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Trade Liberalization, Firm Heterogneity, and Wages

New Evidence from Matched Employer-Employee Data

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Abstract

In this paper, the authors use a linked employer-employee database from Brazil to examine the impact of trade reform on the wages of workers employed at heterogeneous firms. The analysis of the data at the firm-level confirms earlier findings of a differential positive effect of trade liberalization on the average wages at exporting firms relative to non-exporting firms. However, this analysis of average firm-level wages is incomplete along several dimensions. First, it cannot fully account for the impact of a change in trade barriers on workforce composition especially in terms of unobservable (time-invariant) characteristics of workers (innate ability) and any additional productivity that obtains in the context of employment in the specific firm (match specific ability). Furthermore, the firm-level analysis is undertaken under the assumption that the assignment of workers to firms is random. This ignores the sorting of worker into firms and leads to a bias in estimates of the differential impact of trade on workers at exporting firms relative to non-exporting firms. Using detailed information on worker and firm characteristics to control for compositional effects and using firm-worker match specific effects to account for the endogenous mobility of workers, the authors find the differential effect of trade openness on wages in exporting firms relative to domestic firms to be insignificant. Consistent with the models of Helpman, Itskhoki, and Redding (2010) and Davidson, Matusz and Schevchenko (2008), they also find that the workforce composition improves systematically in exporting firms in terms of innate (time invariant) worker ability and in terms the quality of the worker-firm matches.

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Trade Liberalization, Firm Heterogeneity, and Wages: New Evidence from Matched Employer–Employee Data*

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1. Introduction

It is a well-established empirical regularity in the international economics literature that even within narrowly-defined industries, globally-engaged firms and domestic firms differ in terms of their productivity, size, employment composition, and wages (Bernard and Jensen (1995, 1997) and Bernard, Jensen, and Schott (2009)).[1] In the presence of such heterogeneity, trade liberalization will induce inter-firm reallocations within an industry as the more productive, exporting, firms expand and the least productive firms shrink or exit the industry (Melitz, 2003). In an environment with labor market frictions, this within-industry reallocation can have distributional consequences for workers employed in firms with differing levels of global engagement.

The international trade literature has discussed the putative impact of trade liberalization on wages at different levels of aggregation, examining this question alternately at the level of the firm and at the level of the individual worker. There are various channels through which a decline in trade protection could result in differential changes in average wages at exporting firms relative to firms selling only to the domestic market. For instance, if liberalization is associated with a change in relative returns to worker characteristics,[2] differences in work force composition will imply that average firm-level wages in exporting firms will change differentially relative to domestic firms following liberalization. If trade liberalization does not affect the returns to worker characteristics, a differential wage effect may, nevertheless, be observed if changes in trade policy induce compositional changes in the workforce of exporting firms that are different from those in non-exporting firms.[3]

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[1] Recent work extends the findings of Bernard and Jensen (1995) using matched employer-employee data for Germany (Schank, Schnabel, and Wagner (2007) and Klein, Moser, and Urban (2010)), Denmark (Munch and Skaksen 2008), and Mexico (Frías, Kaplan, and Verhoogen 2009). See also Hummels, Jorgensen, Munch and Xiang (2010) for analysis of the impact of outsourcing on wages and employment using matched employer-employee data from Denmark.

[2] A vast empirical literature has examined the effects of globalization on the wage outcomes of workers in the domestic economy with a particular focus on the important question of how trade affects the average wages of workers with different levels of skill. Classic papers in this literature include Lawrence and Slaughter (1993), Leamer (1996), and Feenstra and Hanson (1999). Revenga (1997), Currie and Harrison (1997), and Trefler (2004) analyze the effect of trade liberalization on firm-level wages in Mexico, Morocco, and Canada, respectively, with mixed results. Feenstra and Hanson (2002) and Goldberg and Pavcnik (2007) provide excellent survey treatments.

[3] An expansion in exports could result in differential labor quality upgrading in exporting firms, for example, by inducing these firms to adopt technologies favoring highly skilled workers, as in Yeaple (2005) and Bustos (2011), or by inducing these firms to upgrade product and hence labor quality as in Verhoogen (2008) and Kugler and Verhoogen (2008).

Furthermore, in the presence of labor market frictions, changes in trade protection may result in differential changes, in exporting firms relative to non-exporting firms, in the wages of ex-ante identical workers. For instance, if firms engage in some form of rent sharing with their workers, the wages of workers employed in exporting firms, which experience a relative improvement in their profits or market share after a decline in protection, will correspondingly be better compared to workers employed in firms serving only the domestic market (Egger and Kreickemeier (2009) and Amiti and Davis (forthcoming)). Equally, if the labor allocation process is subject to search and matching frictions, exporting firms may screen (ex-ante identical) workers more intensively, employ workers of higher match-specific-ability, and pay higher wages relative to non-exporting firms as Helpman, Itskhoki, and Redding (2010) have shown in their comprehensive theoretical analysis of the links between trade and labor markets in this context. Here, opening the economy to trade is predicted to alter this wage gap between exporting and non-exporting firms and to affect wage inequality.[4]

Does trade liberalization, in fact, affect differently workers employed in exporting firms relative to non-exporting firms? We explore this issue empirically using a detailed dataset with matched employer-employee data on workers in Brazil for the years 1990-1998. The dataset traces individually identifiable workers across employers over time and contains detailed information on worker characteristics such as age, gender, education, occupation, and tenure at the firm which allows us to suitably account for the role of unobservable worker and firm characteristics in determining wages. We complement this worker-level information with firm-level data on exporter status from the Brazilian Customs Office, and industry-level information on trade protection levels to capture Brazil's main trade policy

[4] More generally, the various channels linking trade protection to wages that we describe are each potentially associated with changes in within-group inequality (inequality between workers with identical observable characteristics), in changes in between-group inequality (inequality between workers with different characteristics, such as, levels of education) and possibly even no change in inequality at all. For instance, under the rent-sharing mechanism, within-group inequality will change following trade liberalization if otherwise identical workers are employed at firms which experience differential profit changes. Alternatively, if trade liberalization changes the returns to worker characteristics, this will result in a change in between-group inequality. Finally, if workers are paid competitive wages and the only impact of trade liberalization is to reallocate workers across firms without any changes in the returns to worker characteristics, there will be no change in either within-group or between-group wage inequality.

reforms.

We begin our analysis by exploring the behavior of average firm-level wages and find that a decline in trade protection is associated with an increase in average wages in exporting firms relative to domestic firms, consistent with the findings of Amiti and Davis (forthcoming) for the liberalization period in Indonesia[5]. However, we argue that the analysis of average firm-level wages, although informative, is incomplete along several dimensions. First, it cannot fully account for the impact of a change in trade barriers on workforce composition in terms of both observable worker characteristics (not available in firm-level datasets), as well as factors that are observable to the managers of the firm and hence impact wages but are unobservable in the data, such as the innate ability of the worker and any additional productivity (ability) that obtains in the context of employment in the specific firm (match specific ability). Furthermore, the firm-level analysis is undertaken under the assumption that the assignment of workers to firms is random and ignores the sorting of worker into firms and the resulting change in the distribution of match-specific ability in different firms. Using matched employer-employee data, we test whether wage behavior at the worker level confirms the maintained assumption of exogenous worker mobility, as in Abowd, McKinney and Schmutte (2010). Consistent with theoretical models emphasizing search dynamics or other frictions as important determinants of the relationship between trade liberalization and wages such as Helpman, Itskhoki, and Redding (2010) and Davidson, Matusz and Schevchenko (2008), we find that the data decisively reject the assumption of exogenous job mobility.

Importantly, using detailed information on worker and firm characteristics to control for compositional effects and using firm-worker match specific effects to account for the endogenous mobility of workers, we find the differential effect of trade openness on wages

[5] To our knowledge Amiti and Davis (forthcoming) is the first paper to incorporate firm-level heterogeneity in empirically studying the impact of trade liberalization on wages. They introduce a general equilibrium model, which combines firm heterogeneity in exporting patterns, trade in intermediate inputs, and firm-specific wages. The latter is incorporated into the model by assuming a fair-wage specification that results in a direct link between firm wages and firm profitability. The model predicts that a decline in final goods tariffs reduces the wages of workers at firms that sell only in the domestic market, but raises the wages of workers at firms that export. Consistent with the model, they find differential changes in average firm-level wages across Indonesian firms with different levels of global engagement following liberalization, which they attribute wholly to firm performance.

in exporting firms relative to domestic firms to be insignificant. Thus, our findings using matched employer employee data suggest a quite different picture of the links between trade liberalization and wages than is obtained by analyzing the data at a more aggregate (firm) level. Consistent with the models of Helpman, Itskhoki, and Redding (2010) and Davidson, Matusz and Schevchenko (2008), we find that the workforce composition improves systematically in exporting firms in terms of innate (time invariant) ability and in terms the quality of the worker-firm matches. This finding also serves to explain the difference between the results at the firm level and those at the worker level. If average worker ability improves systematically in exporting firms following trade liberalization, and this change in not taken into account, it will appear that trade liberalization leads to a differential wage improvement for workers at exporting firms even when this is not the case.

The remainder of this paper is organized as follows. In Section 2, we present a background discussion on Brazil's trade policy reforms and describe the data. We present the empirical methodology and estimation results for the aggregate (firm-level) analysis in Section 3 and for the analysis at the worker level in Section 4. Section 5 concludes.

2. Data and Policy Background

Our main data are administrative records from Brazil for formal-sector workers linked to their employers. We combine this worker-level information with complementary data sources on firm-level exporter status and information on industry-level trade protection during Brazil's main trade policy reform period.

2.1. Brazil's Policy Reforms

The 1990s were a period of dramatic policy reform in Brazil, providing a particularly appropriate setting in which to study the impact of trade liberalization on wages. As compared to the gradual process of globalization in many developed countries, Brazil's trade reform occurred over a relatively short period of time, and with substantial cross-industry variation. Furthermore, many of the policy reforms were arguably unanticipated

and could be viewed as exogenous to changes in wages at the firm and worker level.[6]

The second half of the 20th century in Brazil was characterized by tight import substitution industrialization policies designed to protect the domestic manufacturing sector from foreign competition. Special import regimes and discretionary import controls like the "law of similars", under which goods were banned if they too closely resembled a Brazilian product, were commonplace. Coverage of these quantitative restrictions remained close to 100 percent throughout this period, leaving Brazilian manufacturers highly protected.

The 1990s, however, witnessed sweeping changes in Brazilian trade policy. Beginning with the Collor de Melo administration and continuing with President Cardoso, Brazil began extensive policies of trade liberalization in 1988, which paved the way for the multilateral free trade area, *Mercosul*, with its Southern Cone neighbors (Argentina, Paraguay, and Uruguay) in 1991. Average ad valorem final goods tariff rates fell from 41% to 18% between 1988 and 1989.[7] The federal government abolished all remaining non-tariff barriers inherited from the import substitution era and brought nominal tariffs further down in 1990. Effective rates of protection fell by over 70% in just four years—from approximately 42%, on average, in 1988 to 12%, on average, in 1994 (Kume, Piani, and Souza 2003).

After decades of high inflation and several unsuccessful stabilization attempts, the Brazilian government succeeded with its macroeconomic stabilization plan (*Plano Real*) in 1994 and lastingly ended hyperinflation. The new currency, the *real*, began officially at parity with the U.S. dollar on July 1, 1994, trading freely on international markets and appreciating in its first months. In response, the government partially reversed trade reforms in 1995 after manufacturing industries lost competitiveness due to the *real*'s appreciation;[8] the effective rate of protection climbed slightly in subsequent years from an average of 12% in 1994 to

[6] The same argument could not be made if our analysis focused on industry-level wages, given the substantial evidence for political economy factors influencing trade policy (see, for example, Grossman and Helpman (1994) and Olarreaga and Soloaga (1998) for evidence from Brazil's customs union, *Mercosul*).

[7] These reforms had little impact on import competition however, as non-tariff barriers remained highly restrictive.

[8] Prior to 1994 and the implementation of the new currency, controls on Brazil's former currency, the *cruzado*, had served as yet another form of implicit import protection. In our empirical analysis, we allow for any differential impacts Brazil's exchange rate may have on firms with differing trade exposure.

an average of 18% in 1998.[9]

2.2. Worker data

The linked employer–employee data are from the Brazilian Labor Ministry, which requires by law that all formally-registered firms report to the ministry on all workers in every year.[10] These administrative records have been collected in the *Relação Anual de Informações Sociais* (RAIS) database since 1986. In this paper, we use information from RAIS for the years 1990 through 1998, when we also have complementary data on the export status of firms and industry-level tariff rates.

The main benefit of the RAIS database is the ability to trace individually-identifiable workers over time and across firms. The data include a unique worker-identification number (which remains with the worker throughout his work history), the tax number of the worker's firm, the industrial classification of the worker's firm[11], and the municipality of the worker's firm. These data are particularly valuable as they offer variables beyond the available information in many other firm-level databases, often used in studies like ours. In particular, the data contain detailed information on workers' skill-levels (as defined by occupation[12] and education), which are paramount to this analysis. Other variables of

[9] Trade policy reforms coincided with gradual foreign investment liberalizations and the privatization of state-owned companies, both of which contributed to attracting substantial capital inflows over this time period. Meanwhile, the government's regional development plans also included export promotion policies as explicit elements, helping to boost exports beginning in 1995. In each specification, we include region-specific year dummies to capture the impact of these and other general macroeconomic trends on wages.

[10] The process for firms to report on their workers is extensive and costly. However, this information is used to administer payments of the annual public wage supplements to every formally-employed worker, thus creating a strong incentive for workers to urge their employers to report accurately. In practice, however, only *formally*-employed workers will be properly recorded. Goldberg and Pavcnik (2003) estimate informal workers represent approximately 16% of Brazil's manufacturing labor force over our sample period. The literature on the impact of trade reform on the informal sector offers mixed results (see Goldberg and Pavcnik (2003), Menezes-Filho and Muendler (2007), and Paz (2009)).

[11] The sector classification used in this paper comes from Brazil's statistical office (*Instituto Brasileiro de Geografia e Estatística*, IBGE) and is roughly comparable to a 3-digit NAICS classification (Muendler 2002).

[12] Muendler, Poole, Ramey and Wajnberg (2004) map the Brazilian classification of occupations (*Classificação Brasileira de Ocupações* (CBO)) to the *International Standard Classification of Occupations* (ISCO). The CBO is a detailed, task-oriented classification system, while ISCO reflects a less-detailed and more skill-oriented classification system. The skill classification is intended to incorporate on-the-job experience, informal training, and the technological skill content of the

interest are the worker's annual real wages in Brazilian *reais*[13], tenure at the firm in months, gender, and age.

We restrict observations as follows. First, we draw a one percent random sample from the complete list of workers ever to appear in the national records. We match the sampled workers back to the population data to find all firms in which these workers were ever employed over time to create a complete employment history of this one percent random sample of the population of the Brazilian formal-sector labor force.[14] Next, we keep only workers with valid worker identification numbers to ensure that we can track individuals over time. As is standard in the literature, we include only prime-age workers between the ages of 15 and 64 years, workers with a positive average monthly wage, and workers in private-sector jobs. Finally, for workers with multiple jobs in a given year, only the most recent job is included in the sample. If a worker has multiple current jobs, only the highest paying job is included.

2.3. Complementary data

Export Status Brazilian firms' tax identification numbers are common across many databases, allowing us to match the RAIS data to complementary firm-level data sources. Information on firm-level export transactions is available from the Brazilian Customs Office (*Secretaria de Comércio Exterior*, SECEX). SECEX records all legally-registered firms in Brazil with at least one export transaction in a given year. We match the SECEX firm-level exporter status data to our RAIS worker data by the firm's tax identification code to identify workers at exporting and non-exporting firms. We define an indicator variable equal to one if a worker holds a job at a firm with a positive dollar value of free-on-board exports in a given year and zero otherwise.

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occupation (Elias and Birch 1994). ISCO occupations can be grouped into four broad occupational categories following Abowd, Kramarz, Margolis, and Troske (2001) to reflect the skill-intensity of the occupation.

[13] RAIS reports an average monthly wage (in multiples of the current minimum wage) for each job in which a worker is employed in each year. In combination with information on the number of months a worker was employed during the year and deflated minimum wage information in *reais* from the Brazilian Central Bank, we calculate an annual real wage for each worker.

[14] Note that any firm-level aggregates we use in our analysis, however, are based on the complete population of workers.

As would be predicted by new heterogeneous firm models of international trade, Brazil's trade liberalization in the early 1990s was associated with significant firm-level entry into exporting. The share of exporting firms increased over 50%—from only 8.5% in 1990 to 12.9% in 1994 before leveling off.

Tariffs In our analysis of Brazil's trade policy, we concentrate on two trade protection measures: the final goods output tariff and the effective rate of protection (ERP). The effective rate of protection allows us to incorporate changes in tariffs placed on inputs into a firm's production process. Our data on final goods tariffs are from Muendler (2003), who reports monthly nominal final goods output tariffs at the *Nível 80* Brazilian industrial classification level. We access monthly ERP at the *Nível 80* industrial classification level from Kume, Piani, and Souza (2003).[15] We match the December tariffs from 1990 to 1998 with our annual RAIS worker data by the 2-digit IBGE subsector[16] to identify workers and firms in industries with differential rates of protection and liberalization experiences.

Figure 2.1 displays both the mean and median values of the effective rate of protection in the manufacturing sector during the 1990 to 1998 period. The early 1990s experienced sharp declines in the effective rate of protection. Mean rates fell from approximately 60 percent to 20 percent, while median rates fell from 40 percent to 20 percent in a half-decade. The slight aforementioned protectionist response to the appreciation of the *real* is also evident. Most strongly in the early part of the decade, median ERP was smaller than average ERP, suggesting that the distribution of the effective rate of protection is skewed to the right. Over time, as the sectoral variation narrows, the mean ERP and median ERP converge.

This substantial cross-industry variation in both levels and changes in the ERP is

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[15] Kume, Piani, and Souza (2003) formally measure ERP for sector k as the increase in the value-added due to the structure of tariffs relative to value-added at free trade prices, as follows:

$ERP_k = \tau_k - \frac{\sum a_{mk}\tau_k}{1 - \sum a_{mk}}$, where $a_{mk} = \frac{a^d_{mk}(1+\tau_k)}{1+\tau_m}$ is the input-output matrix at free-trade international prices, a^d_{mk} is the input-output matrix at distorted domestic prices, and τ_k and τ_m are the final goods and intermediate inputs tariffs, respectively.

[16] We follow industry concordances available at *http://econ.ucsd.edu/muendler/brazil* to concord the *Nível 80* classification to the 2-digit IBGE subsector classification used in RAIS. This constitutes the IBGE subsectors 2 through 13.

documented in more detail in Figure 2.2 where we present the distribution of tariffs across industries in 1990 and 1998, and the average annual change in ERP during this period. Note that compared to 1990, the distribution of the effective rate of protection across industries at the end of our sample is much more compressed around a lower mean. We also note substantial variation across sectors in the average annual changes in protection rates.

2.4. Descriptive statistics

The complete matched data include 3,932,297 worker-firm-year observations, with 657,572 workers in 490,884 firms. Our final sample of the manufacturing sector, however, has 504,660 worker-firm-year observations, with 114,042 workers in 58,578 firms. We report detailed descriptive statistics in Table 2.1. Roughly three-quarters of the formal-sector labor force have at most a primary school education. An additional 18% of manufacturing sector workers are high school educated, and only 7% of workers have a college degree. The majority of Brazil's labor force is employed in skilled blue-collar occupations, like machine operators and assemblers. Almost 20% of the manufacturing workforce is in professional or managerial positions, with 11% in unskilled blue-collar jobs. Other white-collar workers (for example, those in secretarial and office assistant occupations) represent only 8% of the formal sector manufacturing labor force. The average number of employees in manufacturing firms is relatively small at 73.

There are 64,212 workers working in 11,143 exporting firms, and 80,895 workers working in 53,537 domestic firms over the sample period. Exporters pay a substantially higher average wage than do non-exporters. Exporters employ a higher share of skilled workers, on average, compared to non-exporting firms, consistent with the existing literature. Almost 10% of workers at exporting firms are college educated and 21% are high-school graduates. In comparison, in non-exporting firms the share of college educated and high-school educated workers are 4% and 17%, respectively.[17] Exporters also employ a higher share of workers in professional, managerial, and technical occupations. Exporters are significantly larger in terms of their average employment compared to non-exporters. The average exporter employs 346 employees, while the average non-exporter has only 37 employees.

[17] Note that in a country like Brazil, the share of workers with a high-school education is a more meaningful representation of skill (Gonzaga, Menezes-Filho, and Terra 2006).

The set of firm level variables available in our dataset allow us to appropriately control for these differences between exporters and domestic firms in identifying the heterogeneous impact of trade policy on wages of these firms.

3. Firm-Level Analysis

We begin our analysis at the firm level to ensure the comparability of our results with those of the existing literature based on firm-level data and to highlight the importance of introducing worker and match heterogeneity into the analysis. To this end, we aggregate the matched employer-employee data to the firm level and estimate the following specification:

$$\ln \overline{y_{jt}} = \gamma_1 t_{kt} + \gamma_2 t_{kt} * Exp_{jt} + \gamma_3 RER_t * Exp_{jt} + \gamma_4 Exp_{jt} + \Psi_j + \delta_{tr} + \beta Z_{jt} + \varepsilon_{jt} \quad (1)$$

where the dependent variable, $\ln(\overline{y_{jt}})$, is the logarithm of average wages at the firm level for firm *j* at time *t.* t_{kt} denotes the level of protection in sector *k* in which the firm operates, and Exp_{jt} is an indicator variable equal to one if firm *j* reports a positive dollar value of exports at time *t* and zero otherwise. The level of protection at the sector-level is measured by both tariffs and the effective rate of protection (ERP). We use the latter measure in our main specifications, since in an environment in which Brazilian firms face declines in both final goods and intermediate input tariffs, the ERP is a more appropriate measure of protection faced by domestic firms. In each specification, we include an interaction term between t_{kt} and Exp_{jt} to allow for changes in protection to have differential effects on exporters and firms serving only the domestic market.

As we noted earlier, the post-liberalization period in Brazil coincided with a period of an appreciation of the currency, the *real*, making Brazilian goods less competitive on international markets, while making imported goods cheaper in *real* terms. Failing to incorporate such fluctuations in exchange rates into our analysis could bias the estimated effect of liberalization on wages. Henceforth, in each specification, we also include an

interaction of Brazil's real exchange rate (RER)[18] and the firm's export status.[19]

The time-varying, firm-level controls, Z_{jt}, include variables available in standard firm-level data sets such as log employment, and the occupational skill composition[20] of the firm in addition to average worker tenure at the firm, and controls for the age[21], gender, and educational skill composition[22] of the firm.[23] Each specification also includes firm fixed effects, ψ_j, accounting for time-invariant, firm characteristics and interactive region-year fixed effects, δ_{tr}, capturing the average effect of policy changes that may differentially impact wages of firms in different regions of Brazil.[24] Here, ε_{jt} is an error term that is assumed to exhibit no serial correlation, and to be orthogonal to all regressors. In each specification, the standard errors are clustered at the industry-year level to account for the possibility of within-industry, across-firm correlation in errors following Moulton (1990).

In interpreting our estimates from specification (1) we focus specifically on the magnitude of the differential change in average firm-level wages at exporters relative to non-exporters (γ_2) as well as the overall wage impact of a decline in protection for exporters ($\gamma_1 + \gamma_2$) and non-exporters (γ_1), separately. The responsiveness of average wages in firms serving only

[18] The real exchange rate series for Brazil is constructed in Muendler (2003) and is available at *http://www.econ.ucsd.edu/muendler/html/brazil.html#brazdata*.

[19] Since the overall impact of the time-varying, economy-wide RER is absorbed by the region-specific year effects (δ_{tr}), we can only separately identify the effect of the RER changes on exporting firms *relative* to domestic firms. We also conduct robustness checks using industry-specific exchange rates.

[20] We define the firm's occupational skill composition as the share of the firm's workforce in four occupational categories: unskilled blue collar, skilled blue collar, other white collar, and professional and managerial workers. Unskilled blue-collar workers are the omitted category.

[21] We define the firm's age composition as the share of the firm's workforce in six age categories: youth (15-17), adolescent (18-24), nascent career (25-29), early career (30-39), peak career (40-49), and late career (50-64). Youth workers are the omitted category.

[22] We define the firm's educational skill composition as the share of the firm in three education categories: less than high school, at least high school, and more than high school. Less than high school is the omitted category.

[23] By including both the educational and the occupational skill composition of the firm, we are able to allow for the possibility that firms use increasingly higher-skilled individuals (as defined by education) in lower-skilled occupations. See Muendler (2008) for evidence on the skill upgrading of occupations in response to trade reform in Brazil.

[24] We consider Brazil's five main geographic regions: the North, Northeast, Center-West, Southeast, and South.

the domestic market to changes in protection is reflected in the coefficient γ_1. A positive γ_1 would suggest that a decline in protection is associated with a decrease in average wages in firms serving solely the domestic market. Note that when ERP is the measure of protection (instead of tariffs), γ_1 would reflect a combined effect of the positive impact of a reduction in input tariffs through prices and access to enhanced variety and quality on firm profits[25], as well as any negative impact of increased import competition due to a decline in output tariffs. Hence, we expect the coefficient to be smaller in magnitude when the measure of protection is ERP compared to the estimated coefficient when protection is measured by (output) tariffs.

The coefficient on the interaction term, γ_2, reflects the differential effect of trade policy changes on average wages in exporting firms relative to firms serving only the domestic market. If a decline in protection results in a differential increase in firm-level average wages in exporting firms, we expect $\gamma_2 < 0$. There are various reasons a decline in protection could result in a differential increase in firm-level average wages of exporting firms. For example, if firms engage in some form of rent-sharing with their workers, workers employed in exporting firms, whose prospects improve as a result of a decline in protection, could experience an increase in their wages. Similarly, if liberalization is associated with a change in the relative returns to skill, and if exporting firms differ in terms of their labor force composition, average firm-level wages in exporting firms will increase relative to domestic firms. Also, the relative increase in wages in exporting firms could be a reflection of a change on workforce composition in terms of factors that are observable to the managers of the firm and hence impact wages, but are unobservable in the data (such as innate worker ability or match-specific ability).

3.1 Estimation Results

[25] See Arkolakis, Demidova, Klenow, and Rodriguez-Clare (2008) and Goldberg, Khandelwal, Pavcnik, and Topalova (2008) for evidence on improved input variety, and Kugler and Verhoogen (2008), Amiti and Konings (2007), Csillag and Koren (2009), and Halpern, Koren and Szeidl (2009) for evidence on improved input quality.

Estimation results from equation (1) with tariffs[26] as the measure of protection are reported in Table 3.1. The results suggest that a decline in tariffs is associated with a decline in average wages at non-exporting firms, consistent with a negative impact of an increase in foreign competition on these firms. We find that a ten percentage point decrease in tariffs leads to a decrease in average firm-level wages by 1.7% for these firms. The negative and significant coefficient on the interaction term between tariffs and export status suggests that the wages in exporting firms increase in response to a decline in tariffs relative to firms serving only the domestic market. We find that a change in tariffs has no statistical impact on average wages at the firm-level for exporting firms.[27] The RER-exporter interaction term suggests that a depreciation in the RER (a decrease in the RER as is it defined in our data) increases the wages in exporting firms relative to non-exporting firms, as expected. All the firm-level controls are statistically significant and enter with the expected signs and magnitudes. Average firm-level wages are increasing with the average tenure and age-profile at the firm, while they are decreasing in the share of female workers employed at the firm. Wages are increasing in both the educational and occupational skill composition of the firm and the size if the firm.

Next, we test whether the differential impact of tariffs we document on exporting firms holds equally for firms operating in all industries. More specifically, we allow for the effect of a change in tariffs to be different for firms operating in Brazil's comparative advantage sectors. We expect that following liberalization, exporters in these sectors will experience a more pronounced increase in profitability relative to exporters in low comparative advantage sectors and hence, expect a stronger impact on average wages at these firms. We divide our sample into high and low comparative advantage sectors using data on each industry's Balassa (1965) comparative advantage.[28] The next two columns in Table 3.1 report our results by the comparative advantage of the firm's sector where high (low) comparative advantage sectors are those with an above (below) median value of the

[26] Our estimation results are essentially the same when we use tariffs weighted by the value added of the industry instead of un-weighted tariffs.

[27] The absolute effect on exporters, the *F-statistic* and corresponding *p-value* on the joint significance of the coefficients are reported at the bottom of Table 3.1.

[28] Industry *k*'s Balassa (1965) comparative advantage in year *t* is constructed in Muendler (2007) as follows:

$$\text{Comp\_Adv}_{k,t} = \frac{X_{k,t}^{Brazil} / \sum_m X_{m,t}^{Brazil}}{X_{k,t}^{World} / \sum_m X_{m,t}^{World}}$$ where X_{kt} are exports.

Balassa (1965) comparative advantage measure across all merchandise trade sectors in 1986.[29] [30] Interestingly, we find the differential impact of trade liberalization on exporters to be prevalent only for firms operating in high comparative advantage sectors. Firms in low comparative advantage sectors experience a decline in average wages regardless of their export status.

During the liberalization period, Brazilian firms faced declines in both final goods and in intermediate input tariffs. While a decrease in final goods tariffs could decrease the profitability of domestic firms by increasing the foreign competition that they face, a decrease in input tariffs is likely to have the opposite effect by decreasing the price of inputs, and improving the variety and possibly quality of inputs that the firm has access to. If the industries that experienced a decline in final goods tariffs also experienced a decline in input tariffs, the product tariff is likely to overestimate the actual decrease in protection that the industry has experienced. To address this issue, we repeat the previous analysis with the effective rate of protection (ERP) instead of (output) tariffs as the measure of protection. The estimation results suggest that a decline in ERP has no significant impact on average wages at non-exporting firms. Improved access to imported intermediates from abroad could explain the difference between these results and those reported in first half of Table 3.1. Note that this coefficient now reflects a combined effect of the positive impact of a reduction in input tariffs through prices and access to variety and quality, as well as any negative impact of increased import competition due to a decline in output tariffs. The negative and significant coefficient on the interaction term suggests that the wages in exporting firms increase in response to a decline in the ERP. A ten percentage point decrease in the ERP increases average wages by 1% at exporting firms. Similar to the results for output tariffs, in the high comparative advantage sector, we find that a decline in ERP is associated with a differential increase in wages in exporting firms relative to their non-exporting counterparts. The increase in wages associated with a ten percentage point

[29] We choose the pre-reform year of 1986 to avoid any possible endogeneity between the comparative advantage measure based on exports and tariff reforms which began in 1988. We also experimented with generating the comparative advantage measure across only manufacturing sectors and using a post-reform year with no difference in the results. Brazil's Balassa (1965) revealed comparative advantage is largely time-invariant.

[30] The following sectors are defined as high comparative advantage sectors: Manufacture of non-metallic mineral products, Manufacture of metallic products, Manufacture of transport equipment, Manufacture of wood products and furniture, Manufacture of footwear, and the Manufacture of food, beverages, and ethyl alcohol.

decrease in ERP is 2.9% for exporters while it is 0.8% (and significant only at 10% level) for firms serving the domestic market. The impact of trade liberalization on average firm wages is not statistically significant for domestic firms or exporters in low comparative advantage sectors. Our results also indicate that the exporter premium is slightly higher for exporters in the high comparative advantage sector.

A potential concern for the results reported in Table 3.1 is the selection bias introduced by the heterogeneity of firms exiting the sample following trade liberalization. If a decline in ERP results in the exit of low productivity firms, the negative effect of liberalization on domestic firms will be underestimated, as the remaining firms in the sample would have high productivity and pay high wages. To evaluate the relevance of this possibility in explaining the differential impact we find on exporting firms, we repeat our analysis for a balanced panel of firms, which were under operation during the entire sample period. Results reported in the first column of Table 3.2 suggest that restricting our sample to those firms, which do not enter or exit, does not alter the magnitude or the significance of our main coefficient of interest, suggesting that our results are not driven by sample-selection.[31]

We provide further robustness checks in the remaining columns of Table 3.2. As we address in the data section, our main firm-level regressions draw on the complete employment history of a 1% random sample of the population of Brazil's formal-sector labor force. To ensure this data is representative of Brazilian firms and industries, in the second column of Table 3.2, we repeat the analysis drawing on the complete employment history of a 5% random sample of formal-sector males living in metropolitan areas. In the third column, we restrict the data to Brazil's main trade reform period (1990-1994) when average protection levels consistently declined. In both cases, our main coefficient of interest remains significant with little change in its magnitude. The results reported in the next column suggest that our findings are robust to replacing the economy-wide real exchange rate with industry-specific real exchange rates in order to capture differences in the relative

[31] Similarly, if the least productive exporters switch out of exporting following liberalization, the magnitude of the differential effect of a decline in ERP on exporters will be biased upwards. In unreported regressions, available by request, we include log total factor productivity as an additional explanatory variable based on estimates from Muendler (2004). We find the effect of log TFP on average firm wages to be insignificant. Importantly, the inclusion of log TFP does not alter the main coefficient of interest.

importance of trading partners across industries.[32]

We also test whether our results are sensitive to the exporting thresholds we use to assign the indicator variable denoting a firm's export status. In the main firm-level specifications, a firm is defined as an exporter if it exported any positive dollar amount that year. Instead, in columns 4 and 5, we only consider firms with an export value more than the 5th percentile (cutoff 5) in that year, and more than the 10th percentile (*cutoff 10*) as exporting firms, respectively. Next, we replace the exporter status dummy in our main specification with two time-invariant measures of export status. *Export90* takes the value 1 if the firm was an exporter as of the beginning of our sample, and zero otherwise. *Export_Once* takes value 1 if the firm exported for at least one year during the sample period. Our estimation results continue to suggest a differential positive impact of liberalization on wages at exporting firms relative to non-exporters. In the last column, we report results from a specification in which we include the logarithm of the value of exports for firms with positive exports. Our results indicate that conditional on exporting, there is a differential increase in wages at firms with higher values of exports following liberalization.

Next, we test whether the differential impact on exporters we document could simply be attributed to compositional differences between exporters and non-exporters in an environment in which the returns to observable worker characteristics are changing as a result of liberalization. Specifically, we allow for changes in the premium paid by firms with different workforce skill compositions, by interacting ERP with measures of labor force composition at the firm level in equation (1) and find no impact on our main coefficient of interest. This finding suggests that the differential effect that we find cannot be explained by changes in the relative returns to observable characteristics following liberalization.[33]

Finally, we consider the possibility that an omission of the importer status of the firm could

[32] We construct industry-specific real exchange rates using time-varying trade weights, as in Goldberg (2004). More specifically, we calculate

$$RER\_ind_t^k = \sum_c \left(\left(.5 \frac{X_t^{kc}}{\sum_c X_t^{kc}} + .5 \frac{M_t^{kc}}{\sum_c M_t^{kc}} \right) RER_t^c \right)$$ where RER_t^c are the bilateral exchange rates for trading partner *c* of Brazil, X_t^{kc} and M_t^{kc} are exports and imports in industry *k* to or from country *c* at time *t*. Note that RER_t^c is also included in this specification.

[33] These results are not reported to conserve space and are available upon request.

bias our results if improved access to foreign intermediate inputs increases wages at the firm level and if the firm's export status is correlated with its import status. Amiti and Davis (forthcoming) find this effect to be important in Indonesia: following a decline in input tariffs, average wages in importing firms increase relative to firms that do not import. Our estimation results based on a sub-sample of firms for which data on import status is available suggest no such differential impact on importing firms in Brazil. Moreover, the differential impact of liberalization on average wages in exporting firms is robust to various specifications controlling for the import status of the firms. Appendix includes a more detailed description of these results.

3.2 Discussion of Firm-Level Analysis

Our firm-level analysis confirms findings in earlier studies regarding the differential impact of trade reform on average wages at firms with differing degrees of trade exposure, especially in high comparative advantage sectors. However, this analysis of average firm-level wages, although informative, is not well suited to examine the differential impact of liberalization on workers in heterogeneous firms for a number of inter-related reasons. In general, we would expect that in addition to observable worker and firm characteristics, the allocation of workers to firms is a function of the worker characteristics that are unobservable in the but the managers can observe and reward, such as the innate (time-invariant) ability of the worker and any additional productivity (ability) that obtains in the context of employment in the specific firm (match-specific ability). Recent theoretical models (including Mortensen (2003), Postel-Vinay and Robin (2002), Lentz (2010), Shimer (2005)) and Gibbons et al. (2005)) variously describe the central role played by ex-ante unobservable worker characteristics in determining the equilibrium assignment of workers to firms. In the context of international trade and labor markets, Helpman, Itskhoki, and Redding (2010), who model the labor allocation process with heterogeneous firms as being subject to search and matching frictions, find that the more productive, exporting, firms will screen workers more intensively and employ workers of higher match-specific ability, i.e., the equilibrium assignment of workers is again a function of unobservable match specific worker ability and is thus non-random.

Importantly, in an environment in which firms are changing the composition and quality of

their labor force in response to liberalization,[34] analysis conducted at the firm level faces at least two problems. If exporting firms respond to liberalization by changing the composition of their workforce systematically, (for example, towards workers with higher innate ability or match-specific ability) our estimates will be biased. This is because part of the differential effect we find for exporters at the firm-level could be due to compositional differences between firms with different trade orientation and not because otherwise identical workers are being paid different wages in different firms. Furthermore, when the job mobility of workers is at least partly determined by their unobservable characteristics (endogenous mobility), estimates of the parameters in (1) will be biased. This is because non-random job assignment implies a correlation between the error term ε_{jt} (which subsumes the un-observables associated with workers in firm *j* at time *t*) and the firm's characteristics represented by the right hand side variables, Ψ_j and Z_{jt}, and thus a failure of the maintained assumption, $E(\varepsilon_{jt} \mid \Psi_j, Z_{jt}) = 0$, underlying the estimation.

In the following section, we describe the worker-level analysis we undertake using matched employer-employee data to account for the issues of unobservable worker ability differences and endogenous job mobility that we have just described. We also describe in much greater detail the issue of endogenous job mobility which persists, in principle, with worker-level analysis, but can be dealt with using firm-worker match effects in the wage specification.

4. Worker-Level Analysis

We begin our worker-level analysis by considering the basic specification of Abowd, Kramarz, and Margolis (1999) in which a worker's wages can be decomposed as follows:

[34] In unreported results available upon request, we provide some evidence of differential skill upgrading at exporting firms relative to non-exporting firms with trade liberalization. Specifically, we re-estimate equation (1) with the share of workers with different levels of education as the dependent variable. While a change in the ERP has no significant impact on the skill composition of workers in non-exporting firms, in exporting firms a decline in the ERP is associated with an increase in the share of college-educated workers and a decrease in the share of workers with less than high school education.

$$\ln y_{ijt} = \alpha_i + \psi_{j(i,t)} + \phi X_{it} + \beta Z_{jt} + \varepsilon_{ijt} \quad (2)$$

where *i* indexes the individual, *j* indexes the firm, *t* indexes time, and $\ln y_{ijt}$ denotes individual-level log wages. The panel of linked worker-firm data allows us to control for a rich array of fixed factors that may influence a worker's wages, in addition to time-varying, observable, firm characteristics (Z_{jt}) and worker characteristics (X_{it}) including indicator variables for the worker's occupation, age, and education, as well as the worker's tenure at the current firm. The model also includes individual fixed effects, α_i, which allow us to control for any are time-invariant unobservable worker characteristics, and firm fixed effects, $\Psi_{j(i,t)}$, for firm *j* at which worker *i* is employed at time *t*, representing firm heterogeneity. Equation (2) could then be augmented to include sector level measures of protection interacted with the firm's export status in order to test the contribution of unobservable differences across workers on the differential response to trade reform.

4.1 Endogenous Worker Mobility

It is important to note that the identifying assumption for equation (2) is that the idiosyncratic disturbance term in each period is independent of observable worker and firm characteristics as well as firm and worker fixed-effects.

$$E(\varepsilon_{it} \mid \alpha_i, \Psi_{j(i,t)}, X_{it}, Z_{jt}) = 0$$

Often referred to in the literature as the assumption of "conditional exogenous mobility" (see, for instance, Abowd, Kramarz, and Margolis (1999), Woodcock (2008) and Soerensen and Vejlin (2009)), the assumption implies that employment mobility could depend on time-varying observable worker or firm characteristics and firm and worker fixed effects, but not ε_{it}. This assumption is at odds with many well-known models of the labor market with directed search, learning, or coordination frictions. For example, as we have discussed before, in Helpman, Itskhoki, and Redding (2010), workers are *ex ante* identical and job

allocation is determined on the basis of match specific ability that is heterogenous *ex post.* Furthermore, high productivity firms (exporters) screen more intensively resulting in higher quality firm-worker matches. In this case the estimates in our firm-level analyses will be biased, due to an omitted worker-firm match effect. Similarly, if workers with certain observable characteristics are more successful at generating good matches than others (for example, because the return from search is higher or due to learning) and hence higher wages, omitted match effects could also bias the estimated returns to observable characteristics in equation (2).

We test the validity of the exogenous mobility assumption using the "match effects test" introduced by Abowd, McKinney, and Schmutte (2010). The test statistic is based on estimated match effects that are computed from the average (over time) residual for a person *i* at a firm *j*. The test rests on the logic that the match effect, under the null of exogenous mobility, should not predict the transitions of workers between firms. Specifically, under exogenous mobility, an individual's average residual, $\overline{\varepsilon_{ijt-1}}$ (within quintiles of the residual distribution) from the most recently completed job should not predict the transition across firms with heterogeneous values of $\Psi_{j(i,t)}$ (say from a particular quintile of the $\Psi_{j(i,t)}$ distribution to another). The test is implemented as follows. First, equation (2) is estimated for the full sample of workers. Then, for workers who switched employers between time *t* and *t-1*, the average residual within person and firm ($\overline{\varepsilon_{ijt-1}}$) is calculated across all years throughout the duration of the match (i.e., until *t-1*); $\overline{\varepsilon_{ijt-1}}$ represents the match effect for the firm and worker pair. Under the null hypothesis, the transition rates across quintiles of the firm effects distribution, from the $\Psi_{j(i,t-1)}$ quintile to the $\Psi_{j(i,t)}$ quintile, are independent of $\overline{\varepsilon_{ijt-1}}$. Importantly, if the null is rejected for our data, this would suggest that the estimation results from equation (2) are biased and that firm-worker match effects should be included in the specification to account for the endogenous mobility of workers.

In constructing the test statistic, we restrict the sample to those workers with at least two years of data and to firms in the manufacturing sector. Since estimates of firm fixed effects for firms with only few movers are likely to be imprecise, Abowd, Kramarz and Margolis

(1999) suggests grouping small firms together and estimating one $\Psi_{j(i,t)}$ for these firms with few movers. We calculate the test statistic both by grouping small firms with less than two movers into one firm and by excluding these small firms from the sample. In both cases, we conduct the test on the largest mobility group, since the firm fixed effects estimated for unconnected groups are not directly comparable with each other.[35] Consistent with the Abowd, McKinney, and Schmutte (2010) finding for the U.S., the test statistic strongly rejects the null hypothesis of exogenous mobility for the sample of job switchers in our data. The match effects test statistic has a value $\chi^2 = 25{,}000$ and is distributed chi-sq with 496 of degrees of freedom (and thus a p-value of 0.000).[36] This finding confirms the relevance of models of labor allocation involving search dynamics or other frictions (such as Helpman, Itskhoki and Redding (2010) and Davidson, Matusz and Schevchenko (2008)) and highlights the importance of allowing for the possibility of firm-worker match heterogeneity in wage determination to account for the endogenous mobility of workers.

To further emphasize this point, we plot, in Figure 4.1, the conditional distribution of quintiles of the firm fixed effects for the previous job, $\Psi_{j(i,t-1)}$, given the quintiles of the individual's average residual from the most recently completed job, for the sample of job changers. Under the assumption of exogenous mobility, the distribution of $\Psi_{j(i,t-1)}$ should not show any variation across quintiles of the average residual. That is to say, the estimation strategy requires that the quality of the firm-worker match in the previous job should not contain any information about the estimated firm-fixed effects for that job. Figure 4.1 clearly demonstrates that this is not the case in our data. For example, while the job changers in the first quintile of the match effect (residual) distribution mostly originate from the second quintile of the $\Psi_{j(i,t-1)}$ distribution, most switchers in the second quintile of the match effect originate from the first quintile of the $\Psi_{j(i,t-1)}$ distribution.

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[35] A connected group includes all the workers who have ever worked for any of the firms in that group as well as all the firms at which any of these workers were employed at during the sample period. Since within each group the mean-deviated firm fixed effects sum to zero, the estimates of $\Psi_{j(i,t)}$ are not directly comparable across unconnected groups. As a possible solution, one can normalize the fixed effects so they have the same mean across groups (Abowd, Creecy, and Kramarz (2002)). Our results are robust to using the full sample of workers and correcting the fixed effects for the unconnected groups in this fashion.

[36] The degrees of freedom are calculated as: $(\#Q(\theta_i)*\#Q(\psi_{j(i,t-1)})*\#Q(\psi_{j(i,t)})-1)*\#Q(\overline{\varepsilon_{it-1}})-1)$ =(5*5*5-1)*(5-1) where #Q denotes the number of quintiles.

In Figure 4.2 we plot the transition rates from a job in a given $\Psi_{j(i,t-1)}$ quintile to a job in $\Psi_{j(i,t)}$ quintile, again for the sample of job changers. Here, we find strong evidence that job transitions are not random; most workers move between jobs within the same employer-effect quintile, which is evident from the rightward movement of the peak of the $\Psi_{j(i,t)}$ distribution with higher quintiles of the original job. Moreover, Figure 4.3 clearly illustrates that these transition probabilities vary considerably by different quintiles of the match effect distribution. Figures 4.3a, 4.3b, and 4.3c plot the transition probabilities for the first, third and fifth quintiles respectively, of the residual distribution. For example, while job switchers are much more likely to improve their employer effect in the middle and (to a lesser extent) in the higher end of the residual distribution, we find this not to be the case in the first quintile of the match effect distribution. This illustrates the failure of the exogenous mobility assumption as the estimated match effects clearly contain information on job-to-job transitions that take place in the data.

Our findings on endogenous worker mobility have interesting implications for the analysis of trade and labor markets, especially since the workings of the labor market have been modeled in a number of different ways in the recent literature on international trade with heterogeneous firms. In the well-known paper of Melitz (2003), heterogeneous monopolistically competitive firms pay their (homogeneous) workers an identical wage, with the assignment of particular workers to firms being (effectively) random. Similarly, in Egger and Kreickemeier (2009) and Amiti and Davis (forthcoming), the assignment of workers to firms is random, even if rent-sharing behavior of firms implies that identical workers may be paid different wages, based on the profits of the firms in which they are employed. The decisive rejection of the assumption of exogenous worker mobility in our data supports a picture of the labor market that is closer in line with the framework of Helpman, Itskhoki, and Redding (2010), where, as we have previously discussed, the labor allocation process is subject to search and matching frictions and the equilibrium assignment of workers to firms is clearly non-random.

4.2 Estimation with Match Effects

If match-specific effects are important in wage determination, the Abowd, Kramarz, and Margolis (1999) specification in equation (2) including only worker and firm fixed effects will result in both biased estimates of these fixed effects, as well as biased estimates of the returns to observable worker and firm characteristics (Woodcock (2008)). For example, if more experienced workers are likely to draw better matches, omission of the match effect will result in an over estimation of the returns to experience. In the context of the international trade literature, if the labor market functions in the manner described by Helpman, Itskhoki and Redding (2010), the screening thresholds for match-specific ability will be different in the resulting equilibrium after liberalization, shifting the distribution of worker abilities (i.e., the quality of matches) within each firm. Given that this shift varies systematically with the export status of the firm, not controlling for match quality will result in biased estimates of the differential effect of trade liberalization on wages in exporting firms.

To account for the fact that a worker's employment history may not be independent of the idiosyncratic part of the residual in equation (2), we now consider a more elaborate specification of wages, in which worker-firm match fixed effects (or spell fixed effects), M_{ij}, which denote a given worker *i*'s employment at a given firm *j*, are included on the right hand side:

$$\ln y_{ijt} = \gamma_1' t_{kt} + \gamma_2' t_{kt} * Exp_{jt} + \gamma_3' RER_t * Exp_{jt} + \gamma_4' Exp_{jt} + M_{ij} + \delta_{tr} + \phi X_{it} + \beta Z_{jt} + \varepsilon_{ijt} \quad (3)$$

Note that since for each spell of a worker within a firm neither the worker nor the firm fixed effect varies, inclusion of match fixed obviates the need for separate inclusion of worker and firm fixed effects.

4.3 Estimation Results

Table 4.1 includes estimation results from equation (3) with spell fixed effects with both tariffs and ERP as the measure of protection. Column 1 presents results with tariffs taken as the measure of trade protection in the sector. Our results suggest that the inclusion of spell fixed effects results in insignificant estimates of both γ_1' and γ_2'. The insignificant effect of a decline in protection on both exporters and firms serving the domestic market also holds

separately for the high and low comparative advantage sectors, although the interaction term is more than three times higher in magnitude for the high comparative advantage sector. These results continue to hold when we use effective rate of protection in the sector as our measure of trade protection.

In Table 4.2 we present a wide array of robustness checks we have conducted. The first two columns report estimation results from specification (3) for various alternative samples. In column 1, we use a 5% random sample of males in metropolitan areas instead of a 1% random sample of the full population. In column 2 we restrict our analysis to only the liberalization period (1990-1994) during which average ERP was decreasing. Our results are robust to these alternative samples and to using an industry-specific real exchange rate measure as in column 3, instead of an economy-wide measure. Next, we test whether our result of insignificant interaction coefficient is sensitive to the exporting thresholds we use to assign the indicator variable denoting a firm's export status. In the baseline specifications a firm is defined as an exporter if it exported a positive dollar amount that year. Instead, in columns 5 and 6, we only consider firms with an export value more than the 5th percentile (cutoff 5), and more than the 10th percentile (cutoff 10) as exporting firms, respectively. While the magnitude of the interaction term increases slightly as we increase the cut off threshold, the coefficient remains statistically insignificantly different than zero. In the next two columns we consider two time-invariant measures of export status. In column 7, a firm is defined as an exporter if it exported a positive dollar value at the beginning of our sample at 1990 (Export90). The export status variable in column 8 takes the value 1 if the firm reported positive exports during every year between 90 and 98. Finally, when we use the magnitude of exports for the sub-sample of exporting firms to represent the relevance of exports to the firm instead, we obtain yet again estimates of γ_1' and γ_2' that are insignificantly different from zero.[37]

Our findings using worker level data and taking endogenous worker mobility into account, suggests an insignificant differential effect of trade policy on the wages of workers

[37] Other robustness checks we have conducted but we do not reported to conserve space include defining a firm as exporter if the firm exported only to non-Mercosur countries (and omitting exporters to Mercosur countries from the sample) and using a more detailed industry classification available only for the 1994-1998 period, which allows us to identify changes in industry level ERP at a more disaggregated level. We find our main conclusions to be robust.

employed in exporting firms. This finding stands in sharp contrast to results obtained using average firm level wages instead. One reason for this difference is simply that the use of detailed worker level data allows us to take into full account any changes in the composition of the workforce (by controlling for both observable and time-invariant unobservable worker characteristics) following trade policy. Furthermore, by taking worker-firm match effects into account, we are able to control for changes in the composition of firms' match quality following trade policy changes. If, following trade liberalization, exporting firms improve their average match quality by hiring workers with better match quality, the estimates of the coefficient γ_2' without controlling for match effects would mistakenly be estimated as significant even in the absence of any true effect.

Table 4.3, which compares the changes in the distribution of estimated match effects in exporting firms relative to non-exporting firms, confirms this point.[38] Comparing the period 1998 to 1990, we see an increase in both the mean and the median match effect in exporting firms, while the mean and median match effect both fall in non-exporting firms. As we have noted earlier, match fixed effects in equation (3) absorb both worker and firm fixed effects in addition to time-invariant match quality of a given employment spell. Consequently, our finding of an improvement in average match effect in exporting firms between 1990 and 1998 summarizes the combined effect of a change in workforce composition in these firms in terms of improvement in worker quality in time invariant worker specific characteristics, such as innate ability (captured by worker fixed effects) and improvement in the quality of the worker-firm matches (captured by match-fixed effects).[39] The improvement in the distribution of match-specific ability in exporting firms is consistent with the Helpman, Itskhoki, Redding (2010) model which predicts that with trade liberalization, exporting firms will screen more intensively and set a higher ability threshold for employment. It is also roughly in line with the prediction of Davidson, Matusz, and Shevchenko (2008) that greater openness leads to better labor market sorting of higher ability workers into higher technology firms. This finding serves to explain the difference between the results at the firm level and those at the worker level: If average quality of the workforce (in terms of match specific ability or innate (time-invariant) ability) improves systematically in

[38] Table 4.3 is for a group of firms, which were included in our sample in both 1990 and 1998 and did not switch export status during this period.
[39] Note that firm-fixed effect for a given firm is constant across time and cannot account for the improvement in match quality between 1990 and 1998.

exporting firms following trade liberalization, not controlling for match effects (as is the case in firm-level analysis), will incorrectly suggest that trade liberalization leads to a differential wage improvement for workers at exporting firms even when this is not the case.

A comparison of estimates obtained from specifications with and without spell fixed effects suggests that match effects matter both qualitatively and quantitatively. Table 4.4 compares estimates obtained from alternate specifications with only firm fixed effects, with both worker and firm fixed effects included, and finally with match fixed effects included. Note that this comparison can only be made for the sample of workers who switch jobs during this period, as worker effects cannot be separately identified from match effects for those workers who do not. As expected, the inclusion of worker fixed effects lowers (absolute value of) the magnitude of γ_2' from 0.242 to 0.151 for tariffs, and from 0.086 to 0.050 for ERP as the measure of protection. The inclusion of match effects lowers the coefficient further from 0.151 to 0.104 and 0.050 to 0.018, when the measure of protection is tariffs and ERP, respectively.

Finally, Table 4.5 reports estimates of γ_1' and γ_2' when workers with different levels of human capital (grouped into two educational categories – high education and low education) are examined separately. Interestingly, there are heterogeneous effects across the two educational groups. We find that the relative wages of workers with higher levels of education employed in exporting firms improve with liberalization compared to similar workers employed in firms serving the domestic market. One explanation for this finding is that the rent sharing mechanism we have discussed earlier, whereby workers share a fraction of the profits made by the firms, may be relevant in the case of high education workers even it remains insignificant in workers with lower levels of education. Alternatively, the improvement in the quality of the labor force in exporting firms, could result in positive productivity spillovers on more educated workers and hence higher wages.

5. Conclusions

In this paper, we use a linked employer–employee database from Brazil to examine the impact of trade reform on the wages of workers employed at heterogeneous firms. Our analysis of the data at the firm-level confirms earlier findings of a differential positive effect of trade liberalization on the average wages at exporting firms relative to non-exporting firms. However, this analysis of average firm-level wages is incomplete along several dimensions. First, it cannot fully account for the impact of a change in trade barriers on workforce composition especially in terms of unobservable (time-invariant) characteristics of workers (innate ability) and any additional productivity that obtains in the context of employment in the specific firm (match specific ability). Furthermore, the firm-level analysis is undertaken under the assumption that the assignment of workers to firms is random. This ignores the sorting of worker into firms and leads to a bias in estimates of the differential impact of trade on workers at exporting firms relative to non-exporting firms. Using detailed information on worker and firm characteristics to control for compositional effects and using firm-worker match specific effects to account for the endogenous mobility of workers, we find the differential effect of trade openness on wages in exporting firms relative to domestic firms to be insignificant. Consistent with the models of Helpman, Itskhoki, and Redding (2010) and Davidson, Matusz and Schevchenko (2008), we also find that the workforce composition improves systematically in exporting firms in terms of innate (time invariant) worker ability and in terms the quality of the worker-firm matches.

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Appendix

For a sub-sample of firms, we can combine RAIS with data from the Brazilian manufacturing survey, *Pesquisa Industrial Anual* (PIA).[40] We construct an indicator variable that takes the value one if a firm reports positive acquisitions of foreign intermediate goods or foreign machinery. This measure captures acquisitions by firms which use imported intermediates even if they don't import them directly, and purchase them from local distributors instead. In order to establish a comparison with our results for the full sample, we start by replicating our main specification for the smaller sample of firm-groups for which we also have data on the importer status of the firm. We find the differential effect on exporters to be prevalent for the restricted sample of PIA firm-groups as well. Our estimates suggest that a one percentage point decrease in ERP is associated with an increase in firm average wages by 0.18%. In column (2) we include the firm's importer status and the interaction terms between importer status with ERP and RER in our main specification. Our main findings on the differential effect of liberalization on exporters continue to hold in this specification. We find that while importing firms pay higher wages on average, the decline in ERP does not have a differential effect on importing firms.

In the PIA sample there is significant overlap in firms that export and import. In fact, conditional on importing, most firms also export. 50 percent of firms that export also import, while only 11% of non-exporters import. In the specification reported in column (3), we test the importance of this overlap on our results. More specifically, we define an exporter dummy which takes the value one for firms that export but do not import; an importer dummy which takes the value one for firms that import but do not export and an exporter-importer dummy which takes the value one for firms that both export and import.[41] The differential effect of a change in ERP on exporters is significant and negative for firms that only export (and do not import) as well as for firms that both export and

[40] PIA is a confidential data set, based on a survey of Brazilian manufacturing firms conducted annually by the Brazilian Census Bureau between 1986 to the present (excluding 1991). Since we do not have access to this confidential dataset, instead, we use random three-to-five firm cells with similar characteristics, constructed by Muendler (2003) to meet confidentiality requirements. The data we use are based on these random aggregates of PIA (firm-groups) as described in Muendler (2003) and Muendler (2004). In our discussion of the PIA data, we use the terms "firms" and "firm-groups" interchangeably.

[41] Although we report the estimated coefficients for column (3) in the same row as columns (1) and (2), we note that in the first two columns, the export variable takes the value one for firms that report positive exports and the import variable takes the value one for firms that report positive imports. In column (3), the export and import variables take the value one for firms that export but do not import, and import but do not export, respectively.

import. We find no differential impact of a decline in ERP on firms that only import. We find the exporter-premium on wages to be higher for exporters that import, compared to exporters that do not. The higher average wages for importers relative to domestic firms reported in column (2) seems mainly to be due to the fact that most of these firms are exporters as well.[42]

[42] We note, however, that there is heterogeneity in this coefficient based on the comparative advantage of the firm's sector. Unreported results suggest that, in high comparative advantage sectors, a decline in ERP increases wages of domestic firms using imported intermediates relative to domestic firms which do not use imported intermediates (at the 10% level of significance); a one percentage point decrease in ERP increases wages at high comparative advantage importers which do not export by 0.21%. By contrast, average wages at importers which do not export, are not statistically different than purely domestic firms post-liberalization in the low comparative advantage sector. Moreover, average wages at exporters (which do not import) and exporter-importers in the low comparative advantage sector increase with a decline in ERP relative to purely domestic firms. These results are available upon request.

Figure 3.1 Time Variation in Effective Rates of Protection, 1990-1998

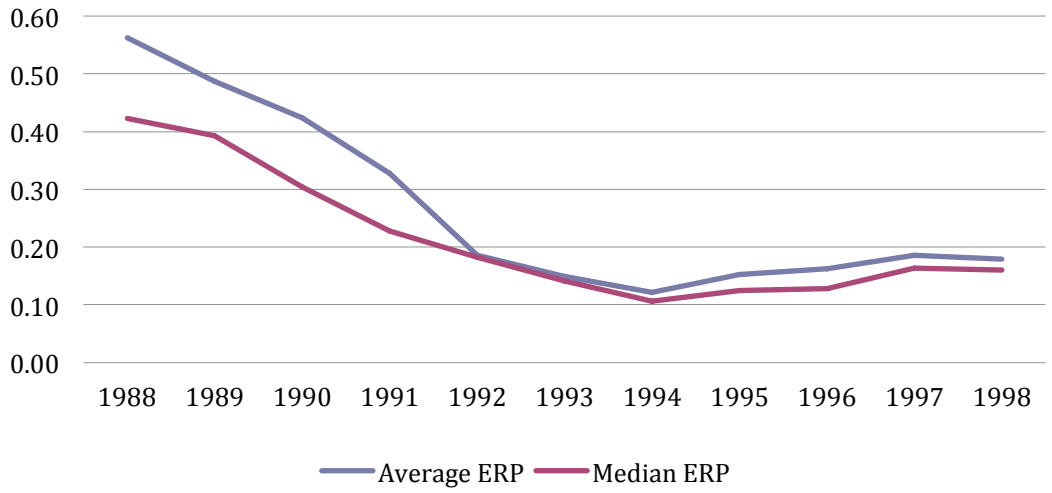
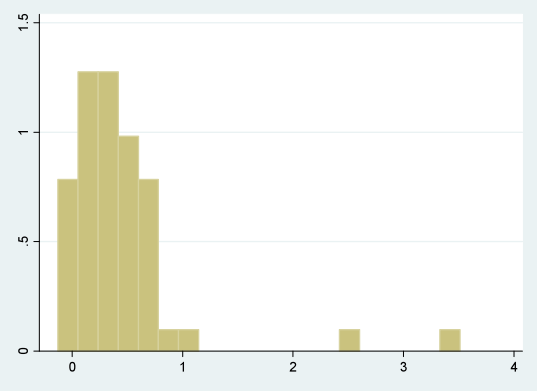
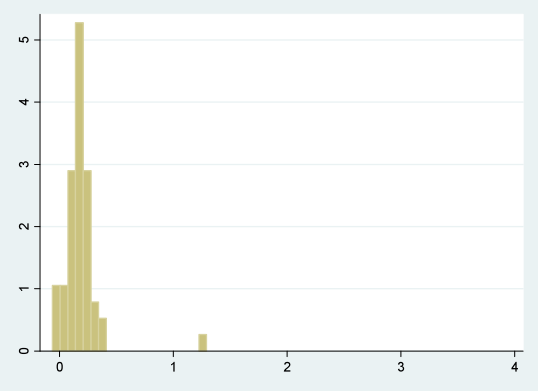


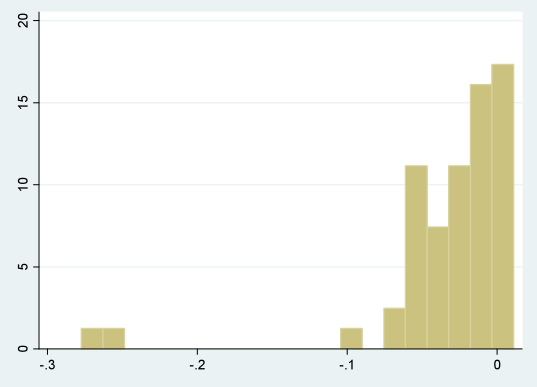
Figure 3.2: Cross-Industry Variation in Effective Rates of Protection



Effective Rates of Protection in 1990



Effective Rates of Protection in 1998



Annual Change in ERP, 1990-1998

Source: Kume, Piani, and Souza (2003).

Figure 4.1 Distribution of Psi ($\Psi_{j(i,t)}$) Quintile by Residual (ε_{ijt-1}) Quintile

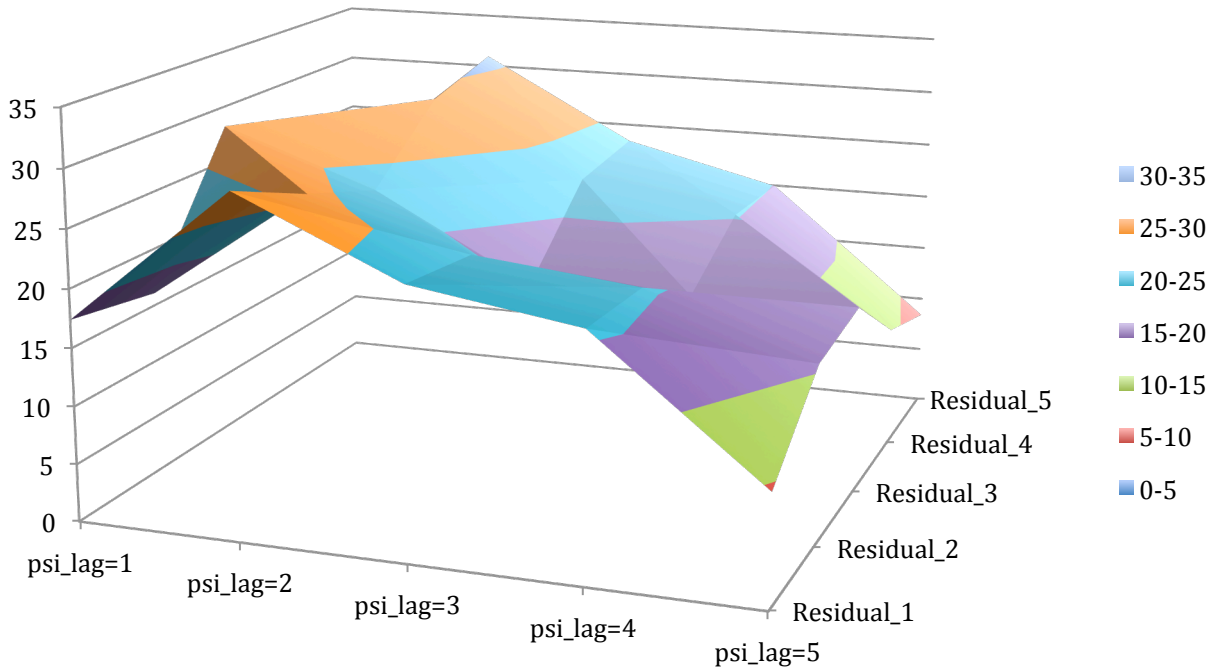


Figure 4.2 Probability of Transition to Each Quintile of the Psi ($\Psi_{j(i,t)}$) Distribution, by Psi ($\Psi_{j(i,t-1)}$) Quintile of Origin, Full Sample of Job Changers

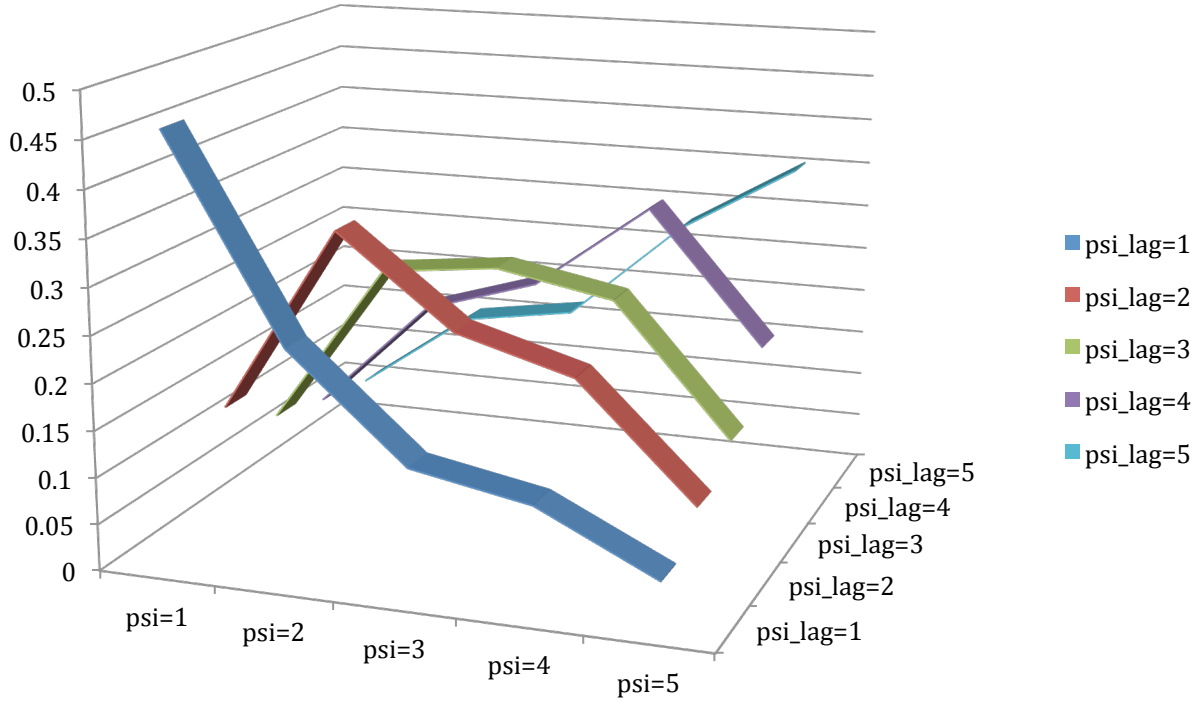


Figure 4.3 Probability of Transition to Each Quintile of $\Psi_{j(i,t)}$, by $\Psi_{j(i,t-1)}$ Quintile

Figure 4.3a Job Changers in Q(ε_{ijt-1})=1

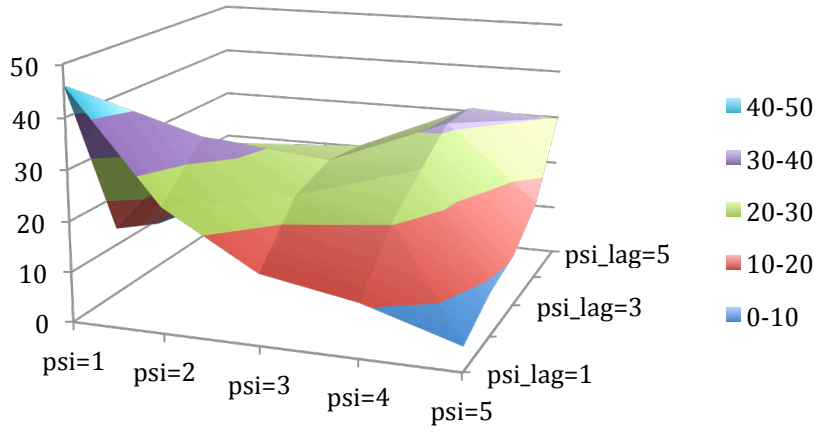


Figure 4.3b Job Changers in Q(ε_{ijt-1})=3

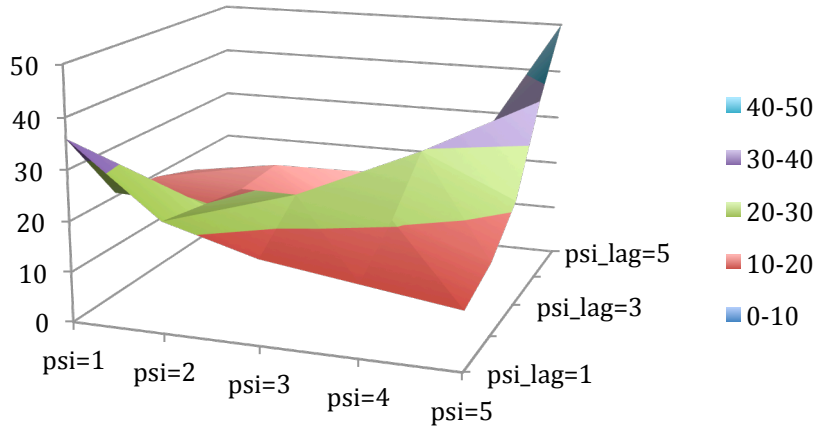


Figure 4.3c Job Changers in Q(ε_{ijt-1})=5

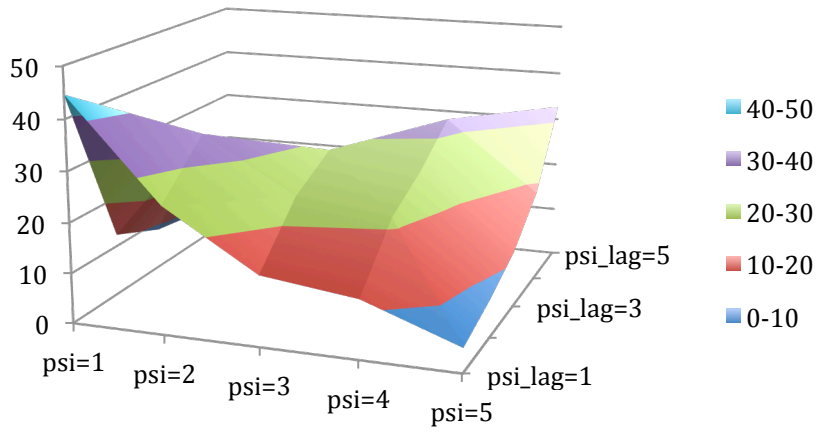


Table 2.1 Descriptive Statistics

	Sample	Exporters	Domestic
Worker Characteristics			
Average Wage in *reais*	3,917	5,512	2,456
Average Log (Wages)	7.630	8.019	7.273
Share of Workers			
Less than High School	0.745	0.696	0.790
High School Graduates	0.184	0.206	0.164
College Graduates	0.066	0.094	0.039
Unskilled Blue Collar	0.109	0.099	0.119
Skilled Blue Collar	0.609	0.599	0.617
Other White Collar	0.078	0.079	0.077
Professional and Managerial	0.179	0.206	0.154
Firm Characteristics			
Average Employment	73	346	37
Average Log (Employment)	2.82	4.75	2.57
Number of Workers	114,042	64,212	80,895
Number of Firms	58,578	11,143	53,537

Table 3.1 Trade Protection and Firm-Level Average Wages

	Tariffs			ERP		
	Full Sample	High Comparative Advantage	Low Comparative Advantage	Full Sample	High Comparative Advantage	Low Comparative Advantage
Tariff	0.170**	-0.058	0.279***			
	(0.079)	(0.182)	(0.095)			
Export*Tariff	-0.248***	-0.562***	-0.117			
	(0.074)	(0.109)	(0.094)			
ERP				-0.011	-0.082*	0.067
				(0.033)	(0.045)	(0.046)
Export*ERP				-0.103***	-0.206***	-0.008
				(0.037)	(0.054)	(0.064)
Export*RER	-0.258***	-0.293***	-0.230***	-0.233***	-0.247***	-0.199**
	(0.058)	(0.076)	(0.081)	(0.055)	(0.074)	(0.081)
Export	0.328***	0.414***	0.275***	0.279***	0.318***	0.219**
	(0.074)	(0.094)	(0.104)	(0.067)	(0.088)	(0.100)
N	505,369	258,374	246,995	505,369	258,374	246,995
Firm-Fixed Effects	YES	YES	YES	YES	YES	YES
Region-Specific Year Dummies	YES	YES	YES	YES	YES	YES
Detailed Firm-Level Controls	YES	YES	YES	YES	YES	YES
Impact on Exporters						
Tariff	-0.078	-0.620***	0.162	-0.114**	-0.289***	0.058
F-Statistic	0.50	9.37	1.40	4.86	14.58	0.59
p-value	0.48	0.00	0.24	0.03	0.00	0.45

Table 3.2 ERP and Firm-Level Wages: Robustness

	Alternative Samples			Industry Specific RER	Export Status				Size of Exports
	Balanced Panel	5% Metro Sample	Lib. period 90-94		Cutoff5	Cutoff10	Export90	Export_All	
ERP	0.004	-0.021	-0.000	-0.031	-0.011	-0.011	-0.013	-0.001	0.284***
	(0.038)	(0.033)	(0.029)	(0.031)	(0.033)	(0.033)	(0.034)	(0.033)	(0.091)
Export*ERP	-0.095**	-0.083**	-0.125***	-0.055*	-0.108***	-0.110***	-0.115***	-0.086**	-0.025***
	(0.042)	(0.033)	(0.034)	(0.031)	(0.038)	(0.039)	(0.042)	(0.037)	(0.007)
Export*RER	-0.009***	-0.210***	-0.098***	-0.005***	-0.233***	-0.238***	-0.261***	-0.190***	-0.022***
	(0.002)	(0.050)	(0.032)	(0.001)	(0.056)	(0.056)	(0.057)	(0.046)	(0.008)
Export	0.284***	0.251***	0.145***	0.466***	0.279***	0.283***			0.034***
	(0.067)	(0.060)	(0.039)	(0.060)	(0.067)	(0.068)			(0.010)
N	204,437	354,564	270,400	505,369	505,369	505,369	505,369	505,369	58,418
Firm-Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Region-Specific Year Dummies	YES	YES	YES	YES	YES	YES	YES	YES	YES
Detailed Firm-Level Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES

Table 4.1 Trade Protection and Worker-Level Wages

	Tariffs			ERP		
	Full Sample	High Comparative Advantage	Low Comparative Advantage	Full Sample	High Comparative Advantage	Low Comparative Advantage
Tariff	0.133	-0.018	0.273			
	(0.177)	(0.300)	(0.256)			
Export*Tariff	-0.110	-0.180	-0.055			
	(0.090)	(0.129)	(0.127)			
ERP				-0.020	-0.083	0.198
				(0.073)	(0.074)	(0.130)
Export*ERP				-0.045	-0.042	0.004
				(0.045)	(0.051)	(0.074)
Export	0.133*	0.067	0.184	0.113*	0.024	0.154
	(0.072)	(0.074)	(0.125)	(0.061)	(0.064)	(0.115)
Export*RER	-0.094*	-0.025	-0.147	-0.085*	-0.004	-0.132
	(0.056)	(0.059)	(0.095)	(0.051)	(0.055)	(0.091)
N	504,424	266,463	237,961	504,424	266,463	237,961
Detailed Worker Level Controls	YES	YES	YES	YES	YES	YES
Detailed Firm Level Controls	YES	YES	YES	YES	YES	YES
Region-Specific Year Dummies	YES	YES	YES	YES	YES	YES
Spell-Fixed Effects	YES	YES	YES	YES	YES	YES

Table 4.2 ERP and Worker-Level Wages: Robustness

	Alternative Samples			Export Status				
	5% Metro Male Sample	Liberalization period 90-94	Industry Specific RER	cutoff5	cutoff10	Export90	Export_All	Size of Exports
ERP	-0.044	-0.040	-0.025	-0.015	-0.012	-0.026	-0.030	-0.170
	(0.059)	(0.092)	(0.074)	(0.073)	(0.073)	(0.072)	(0.074)	(0.196)
Export*ERP	-0.031	-0.046	-0.035	-0.054	-0.058	-0.037	-0.021	0.007
	(0.050)	(0.045)	(0.044)	(0.046)	(0.044)	(0.047)	(0.040)	(0.015)
Export	0.054	0.076	-0.003***	0.115*	0.122**			0.006
	(0.055)	(0.058)	(0.001)	(0.062)	(0.062)			(0.012)
Export*RER	-0.019	-0.053	-0.003***	-0.084	-0.089*	-0.099**	-0.024	0.001
	(0.047)	(0.051)	(0.001)	(0.051)	(0.051)	(0.043)	(0.026)	(0.009)
N	447,957	269,951	504,424	504,424	504,424	504,424	504,424	241,147
Detailed Worker Level Controls	YES	YES	YES	YES	YES	YES	YES	YES
Detailed Firm Level Controls	YES	YES	YES	YES	YES	YES	YES	YES
Region-Specific Year Dummies	YES	YES	YES	YES	YES	YES	YES	YES
Spell-Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES

Table 4.3 Change in Estimated Match Effects Over Time

	Match Effect			
	1990		1998	
	Median	Mean	Median	Mean
Exporters	0.145	0.135	0.169	0.160
Non-exporters	0.018	-0.019	-0.056	-0.068
All Firms	0.065	0.031	0.059	0.034

Table 4.4 Alternative Specifications for Switchers

	Tariffs			ERP		
	Only Firm Fixed Effects	Both Firm and Worker Fixed Effects	Match Fixed Effects	Only Firm Fixed Effects	Both Firm and Worker Fixed Effects	Match Fixed Effects
Tariff	0.552***	0.158*	0.056			
	(0.180)	(0.149)	(0.232)			
Export*Tariff	-0.242*	-0.151**	-0.104			
	(0.124)	(0.092)	(0.101)			
ERP				0.087	0.020	-0.011
				(0.084)	(0.063)	(0.095)
Export*ERP				-0.086	-0.050	-0.018
				(0.070)	(0.050)	(0.058)
Export	0.255***	0.182	0.133	0.198**	0.144	0.098
	(0.094)	(0.074)	(0.083)	(0.086)	(0.066)	(0.076)
Export*RER	-0.140*	-0.117**	-0.089	-0.109	-0.097	-0.071
	(0.072)	(0.057)	(0.066)	(0.070)	(0.054)	(0.063)
N	226,193	226,193	226,193	226,193	226,193	226,193
Firm Controls	YES	YES	YES	YES	YES	YES
Worker Controls	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	NO	YES	YES	NO
Worker Fixed Effects	NO	YES	NO	NO	YES	NO
Spell-Fixed Effects	NO	NO	YES	NO	NO	YES

Table 4.5 ERP and Worker Level Wages, by Skill

	Less than High-School	More than High-School
ERP	-0.005	-0.055
Export*ERP	-0.035	-0.094*
Export	0.116*	0.127
Export*RER	-0.088	-0.088
Firm Controls	YES	YES
Worker Controls	YES	YES
Spell-Fixed Effects	NO	NO

Table A.1 ERP and Firm-Level Wages: Importers and Exporters

	Only Export Status	Export and Import Status	Detailed Trade Status
ERP	0.090	0.084	0.089
	(0.073)	(0.074)	(0.072)
Export*ERP	-0.178***	-0.175***	-0.185***
	(0.036)	(0.038)	(0.045)
Export*RER	-0.154***	-0.117**	-0.088*
	(0.051)	(0.051)	(0.051)
Export	0.226***	0.184***	0.163**
	(0.064)	(0.065)	(0.065)
Import		0.128***	-0.034
		(0.042)	(0.135)
Import*ERP		0.001	-0.034
		(0.042)	(0.135)
Import*RER		-0.112***	0.065
		(0.034)	(0.101)
Export_Import *ERP			-0.175***
			(0.045)
Export_Import *RER			-0.219***
			(0.050)
Export_Import			0.306***
			(0.064)
N	11,372	11,372	11,372
Firm-Fixed Effects	YES	YES	YES
Year Dummies	YES	YES	YES
Detailed Firm-Level Controls	YES	YES	YES