Wage Effects of Trade Reform with Endogenous Worker Mobility[*]

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Abstract

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 data at the firm level confirms earlier findings of a differential positive effect of trade liberalization on average wages at exporting firms relative to non-exporting firms. However, the 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) worker characteristics (innate ability) and any additional productivity that results from employment in a 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 workers into firms and leads to a bias in estimates of the differential impact of trade on average wages at exporting firms relative to non-exporting firms. Using detailed information on worker and firm characteristics to control for compositional effects and allowing for the endogenous assignment of workers to firms due to time-invariant firm-worker match-specific productivity effects, we find an insignificant differential effect of trade openness on wages at exporting firms relative to domestic firms. We also show that workforce composition post-liberalization improves systematically in exporting firms in terms of the combination of innate worker ability and the quality of the worker-firm matches. Our findings confirm the importance of labor market matching mechanisms in determining the effects of trade policy changes on wages.

Keywords: Trade liberalization, linked employer-employee data, endogenous mobility.
JEL Classification: F16

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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)). 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 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,[1] differences in workforce composition will imply that average firm-level wages in exporting firms will change differentially relative to non-exporters following liberalization. Even if trade liberalization does not affect the returns to worker characteristics, a differential wage effect may 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. An expansion in exports may result in such 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 (forthcoming).

[1] 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.

In the presence of labor market frictions, changes in trade protection may result in differential changes in the wages of workers employed in exporting firms relative to the wages of *ex-ante* identical workers employed in non-exporting firms. 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 profits or market share after a decline in protection,[2] will correspondingly be higher compared to workers employed in firms serving only the domestic market (Egger and Kreickemeier (2009) and Amiti and Davis (forthcoming)). Furthermore, exporters wishing to improve their product quality for foreign markets could respond to a decline in protection by paying (higher) efficiency wages in order to induce increased effort from otherwise identical workers (Frías, Kaplan, and Verhoogen (2009) and Davis and Harrigan (2011)). Moreover, if the labor allocation process is subject to search and matching frictions, exporting firms may screen 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) model in their analysis of the links between trade and labor markets with heterogeneous firms and (*ex-post*) heterogeneous workers. In this context, an opening of the economy to trade increases the wage gap between exporting and non-exporting firms and increases wage inequality. Similarly, Davidson, Matusz, and Shevchenko (2008) consider labor markets which are subject to search and matching frictions and show that globalization improves the efficiency with which workers are matched to firms in export-oriented sectors, but predict a less efficient allocation of workers to firms in import-competing sectors.[3]

[2] In the well-known framework of Melitz (2003) with heterogeneous, monopolistically-competitive firms, the relative profits of exporting firms compared to non-exporting firms will rise with trade liberalization, undertaken unilaterally by the liberalizing country or on a reciprocal basis with liberalizing partners. Relatedly, the disciplining effect of product-market competition and foreign entry in product markets after trade reform may erode mark-ups and hence rents especially in firms serving solely the domestic market (Levinsohn (1993) and Hay (2001)). In addition, improved access to cheaper and a wider variety of imported inputs may result in export market entry through an improvement in firm-level productivity (Amiti and Konings (2007)) or the introduction of new final goods (Goldberg, Khandelwal, Pavcnik, and Topalova (forthcoming)).

[3] Here, liberalization increases the wage gap between high productivity and low productivity firms, and by altering the degree of assortative matching between high productivity workers and high productivity firms, changes the extent of within-group wage inequality (inequality between workers with identical observable characteristics). More generally, the various channels linking trade protection to wages that we describe are each potentially associated with changes in within-group inequality, 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

Does trade liberalization, in fact, affect differently workers employed in exporting firms and non-exporting firms? We explore this issue empirically using a detailed matched employer-employee dataset from Brazil for the years 1990-1998.[4] 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 both observable and unobservable (time-invariant) worker, firm, and match 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 reforms.

We begin our analysis by exploring the behavior of average firm-level wages in order to ensure the comparability of our results with earlier work and to highlight the contribution of the empirical strategy employed in this paper using matched employer-employee data. Consistent with the findings of Amiti and Davis (forthcoming) for the liberalization period in Indonesia,[5] we find that a decline in trade protection is associated with an increase in

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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.

[4] We are not the first to study the links between trade openness and wages using matched employer-employee data. For instance, recent studies confirm and extend the findings of Bernard and Jensen (1995) for Germany (Schank, Schnabel, and Wagner (2007) and Klein, Moser, and Urban (2010)) and Denmark (Munch and Skaksen (2008)). Our study differs from these in our emphasis on the differential impacts of liberalization across heterogeneous firms. Frías, Kaplan, and Verhoogen (2009) exploit exogenous variation in exchange rates to study wage differentials between exporting and non-exporting firms. Hummels, Jorgensen, Munch, and Xiang (2011) study the impact of outsourcing on wages using matched employer-employee data from Denmark. Davidson, Heyman, Matusz, Sjoholm, and Zhu (2011) study labor market sorting using matched employer-employee data from Sweden, and point to a new source of gains from trade in the form of an improvement in match quality between workers and firms in export-oriented industries. Finally, Helpman, Itskhoki, Muendler and Redding (2011) study the effect of trade on inequality in Brazil using a structural model based on estimated parameters from matched data. However, as we discuss in greater detail later in the paper, we are the first in trade literature to highlight the problematic issue of the endogenous mobility of workers across firms and its potential to lead to biased parameter estimates in both reduced form and structural analysis.

[5] To our knowledge, Amiti and Davis (forthcoming) is the first paper to incorporate firm-level heterogeneity in an empirical analysis of the impact of trade liberalization on wages. They introduce a general equilibrium model, which combines firm heterogeneity, 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

average wages in exporting firms relative to domestic firms. 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 observable worker characteristics (that are not available in most 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 (time-invariant) ability of the worker, and any additional productivity that arises in the context of employment in the specific firm due for example, to production complementarities between the worker and the 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 workers into firms and the resulting change in the distribution of match-specific ability across firms with different modes of globalization. Using matched employer-employee data, we test whether wage behavior at the worker level confirms the maintained assumption of random worker-firm assignment (i.e., exogenous worker mobility), using a test statistic introduced in Abowd, McKinney, and Schmutte (2010). Consistent with theoretical models emphasizing non-random allocation of workers across production activities as important determinants of the relationship between trade liberalization and wages, such as Yeaple (2005) and Bustos (2011) in a perfectly competitive labor market context, and Helpman, Itskhoki, and Redding (2010) and Davidson, Matusz, and Schevchenko (2008) in the presence of labor market frictions, our data decisively reject the assumption of exogenous worker mobility.

Importantly, using detailed information on worker and firm characteristics to control for compositional effects and allowing for the endogenous assignment of workers to firms, which arise due to unobserved (time-invariant) firm-worker match-specific productivity effects, we find an insignificant differential effect of trade openness on wages at exporting firms relative to domestic firms. Consistent with the models of Yeaple (2005), Davidson, Matusz, and Schevchenko (2008), and Helpman, Itskhoki, and Redding (2010) we find that workforce composition improves systematically in exporting firms relative to domestic firms, in terms of innate worker ability and in terms of the quality of their worker-firm

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.

matches post-liberalization. This finding also serves to explain the difference between the results at the firm level and those at the worker level. If average (innate or match-specific) worker ability improves systematically in exporting firms following trade liberalization, and this change is not addressed, it will appear that trade liberalization leads to a differential wage improvement for workers at exporting firms. Our findings imply that following trade liberalization, a given worker (with fixed innate ability) who continues to be employed at a given exporting firm (with a fixed worker-firm match effect) will not experience any differential effect on her wage relative to another worker who continues to be employed at a non-exporting firm. *Ceteris paribus*, workers who transition to firms with which they are better matched will, however, earn higher wages because of their higher productivity there. Exporting firms will pay a differentially higher *average* wage post-liberalization because of the improvement in the composition of their workforce in terms of (time-invariant) innate ability and worker-firm match quality. Thus, our findings using matched employer-employee data suggest a quite different picture of the links between trade liberalization and wages than those obtained by analyzing the data at a more aggregate (firm) level and underscore the importance of non-random allocation mechanisms, driven by search and matching frictions and/or complementarities between worker characteristics and firm technology (or productivity) in determining the effects of trade policy changes on wages.

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. In Section 4, we discuss the biases which may arise due to endogenous worker mobility and test for it using matched employer-employee data. Section 5 describes the analysis at the worker level, and Section 6 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 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[6], 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.[7]

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% throughout this period, leaving Brazilian manufacturers heavily protected.

The 1990s, however, witnessed sweeping changes in Brazilian trade policy. Beginning in 1988, the government, under the scope of a "New Industrial Policy", reduced average manufacturing tariffs (Moreira and Correa (1998)). Average *ad valorem* final goods tariff rates fell from 47% to 36% between 1988 and 1989 (Muendler (2003b)). These reforms had little impact on import competition however, as non-tariff barriers remained highly restrictive. Effective trade policy changes began with the Collor administration's "Industrial and Foreign Trade Policy" in 1990. The federal government abolished all remaining non-tariff barriers inherited from the import substitution era and brought nominal tariffs further down. Final goods tariffs fell by over 50% in just five years—from 23.3%, on average, in 1990 to 10.6%, on average, in 1995.

In 1994, after decades of high inflation and several unsuccessful stabilization attempts, the Brazilian government succeeded with its macroeconomic stabilization plan (*Plano Real*),

[6] Across Brazil's top export destinations in 1990—Europe, the U.S., and Japan account for almost 70% of Brazilian exports—average tariff rates were already low and declined only marginally during the 1990s.

[7] Brazil's reforms were largely driven by the government's stated goal to bring down trade barriers. Industries with the highest pre-reform protection experienced the strongest liberalization. For this reason, in the analysis that follows, we consider Brazil's liberalization as exogenous (Goldberg and Pavcnik (2005)).

designed to help correct a large fiscal deficit and lastingly end hyperinflation. The new currency, the *real*, was pegged to the U.S. dollar, and began at parity on July 1, 1994. Officially, the *real* was set to a crawling peg which permitted the currency to depreciate at a controlled rate against the U.S. dollar. However, as the country's persistent effort to control inflation materialized[8], the real exchange rate actually appreciated in the first months (see Figure 2.1).[9] In response, the government partially reversed trade reforms in 1995 after manufacturing industries lost competitiveness due to the *real*'s appreciation.[10] Final goods tariffs climbed slightly in subsequent years from an average of 11% in 1995 to an average of 15% in 1998.[11]

2.2. Worker data

The Brazilian Labor Ministry requires by law that all legally-registered firms report to the ministry on all workers in every year. 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 protection rates.

The main benefit of the RAIS database is the ability to trace individually-identifiable workers over time and across jobs. A unique job-level observation includes a worker identification number (which remains with the worker throughout his work history), the tax number of the worker's firm, the month-year of the worker's accession to the firm, and the month-year of the worker's separation from the firm. The RAIS data are particularly

[8] Annualized inflation rates, which reached 8,000% in 1994, fell to 1.7% in 1998. Pinheiro, Giambiagi, and Moreira (2001) remark that Brazil had the same inflation rate for a year that it had in a single day prior to the *Plano Real*.
[9] The real exchange rate series for Brazil is constructed in Muendler (2003a).
[10] 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 differential impacts of exchange rate fluctuations on firms with differing trade exposure.
[11] 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. Furthermore, in robustness checks we also include sector-specific year dummies to account for the possibility that these reforms exhibit sector-time variation, not fully captured by our time-varying controls.

valuable as they offer variables beyond the available information in firm-level databases, often used in studies like ours. In particular, the data contain detailed information on workers' skill-levels (as defined by occupation, education, and reported tenure at the firm in months) and average monthly earnings for each job in which a worker is employed. Our measure for a worker's annual compensation is the annual real wage in *reais*. We also have information on the gender and age of the worker, and the industrial classification and municipality in which the firm operates. Appendix A offers a detailed discussion of the data, including further information on the variables of interest.

To create our samples for estimation, we restrict observations as follows. First, RAIS was made available to us in the form of a random sample from the complete list of workers across all sectors of the economy ever to appear in the national records. The sampled workers are matched to the population data to find all firms in which these workers were ever employed over time, creating a complete employment history of a 1% random sample of the population of the Brazilian formal-sector labor force. Next, following earlier work using RAIS (see, for example, Menezes-Filho, Muendler, and Ramey (2008) and Poole (forthcoming)), 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 monthly wage, and workers in private-sector jobs. Finally, for workers with multiple jobs in a given year, we include only the most recent job in the sample. If a worker has multiple current jobs, only the highest paying job is included. Our implicit assumption is that workers consider the last and highest paying job of the year for annual job transitions.

2.3. Complementary data

Trade protection In our analysis of Brazil's trade policy, we concentrate on two trade protection measures: the final goods 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 as well as changes in the final goods tariffs. Our data on final goods tariffs and ERP are from Kume, Piani, and Souza (2003), who report monthly protection rates at the *Nível 80* Brazilian industrial classification level. In Appendix A, we describe in detail the construction of these measures. We match the December tariffs and

ERP from 1990 to 1998 with the worker and firm data by the 2-digit industrial classification found in RAIS, following publicly-available concordances, to identify workers and firms in industries with differential rates of protection and liberalization experiences.

Figure 2.2 displays both the mean and median values of the effective rate of protection in the manufacturing sector for our sample period. The early 1990s experienced sharp declines in the effective rate of protection. Mean rates fell from around 42% in 1990 to approximately 14% in 1994, while median rates fell from 35% to 14% over the same time period. The slight aforementioned protectionist response to the appreciation of the *real* beginning in 1994 is also evident. Most strongly in the early part of the decade, the median ERP is smaller than the 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.

The substantial cross-industry variation in both levels and changes in the ERP is documented in more detail in Figure 2.3 where we present the distribution of effective rates 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. The standard deviation for ERP across sectors was 0.23 in 1990 and 0.08 in 1998. We also note substantial variation across sectors in the average annual changes in protection rates. While across all sectors the ERP declined approximately 25 percentage points between 1990 and 1998, some sectors liberalized more than others. Specifically, as we mention earlier in the section, industries with the highest pre-reform ERP experienced the most dramatic liberalization. The manufacture of transport equipment endured the steepest declines of over 60 percentage points, while footwear manufacturing faced a mere 9 percentage point decline in ERP over our sample period.

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 all 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. In our baseline specification, we define an indicator variable equal to one for firms with a positive dollar value of free-on-board exports in a given year and zero otherwise.

Our data indicate that during the early 1990s, there was 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. Over the sample period, approximately 10% of firms switch export status—9.0% begin exporting, while another 6.3% of firms switch out of exporting.

2.4. Descriptive statistics

Worker data The base sample described in Section 2.2 includes 2,173,888 worker-firm-year observations—494,229 workers employed in 321,427 firms across 26 broad industries of the economy. For our analysis on the wage impacts of trade reform, we further restrict the data to the manufacturing sector for which we have information on trade protection levels. The longitudinal nature of the data allows us to control for unobservable time-invariant worker, firm, and match quality through fixed effects regressions. However, as is well-documented in the literature, a proper identification of both worker and firm fixed effects relies on worker mobility across firms. Therefore, following Abowd, Kramarz, and Margolis (1999), we group small firms with few movers together for a more precise estimation of the firm fixed effects. In addition, to increase the estimated precision of the worker fixed effects, we also restrict the sample to those workers with at least two years of data. Finally, we conduct our tests on the largest mobility group, as firm fixed effects across unconnected groups are not directly comparable.[12]

Generated in this manner, the sample for our worker-level estimations consists of 504,424 worker-firm-year observations, characterizing 114,042 workers employed in 58,578 firms. The average worker is represented in the data sample for 5.6 years and employed in 1.7 different firms. Almost 40% of workers switch firms at least once during the 9-year sample period. Approximately 27% of workers switch between exporting and non-exporting firms—15.1% switch into exporting firms and 12.3% switch out of exporting firms.

[12] A connected group includes all the workers who have ever worked for any of the firms in that group, as well as all of the firms at which any of these workers were ever employed during the sample period.

We report detailed descriptive statistics across firm-types in Table 2.1. The set of variables available in our dataset allows us to appropriately control for differences between exporting and non-exporting workers and firms in identifying the heterogeneous impact of trade policy on wages. The top panel reports information for worker-level characteristics. Across the 114,042 workers in our sample, the average annual wage is 3,917 *reais*. Over the sample period, average annual wages increase by 36% (from 3,235 *reais* in 1990 to 4,417 *reais* in 1998). Roughly three-quarters of the formal-sector labor force have at most a middle school education. An additional 18% of manufacturing sector workers are high-school educated, and only 7% of workers have a college degree. The manufacturing formal workforce becomes more educated over the sample period. By 1998, 30% of workers have at least a high school education. 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 approximately 10% 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.

Fifty-six percent (64,212) of workers in our sample are employed for at least one year in an exporting firm. Exporters pay a substantially higher average wage than do non-exporters—5,512 *reais* per year on average as compared to 2,456 *reais* per year on average, respectively. Wage growth over the sample period is also higher at exporters, pointing to an increase in the exporter-domestic wage gap. Exporters employ a higher share of skilled workers, on average, compared to non-exporting firms, consistent with the existing literature. Almost 30% of workers employed in exporting firms have at least a high school degree. In comparison, the share of workers with at least a high school education at non-exporters is 20%. Exporters also employ a higher share of workers in professional, managerial, and technical occupations.

Firm data In the bottom panel, we report firm-level statistics. Note that for the firm-level analyses that follow, we form a panel of firm aggregates generated from the complete RAIS formal-sector population for the 321,427 firms in our base sample, producing in the manufacturing sector. That is, firm-level employment characterizes the true population of workers employed at the firm in a year, rather than sampled workers employed at the firm in a year. Our complete firm-year sample includes 505,369 firm-year observations.[13]

The average number of employees in manufacturing firms is relatively small at 73. In 1990, though exporters characterize 43% of employment, they represent only 9% of firms, pointing to a significant difference between exporters and non-exporters in terms of average employment. Over the complete sample period, the average exporter employs 346 employees, while the average non-exporter employs only 37 workers. Average employment decreases by more across exporting firms during the sample period than non-exporting firms—possibly driven by labor productivity changes among existing exporters or compositional changes due to firm entry into the export market (13% of firms are exporters by 1998). Similarly, as new export market entrants tend to be smaller on average, average export sales decrease over the sample.

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, as we discuss in Section 2.4, we form a firm-level panel of population aggregates from RAIS and estimate the following specification:

$$\ln \overline{y_{jt}} = \gamma_1 \mathrm{Pr}otect_{kt} + \gamma_2 \mathrm{Pr}otect_{kt} * Exp_{jt} + \gamma_3 RER_t * Exp_{jt} + \gamma_4 Exp_{jt} + \delta_{tr} + \beta Z_{jt} + \psi_j + \varepsilon_{jt} \qquad (1)$$

where the dependent variable, $\ln \overline{y_{jt}}$, is the logarithm of average wages at the firm level for firm j at time t, $\mathrm{Pr}otect_{kt}$ denotes the level of protection in sector k in which firm j 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 for each sector is measured by

[13] We note that this sampling strategy produces a firm sample that may be biased towards larger employers. In unreported results available by request, we also evaluate a random sample of firms ever to appear in RAIS in order to ascertain the importance of worker sampling on the firm sample. Our firm-level results are robust to this alternative sample of firms.

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 firms. In each specification, we include an interaction term between $\Pr otect_{kt}$ 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 appreciation of the currency, the *real*, making Brazilian goods less competitive in 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) and the firm's export status.[14] The time-varying, firm-level controls, Z_{jt}, include variables available in standard firm-level datasets such as log employment and the occupational skill composition[15] of the firm, in addition to average worker tenure at the firm, and controls for the age[16], gender, and educational skill composition[17] of the firm.[18]

Each specification also includes firm fixed effects, ψ_j, to account for time-invariant firm characteristics, and interactive region-year fixed effects, δ_{tr}, to capture the average effect of

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[14] 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 RER changes on exporting firms relative to domestic firms. We also conduct robustness checks using industry-specific real exchange rates.

[15] 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.

[16] 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.

[17] 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.

[18] 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.

policy changes that may differentially impact wages of firms in different regions of Brazil.[19] 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).[20]

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). The responsiveness of average wages in firms serving only 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 of inputs[21]), as well as any negative impact of increased import competition due to a decline in output tariffs. All else equal, if the industries that experienced a decline in final goods tariffs also experienced a decline in input tariffs, the output tariff is likely to overestimate the actual decrease in protection that the industry has experienced. 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 estimated 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 $\gamma_2 < 0$.

3.1. Estimation results

[19] We consider Brazil's five main geographic regions: the North, Northeast, Center-West, Southeast, and South.
[20] Results are robust to clustering the standard errors at the firm-level.
[21] See Arkolakis, Demidova, Klenow, and Rodriguez-Clare (2008) and Goldberg, Khandelwal, Pavcnik, and Topalova (forthcoming) for evidence on improved input variety, and Kugler and Verhoogen (forthcoming), Amiti and Konings (2007), Csillag and Koren (2011), and Halpern, Koren, and Szeidl (2011) for evidence on improved input quality.

Estimation results from equation (1) with tariffs[22] as the measure of protection are reported in the left panel of 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. The RER-exporter interaction term suggests that a RER depreciation (a decrease in the RER as it is defined in our data) increases the wages in exporting firms relative to non-exporting firms, as expected. Our data also report a strong, positive exporter premium, consistent with previous studies. Estimated coefficients for all the (unreported) 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 of 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.[23] Intuition suggests that following liberalization, exporters in these sectors will experience a more pronounced increase in profitability relative to exporters in low comparative advantage sectors.[24] We therefore, expect a stronger impact on average wages at these firms. We divide our sample into high and low comparative advantage sectors using

[22] Our estimation results are qualitatively the same when we use tariffs weighted by the value-added of the industry instead of unweighted tariffs.

[23] See Bernard, Redding, and Schott (2007), Menezes-Filho and Muendler (2011), and Davidson, Matusz, and Shevchenko (2008) for discussions of the differential effects of trade liberalization in high and low comparative advantage sectors in heterogeneous firm settings.

[24] Menezes-Filho and Muendler (2011) report a differential increase in hiring rates for exporters in comparative advantage sectors relative to exporters in comparative disadvantage sectors post-liberalization. Employment growth is a plausible proxy for the profitability of a firm if an increase in firm profits is associated with an expansion of the firm. For a small sub-sample of firms, for which we also have information on sales and output, we confirm these findings. Unreported results, available by request, suggest that although exporters in both low and high comparative advantage sectors increase sales and output with liberalization, the impact of a decline in tariffs on sales and output is greater for exporters in high comparative advantage sectors.

data on each industry's Balassa (1965) comparative advantage.[25] 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 Balassa (1965) comparative advantage measure across all merchandise trade sectors in 1986.[26] These results suggest that the differential impact of trade liberalization on exporters we report in the first column is largely driven by firms operating in high comparative advantage sectors. A ten percentage point decrease in final goods tariffs increases average wages of exporters in comparative advantage sectors by 6.2%, while an equal decrease in tariffs has no statistical impact on average wages at comparative disadvantage exporters.[27]

In the second half of Table 3.1, we report estimation results for 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 the first half of Table 3.1. The negative and significant coefficient on the interaction term suggests that average wages in exporting firms increase relative to non-exporting firms in response to a decline in the ERP. A ten percentage point decrease in ERP increases average wages by 1.1% 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 decrease in ERP is 2.9% for exporters and 0.8% (and significant only at the 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

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

$$CA_{kt} = \frac{X_{k,t}^{Brazil} / \sum_k X_{k,t}^{Brazil}}{X_{k,t}^{World} / \sum_k X_{k,t}^{World}}$$ where X_{kt} are exports.

[26] 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.

[27] Unreported F-statistics and corresponding p-values are available by request.

that the exporter premium is slightly higher for exporters in the high comparative advantage sector.

We next conduct a wide array of robustness checks for the reported results on the differential impact of liberalization on firm-level average wages at exporters relative to non-exporters (see Appendix B for a detailed discussion). First, we estimate equation (1) for various sub-samples. Specifically, we consider a balanced panel of firms which were under operation throughout the entire sample period, a 5% random sample of formal-sector males living in metropolitan areas, and a shorter panel that covers Brazil's main trade reform period (1990-1994) when average protection levels consistently declined. Our results are robust to these alternative samples. The main coefficient of interest is also robust to including industry-specific real exchange rates, instead of the economy-wide RER, and sector-specific year dummies to allow for the possibility that policy reforms exhibit sector-time variation not fully-captured by our time-varying controls. Next, we test whether our results are sensitive to how we assign the indicator variable denoting a firm's export status. We find our results to be robust to export status variables constructed using different thresholds and time-invariant measures. We also exploit the intensive export margin and find a stronger effect at larger exporters. Then, 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 change as a result of liberalization. Our results suggest that the differential effect that we find is not explained by changes in the relative returns to observable characteristics following liberalization. Finally, we consider the possibility that an omission of the importer status of the firm could 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 use of imported intermediate inputs. Our estimation results, based on a sub-sample of firms for which data on import status is available, suggest that the differential impact of liberalization on average wages in exporting firms is robust to various specifications controlling for the import status of firms.

4. Estimation Bias due to Endogenous Worker Mobility

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, the analysis of average firm-level wages, although informative, is not well suited to examine the differential impact of liberalization on otherwise identical workers in heterogeneous firms for a number of inter-related reasons. For instance, in addition to observable worker and firm characteristics, the matching of workers to firms is likely a function of worker characteristics that are unobservable in the data but that managers of the firm can observe and reward, such as the innate ability of the worker and any additional productivity that may result from a worker's employment in a specific firm due, for example, to production complementarities between the worker and the firm (match-specific ability). Specifically, in an environment in which firms are changing the composition and quality of their labor force in response to liberalization, analysis conducted at the firm level faces at least two problems. If exporting firms differentially respond to liberalization by systematically changing the composition of their workforce, for example, towards workers with higher innate ability or match-specific ability, our firm-level estimates will be biased.[28] 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 across firms with different modes of globalization.

Additionally, if the job mobility of workers is at least partly determined by unobservable worker-firm match quality (endogenous mobility), estimates of the differential effect of trade on workers in exporting firms in equation (1) will be further biased. This is because non-random job assignment implies a correlation between the error term ε_{jt} (which subsumes the unobservable characteristics associated with workers matched to firm j at time t) and the firm's characteristics represented by the right hand side variables, and thus a failure of the maintained assumption underlying the estimation.

In the context of the literature on international trade and labor markets, recent contributions have emphasized the role played by observable and *ex-ante* unobservable worker characteristics in determining job assignment and wages. Yeaple (2005) proposes a

[28] In unreported results, available upon request, we provide evidence of such differential workforce upgrading in terms of (observable) skill at exporting firms relative to non-exporting firms with trade liberalization. Specifically, we re-estimate a version of equation (1) with the share of workers with different levels of education as the dependent variable. While a change in ERP has no significant impact on the workforce skill composition at non-exporting firms, a decline in ERP is associated with a relative increase in the share of workers with high-school or more at exporting firms.

model in which firm heterogeneity arises when firms endogenously choose to employ different technologies and systematically hire different types of workers. In equilibrium, due to the complementarities between technology and skill (or ability), high skilled workers sort into high technology firms.[29] Helpman, Itskhoki, and Redding (2010) model the labor allocation process with heterogeneous firms as subject to search and matching frictions with *ex-ante* identical workers but *ex-post* match-specific heterogeneity in worker ability.[30] Firms have an incentive to screen workers to exclude those with lower abilities due to complementarities between worker ability and firm productivity. Since larger firms have higher returns to screening, more productive firms (exporters) screen more intensively, and have workforces of higher average ability than less productive firms. Search frictions induce multilateral bargaining between a firm and its workers, and higher ability workforces are more costly to replace. This results in higher wages in more productive, exporting firms, and an equilibrium assignment of workers to firms that is a function of unobservable, match-specific worker ability and thus non-random. Similarly, Davidson, Matusz, and Shevchenko (2008) study the effects of globalization on labor market matching. They model a perfectly competitive industry populated by heterogeneous firms that differ in the sophistication of the technology that they use (high- and low-tech), and heterogeneous workers with different levels of ability. High-ability workers are better suited for the jobs created by high-tech firms, making positive assortative (and thus non-random) matching optimal, even though due to labor market frictions equilibrium sorting may be imperfect (i.e., some high-ability workers may accept low-tech jobs).

Having discussed the potentially problematic issue of the endogenous assignment of workers to firms, and the central role of this process in recent theoretical contributions studying the links between trade and labor markets, we now proceed to explicitly test for the presence of endogenous worker mobility in our data. As we describe in the following section, we use matched employer-employee data and closely follow the recent work of Abowd, McKinney, and Schmutte (2010) in constructing suitable tests of endogenous worker mobility.

[29] Bustos (2011) builds on this framework in a monopolistically-competitive setting.

[30] While Helpman, Itskhoki, and Redding (2010) discuss the case in which worker ability is assumed to be match-specific and independently-distributed across matches, they note that the static model equally admits another interpretation in which ability is the innate talent of a worker that does not depend on his match and is *ex-ante* unobservable to both workers and firms.

4.1. Testing for endogenous worker mobility

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

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

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 factors that may influence a worker's wages, such as time-varying, observable, firm characteristics (Z_{jt}) and worker characteristics (X_{it}). The vector Z_{jt} is as in the firm-level analysis, and the vector X_{it} includes 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 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.

It is now a well-established empirical regularity that both worker and firm heterogeneity contribute to worker-level employment outcomes, such as wages, as in equation (2). It is important to note, however, that the classic 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:

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

Often referred to in the literature as the assumption of "conditional exogenous mobility" (see, for instance, Abowd, Kramarz, and Margolis (1999), Woodcock (2011), and Sørensen and Vejlin (2011)), the assumption implies that the assignment of workers to employers depends on time-varying observable worker and firm characteristics, and firm and worker fixed effects, but not ε_{ijt}. This assumption is at odds with many well-known models of the labor market with directed search, learning, or coordination frictions.[31] 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 heterogeneous *ex-post.* Furthermore, high productivity firms (exporters) screen more intensively, due to the complementarities between firm productivity and average worker ability, resulting in higher quality firm-worker matches. In this case, the estimates of equation (2) will be biased due to omitted worker-firm match quality (Woodcock (2011)). Similarly, if workers with certain observable characteristics are more successful at generating good matches (for example, because the return from search is higher, or due to learning) and hence earn higher wages, omitted match heterogeneity 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 computed from the average (over time) residual for a worker *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 from the most recently completed job, $\overline{\varepsilon_{ijt-1}}$, (within quintiles of the residual distribution) should not predict the transition across firms with heterogeneous firm fixed effects (say from a particular quintile of the $\psi_{j(i,t)}$ distribution to another quintile of the $\psi_{j(i,t)}$ distribution).

The test is implemented as follows. First, we estimate equation (2) for the sample of workers described in Section 2.4. Then, for workers who switched employers between time *t* and *t-1*, the average residual within worker and firm ($\overline{\varepsilon_{ijt-1}}$) is calculated for the complete duration of the match (i.e., until *t-1*). $\overline{\varepsilon_{ijt-1}}$ then represents the "match effect" for the firm and worker pair (at the employer in *t-1*). Under the null hypothesis, the transition rates between quintiles of the firm effects distribution—from the previous employer's $\psi_{j(i,t-1)}$

[31] As Abowd, McKinney, and Schmutte (2010) note, a number of theoretical models have variously described how the error term in equation (2) could be structurally related to the assignment of workers to employers through search dynamics, as in Mortensen (2003), Postel-Vinay and Robin (2002), and Lentz (2010), coordination frictions as in Shimer (2005), or learning as in Gibbons, Katz, Lemieux, and Parent (2005).

quintile to the current employer's $\psi_{j(i,t)}$ quintile—should be independent of $\overline{\varepsilon_{ijt-1}}$. Importantly, if the null hypothesis is rejected for our data, this would suggest that the estimation results from equation (2) are biased. Allowing for endogenous mobility due to unobservable firm-worker match-specific productivity requires the inclusion of firm-worker match effects in the specification. We discuss our approach to this issue in detail in the following section.

As we discuss in Section 2.4, a proper identification of worker and firm fixed effects relies on worker mobility across firms. Therefore, in constructing the test statistic, we restrict the base manufacturing sample to those workers with at least two years of data to ensure a more precise identification of worker fixed effects. Moreover, since estimates of firm fixed effects for firms with few movers are likely to be imprecise, Abowd, Kramarz, and Margolis (1999) suggest grouping small firms together to estimate 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.[32]

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, distributed chi-squared, has a value $\chi^2 = 8{,}600$ when we group small firms, and $\chi^2 = 19{,}000$ when we omit these firms from the sample, with 496 of degrees of freedom[33] (and thus a p-value of 0.000). This finding confirms the relevance of models of labor allocation involving search dynamics and sorting, and highlights the importance of allowing for the possibility of firm-worker match heterogeneity in wage determination.

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[32] 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 ever employed 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 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.

[33] The degrees of freedom are calculated as $(\#Q(\alpha_i)*\#Q(\psi_{j(i,t-1)})*\#Q(\psi_{j(i,t)})-1)*\#Q(\overline{\varepsilon_{ijt-1}})-1)$ = (5*5*5-1)*(5-1) = 496 where #Q denotes the number of quintiles.

To further emphasize this point, following Abowd, McKinney, and Schmutte (2010) and Schmutte (2010), we illustrate worker employment transitions in a series of plots. Figure 4.1 maps the conditional distribution of quintiles of the firm fixed effects for the previous job, $\psi_{j(i,t-1)}$, given quintiles of individual average residuals from the most recently completed job ($\overline{\varepsilon_{ijt-1}}$), 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 demonstrates that this is not the case in our data. For example, while job changers in the extremes of the match effects distribution (Q($\overline{\varepsilon_{ijt-1}}$) = 1 and Q($\overline{\varepsilon_{ijt-1}}$) = 5) are most likely to originate from the lower-middle of the $\psi_{j(i,t-1)}$ distribution (Q($\psi_{j(i,t-1)}$) = 2), job changers in the middle of the match effect distribution (Q($\overline{\varepsilon_{ijt-1}}$) = 3) most often originate from the first quintile of the $\psi_{j(i,t-1)}$ distribution.

In Figure 4.2, we plot the transition rates from a job in $\psi_{j(i,t-1)}$ 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, Figures 4.3a-c illustrate that these transition probabilities vary across 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 of the match effects distribution, respectively. The figures indicate the differences across job switchers in different quintiles of the match effects distribution. For example, job switchers at the extremes of the match effects distribution are more likely to transition within the same employer effect than job changers in the middle of the match effects distribution—most notably for Q($\overline{\varepsilon_{ijt-1}}$) = 5 in Figure 4.3c. By contrast, job switchers at the median of the match effects distribution are more likely to improve their employer effect than are workers at either the top or the bottom quintiles of the match effects distribution, as is evidenced by the relatively flat surface in lower-right quadrant of Figure 4.3b.[34] 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, also reflected in the value of the test statistic for exogenous mobility that we have previously discussed.

Finally, in results available by request, we compute the Abowd, McKinney, and Schmutte (2010) test statistics separately for the following subsamples of job switchers: those who switched between domestic firms, those who switched between exporting firms, and importantly in our context, those workers who make transitions between domestic firms and exporting firms. In every case, we find a decisive rejection of the assumption of exogenous worker mobility.

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 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 the 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 random allocation of workers to firms is also a feature of Davis and Harrigan (2011) in which heterogeneous firms, with different ability to monitor the effort of their workers, pay identical workers different (efficiency) wages in equilibrium. The decisive rejection of the assumption of exogenous worker mobility in our data supports a picture of the labor market that is more in line with the framework of Yeaple (2005), Davidson, Matusz, and Schevchenko (2008), and Helpman, Itskhoki, and Redding (2010), where, as we have previously discussed, the equilibrium assignment of workers to firms is non-random.

[34] This is consistent with the finding in Abowd, McKinney, and Schmutte (2010) for the United States that the likelihood for upgrading the firm effect is strongest for workers at the median match effect.

5. Worker-Level Analysis with Match Fixed Effects

If match-specific productivity is 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 (2011)). For example, if more experienced workers are likely to draw better matches, omission of the match effect will result in an overestimation 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 post-liberalization equilibrium, 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, specifications lacking controls for match quality will result in biased estimates of the differential effect of trade liberalization on wages.

To allow for the fact that a worker's job assignment may not be independent of the idiosyncratic part of the residual in equation (2), but may instead be determined by unobserved, time-invariant, firm-worker match-specific productivity effects, as specified in a number of recent theories of trade and labor market allocation, we now consider a more elaborate specification of wages, in which we include worker-firm match fixed effects (or job-spell fixed effects), M_{ij}, denoting a given worker *i*'s employment at a given firm *j*:

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

Since for the duration of a worker's employment within a firm, neither the worker nor the firm varies, the inclusion of match fixed effects obviates the need for the separate inclusion of worker and firm fixed effects. This is not costly for us, as our primary interest in this exercise lies in estimating the differential effect of trade liberalization (γ_2') controlling for worker, firm, and match effects rather than in estimating separately the worker and firm effects.[35] The estimated coefficient of the interaction term reflects any differential effect of a change in protection on the wages of workers employed in exporting firms relative to otherwise identical workers employed in firms serving only the domestic market. Note that the interpretation of γ_2' is different than the analogous coefficient in equation (1) as it no longer reflects the differential change in the firm's workforce composition based on (time-invariant) unobservable worker characteristics or the quality of the firm-worker match.

We should note that the inclusion of time-invariant match effects, M_{ij}, on the right hand side of equation (3) allows us to account for the endogenous assignment of workers to firms due to the component of match-specific ability that is time-invariant. Thus, in estimating specification (3), the implicit assumption is that worker mobility is random conditional on time-invariant match-specific worker ability and time-varying worker and firm characteristics, X_{it} and Z_{jt}, as follows:[36]

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

5.1. Estimation results

Table 5.1 reports estimation results from equation (3) with match fixed effects for both tariffs and ERP as the measure of protection.[37] In the first column, we present results with tariffs taken as the measure of trade protection in the sector. We concentrate our interpretation on γ_2', the coefficient on the differential impact of trade reform on otherwise identical workers employed in exporting firms. Our estimates suggest that the inclusion of

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[35] If one is interested in obtaining unbiased estimates of firm and worker fixed effects, as in exercises examining the sorting patterns of workers into firms (Davidson, Heyman, Matusz, Sjoholm, and Zhu 2010) or decomposing wage variation into variation arising from firm heterogeneity and worker heterogeneity (Frías, Kaplan, and Verhoogen 2009) *inter alia*, match effects should be included in addition to firm and worker fixed effects in equation (3). See Woodcock (2011) for details.

[36] As discussed in Abowd, McKinney, and Schmutte (2010), accounting for endogenous worker assignment that may occur due to *time-varying* match-specific effects requires knowledge of the source of the time variation in worker's firm-specific ability (for instance, due to firm-specific on-the-job learning). Such features, while clearly important, have generally not been the focus of theories linking international trade and labor markets and their estimation is outside of the scope of the present analysis. Our specification allows for an important source of endogenous worker mobility, which may arise due to time-invariant match-specific productivity effects, but does not control for all forms of endogenous worker mobility. Nevertheless, our estimation, which proceeds on the basis of a weaker exogeneity assumption, constitutes an improvement over existing work in this area.

[37] In our estimation of equation (3), we cluster standard errors at the industry-year level.

match 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 when we consider separately the high and low comparative advantage sectors, although the point estimate on the interaction term is more than three times larger in magnitude for the high comparative advantage sector. These results continue to hold when we use the effective rate of protection in the sector as our measure of trade protection.[38]

In Table 5.2, we conduct a wide array of robustness checks. The first two columns report estimation results from specification (3) for various alternative samples. As we describe in the data section, the regressions in Table 5.1 draw on the complete employment history of a 1% random sample of the population of Brazil's formal-sector labor force. In the first column, 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 next column, we restrict our analysis to Brazil's main trade reform period (1990-1994) during which the average ERP continuously decreased. Next, we replace the economy-wide real exchange rate with industry-specific real exchange rates in order to capture differences in the relative importance of trading partners across industries.[39] Then, we include sector-specific year dummies in specification (3) in order to account for other economic changes or reforms (such as prices, FDI inflows, privatization) that might exhibit sector-level variation, and that may not be fully captured by firm fixed effects or region-specific year dummies.[40] Our results are robust to these alternative samples, to using an industry-specific real exchange rate measure, and to the inclusion of sector-year indicator variables in the specification.

[38] Note that our estimates in the baseline specification reported in Table 5.1 suggest that, consistent with the evidence from Mexico in Verhoogen (2008), relative wages of workers in exporting firms increase following a RER depreciation (significant only at 10%). A potential explanation for this differential effect could be that RER changes are perceived as transitory by firms and that firms respond by paying existing workers higher wages (through rent-sharing or efficiency wages) instead of changing their workforce composition. While we find the discrepancy between the results for changes in RER and in protection potentially interesting, we do not emphasize this result as this is not a robust finding (see Table 5.2). We thank Rodney Ludema for a very helpful discussion on this point.

[39] We construct industry-specific real exchange rates using time-varying trade weights, as in Goldberg (2004). More specifically, we calculate $RER\_ind_t^k = \sum_c ((0.5\frac{X_{t-1}^{kc}}{\sum_c X_{t-1}^{kc}} + 0.5\frac{M_{t-1}^{kc}}{\sum_c M_{t-1}^{kc}}) * RER_t^c)$ where RER_t^c are the bilateral exchange rates for trading partner *c* of Brazil, X^{kc} and M^{kc} are exports and imports in industry *k* to or from country *c* at time *t-1*.

[40] Note that the level effect of ERP, which varies by sector and time, is absorbed by sector-specific year dummies and cannot be separately identified in this specification.

Next, we test whether our insignificant interaction coefficient is sensitive to the exporting thresholds we use to assign the indicator variable denoting a firm's export status. In our main specifications, a firm is defined as an exporter if it exported any positive dollar amount that year. There is, however, substantial heterogeneity within our sample of exporters.[41] The average worldwide export sales over our sample period is approximately $3.9 million, while the median exported value is much lower at roughly $150,000. Consistent with the idea of learning about potential exporting markets (Albornoz, Calvo, Corcos, and Ornelas (2012)), there are many exporters with positive, but very small, worldwide export sales. We note this may generate spurious entry into and exit from exporting. Therefore, in the next columns, we instead only consider firms with an export value greater than the 5th percentile (*Cutoff5*) in that year, and greater than the 10th percentile (*Cutoff10*) in that year as exporting firms, respectively.[42] Our results are robust to these changes in the exporting cutoff. While the magnitude of the interaction term increases slightly as we increase the cutoff threshold, the coefficient remains statistically insignificantly different from zero.

In the next two columns, to alleviate potential concerns regarding endogeneity of the export decision, we consider two time-invariant measures of export status. First, we define a firm as an exporter if it exported a positive dollar value at the beginning of our sample (1990), and zero otherwise (*Export90*). Next, the export status variable *Ever_Export* takes the value one if the firm reported positive exports in any year between 1990 and 1998. Our main coefficient of interest remains insignificant, and the point estimate is similar in magnitude.

Until now, all of the specifications have relied on the extensive margin of exporting. In the final column, we exploit the intensive export margin, conditional on exporting. When we use the magnitude of exports for the sub-sample of exporting firms to represent the relevance of exports to the firm, we obtain yet again estimates of γ_1' and γ_2', that are insignificantly different from zero.[43]

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[41] This is consistent with evidence presented from a wide range of countries, including France (Eaton, Kortum, and Kramarz (2004)), the United States (Bernard and Jensen (1995)), Chile (Blum, Claro, and Horstmann (2011)) and Colombia (Eaton, Eslava, Kugler, and Tybout (2008)).

[42] Over our sample period, the 5th percentile of the value of exports is roughly $2,800, and the 10th percentile of the value of exports is approximately $5,700.

[43] Other robustness checks we performed but do not report to conserve space include defining a firm as exporter if the firm exported only to non-Mercosur countries (and omitting exporters to Mercosur

Our findings, using worker-level data to account for compositional changes and match effects to allow for endogenous worker mobility (as discussed in the previous section), suggest an insignificant differential effect of trade policy on the wages of workers 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 account any changes in the composition of the workforce (by controlling for both observable and time-invariant unobservable worker characteristics) following trade policy. Also, by including worker-firm match effects, we allow for compositional changes in terms of firms' (time-invariant) match quality following trade policy changes. If, following trade liberalization, exporting firms improve their average match quality by hiring better matched workers, the estimates of the differential impact of trade reform on average firm-level wages at exporters from equation (1) without controlling for match effects would mistakenly be estimated as significant even in the absence of any true effect.

Table 5.3, which compares the changes in the distribution of estimated match effects in exporting firms and non-exporting firms, confirms this point. Comparing the beginning (1990) and the end (1998) of our sample period, we document 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. Note that the magnitude of the estimated match effect is larger for exporters than for non-exporters in both 1990 and 1998. As we have previously discussed, the match fixed effects in equation (3) absorb both worker and firm fixed effects in addition to the time-invariant match quality of a given employment spell. Consequently, our finding of an increase in the average match effect in exporting firms between 1990 and 1998 summarizes the combined effect of changes in the workforce composition in these firms in terms of improvements in worker quality as measured by time-invariant worker-specific characteristics, such as innate ability, and in terms of an improvement in the quality of the worker-firm matches.[44] [45] The improvement in the distribution of match-specific

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.

[44] Note that the firm fixed effect for a given firm is constant across time and cannot account for the improvement in match quality between 1990 and 1998.

ability in exporting firms is consistent with models emphasizing non-random allocation of workers across production activities. For instance, in Yeaple (2005) and Bustos (2011), exporting firms adopt skill-intensive technologies and employ higher skilled workers. Similarly, the model in Helpman, Itskhoki, and Redding (2010) predicts that with trade liberalization, exporting firms will screen more intensively and set a higher ability threshold for employment. Our result 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 in export-oriented industries.

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 (time-invariant) match-specific ability or innate worker-specific ability) improves systematically in exporting firms following trade liberalization as we show in Table 5.3, failing to control for match effects (as is the case in the firm-level analysis) will incorrectly suggest that trade liberalization leads to a differential wage improvement for workers at exporting firms. Our findings imply that following trade liberalization, a given worker (with fixed innate ability) who continues to be employed at a given exporting firm (with a fixed worker-firm match effect) will not experience any differential effect on her wage relative to another worker who continues to be employed at a non-exporting firm. *Ceteris paribus*, workers who transition to firms with which they are better matched will, however, earn higher wages because of their higher productivity there. Exporting firms will pay a differentially higher *average* wage post-liberalization because of the improvement in the composition of their workforce in terms of innate ability and worker-firm match quality. In unreported results, we, indeed, find this to be the case; when we include the estimated average firm-level match effect from specification (3) in specification (1) with firm-level average wages as the dependent variable, the differential effect of liberalization on exporting firms becomes insignificant.[46]

[45] One possible concern is that there are differences in trends for exporters and non-exporters. In unreported results available by request, we include an interaction between the exporter dummy and a time trend in our estimation of match effects from specification (3). Controlling for differential trends, the main results from Table 5.3 continue to hold. Similarly, the differential increase in match quality that we find for exporters does not merely reflect differential changes at more productive firms (correlated with export status); large (more productive) non-exporters also experience declines in average match quality over the sample period.

[46] While our findings of endogenous labor mobility and improvement in the distribution of match effects in exporting firms are clearly consistent with the models of Helpman, Itskhoki, and Redding (2010) and Davidson, Matusz, and Shevchenko (2008), we should note that there are indeed other dimensions along which the data are not entirely in line with the predictions of these theories. For

A comparison of estimates obtained from specifications with and without match fixed effects suggests that match effects matter both qualitatively and quantitatively. Table 5.4 compares estimates obtained from alternate specifications with only firm fixed effects, with separate 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 fixed effects cannot be separately identified from match fixed effects for those workers who do not switch firms. As expected, the inclusion of worker fixed effects lowers (in absolute value) the magnitude of the point estimate on 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 fixed 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.

6. Conclusions

Our interest in this paper is to study the effect of trade liberalization on labor markets, specifically the impact of trade liberalization on the wages of workers at exporting firms relative to the wages of otherwise identical workers at non-exporting firms. In doing so, we highlight some important methodological deficiencies in earlier work undertaken at the firm level and at the level of individual workers. Most importantly, while earlier empirical studies on this topic have treated the assignment of workers to firms as random, we study the endogenous job mobility of workers and find this to be an important feature of the data. This finding is consistent with recent theoretical papers studying the relationship between trade and labor markets, where heterogeneous workers sort into heterogeneous firms. Allowing for the endogenous mobility of workers which may arise due to unobservable and time-invariant worker-firm match-specific productivity, we find an insignificant differential effect of trade liberalization on worker-level wages. We also find that the workforce composition improves systematically in exporting firms in terms of innate worker ability and in terms of the quality of their worker-firm matches post-liberalization, resulting in a

instance, complementarities between workers' abilities and firm productivity in Helpman, Itskhoki, and Redding (2010) imply that with the improvement in average match quality within an exporting firm, the wages of all remaining workers in the firm should increase. A similar cross-worker spillover effect occurs under Nash bargaining in Davidson, Matusz, and Shevchenko (2008) as a worker with high productivity increases firm revenues and hence, wages for all other workers, a prediction that does not find broad support in our data (as indicated by the insignificance of our estimate of γ_2').

differential increase in the *average* wage in exporting firms. These findings suggest a different picture of the links between trade liberalization and wages than those obtained by analyzing the data at a more aggregate (firm) level and underscore the importance of labor market allocation mechanisms in determining the effects of trade policy changes on wages.

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Appendix A: Data Description

Administrative records Our main data are administrative records from Brazil for formal-sector workers in any sector of the economy linked to their employers. Brazilian law dictates that registered establishments report to the Ministry of Labor each year on all workers. These records are processed annually in the *Relação Anual de Informações Sociais* (RAIS). Worker characteristics of interest include earnings, education, occupation, tenure, gender, and age. Firm characteristics of interest include the sector and municipality of production.

The process for firms to report on their workers is extensive and costly. However, RAIS is used to assess payments of the annual public wage supplement, approximately one monthly minimum wage, to all formally-employed workers during the year. Thus, workers have a strong incentive to encourage employers to report accurately. In practice, however, only formally-employed workers will be properly recorded.[47]

As we report in the text, a unique job-level observation includes a worker identification number, the tax number of the worker's firm, the month-year of the worker's accession to the firm, and the month-year of the worker's separation from the firm. In practice, a unique worker ID may be reported more than one time within a firm-year. This turnover is in part due to a Brazilian labor law (*Fundo de Garantia de Tempo de Serviço* (FGTS)) in which formally-employed workers receive a guaranteed fund, comprising monthly employer contributions, upon termination. If the worker is dismissed without justification, the firm has to pay the worker 40% of the FGTS contributions. In order to avoid these spurious transitions, we consider only the last paying job within a year for each worker.

RAIS reports a worker's average monthly earnings (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

[47] Goldberg and Pavcnik (2003) estimate that 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 (2011), and Paz (2009)).

minimum wage information in *reais* from the Brazilian Central Bank, we calculate our main dependent variable—annual real earnings in *reais*.

Muendler, Poole, Ramey, and Wajnberg (2004) map the Brazilian classification of occupations (*Classificação Brasileira de Ocupações* (CBO)) found in RAIS to the *International Standard Classification of Occupations* (ISCO). The CBO is a detailed, task-oriented classification system, while the 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 occupation (Elias and Birch (1994)). ISCO occupations can be grouped into four broad occupational categories—professional and managerial, unskilled white collar, skilled blue collar, and unskilled blue collar—following Abowd, Kramarz, Margolis, and Troske (2001) to reflect the skill-intensity of the occupation.

Sectors in RAIS are identified using the classification of Brazil's statistical office (*Instituto Brasileiro de Geografia e Estatística* (IBGE)). This classification is roughly comparable to a 3-digit NAICS classification (Muendler (2002)) and has also been used by Menezes-Filho, Muendler, and Ramey (2008).

Trade protection Our data on protection rates are from Kume, Piani, and Souza (2003). They report *ad valorem* nominal output tariffs and effective rates of protection at the *Nível 80* Brazilian industrial classification level. ERP for sector k is formally measured as the increase in value-added due to the structure of tariffs relative to value-added at free trade prices, as $ERP_k = \tau_k - \frac{\sum_m a_{mk}\tau_k}{1-\sum_m a_{mk}}$, where $a_{mk} = \frac{a_{mk}^d(1+\tau_k)}{1+\tau_m}$ are input-output shares at free-trade international prices, a_{mk}^d are input-output shares at distorted domestic prices, and τ_k and τ_m are the final goods and intermediate inputs tariffs, respectively. We concord the *Nível 80* classification with the IBGE classification found in RAIS using a publicly-available concordance.[48] For each year, we calculate the simple-average across *Nível 80* industries within a 2-digit IBGE subsector.

[48] We follow industry concordances available at *http://econ.ucsd.edu/muendler/brazil*.

Appendix B: Robustness of Firm-Level Estimation Results

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 on the magnitude of 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 B.1 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.

In the next column of Table B.1, we use the complete employment history from a 5% random sample of males in metropolitan areas instead of the 1% random sample of the full population. 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 minimal changes in the magnitude. The results reported in the next column suggest that our findings are also robust to replacing the economy-wide real exchange rate with industry-specific real exchange rates. Next, we estimate specification (1) by including sector-year indicator variables in order to account for other concomitant economic changes or reforms that might exhibit sector-level variation and are not fully captured by our other controls.[49] We find that the differential impact of trade reform on average firm wages at exporters relative to non-exporters remains.

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 had any positive worldwide export sales in that year. We note this may generate spurious entry into and exit from exporting. As such, in the next columns, we instead, only consider firms with worldwide export sales greater than the 5th percentile (*Cutoff5*), and greater than the 10th percentile (*Cutoff10*) as exporting firms,

[49] Note that the level effect of ERP, which varies by sector and time, is absorbed by the sector-specific year dummies and cannot be separately identified.

respectively. Our results are robust to these changes in the exporting cutoff.

Next, we replace the exporter status dummy in our main specification with two time-invariant measures of export status. *Export90* takes the value one if the firm was an exporter as of the beginning of our sample (1990), and zero otherwise. *Ever_Export* takes the value one if the firm exported in any year during the sample period. Our estimation results continue to suggest a differential positive impact of liberalization on average wages at exporting firms relative to non-exporters.[50]

Our results until now have strictly relied on the extensive margin of exporting. 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 (the intensive export margin). Our results indicate that conditional on exporting, there is a differential increase in wages at firms with higher values of exports following liberalization. When we evaluate this result at the mean value of log export sales, a ten percentage point decrease in the effective rate of protection increases wages at exporting firms by 0.1%. We note a stronger effect at larger exporters. Wages at exporters in the 90th percentile of log export sales increase by 1.0% with an equivalent decrease in the effective rate of protection.

Next, in unreported results available by request, 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.

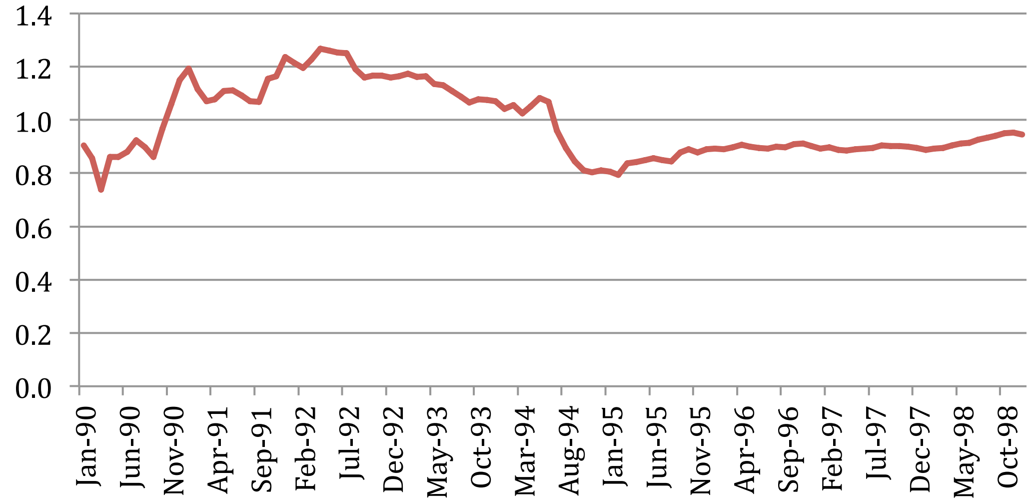
Finally, for a sub-sample of firms, we can combine RAIS with data from the Brazilian

[50] Moreover, our main coefficient of interest on the interaction term remains negative and significantly different from zero when we limit the sample only to firms that switched export status.

manufacturing survey, *Pesquisa Industrial Anual* (PIA).[51] 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. Omission of importer status of the firm could 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 results, available on request, suggest that the inclusion of firm's importer status and the interaction terms between importer status with ERP and RER in our main specification do not alter our main findings on the differential effect of liberalization on exporters.

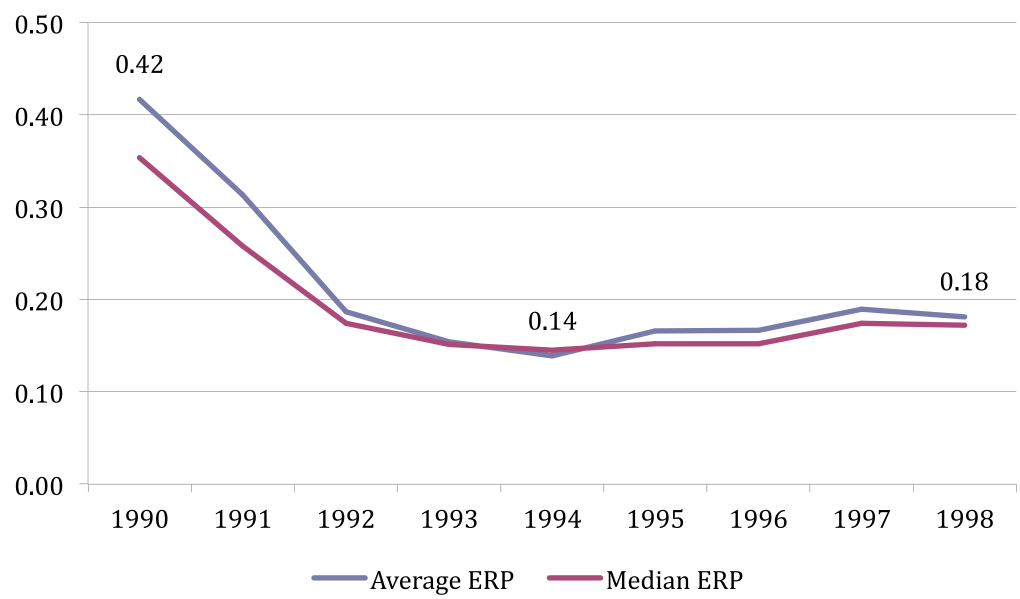
[51] PIA is a confidential dataset, based on a survey of Brazilian manufacturing firms conducted annually by the Brazilian Census Bureau between 1986 to the present (excluding 1991). Since we do not have access to this confidential dataset, instead, we use random three-to-five firm cells with similar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

Figure 2.1 Time Variation in Real Exchange Rates (R/$), 1990-1998



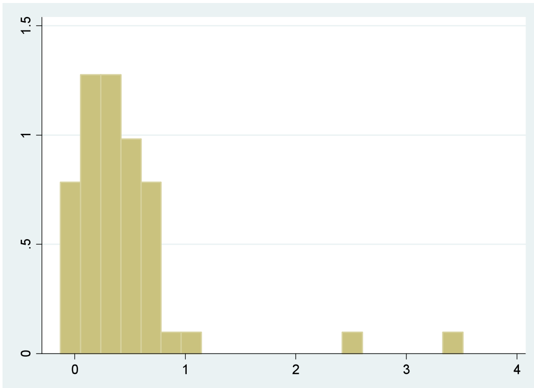
Source: Muendler (2003a)

Figure 2.2 Time Variation in Effective Rates of Protection, 1990-1998

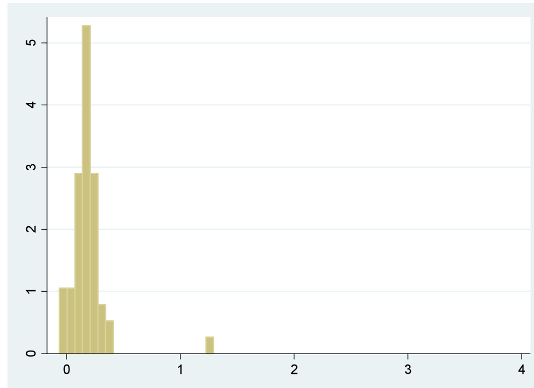


Source: Kume, Piani, and Souza (2003)

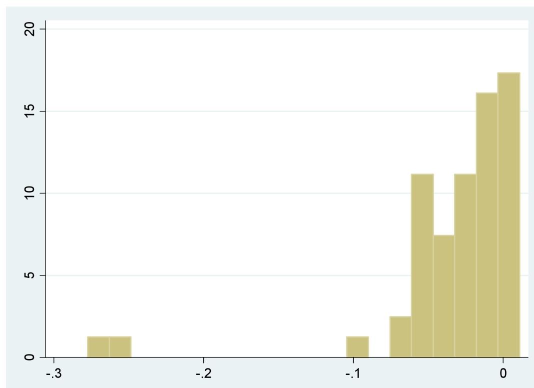
Figure 2.3 Cross-Industry Variation in Effective Rates of Protection



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 $\Psi_{j(i,t-1)}$ Quintile by Average Residual ($\overline{\varepsilon_{ijt-1}}$) Quintile

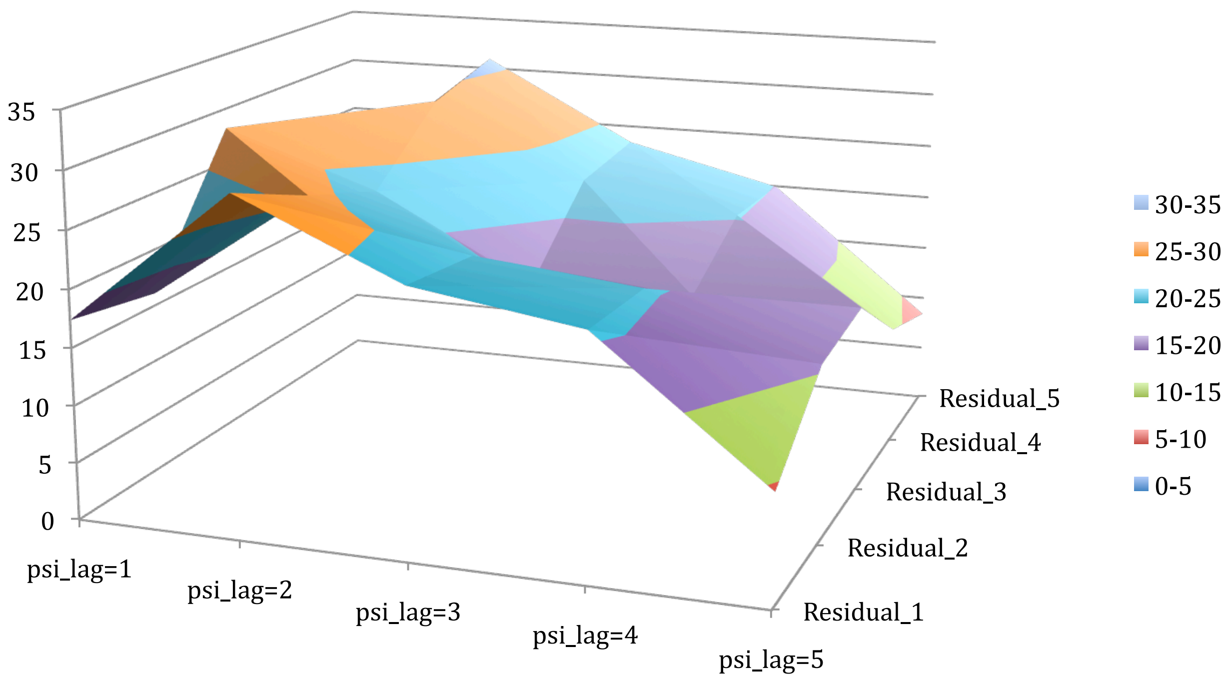


Figure 4.2 Probability of Transition to Each Quintile of the $\Psi_{j(i,t)}$ Distribution, by $\Psi_{j(i,t-1)}$ Quintile of Origin, Full Sample of Job Changers

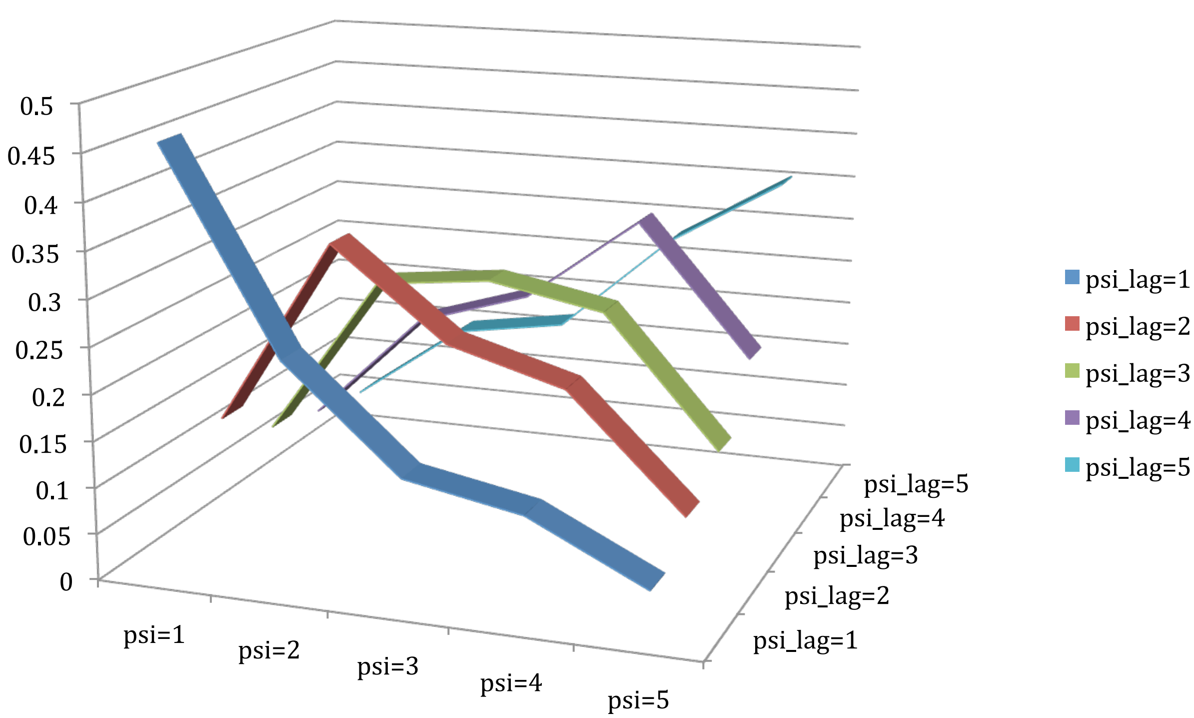


Figure 4.3 Probability of Transition to Each Quintile of $\Psi_{j(i,t)}$, by $\Psi_{j(i,t-1)}$ Quintile

Figure 4.3a Job Changers in Q($\overline{\varepsilon_{ijt-1}}$)=1

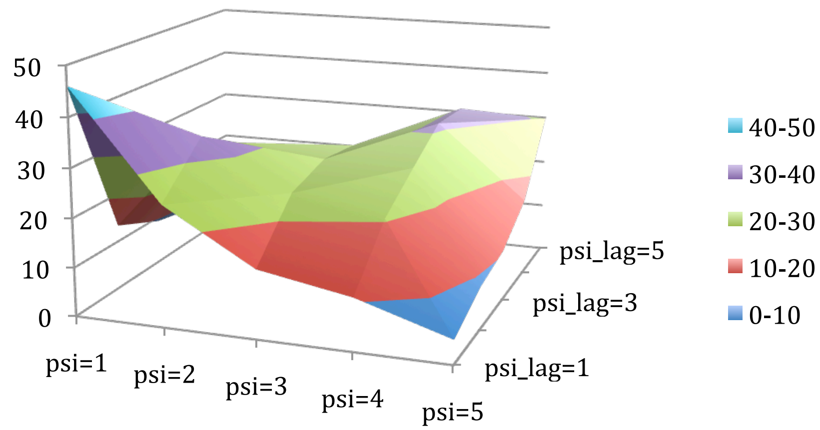


Figure 4.3b Job Changers in Q($\overline{\varepsilon_{ijt-1}}$)=3

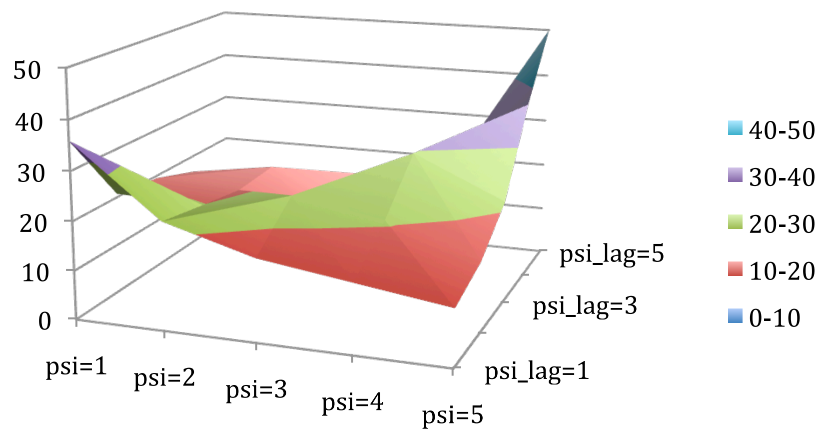


Figure 4.3c Job Changers in Q($\overline{\varepsilon_{ijt-1}}$)=5

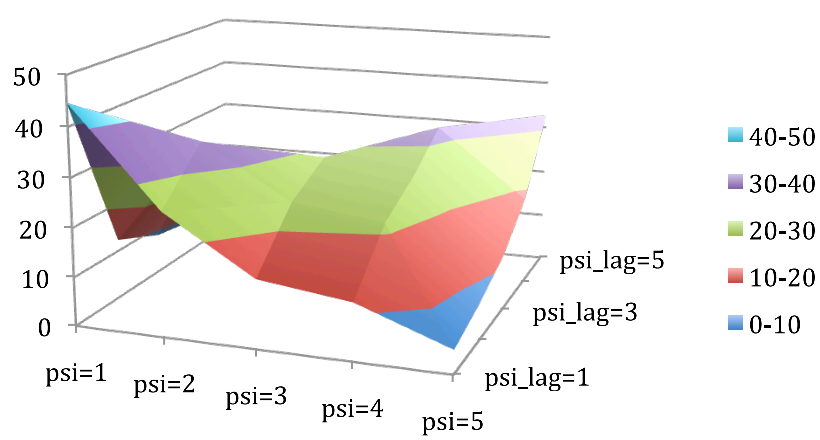


Table 2.1 Descriptive Statistics

	Sample	Exporters	Domestic
Worker Characteristics			
Average Wage in *reais*	3,917	5,512	2,456
Average Log (Wages)	7.630	8.019	7.273
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
Number of Workers	114,042	64,212	80,895
Firm Characteristics			
Average Employment	73	346	37
Average Log (Employment)	2.82	4.75	2.57
Number of Firms	58,578	11,143	53,537

Source: RAIS, manufacturing sector, 1990-1998.

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

Note: Robust standard errors, clustered at the industry-year level, are in parenthesis. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level.

Table 5.1 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*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)
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)
N	504,424	266,463	237,961	504,424	266,463	237,961
Detailed Firm-Level Controls	YES	YES	YES	YES	YES	YES
Detailed Worker-Level Controls	YES	YES	YES	YES	YES	YES
Region-Specific Year Dummies	YES	YES	YES	YES	YES	YES
Match Fixed Effects	YES	YES	YES	YES	YES	YES

Note: Robust standard errors, clustered at the industry-year level, are in parenthesis. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level.

Table 5.2 ERP and Worker-Level Wages: Robustness

	Alternative Samples		Industry-Specific RER	Sector-Year Dummies	Export Status				Log Value of Exports
	5% Metro Male Sample	Lib. period 90-94			Cutoff5	Cutoff10	Export90	Ever Export	
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	-0.012	-0.054	-0.058	-0.037	-0.021	0.007
	(0.050)	(0.045)	(0.044)	(0.036)	(0.046)	(0.044)	(0.047)	(0.040)	(0.015)
Export*RER	-0.019	-0.053	-0.003***	-0.076	-0.084	-0.089*	-0.099**	-0.024	0.001
	(0.047)	(0.051)	(0.001)	(0.050)	(0.051)	(0.051)	(0.043)	(0.026)	(0.009)
Export	0.054	0.076	0.252***	0.096	0.115*	0.122**			0.006
	(0.055)	(0.058)	(0.001)	(0.060)	(0.062)	(0.062)			(0.012)
N	447,957	269,951	504,424	504,424	504,424	504,424	504,424	504,424	241,147
Detailed Firm-Level Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Detailed Worker-Level Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Region-Specific Year Dummies	YES	YES	YES	YES	YES	YES	YES	YES	YES
Match Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES

Note: Robust standard errors, clustered at the industry-year level, are in parenthesis. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level.

Table 5.3 Change in Estimated Average Match Fixed 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 5.4 Alternative Specifications for Switchers

	Tariffs			ERP		
	Only Firm Effects	Both Firm and Worker Effects	Match Effects	Only Firm Effects	Both Firm and Worker Effects	Match 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*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)
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)
N	226,193	226,193	226,193	226,193	226,193	226,193
Detailed Firm-Level Controls	YES	YES	YES	YES	YES	YES
Detailed Worker-Level Controls	YES	YES	YES	YES	YES	YES
Region-Specific Year Dummies	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	NO	YES	YES	NO
Worker Fixed Effects	NO	YES	NO	NO	YES	NO
Match Fixed Effects	NO	NO	YES	NO	NO	YES

Note: Robust standard errors, clustered at the industry-year level, are in parenthesis. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level.

Table B.1 ERP and Firm-Level Wages: Robustness

	Alternative Samples			Industry-Specific RER	Sector-Year Dummies	Export Status				Log Value of Exports
	Balanced Panel	5% Metro Sample	Lib. period 90-94			Cutoff5	Cutoff10	Export90	Ever Export	
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*	-0.093***	-0.108***	-0.110***	-0.115***	-0.086**	-0.025***
	(0.042)	(0.033)	(0.034)	(0.031)	(0.033)	(0.038)	(0.039)	(0.042)	(0.037)	(0.007)
Export*RER	-0.009***	-0.210***	-0.098***	-0.005***	-0.176***	-0.233***	-0.238***	-0.261***	-0.190***	-0.022***
	(0.002)	(0.050)	(0.032)	(0.001)	(0.046)	(0.056)	(0.056)	(0.057)	(0.046)	(0.008)
Export	0.284***	0.251***	0.145***	0.466***	0.217***	0.279***	0.283***			0.034***
	(0.067)	(0.060)	(0.039)	(0.060)	(0.055)	(0.067)	(0.068)			(0.010)
N	204,437	354,564	270,400	505,369	505,369	505,369	505,369	505,369	505,369	58,418
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Region-Specific Year Dummies	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Detailed Firm-Level Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Note: Robust standard errors, clustered at the industry-year level, are in parenthesis. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level.