcement of the labor laws in firm j in country c, Z captures firm level pre-determined characteristics of interest of firm j located in country c and η_c and μ_s are time invariant fixed effects capturing country and sector unobserved characteristics. The country dummies capture country level unobserved institutional characteristics and policies which could be simultaneously correlated with firm size and skill composition of the workforce and with the strictness of labor regulations in the field (captured by the interaction term R*E). In Z we include a set of pre-determined firm characteristics, among others, age of the firm and ownership characteristics (public or foreign capital) and export status.

Our main coefficient of interest is β. Take for example the case where Y is the logarithm of total employment in the firm. The point estimates for β quantify the percentage point difference in logarithm of firm size for a firm facing the 90^{th} percentile of the enforcement of the labor regulation relative to a firm facing the 10^{th} percentile in the enforcement in a country with rigid employment protection (e.g., in the 90^{th} percentile of the rigidity in firing index) versus the same differential in a country with looser labor regulations (e.g., in the 10^{th} percentile of the rigidity in firing index). This difference is computed assuming that firms operate in the same sector and have the same set of observable characteristics captured in Z_j. Our approach assumes that the degree of enforcement of the labor law is a key factor when identifying the effects of regulation, since in developing countries there is a

large gap between the de facto regulation and its effective implementation. In particular, it is possible that the effects of employment protection are more visible for an economy with a moderate code rather than for an economy with a stronger code, as long as the former has a stricter enforcement of the law.

This empirical approach is similar in spirit to a "differences-in-differences" approach as implemented by Rajan and Zingales (1998) (see also Micco-Pagés, 2006).[9] We look at whether EPL affects labor market outcomes by examining one channel for such a relation: that EPL will have different effects depending on the degree of enforcement of the law. In particular, we ask whether firms for which enforcement is stricter have smaller size (or a higher share of skilled workers) in countries where de jure labor regulations are stronger. It is worth stressing that we are not exploring the variation in enforcement of the labor law alone, which is more likely to be endogenous to the firm outcomes. In particular, we have seen in figures 1 and 2 that enforcement of the labor law is stricter for the more visible and larger firms. We will be assuming that firm outcomes will be different depending on the degree of enforcement. This is identified by the differential effect of enforcement in countries with different rigidities in the labor law, after controlling for country-sector

[9] Following the idea of Rajan and Zingales (1998), Micco and Pages (2006) use industry level data from a sample of developed and developing countries to examine the differential effects of EPL on job turnover. In line with their hypothesis, they find that more stringent EPL slows down job turnover, and this effect is stronger in sectors that are intrinsically more volatile.

fixed effects, firm characteristics and the enforcement of labor (and also of other types of regulations). By controlling for country level fixed effects we are able to account for unobserved country characteristics that are likely to be correlated with de facto labor regulations and with firm size (e.g., level of development in the country). Finally, controlling for the degree of enforcement of labor regulations, we account for (unobservable) firm characteristics that could be simultaneously correlated with the enforcement and with firm size in countries with different degrees of rigidities in the labor law.

It is worth stressing that the empirical methodology that we explore is different from a pure cross-country approach, which explores simply the cross-country variation in the de jure regulation. With aggregate cross country data, and in the absence of any time series variation, it is not possible to account for country time invariant effects when the labor regulation does not vary at the regional level (as it is the case in most developing countries). This raises serious concerns regarding potential country level omitted variables, such as institutions or policies, which are likely to affect firm size or workforce composition. Moreover, it is likely that countries with larger firm sizes (and possibly lower employment creation) could demand stricter levels of EPL. This would make it hard to determine the direction of causality when exploring only cross-country variation in de jure regulation. Our methodology explores the interaction term between the degree of enforcement faced by each firm and the de jure EPL as it is stated in each country's labor codes, after

controlling for the level of enforcement itself and by country fixed effects. This will allow us to overcome the omitted variables problem at the country level by exploring only within country variation. By controlling for the country fixed effects and the firm characteristics, we also hope to minimize the potential reverse causality problem (i.e., causality running from firm size to strictness of enforcement of labor regulations) that plagues most of the cross country work.

However, one could argue that the identification put forth in the specification above still presents some shortcomings. Take again the case where the variable of interest is total employment. The main worry in this case is whether enforcement of labor market regulations proxy for something else. In principle, firm size within a country might be determined by a long list of factors. However, our results will only be explained by these alternative factors if enforcement at the firm or regional level is correlated with the dependence of the firm on these other factors and if labor market regulations are a good proxy for these factors. We will discuss below some factors where this could be the case.

First, assume for example that enforcement of labor regulations is correlated with regional differences in the economic environment and that these affect the way the general level of development in a country affects firm size. In this case β would not be capturing the effect of labor regulations but rather the effect of the general level of development in a country. To test this hypothesis we will include the interaction of labor inspections with a general measure of the development in the

country (GDP pc, proxy for government control and rule of law). Applying a similar reasoning one could think that firm size is also affected by the institutional quality of the country or the quality of its regulatory environment. Again, to the extent that these are correlated with labor regulations and affect firm size differently depending on the degree of enforcement (in turn correlated with the regional level of regulatory quality), β would be biased. Also, labor regulations might correlate with entry regulations and the latter also affect firm size. Similarly, we will test the robustness of our findings to the inclusion of an interaction of labor inspections with a country measure of the ease to start a business.

Second, one could argue that, within each country, there are sector specific factors or characteristics determining firm size. For example, sectors subject to more product, health and/or security regulations could be, in general, more likely to evade the law and enforcement in these sectors could be stricter. To the extent that these sector regulations are correlated with labor regulations across countries, β could again be biased. To account for this, we will control for a country-sector fixed effect in the reduced form equation. As a consequence we will not be able to include country level fixed effects but we will include country characteristics that capture the degree of development in the country (e.g., GDP per capita). Similarly, there could be factors regional and sector level for which we account for region-sector fixed effects.

Third, it is possible that labor inspections are a poor proxy for the quality of the enforcement of labor market regulations. In particular, it is possible that one of the motives for labor inspections (as well as other types of inspections) is related to corruption and bribery. In this case, one would expect that firms where it is easier for inspectors to extract rents are also more likely to be inspected. Although one could observe that in these types of firms the number of inspections is larger, this would not necessarily translate into a stricter enforcement of the labor (or other) law. As a corollary, there would be a spurious (positive or negative) correlation between firm size (or with the other variables of interest) and labor inspections.[10] To minimize this problem, we are already controlling for several observable firm characteristics in Z, which we know are correlated with enforcement and also with firm size. Nevertheless, one could still argue that this is likely to be a problem as long as the quality of the variable labor inspections as a proxy for enforcement depends on the degree of development of the country (which could also be systematically correlated with the stringency of the labor law). To minimize this concern we will interact total number of inspections (related with labor but also other regulations) with de jure labor regulations (after including the variable also in levels). The inclusion of this variable is likely to minimize this concern as long as the probability of having an

[10] In this case, firms may be subject to labor inspections because they are not abiding by the labor law or it may be the case that labor inspections reflect a better quality regulatory environment and not unlawful activity on the part of the firms.

inspection in order to extract rents is not higher for the labor inspections than for other inspections (including health and safety inspections) in countries with different degrees of rigidity of labor laws. We also try to address this concern that enforcement is simply capturing the effect of bribes by including the interaction of labor regulations with firm level measures of evasion and bribes. In particular, we will explore the fact that firms report the share of management time that is spent dealing with government officials (although not necessarily labor inspectors) and on their own perception of the extent to which the degree of property rights is coherently enforced in the country. If inspections are simply capturing these, we would expect the point estimates for β to become insignificant.

Finally, there could still be a general concern regarding the endogeneity of $R_c * E_{jrc}$. Ideally, we would like to find an instrument that is simultaneously correlated with enforcement of labor regulation and is reasonably exogenous to the outcomes of interest. One possibility would be to follow Almeida and Carneiro (2006, 2007) and compute a measure of how costly it is to supply enforcement in each region.[11] Unfortunately, unlike in the case of Brazil, we do not know with detail the cities where each firm is located, and hence it would be difficult to find an analogous

[11] Almeida and Carneiro (2006 and 2008) use as instrument the average distance between the city where the firm is located and all the cities within the same state where a *subdelegacia* of the Ministry of Labor is located. Distances are measured in hours of travel by car, the type of transportation used by labor inspectors in Brazil.

instrument. We will test the robustness of our findings when we use the average number of labor inspections in the region-sector where the firm is located. This variable is likely to be positively and strongly correlated with the firm's own reported inspections and it minimizes the endogeneity concerns.

4. Enforcement of EPL Regulations and Firm Outcomes

4.1 EPL and Firm Size

Strict job security regulations, when enforced, increase the cost of firing and discourage firms from adjusting their workforce when facing adverse economic conditions. However, job security provisions might also alter the firm's hiring decisions. In good times, firms hire fewer workers because they take into account that these workers may have to be laid off in the future, and that is costly. Thus, the overall impact of job security regulations on firm size is likely to be undetermined as it depends on whether the negative effect on layoffs is offset by the reduction in hiring rates. Table 2 reports the main findings of estimating equation (1) when the dependent variable is firm size (proxied by number of permanent workers in the firm). In all specifications we control for a basic set of predetermined firm characteristics captured by X_{ijc}, (age, export status, dummies for foreign and government ownership) and for country sector fixed effects. Column (1) shows that, after controlling for country-fixed effects, firms that are exposed to a stricter enforcement of firing regulations are smaller in size in countries with more rigid firing regulations.

Since we are exploring the within country variation in the enforcement of regulations in countries with different degrees of stringency of regulations, we quantify the differential in the log of firm size by the using the point estimate in column (1). This coefficient is approximately (-0.0006) and remains stable across different specifications. The 90-10 differential is -0.21. This differential is interpreted as saying that a firm facing the 90^{th} percentile of the enforcement of labor regulation relative to a firm that is facing the 10^{th} percentile is 21 percent smaller in a country with rigid firing regulations (which is in the 90^{th} percentile of the Difficulty of Firing Index, e.g. in Portugal)) than in a country with looser regulations (10^{th} percentile, e.g., in Bulgaria). This differential is not large since the average log firm size in the sample is 3.7. This negative and significant correlation will maintain and remain robust to several tests, shown in columns (2) – (9). In column (2) we proceed by adding a set of firm level predetermined controls. In column (3) we check for the possibility that it is possible that inspections are a poor proxy for the quality of the enforcement, i.e. Inspectors may have rent extraction motives. Firms where it is easier to extract rents will have more inspections. This does not necessarily translate into stricter enforcement of the law and cause spurious correlation. To minimize this problem, we are already controlling for observable firm characteristics in Z, which we know are correlated with enforcement and also with firm size. The inclusion of this variable is likely to minimize this concern as long as the probability of having an inspection in order to extract rents is not higher for the labor inspections than for

other inspections (including health and safety inspections) in countries with different degrees of rigidity of labor laws.

In column (4) we check for the possibility that the stringency of EPL is simply capturing the degree of development in the country.[12] This is likely to be the case since it is well documented in Heckman and Pagés (2004) and Botero et al. (2004) that there is a negative correlation between the strictness of employment protection legislation and income levels. To address this we control for the interaction between enforcement of labor regulations at the firm level and the GDP per capita in each country. Our coefficient of interest hardly changes.

Along the same lines, we further test whether labor regulations are not capturing the effect of other country level institutions, omitted from the analysis but likely to be correlated with labor regulations. Therefore, we test our results to make sure if they are not driven by the cross country differences in the quality of governance. Throughout column (5) to column (7) we separately control for several measures of institutional quality. In particular, we control for an interaction term between labor inspections and rule of law, regulatory quality and government control of corruption and find that our results remain the same. The positive and significant coefficient of the interaction term emphasizes the positive impact of better quality

[12] Indeed, when we run a cross-country regression of the difficulty of firing index on per capita gdp, we get a significant and negative coefficient which is also robust to controlling for rigidity of employment and mean days of labor inspections.

institutions on firm size. Further, we investigate whether our results are capturing the impact of differences in entry regulations other than the effect of labor regulations. This is a useful exercise since Botero et al. document a positive correlation between the two. To account for this we control for the interaction between enforcement at the firm level and the number of procedures to start a business which is a measure of how costly it is to establish a business in a given country. Results in column (8) show that our results are the same. We also want to investigate by controlling for the rigidity of employment index which accounts for the rigidities in both hiring and firing regulations as well as the rigidities in work hours. Column (9) shows that our results remain robust.

Finally, it could be the case that firms in developing countries may differ in their propensity to bribe which in turn may be correlated with enforcement and firm size. To account for this, we proxy the propensity to bribe by the management's time spent dealing with officials which are not only labor officials but others as well. Additionally, the firm's perception of the enforcement of property rights as well as the consistency in the application of the regulations and the efficiency of government may be correlated with the firms' choice of size of its workforce and enforcement. Corresponding results are presented in columns (2) to (5) of Table 3. Although our main results about the enforcement of regulations remain qualitatively the same, we get useful insights from these checks. Our results suggest that firms that report spending more time with officials are smaller in size. Surprisingly, we also find that

the stronger the perceptions of an efficient government the smaller the firms are. We also find evidence that the stronger the perceptions are about the consistency of regulations and enforcement of property rights, the larger the firms are, which confirms the conduciveness of a favorable business environment.

4.2 Heterogeneity and Robustness of EPL and Firm Size

One of the main shortcomings of this analysis is that enforcement at the firm level could be endogenous. In other words, the days of inspections by the labor inspectors are not random. One way to address this problem is to find an instrument which is simultaneously correlated with enforcement of labor regulation and exogenous to the outcome of interest. An ideal way to do this is to compute a measure of how costly it is to enforce the regulations in each city. Unfortunately, unlike in the case of Brazil, we do not know with detail the cities in which each firm and labor offices are located, and hence it would be difficult to find an analogous instrument. To overcome this, we use the average number of labor inspections in the region-sector where the firm is located. The results are presented in column (1) of Table 3. Although the magnitude of our coefficient of interest changes substantially, it remains negative and significant.

One might argue that firms may differ in their needs of labor adjustment depending on the degree of the sophistication of their technology. In order to analyze firms with different levels of technology, we restrict our sample only to manufacturing firms. Column (1) in Table 4 displays the results when we repeat the

analysis for the manufacturing firms. In columns (2) and (3) we run the regression separately for low-tech and high-tech firms.[13] Although our main results do not change, the impact of enforcement on firm size is more pronounced in low-tech firms than in high-tech ones. These findings accord well with the idea in Micco and Pagés (2006) that low-tech firms which are relatively more dependent on labor are affected more by labor regulations.

An additional concern is that our results may be driven by the sorting of firms which implies that firms locate based on the strictness of enforcement. Considering foreign firms are more mobile in their location decisions, we further restrict our sample to domestic firms. Results presented in Column (4) suggest that the effect of enforcement on firm size does not capture the sorting of firm to locations where enforcement is looser.

One might argue that labor law does not apply to firms below a given size. Abidoye et. al (2008), Schivardi and Torrini (2005) and Boeri and Jimeno (2003) study the impact of firing regulations in countries where labor regulations do not bind for firms below a threshold size. Boeri and Jimeno (2003) document that workers employed in firms subject to stricter regulations face a lower dismissal probability. Along similar lines, Schivardi and Torrini (2005) firms' propensity to grow is lower around the thresholds. Also, Abidoye et. al examine firm-level panel

[13] We follow Parisi et al. (2006) and define low-tech industries as follows: Beverages, food, garments, leather, non-metallic and plastic materials, other manufacturing, textiles and wood and furniture.

data from Sri Lanka and find that (among non-export-processing zone firms) the likelihood of employment growth for firms [14] at the threshold employment level of 15 workers is lower than the corresponding likelihood for firms above this threshold. Differential enforcement of regulations is actually observed in our sample in the form of average days of labor inspections rising with firm size as shown in Figure 1. To address this criticism, we perform our baseline specification, which is column (4) of Table 2, for firms that employ more than 5, 10, 15 and 20 workers. Column (5) in Table 7 presents the results only for the subsample of firms with more than 5 workers. Our results are robust to exclusion of smaller size firms which may be argued to face weaker enforcement.[15]

EPL may have differential impacts on firm size through hiring and firing decisions of the firms depending on the age of the firm. One could argue that older firms already made their workforce decisions and hence will react less to the EPL while younger firms which have made their decisions more recently will respond more. Indeed, when we split our sample into older and younger firms by defining the

[14] They restrict their sample to firms which are not in export processing zones because of the wide-spread belief of lax enforcement policy enforcement for those firms.

[15] Our findings are also robust to restricting the sample to firms with more than 10, 15 and 20 workers, not presented in the appendix .

latter as those younger than 5 years of age, we find that younger firms are indeed affected more by firing regulations (Columns (8) and (9) in Table 5).[16]

Table 5 presents further robustness checks. A relevant concern in cross-country analysis is that some particular outlier country or region might be driving the results. To address this concern, we repeat the estimation by excluding one region from the sample at a time. Columns (2) to (5) of Table 6 confirm that our results are not driven by an outlier region. Similarly, we check to see if low income countries are driving our results and hence perform the analysis by excluding the low income countries from the sample.[17] Our results remain the same.

Finally, we also divide the sample into two sub-samples according to their score on the Rule of Law index.[18] We find that our main results still hold in the sample of good rule of law countries but not in the bad rule of law countries. This finding is very interesting given the motivation of our paper. It confirms not only the

[16] Young firms have made their workforce decisions more recently and therefore could have responded more easily to regulation and its enforcement. In contrast, older firms could have made their technological and labor intensity decisions much earlier, and since then we do not expect firm migration to be a frequent event, would be less affected. We split the sample into young (less or equal than 5 years of age) and old (more than 5 years of age) firms, and then re-estimated our models for the two subsamples. The results in Table 5 columns show that the impact of regulations are stronger for younger than older firms.

[17] Income classification is according to the World Bank's classification.

[18] We divide the sample according to the median score on the Kauffman and Kraay index.

correctness of suspicion about the quality of inspections as a proxy of enforcement in countries where the rule of law is weak but also the main idea of our paper that regulations will be more binding in countries where enforcement is stricter.

4.3 EPL & the Skill Composition of the Workforce

In addition to the impact on the level of employment, EPL affect the type of workers employees hire and hence may have distortionary effects on the distribution of employment. Theoretically, the EPL is expected to shift the choices of employers toward higher skilled workers which may result from switching to more capital intensive techniques of production. This in turn leads to increased preference for higher skilled workers who are better at working with capital. Similarly, due to the expectations of longer job duration, firms may choose to employ better skilled workers who will be likelier to be more productive. Having established the result that stricter enforcement of firing regulations constrains firm size, we next check for the impact of enforcement on the employment of different types of workers. We analyze whether enforcement of labor regulations affect the share of skilled workers at the firm level which we measure by the share of professionals, managerial and skilled production workers in the total workforce as reported by the firm. Table 6 presents the results from estimating our baseline specification by using the share of skilled workers as the outcome variable. Results in columns (1) through (9) confirm that stricter enforcement of firing regulations in countries where regulations are more rigid is positively correlated with a higher share of skilled workers. The 90-10

percentile differential in this case is 0.02 which is not particularly large considering the mean share of skilled workers in our sample is 0.63. This finding is in line with what Montenegro and Pages (2003) and Autor, Kerr and Kugler (2007) find for India and US, respectively.

4.4 Heterogeneity and Robustness of EPL & Skill Composition of the Workforce

We proceed to test the robustness of our findings on skill composition of the workforce and report these checks in tables 7 and 8. In summary, results in Table 7 show that while our main findings on the impact of EPL on the share of skilled workers still hold in the subsample of manufacturing firms, this effect is now smaller in magnitude. In addition, as shown in columns (2) – (4), the impact of regulations no longer persists when we distinguish between the technology used by the firms and when we restrict our sample to domestically-owned firms. The latter finding may be interpreted as saying that our main findings are driven by foreign firms which shift their workforce composition toward higher skilled workers when faced by stricter EPL. Column (5) displays the results for restricting the analysis to firms employing more than 5 workers. In this case, the coefficient is identical to the one in the baseline specification in column (4) of Table 6.

Table 8 presents the results when we test the robustness of our findings to alternative samples. We find that our coefficient of interest changes signs and turns

insignificant when we exclude the low income countries. Columns (2) to (4) reveal that our results are not robust to exclusion of each region from the sample which implies that our results may have been driven by firms in some particular regions. Finally, and more interestingly, while we find a negative and imprecisely estimated coefficient in the sample of good rule of law countries, our results still hold in bad rule of law countries. This finding may be interpreted as an implication of the fact that there is relatively low variation in the number of days of inspections in good rule of countries compared to the bad rule of law countries.[19]

5. Conclusion

This paper examines the impact of firing regulations on employment and the composition of employment at the firm level in developing countries. We explore the within-country variation in the enforcement of regulations in countries with different stringencies of labor law using a rich firm-level dataset. Our findings are as follows. First, we find evidence that stricter firing rules in countries where *de jure* regulations are more rigid is associated with significantly smaller firm size controlling for firm characteristics and country-sector fixed effects. These effects are stronger in manufacturing and labor intensive firms. Second, stricter enforcement of firing regulations shifts the composition of the workforce away from unskilled and non-production workers toward skilled workers.

[19] Indeed, while the standard deviation of the days of labor inspections in good rule of countries is 4.2 days, it is 5.9 days in bad rule of countries.

Our results have important policy implications. First, employment protection legislation constrains firm size, measured by the number of workers. Constraints to firm size can be detrimental growth if we consider that most of economic growth is due to growth in the existing firms as opposed to growth in the creation of new firms, as documented in Rajan and Zingales (1998) and Kumar, Rajan and Zingales (1999). Second, this impact on firm size may translate into lower employment if the numbers of firms in the economy do not rise with stricter EPL to compensate the fall in firm size. Third, EPL may also distort the optimal size distribution in the economy by shifting the distribution of firms toward smaller ones. Finally, EPL has significant impacts also on the skill mix of the firms' workforce and therefore bears distributional implications. Therefore, an effective EPL should be designed with the impact of different sub-groups of the labor force whom it intends to protect. While these findings certainly lead to efficiency and equity considerations, the ultimate effect of the change in the skill composition of the workforce on productivity needs further investigation and is, therefore, left for future research.

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Specificity of Human Capital Investments” American Economic Review, (96) 3: 811-831

Appendix

Figure 1

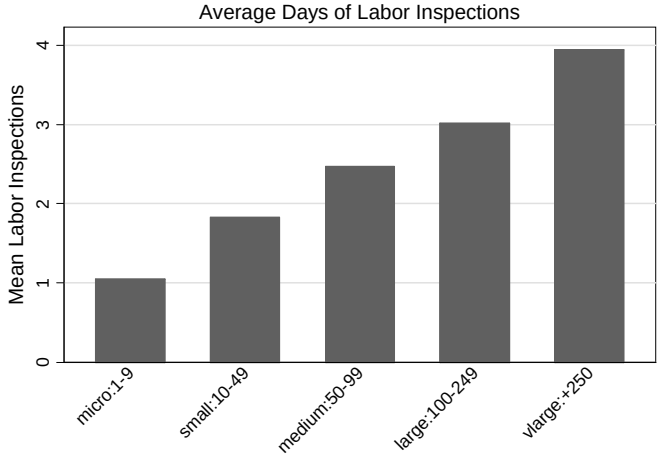

Figure 2

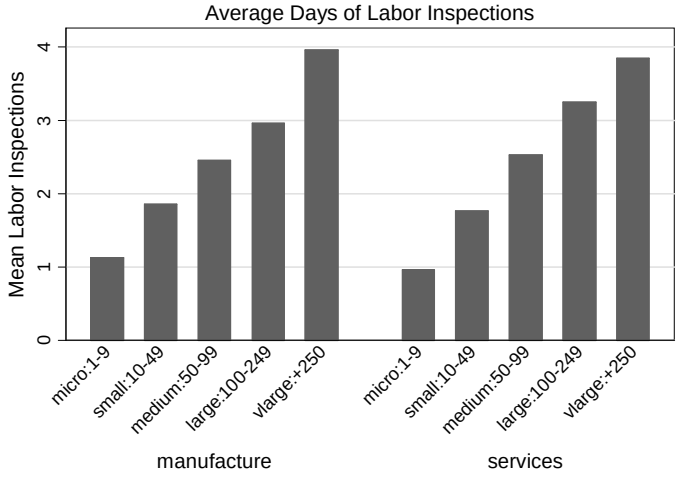

Figure 3: Difficulty of Firing and Labor Inspections

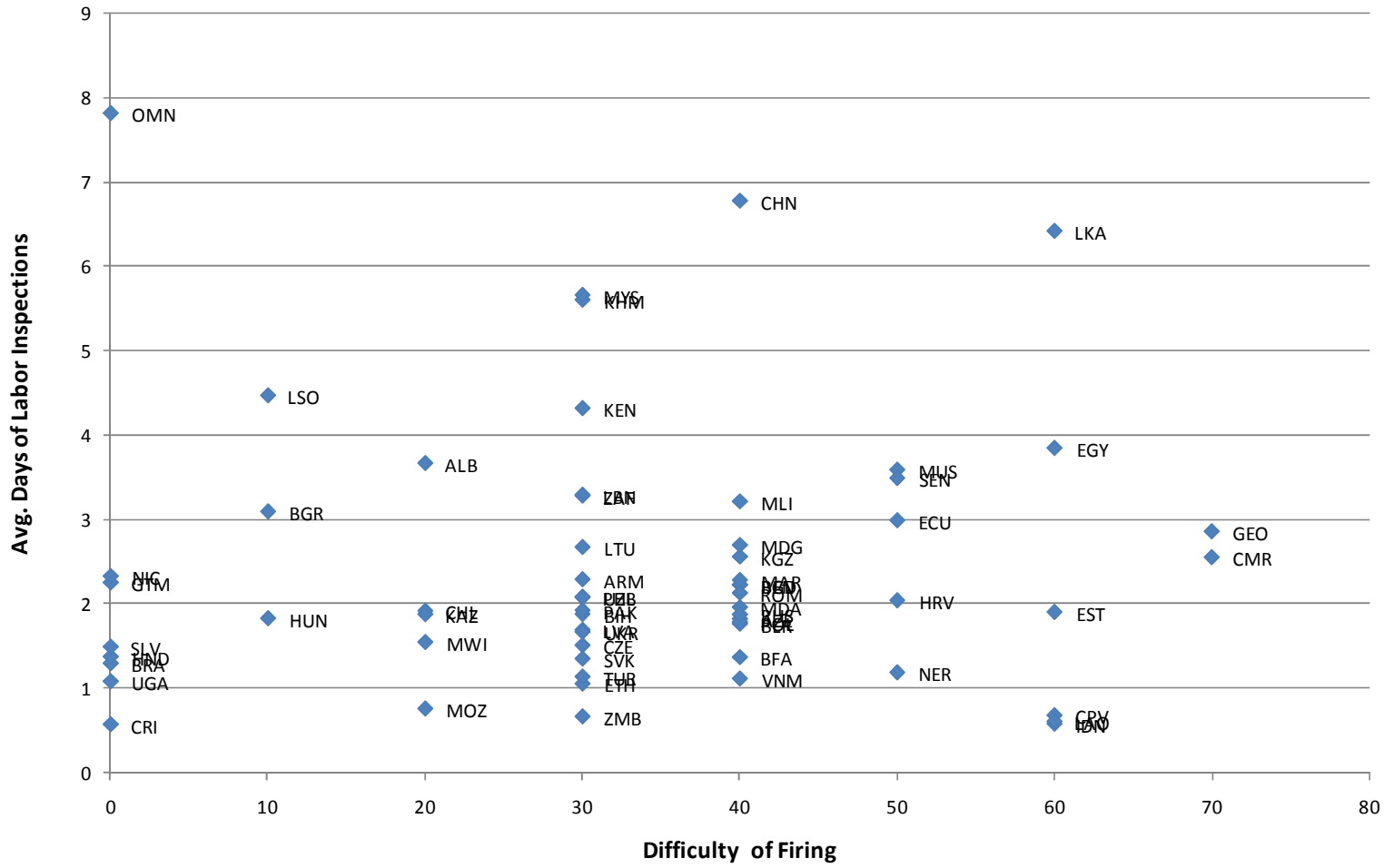

Table A.1. Variables Definitions

Variable	Definition
Firm Size	Total number of permanent workers
Share of Skilled Workers	Percentage of the firm's workforce that are managers, professionals or skilled production workers.
Fully Foreign-Owned	Dummy variable equal to 1 if the firm's capital is entirely owned by foreigners.
Majority Foreign-Owned	Dummy variable equal to 1 if more than 50% of the firm's capital is owned by foreigners.
Minority Foreign-Owned	Dummy variable equal to 1 if more than 0% but less than 50% of the firm's capital is owned by foreigners.
Exporter	Dummy variable equal to 1 if the firm exports directly or indirectly.
Age	Year of the survey minus the year when the firm started operations.
Micro, Small, Medium, Large and Very Large	Dummy variables equal to 1 if the total number of employees in the firm is between 1 and 10, between 11 and 50, between 51 and 150, between 150 and 249 and greater than 250 respectively.
Public Ownership	Dummy variable equal to 1 if the share of the firm's capital owned by the government or state is positive.
Labor Inspections	Number of visits to the firm by labor inspectors.
Total Inspections	Number of visits to the firm by all types of inspections (including taxes, heath, safety and labor).
Difficulty of Firing	Difficulty of Firing Index (Doing Business)
Firing Costs	Firing costs (weeks of wages) (Doing Business)
Regulatory Quality	Government Regulatory Quality (Kaufmann & Kraay)
Rule of Law	Government Rule of Law (Kaufmann & Kraay)
Gov. Control Corruption	Government Control Corruption (Kaufmann & Kraay)

Source: Enterprise Surveys unless otherwise stated.

Table A2. Country/Year Composition of the Sample

	Obs	Freq.
SUB-SAHARAN AFRICA		
Benin2004	194	0.01
BurkinaFaso2006	50	0.00
Cameroon2006	118	0.00
CapeVerde2006	47	0.00
Ethiopia2002	367	0.01
Kenya2003	242	0.01
Lesotho2003	52	0.00
Madagascar2005	206	0.01
Malawi2005	155	0.01
Mali2003	132	0.00
Mauritius2005	174	0.01
Mozambique2002	107	0.00
Niger2006	125	0.00
Senegal2003	227	0.01
SouthAfrica2003	578	0.02
Uganda2003	297	0.01
Zambia2002	194	0.01
Total	3,265	
EAST ASIA & PACIFIC		
Cambodia2003	184	0.01
China2003	3,356	0.11
Indonesia2003	711	0.02
Laos2005	244	0.01
Malaysia2002	708	0.02
Philippines2003	608	0.02
Thailand2004	1,384	0.05
Vietnam2005	1,471	0.05
Total	8,666	
MIDDLE EAST & NORTH AFRICA		
Egypt2006	989	0.03
Lebanon2006	292	0.01
Morocco2004	827	0.03
Oman2003	268	0.01
Total	2,376	

	Obs	Freq.
EUROPE & CENTRAL ASIA		
Albania2005	201	0.01
Armenia2005	333	0.01
Azerbaijan2005	187	0.01
Belarus2005	308	0.01
BiH2005	173	0.01
Bulgaria2005	278	0.01
Croatia2005	166	0.01
Czech Rep.2005	261	0.01
Estonia2005	139	0.00
Georgia2005	188	0.01
Hungary2005	515	0.02
Kazakhstan2005	544	0.02
Kyrgyzstan2005	200	0.01
Latvia2005	179	0.01
Lithuania2005	185	0.01
Moldova2005	338	0.01
Poland2005	719	0.02
Romania2005	559	0.02
Russia2005	534	0.02
Slovakia2005	156	0.01
Turkey2005	1,641	0.05
Ukraine2005	553	0.02
Uzbekistan2005	240	0.01
Total	8597	
LATIN AMERICA & CARIBBEAN		
Brazil2003	1,634	0.05
Chile2004	933	0.03
CostaRica2005	343	0.01
Ecuador2003	434	0.01
ElSalvador2003	465	0.02
Guatemala2003	435	0.01
Honduras2003	449	0.01
Nicaragua2003	452	0.01
Total	5,145	
SOUTH ASIA		
Bangladesh2002	949	0.03
Pakistan2002	939	0.03
SriLanka2004	414	0.01
Total	2,302	
TOTAL	**30,351**	

Table 1. Summary Statistics of the Main Variables

Variable	N	Mean	S.D.	Min	Max
Firm Size	30351	211.7	1021.9	1	67598
Log Firm Size	30351	3.83	1.65	0	11.12
Micro (1-9)	30351	0.16	0.37	0	1
Small (10-49)	30351	0.38	0.49	0	1
Medium (50-99)	30351	0.14	0.35	0	1
Large (100-249)	30351	0.15	0.35	0	1
Very Large (+250)	30351	0.17	0.38	0	1
Large City (dummy)	30061	0.62	0.32	0	1
Share Skilled Workers	30061	0.62	0.32	0	1
Age of the firm	29435	17.36	16.58	0	215
Exporter	29918	0.31	0.46	0	1
Minority Foreign Ownership	30246	0.86	0.35	0	1
Majority Foreign Ownership	30246	0.03	0.17	0	1
Full Foreign Ownership	30246	0.05	0.21	0	1
Public Ownership	30151	0.08	0.27	0	1

Source: Author's calculations based on the Enterprise Surveys.

Table 2: Firing Regulations and Firm Size

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Rigidity in Firing * Labor Inspections	-0.00086	-0.00073	-0.00068	-0.00062	-0.00067	-0.00059	-0.00062	-0.00068	-0.00058
	[0.00013]***	[0.00011]***	[0.00014]***	[0.00014]***	[0.00015]***	[0.00014]***	[0.00014]***	[0.00014]***	[0.00014]***
Rigidity in Firing * Total Inspections	-	-	-0.00007	-0.00007	-0.00007	-0.00007	-0.00007	-0.00006	-0.00007
			[0.00005]	[0.00005]	[0.00005]	[0.00005]	[0.00005]	[0.00005]	[0.00005]
Labor Inspections	0.0661	0.0504	0.02879	0.0066	0.04458	0.06889	0.05867	0.06119	-0.02133
	[0.00491]***	[0.00432]***	[0.00526]***	[0.02000]	[0.02868]	[0.02873]**	[0.02767]**	[0.02312]***	[0.02329]
Total Inspections	-	-	0.01252	0.01255	0.01253	0.01244	0.01249	0.01245	0.00585
			[0.00195]***	[0.00195]***	[0.00194]***	[0.00195]***	[0.00194]***	[0.00194]***	[0.00306]*
GDP pc * Labor Inspections	-	-	-	0.0028	-0.00191	-0.00558	-0.00378	0.00001	0.00465
				[0.00253]	[0.00354]	[0.00374]	[0.00348]	[0.00255]	[0.00265]*
Rule of Law * Labor Inspections	-	-	-	-	0.00915	-	-	-	-
					[0.00513]*				
Regulatory Quality * Labor Inspections	-	-	-	-	-	0.01725	-	-	-
						[0.00522]***			
Gov. Control Corruption * Labor Inspections	-	-	-	-	-	-	0.01342	-	-
							[0.00490]***		
Procedures to Start a Business * Labor Inspections	-	-	-	-	-	-	-	-0.00281	-
								[0.00071]***	
Rigidity Employment * Labor Inspections	-	-	-	-	-	-	-	-	0.0005
									[0.00024]**
Basic Firm Level Controls Included?	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country_Sector Fixed Effects Included?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	30351	27387	27387	27387	27387	27387	27387	27387	27387
P90-P10 Differential in Firm Size=β[E(90)-E(10)][R(90)-R(10)]	-0.21	-0.18	-0.17	-0.15	-0.17	-0.15	-0.15	-0.17	-0.14

Source: Author's calculations based on the Investment Climate Surveys.

Dependent variable is the logaritm of firm size (measured by the total number of permanent employees). Robust standard errors are in brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. P90-P10 differential quantifies the impact of increasing enforcement from 10^{th} less strict percentile to the 90^{th} strictest percentile in a country with loose regulations (at the 10^{th} percentile) relative to a country with stricter regulations (at the 90^{th} percentile). E(α) refers to the α^{th} percentile of the labor inspections, R(α) refers to the α^{th} percentile of the difficulty in firing variable. All variables are defined in Table A.1.

Table 3: Firm Size and Rigidity in Firing Regulations: Robustness Checks

	Av. Labor Inspections in the City-Size-Sector	Inspections at the Firm Level			
	(1)	(2)	(3)	(4)	(5)
Rigidity in Firing * Labor Inspections	-0.0017	-0.00045	-0.00053	-0.00038	-0.00049
	[0.00043]***	[0.00018]**	[0.00015]***	[0.00017]**	[0.00017]***
Labor Inspections	0.1073	-0.03179	-0.0263	-0.11384	-0.01177
	[0.05244]**	[0.02273]	[0.02112]	[0.02366]***	[0.02813]
Rigidity in Firing * Total Inspections	0.0000	-0.00022	-0.00003	-0.00015	-0.00011
	[0.00014]	[0.00007]***	[0.00006]	[0.00006]**	[0.00007]
Total Inspections	0.0255	0.0186	0.01327	0.01802	0.01528
	[0.00529]***	[0.00264]***	[0.00202]***	[0.00226]***	[0.00256]***
Rigidity in Firing * Management Time Spent Dealing with Officials	-	-0.00046	-	-	-
		[0.00010]***			
Rigidity in Firing * Property Rights Enforced in the Country	-	-	0.14424	-	-
			[0.01823]***		
Rigidity in Firing * Regulations Consistent in the Country	-	-	-	0.04739	-
				[0.01929]**	
Rigidity in Firing * Government Efficient in the Country	-	-	-	-	-0.03911
					[0.02896]
GDP pc * Labor Inspections	Yes	Yes	Yes	Yes	Yes
Basic Firm Level Controls Included?	Yes	Yes	Yes	Yes	Yes
Country_Sector Fixed Effects Included?	Yes	Yes	Yes	Yes	Yes
Observations	29,205	24,140	23,789	20,522	8,991
P90-P10 Differential in Firm Size=β[E(90)-E(10)][R(90)-R(10)]	-0.48	-0.11	-0.13	-0.09	-0.12

Source: Author's calculations based on the Investment Climate Surveys.

Dependent variable is the logaritm of firm size (measured by the total number of permanent employees). Table reports the. specification which includes all the variables in column (4) of table 2 (including country_sector fixed effects). Robust standard errors are in brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. P90-P10 differential quantifies the impact of increasing enforcement from 10th less strict percentile to the 90th strictest percentile in a country with loose regulations (at the 10th percentile) relative to a country with stricter regulations (at the 90th percentile). E(α) refers to the αth percentile of the labor inspections, R(α) refers to the αth percentile of the difficulty in firing variable. All variables are defined in Table A.1.

Table 4: Firm Size and Firing Regulations: Robustness to the Manufacturing Sector

	All	High-Tech Manufacturing	Low-Tech Manufacturing	Domestic Firms	"Firms with more than 5 employees"
	(1)	(2)	(3)	(4)	(5)
Rigidity in Firing * Labor Inspections	-0.0009	-0.0006	-0.0009	-0.00075	-0.00077
	[0.00015]***	[0.00033]*	[0.00017]***	[0.00018]***	[0.00015]***
Labor Inspections	0.0351	-0.1058	0.0715	0.0320	0.0157
	[0.02129]*	[0.04317]**	[0.02343]***	[0.02585]	[0.01994]
Basic Firm Level Controls Included?	Yes	Yes	Yes	Yes	Yes
Country_Sector Fixed Effects Included?	Yes	Yes	Yes	Yes	Yes
Observations	20,766	6,825	13,941	16336	19546
P90-P10 Differential in Firm Size=β[E(90)-E(10)][R(90)-R(10)]	-0.26	-0.15	-0.22	-0.19	-0.23

Source: Author's calculations based on the Investment Climate Surveys.

Dependent variable is the logarithm of total firm size (measured by the total number of permanent employees in the firm) . Table reports different robustness checks over our baseline specification (in column (4) of table 2). Robust standard errors are in brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. P90-P10 differential quantifies the impact of increasing enforcement from 10th less strict percentile to the 90th strictest percentile in a country with loose regulations (at the 10th percentile) relative to a country with stricter regulations (at the 90th percentile). E(α) refers to the αth percentile of the labor inspections, R(α) refers to the αth percentile of the difficulty in firing variable. All variables are defined in Table A.1. In column (3), the low tech industries include beverages, food, garments, leather, non-metallic and plastic materials, paper, other manufacturing, textiles, and wood and furniture.

Table 5: Firm Size and Firing Regulations: Robustness to Alternative Samples

	Excluding:					"Good" Rule of Law Countries	"Bad" Rule of Law Countries	Younger Firms-Age<5	Older Firms-Age >=5
	Low Income Countries	East Asia and Pacific	ECA	LAC	North Africa & MENA				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Rigidity in Firing * Labor Inspections	-0.0004	-0.0006	-0.0008	-0.0006	-0.0003	-0.0009	-0.0002	-0.0009	-0.0006
	[0.00016]***	[0.00018]***	[0.00014]***	[0.00019]***	[0.00016]**	[0.00021]***	[0.00021]	[0.00047]*	[0.00015]***
Labor Inspections	-0.0734	-0.0082	0.0663	0.0296	-0.1112	-0.0055	0.0945	0.0814	0.0031
	[0.03298]**	[0.02459]	[0.01985]***	[0.02225]	[0.02680]***	[0.04068]	[0.03114]***	[0.06196]	[0.02127]
Basic Firm Level Controls Included?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country_Sector Fixed Effects Included?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,685	17,880	18,805	22444	23032	12887	14500	3121	24266

Source: Author's calculations based on the Investment Climate Surveys.

Dependent variable is the logarithm of total firm size (measured by the total number of permanent employees in the firm). Table reports different robustness checks over our baseline specification (in column (4) of table 2). Column (1) to (5) estimates the base model excluding from the sample low income countries in column (1), firms in East Asia and Pacific in column (2), firms in Eastern Europe in column (3), Latin America and Caribbean, and North Africa and Middle East**.** Robust standard errors are in brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. All variables are defined in Table A.1.

Table 6: Firing Regulations and Share of Skilled Workers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Rigidity in Firing * Labor Inspections	0.00007	0.00007	0.00012	0.00008	0.0001	0.00009	0.00009	0.00012	0.00011
	[0.00002]***	[0.00003]***	[0.00003]***	[0.00003]**	[0.00003]***	[0.00003]***	[0.00003]***	[0.00003]***	[0.00003]***
Rigidity in Firing * Total Inspections	-	-	-0.00002	-0.00002	-0.00002	-0.00002	-0.00002	-0.00002	-0.00002
			[0.00001]**	[0.00001]*	[0.00001]**	[0.00001]**	[0.00001]**	[0.00001]**	[0.00001]*
Labor Inspections	-0.00322	-0.00328	-0.00278	0.01068	-0.00292	-0.00216	-0.00275	-0.0123	0.00119
	[0.00092]***	[0.00096]***	[0.00112]**	[0.00445]**	[0.00112]***	[0.00116]*	[0.00113]**	[0.00200]***	[0.00176]
Total Inspections	-	-	-0.0003	-0.0003	-0.0003	-0.0003	-0.0003	-0.0003	-0.0011
			[0.00037]	[0.00037]	[0.00037]	[0.00037]	[0.00037]	[0.00037]	[0.00061]*
GDP pc * Labor Inspections	-	-	-	-0.00169	-	-	-	-	-
				[0.00054]***					
Rule of Law * Labor Inspections	-	-	-	-	-0.00221	-	-	-	-
					[0.00079]***				
Regulatory Quality * Labor Inspections	-	-	-	-	-	-0.00254	-	-	-
						[0.00078]***			
Gov. Control Corruption * Labor Inspections	-	-	-	-	-	-	-0.00257	-	-
							[0.00081]***		
Procedures to Start a Business * Labor Inspections	-	-	-	-	-	-	-	0.00082	-
								[0.00015]***	
Rigidity Employment * Labor Inspections	-	-	-	-	-	-	-	-	-0.00014
									[0.00005]***
Basic Firm Level Controls Included?	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country_Sector Fixed Effects Included?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	30061	27158	27158	27158	27158	27158	27158	27158	27158
P90-P10 Differential in Share of skilled workers=β[E(90)-E(10)][R(90)-R(10)]	0.017	0.017	0.030	0.020	-	-	-	-	-

Source: Author's calculations based on the Investment Climate Surveys.

Dependent variable is the ratio of managerial, professional and skilled production workers to firm size. Robust standard errors are in brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. P90-P10 differential quantifies the impact of increasing enforcement from 10^{th} less strict percentile to the 90^{th} strictest percentile in a country with loose regulations (at the 10^{th} percentile) relative to a country with stricter regulations (at the 90^{th} percentile). E(α) refers to the αth percentile of the labor inspections, R(α) refers to the α^{th} percentile of the difficulty in firing variable. All variables are defined in Table A.1.

Table 7: Firing Regulations & Share of Skilled Workers: Robustness to the Manufacturing Sector

	All	High-Tech Manufacturing	Low-Tech Manufacturing	Domestic Firms	"Firms with more than 5 employees"
	(1)	(2)	(3)	(4)	(5)
Rigidity in Firing * Labor Inspections	0.0001	0.00011	0.00005	0.00006	0.00008
	[0.00004]***	[0.00008]	[0.00004]	[0.00004]	[0.00004]**
Labor Inspections	0.00977	0.03716	-0.00021	0.01416	0.01212
	[0.00484]**	[0.01316]***	[0.00510]	[0.00573]**	[0.00475]**
Basic Firm Level Controls Included?	Yes	Yes	Yes	Yes	Yes
Country_Sector Fixed Effects Included?	Yes	Yes	Yes	Yes	Yes
Observations	20620	6782	13838	16229	19412
P90-P10 Differential in Firm Size=β[E(90)-E(10)][R(90)-R(10)]	0.03	0.03	0.01	0.01	0.02

Source: Author's calculations based on the Investment Climate Surveys.

Dependent variable is the ratio of managerial, professional and skilled production workers to firm size. Robust standard errors are in brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. P90-P10 differential quantifies the impact of increasing enforcement from 10^{th} less strict percentile to the 90^{th} strictest percentile in a country with loose regulations (at the 10^{th} percentile) relative to a country with stricter regulations (at the 90^{th} percentile). E(α) refers to the α^{th} percentile of the labor inspections, R(α) refers to the α^{th} percentile of the difficulty in firing variable. All variables are defined in Table A.1.

Table 8: Firing Regulations & Share of Skilled Workers: Robustness to Alternative Samples

	Excluding:					"Good" Rule of Law Countries	"Bad" Rule of Law Countries
	Low Income Countries	East Asia and Pacific	ECA	LAC	North Africa & MENA		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Rigidity in Firing * Labor Inspections	-0.00001	0.00009	0.00006	0.00011	0	-0.00002	0.00026
	[0.00004]	[0.00004]**	[0.00004]	[0.00003]***	[0.00004]	[0.00005]	[0.00006]***
Labor Inspections	0.03625	0.01996	-0.00385	0.00834	0.01977	0.00709	0.00692
	[0.00729]***	[0.00569]***	[0.00532]	[0.00467]*	[0.00509]***	[0.00924]	[0.00754]
Basic Firm Level Controls Included?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country_Sector Fixed Effects Included?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20505	22844	17700	18678	22252	12771	14387

Source: Author's calculations based on the Investment Climate Surveys.

Dependent variable is the ratio of managerial, professional and skilled production workers to firm size. Robust standard errors are in brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. All variables are defined in Table A.1.

Chapter 3: Productivity, Wages and Exports in Indian Manufacturing Firms: Evidence from Quantile Regressions

1. Introduction

Several studies have empirically examined the superior characteristics of exporting firms using firm-level data since the 1990s. The literature, starting with Bernard and Jensen (1995), documents the superior characteristics of exporting firms relative to non-exporting firms. The common finding in almost all studies is that exporters are larger, more productive, more capital-intensive, employ a higher share of skilled workers and pay higher wages. The magnitude and cause of this premium between exporters and non-exporters has been widely investigated in several papers.

There are two different explanations for this export premium: (i) self-selection and (ii) learning by exporting. The self-selection hypothesis suggests that only better firms select into export markets due to the existence of additional costs associated with exporting that may act as barriers to entry into international markets.

These costs involve costs of transportation, obtaining information about the markets, establishing marketing and distribution channels and modifying the current products according to the tastes of foreign consumers. Therefore, only the more productive firms are able to overcome the barriers associated with exporting. Along similar lines Alvarez and López (2005) proposed that firms may be "consciously" increasing their productivity for the purpose of being able to export. In this case, it is the decision to export that ensues from the higher returns available in foreign markets and this causes firms to increase their productivity. According to the "conscious self-selection" hypothesis, in developing countries, goods that are produced for export markets are typically higher quality than the ones that are produced for the domestic market. Therefore, firms that decide to participate in international markets have to invest in new capital and produce a higher quality version of the domestic good. Since the introduction of the new technology increases the value of production, productivity of the exporting firms is higher for firms producing for the international markets relative to the domestic one. Alvarez and López (2005) find evidence for this hypothesis using plant-level data from Chile.

The alternative to the self-selection hypothesis is the learning by exporting hypothesis. This view suggests that there are post-entry rewards from exporting and that the firm's performance improves due to exposure in international markets. According to this view, more intense competition in international markets and interacting with foreign customers makes firms more efficient.

These two hypotheses are not mutually exclusive. However, previous studies have found strong evidence suggesting self-selection and only moderate support for the learning by exporting. Studies that find evidence for the self-selection hypothesis for labor productivity include Bernard and Jensen (1995, 1999, 2004) for the US, Aw Chung and Roberts (2000) for Taiwan, Bernard and Wagner (1997) for Germany, Clerides, Lach and Tybout (1998) for Colombia, Mexico and Morocco, Baldwin and Gu (2003) for Canada, Chiara, Serti and Thomasi (2007) for Italy. While the robust finding from these studies is that better firms become exporters, there is some evidence of post-entry productivity growth, mostly in entrants and in the first years after starting to export (Kraay, (1999) for China, Baldwin and Gu (2003) for Canada, Alvarez and Lopez (2005) for Chile).

As an extension to the literature, Yasar, Nelson and Rejesus (2005) examine the effects of exporting on productivity at different quantiles of the output distribution. Using plant level data on Turkish firms and employing quantile regression techniques, they document that the export premium is significant and increasing at all points throughout the output distribution. They also find that exporting firms that continuously export perform better compared to export starters, quitters and switchers.

In this paper, I initially identify the export premium on labor productivity and average wages at different points of their respective conditional distributions and then test the two hypotheses in order to explain the superior performance of

exporters. Quantile regression results confirm the superior characteristics of exporting firms. Looking at the export premia at different points of the productivity distribution reveals patterns that are not readily detectable by analyses at the conditional mean. I find that, after controlling for industry and location as well as firm size and work force composition, the export premium is not uniform through the conditional productivity distribution but rather increases in the second half of the conditional productivity distribution. The same holds for average wages paid by the firms. Looking at the ex-ante differences in performance characteristics of the firms, I find no evidence for the self-selection of firms on the basis of productivity. However, I find some suggestive evidence of productivity enhancing effects of exporting for export starters.

2. Data and Basic Patterns

This study is based on data from the survey for India from the Enterprise Surveys of the World Bank.[1] The surveys were conducted between 2002 and 2005 and the samples were designed to be representative of the population of firms according to their industry and location within each country. In India the surveys were conducted in 2002, 2005 and 2007. A panel component is available in the 2002 and 2005 waves. The dataset includes 1047 firms that were surveyed in both years. With the

[1] For an in-depth description of the Enterprise Surveys and a survey of other papers that utilize the dataset, see Almeida and Susanli, (2009)

availability of retrospective questions, this provides information on several characteristics of the firms for the years 2000, 2001, 2003 and 2004 and makes a balanced panel. While for many of the variables, information on 2002 is available, I do not use information on 2002 due to high number of missing observations on the sales variable.[2] The survey includes rich information on activities of the firms both domestic and international, including the sales, the amount of exports and the export destination of the firms as well as the first time firms started exporting to a particular destination. In addition the survey collects detailed information on labor force variables, such as size and workforce composition. The final sample is a balanced panel of 760 firms with a complete set of variables, distributed across a wide set of manufacturing industries - auto and auto components, beverages, chemicals, electronics, food, garments, leather, metals and machinery, non-metallic and plastic materials, paper, textiles, wood and furniture.

Table 1 provides the descriptions of the main variables in the analysis. I proxy labor productivity by the value added per worker.[3] I calculate value added by subtracting the cost of raw materials (excluding fuel) from the total value of production. I categorize firm size into five groups: micro, small, medium, large and very large.

[2] I also make use of retrospective questions about exporting history to verify whether a firm has continuously exported or not during the panel period.

[3] In this paper, I abuse notation in that I use labor productivity and productivity interchangeably.

Table 2 shows some useful statistics about the export behavior of firms. A firm is considered an exporter if it reports exporting a positive amount of its output in a given year.[4] Entrants are defined as not having exported the year before but exporting the current year and symmetrically exiting is defined as having exported the year before but not exporting this year. On average, 6.2 and 4 percent of the firms enter and exit export markets in a given year.

Table 3 presents the summary statistics for the main variables that are used in the analysis.[5] As found in other studies, exporting firms are more productive, employ a higher share of skilled workers, and pay higher wages than non-exporting firms. While these results are suggestive of an export premium, a more formal and systematic analysis that also takes into account the correlation between export status and labor productivity throughout the conditional productivity distribution will be given in the section 3.

3. Export Premia

The summary statistics presented in Section 2 suggest that exporting firms are superior to non-exporting firms in terms of productivity, quality of their workers and

[4]Retrospective information is only available for direct exports but not indirect exports that are made through intermediary firms. To provide continuity in the panel length, I only consider direct exporters.

[5] The breakdown of firms with respect to city of location and sector of activity is also available, but not presented in the text to conserve space. They are available from the author upon request.

the average wages they pay to their workforce. A more systematic analysis of these differences involves estimating the export premia controlling for other firm attributes such as location and sector of activity and also for year fixed effects.

Following Bernard and Jensen (1999), I estimate the export premium by the following equation:

$$lnY_{it} = \alpha + \beta Export_{it} + \delta Control_{it} + \varepsilon_{it} \qquad (1)$$

where Y_{it} is an outcome of interest for firm i at time t, either the logarithm of value-added per worker or the logarithm of average wage at the firm. $Export_{it}$ is a dummy variable that is equal to 1 if the firm exports a positive amount of its production. The coefficient of interest, β, quantifies the (ceteris paribus) percentage difference in the outcome variable between exporters and non-exporters. $Control_{it}$ is a vector of variables including industry, year and city dummies and the firm's size and share of skilled workers. Dummy variables for firm size categories are included in the regression to capture differences in labor productivity across firms of different work force size. City and industry fixed effects control for the differences in productivity across cities and industries. Year fixed effects control for the changes in the macroeconomic and intuitional environment over time.

In addition, I estimate the above equation by using quantile regression techniques to provide insights about export premia over the conditional productivity distribution. Quantile regression technique was introduced into this area of research by Yasar, Nelson and Rejesus (2005) to examine the productivity effects of exporting at different points of the conditional output distribution. Quantile regression estimates are known to be robust relative to least squares estimates in the presence of outliers in the sample and also in cases of departures from normality. To provide a snap-shot of the exporter premium across the productivity distribution, I estimate (1) for different quantiles of the productivity distribution. In particular I estimate the linear regressions:

$$lnY_{it} = \alpha(\theta) + \beta(\theta)Export_{it} + \delta(\theta)Control_{it} + u_{it} \qquad (2)$$

with $Q_\theta(Y_{it}|X_{it}) = \alpha(\theta) + \beta(\theta)Export_{it} + \delta(\theta)Control_{it}$

where ln Y_{it} and $Control_{it}$ is defined as in (1), $\beta(\theta)$ and $\delta(\theta)$ are the parameters to be estimated and u_{it} is a vector of residuals. $Q_\theta(Y_{it}|X_{it})$ denotes the θ^{th} conditional quantile of lnY_{it} given the vector of covariates X_{it}, $0<\theta<1$.

I estimate equations (1) and (2) using data pooled across all the years in the sample controlling for year fixed effects. Table 4 presents the results. On average, exporters do better than non-exporters in labor productivity and pay higher wages. Controlling for year dummies, labor productivity in exporters is on average 43

percent higher. This premium falls down to 30 percent when we compare firms that are in the same industry, city, of the same size and with the same share of skilled workers. Similarly, exporting firms pay 30 percent higher wages on average, and this premium on wage falls down to 10 percent after controlling for firms size, share of skilled workers, industry and city dummies.

As for quantile regressions, the first row of Table 4 shows that the export premium on productivity increases throughout the distribution. Results in Table 4 (second row) indicate that this premium ranges from 0.16 to 0.35 when a larger set of covariates are included. These results can be interpreted as follows: in the middle of the conditional productivity distribution, the premium on exporting is 41 percent in a given year, however it falls to 25 percent when firms of the same size, employing the same share of skilled workers, in the same industry and located in the same city are considered in the same year. Similarly, Table 5 shows that the export premia on the log of average wages decline when size, the share of skilled workers, industry and city dummies are controlled for. This time the premium at the mean falls from 30 percent to 20 percent. Regression quantiles at all points range from 14 to 51 percent when only controlling for year fixed effects and they range between 11 and 14 percent when a larger set of controls are included.

While these findings are in line with the existing literature, they are not informative about the direction of causality. Therefore, I address that question in the next section.

4. Direction of Causality: Self-selection or Learning by Exporting?

To test for self-selection of firms into exporting, it is essential to compare the performance of firms that start to export, i.e., export-starters, and non-exporters in the years prior to exporting. There are different ways of defining an export starter in the literature. These definitions typically depend on the length of the panel available. Serti and Thomasi (2007) defines export starters as firms that do not export at least for two years and continue to export after entry. Given the short length of the panel, I follow Serti and Thomasi (2007) and define export-starters as those firms that did not export in the years 2000, 2001 and 2002 and exported in 2003 and 2004. According to this definition of export-starters, there are 45 export-starters and 487 firms that never exported at any point in time in the sample.

To compare the ex-ante differences between export-starters and non-exporters in the years in which none of these firms were exporting, following Bernard and Jensen (1999) I estimate the following equation:

$$lnY_{it} = \alpha + \beta Export_{iT} + \delta Control_{it} + \varepsilon_{it} \qquad (3)$$

where $Expor_{iT}$ is a dummy for being an export starter in year T (T=2003 in this study) and Y_{it} is the outcome variable in the years prior to entry. $Control_{lit}$ is vector

of firm-characteristics, including firm size, share of skilled workers as well as industry, city and year dummies. I estimate equation (3) using data on firms that did not export in 2000, 2001 and 2002 but that may or may not start to export in 2003. In addition, I estimate (3) using quantile regression.

Table 6 presents the findings from ordinary least squares and quantile regressions.[6] When only controlling for a year dummy, there is evidence only at the 75^{th} and 90^{th} percentile of the conditional productivity distribution. However, this effect is diminishes and even reverses at the lower tail of the conditional productivity distribution when industry, city dummies and firms size and skill share are controlled for. Regarding wages, Table 6 shows that after controlling for the year dummy, in the years prior to exporting today's exporters pay on average 17 percent higher wages than today's non-exporters, and this premium in wages range widely from 10 percent to 70 percent. However, this finding does not persist when industry, city and

[6] Before running regressions, I checked the skewness and kurtosis of the dependent variables. Both the log of value added per worker and the log of average wages are positively skewed (0.44 and .52, respectively). Kurtosis values of 9.1 and 12.1 also imply heavy tailed underlying distributions which imply departures from normality and justify the use of quantile regression analysis.

firm size and composition are controlled for (in row 4).[7] Therefore, there is a lack of evidence for the self-selection of firms into export markets.

The next natural question to ask is whether exporting brings success to the firms. To analyze the direction of causality from export market entry to improved performance, I initially examine differences in productivity of firms with different exporting behavior during the entire panel period and then look at the post-entry growth of productivity and wages.

In particular, I group firms into 4 different groups based on their export behavior[8] : Non-exporters, switchers, export-starters, and quitters. Non-exporters are firms that did not export at any point in time. Continuous exporters are defined as firms which exported during all years. Export-starters are defined as discussed above. Quitters are firms that exported continuously in the first three years of the panel and did not export at all in the last two years. Switchers are firms that exported in at least one year but that are not continuous exporters, starters or quitters.

Initially I run a regression of the following form:

[7] One possible explanation may be related to the fact that labor regulations are set at the state level in India and hence the city fixed effects may be capturing the premium on wages if exporting firms are clustered certain cities in a state.

[8] Although I do not use the observations from year 2002 for productivity and wages, I use information on exports to verify export behavior throughout the sample.

$$lnY_{it} = \alpha + \beta Exp - starter + \gamma Cont - exporter + \mu Switch + \eta Quit + \delta Control_{it} + \varepsilon_{it} \quad (4)$$

where non-exporters are the base category.

Results presented in Table 7 show that continous exporting, switching and quitting are associated with higher levels of higher wages relative to selling only in the domestic market while starting to export do not bring any benefits for productivity. Controlling for the broader set of variables, labor productivity and average wages are on average 46 and 41 percent higher in continuous exporting firms, respectively. This suggests that benefits to exporting are more pronounced in firms that may be considered experienced exporters. An interesting result in Table 7 is that quiting export markets, relative to not exporting at all, is associated with higher productivity which leads one to think that those firms may still be reaping the benefits from exporting, if any. It is also noteworthy that exporting at some point during the panel is associated with 29 percent higher productivity relative to never exporting, when a larger set of controls are included. This is at odds with the prior that if exporting is beneficial to firms, then the benefits would be experienced only when firms are continuously exposed to international markets. I, therefore, test if the coefficient of the variable continous exporter is greater than that of switcher. In line with expectations, I fail to reject the null hypothesis at 10 percent level of significance that the coefficient of continous exporter is larger than that of switcher.

Although the above analysis is informative, a better understanding of export entry and subsequent performance requires examining growth rates of outcome variables. Therefore, to measure the post-entry premia in growth rates, I follow Bernard and Jensen (1997) and estimate the following equation:

$$lnY_{it} - lnY_{it-2} = \alpha + \beta Exp\_starter_{it-2} + \delta Control_{it-2} + \varepsilon_{it} \qquad (5)$$

Ideally, I would like to compare the post-entry growth of productivity and wages of firms, that have not exported for a period, but then which started to export continuously to those firms that have not exported at all. Given the short span of the panel, I am limited to firms that started to export in 2003 and continued to export in 2004. Therefore , I will compare the productivity growth of firms that started to export to those that did not export at any point in time. Results reported in Table 8 indicate that although export starters experience a labor productivity growth relative to non-exporters in the first two years after entry, this effect disappears when industry and location, as well as workforce size and composition are controlled for. In terms of growth average wages, although average wages paid by the firms that enter export markets grow 36 percent faster, this growth premium is not robust to controlling for industry and city dummies.

5. Conclusion

This paper studies the significant difference in labor productivity and wages between exporters and non-exporters. The literature has already reached a consensus on the superior characteristics of exporters. Using data from Indian firms, I show that exporters are better than non-exporters in terms of labor productivity and wages not only on average but also at different points of their respective distributions.

I test whether self-selection or learning by exporting are behind the export premia that are observed. While I find some evidence suggesting learning effects for labor productivity and some weak evidence for productivity growth in the first years after entry, I do not find evidence for the self selection hypothesis.

These results imply that there is little improvement from exporting in the first years following entry. This further implies that the higher productivity and wages of exporters are most likely due to persistence in exporting. As for policy making, if there are post-entry improvements from entering into the export markets, then a sound policy to promote export-led growth would be to formulate and implement programs that would support sustaining participation in the international markets.

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Appendix

Table 1: Variables Definitions

Variable	Definition
Value added per worker	Logaritm of the difference between sales and costs of raw materials excluding fuel (deflated to 2000 Rupees)
Average wages	Logaritm of the total costs of labor (including wages and bonuses) divided by number of workers (deflated to 2000 Rupees)
Exporter	Dummy variable equal to 1 if the firm exports a positive amount of its output
Micro, Small, Medium, Large and Very Large	Dummy variables equal to 1 if the total number of employees in the firm is between 1 and 10, between 11 and 50, between 51 and 150, between 150 and 249 and greater than 250 respectively.
Share of Skilled Workers	Percentage of the firm's workforce that are managers, professionals or skilled production workers.

Source: Enterprise Surveys.

Table 2: Export Behavior of Firms

Year	Number of Firms	Exporters (%)	Entrants (%)	Exiters (%)
2000	760	19.2	-	-
2001	760	24.3	5.4	0.3
2003	760	20.4	8.6	12.5
2004	760	22.0	4.7	3.2
Sample average	760	21.5	6.2	4.0

Source: Author's calculations using Enterprise Surveys

Table 3: Summary Statistics

Exporters	Observations	Mean	Std. Dev.	Min	Max
Variable					
Log(Value added per worker)	653	5.06	1.58	-3.66	13.26
Log(Average wage)	618	3.25	1.42	-5.47	10.30
Firm size - Micro	653	0.16	0.36	0	1
Firm size - Small	653	0.23	0.42	0	1
Firm size - Medium	653	0.15	0.36	0	1
Firm size- Large	653	0.20	0.40	0	1
Firm size- Very Large	653	0.26	0.44	0	1
Share of skilled workers	653	0.15	0.12	0.00	0.78
Non-Exporters					
Log(Value added per worker)	2387	4.62	1.36	-2.84	13.36
Log(Average wage)	2293	2.95	1.35	-5.57	13.84
Firm size - Micro	2387	0.47	0.50	0	1
Firm size - Small	2387	0.29	0.45	0	1
Firm size - Medium	2387	0.11	0.31	0	1
Firm size- Large	2387	0.08	0.27	0	1
Firm size- Very Large	2387	0.06	0.23	0	1
Share of skilled workers	2387	0.14	0.13	0	1

Source: Author's calculations based on the Enterprise Surveys.

Table 4: Export Premia

Dependent Variable	OLS	Quantile Regressions				
		0.1	0.25	0.5	0.75	0.9
Log (Value added per worker)[a]	0.43	0.47	0.41	0.41	0.59	0.64
	[0.061]*	[0.092]*	[0.059]*	[0.047]*	[0.060]*	[0.111]*
Log (Value added per worker)[b]	0.30	0.22	0.16	0.25	0.35	0.32
	[0.067]*	[0.065]*	[0.046]*	[0.051]*	[0.041]*	[0.080]*

* significant at 10%; ** significant at 5%; *** significant at 1%

Specifications denoted by "a" includes year fixed effects, and "b" includes year, industry and city fixed effects as well as firm size categories and share of skilled workers. All regressions include 3040 observations.

Table 5: Export Premia

Dependent Variable	OLS	Quantile Regressions				
		0.1	0.25	0.5	0.75	0.9
Log (Average wages)[a]	0.3	0.14	0.31	0.28	0.35	0.52
	[0.060]*	[0.127]	[0.057]*	[0.050]*	[0.028]*	[0.057]*
Log (Average wages)[b]	0.10	0.11	0.07	0.12	0.12	0.14
	[0.024]*	[0.038]*	[0.029]**	[0.026]*	[0.027]*	[0.055]*

*** significant at 10%; ** significant at 5%; * significant at 1%

Specifications denoted by "a" includes year fixed effects (2911 observations), and "b" includes year, industry and city fixed effects as well as firm size categories and share of skilled workers (2903 observations).

Table 6:

Dependent Variable	OLS	Quantile Regressions				
		0.1	0.25	0.5	0.75	0.9
Log(Value added per worker)[a,1]	0.05	-0.19	0.01	-0.07	0.11	0.22
	[0.129]	[0.127]	[0.049]	[0.078]	[0.021]*	[0.121]***
Log(Value added per worker)[b,1]	0.03	-0.19	-0.02	-0.11	0.06	0.17
	[0.130]	[0.112]*	[0.062]	[0.028*	[0.108]	[0.083]**
Log(Average wages)[a,2]	0.17	0.16	0.71	0.33	0.18	0.10
	[0.099]***	[0.229]	[0.100]*	[0.092]*	[0.043]*	[0.108]
Log(Average wages)[b,2]	0.01	-0.13	-0.02	0.05	0.08	-0.05
	[0.100]	[0.025]*	[0.034]	[0.038]	[0.062]	[0.064]

*** significant at 10%; ** significant at 5%; * significant at 1%

Specifications denoted by "a" includes year fixed effects, and "b" includes year, industry and city fixed effects as well as firm size categories and share of skilled workers.Superscripts 1 and 2 denote 1064 and 998 observations, respectively.

Table 7:

Dependent Variable	Log (VA) - in Levels[a]	Log (VA) - in Levels[b]	Log (Av. Wage) - in Levels[a]	Log (Average Wage) - in Levels[b]
Switcher	0.31	0.29	0.17	0.12
	[0.075]*	[0.081]*	[0.075]**	[0.078]
Continuous exporter	0.64	0.46	0.51	0.41
	[0.087]*	[0.095]*	[0.086]*	[0.093]*
Export starter	0.09	-0.05	0.17	-0.01
	[0.107]	[0.111]	[0.105]*	[0.106]
Quitter	0.32	0.37	0.28	0.33
	[0.094]*	[0.097]*	[0.092]*	[0.093]*
Observations	3040	3032	2911	2903
P-Value for Ho: b[Continuous-exp.] >b[Switcher]	0.00	0.11	0.00	0.97

*** significant at 10%; ** significant at 5%; *significant at 1%

Specifications denoted by "a" includes year fixed effects, and "b" includes year, industry and city fixed effects as well as firm size categories and share of skilled workers.

Table 8:

Dependent Variable	Log (VA) - Growth[a]	Log (VA) - Growth[b]	Log (Av. Wage) - Growth[a]	Log(Av. Wage) - Growth[b]
Export starter	0.33	0.26	0.36	0.03
	[0.169]***	[0.177]	[0.168]**	[0.164]
Observations	1064	1064	992	992

***significant at 10%; ** significant at 5%; * significant at 1%

Specifications denoted by "a" includes year fixed effects, and "b" includes year, industry and city fixed effects as well as firm size categories and share of skilled workers.