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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 evaluate the wage effects of trade reform. With an aggregate (firm-level) analysis of this question, we find that a decline in trade protection is associated with an increase in average wages in exporting firms relative to domestic firms, consistent with earlier studies. However, using disaggregated, employer–employee level data, and allowing for the endogenous assignment of workers to firms due to match-specific productivity, we find that the premium paid to workers at exporting firms is economically and statistically insignificant, as is the differential impact of trade openness on the wages of workers at exporting firms relative to otherwise identical workers at domestic firms. We also find that workforce composition improves systematically in exporting firms, in terms of the combination of worker ability and the quality of worker–firm matches, post-liberalization. These results stand in stark contrast to the findings reported in many earlier studies and underscore the importance of endogenous matching and, more generally, non-random labor market allocation mechanisms, in determining the effects of trade policy changes on wages.

1. Introduction

Central to academic and policy discussions about the process of globalization is the question of how greater trade openness impacts the labor market and the distribution of incomes in society. More narrowly, the theoretical literature in trade has recently focused on the question of whether the wage effects of trade depend upon the mode of globalization of the firm at which the worker is employed; that is, whether workers employed at exporting firms earn higher wages and experience different wage changes following trade liberalization than workers employed at non-exporting firms.

This theoretical literature offers a wide range of predictions concerning the distributional impact of trade liberalization. In neoclassical settings with competitive goods and factor markets, identical workers must earn identical wages; trade does not differentially impact the wages of workers based on the nature of the firm at which the worker is employed. However, product markets and the labor market may both be imperfectly competitive. For instance, monopolistically competitive firms of heterogeneous productivity, as in Melitz (2003), may engage in rent-sharing with homogeneous and randomly-allocated workers, as in Egger and Kreickemeier (2009) and Amiti and Davis (2012). In this case, the wages of workers employed in the more productive, exporting firms, which experience a relative improvement in profits or market share after a decline in protection, may increase compared to workers employed in firms serving only the domestic market.[1] Alternatively, 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

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[1] This follows the logic of the well-known framework of Melitz (2003) with heterogeneous, monopolistically competitive firms, where 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. Likewise, 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 et al. (2010)).

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workers (as in Verhoogen (2008), Frías et al. (2009), and Davis and Harrigan (2011)). Moreover, if the labor allocation process is non-random and characterized by complementarities between (unobservable) worker quality and firm technology, as in Yeaple (2005), or subject to search and (*ex-ante* unobservable) worker–firm-specific matching frictions, as in Helpman et al. (2010) and Davidson et al. (2008), the opening of the economy to trade may increase inequality by increasing the wage gap between workers employed in exporting and non-exporting firms. Thus, the theoretical predictions concerning the impact of trade liberalization on the wages of identical workers employed at exporting and non-exporting firms depend variously upon assumptions about the competitiveness of the labor market, the nature of the labor allocation process matching workers with firms, and the interplay of these factors with the product market structure, among other things. For this reason, empirical analysis, which allows for these various possibilities, is necessary to evaluate the actual outcomes.

A number of previous studies have indeed examined the links between trade and average *firm-level* wages, finding a relative increase in wages for workers at exporting firms post-trade reform (see, for example, Amiti and Davis (2012)). We argue, however, 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. Firm-level analyses also cannot account for 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). Finally, the firm-level analysis is undertaken under the assumption that the assignment of workers to firms is conditionally random (conditional on the observable characteristics of workers and firms), thus ignoring the sorting of workers into firms based on unobservable characteristics, and any changes in the distribution of match-specific ability across firms following trade liberalization.

Our paper empirically studies the question of whether trade openness affects differently workers employed in firms with different modes of globalization, placing particular emphasis on the possibility of the non-random matching of workers to firms. We use a matched employer–employee dataset from Brazil for the years 1990–1998 (covering the country's main trade liberalization episode), which traces individually-identifiable workers across employers over time and contains detailed information on wages and worker characteristics. [2]

While our analysis is primarily conducted at the disaggregated level of individual workers, in order to ensure the comparability of our results with earlier work, we begin by studying the links between trade and aggregate (average) firm-level wages instead. Consistent with earlier findings in the literature, we observe that a decline in trade protection is associated with an increase in average wages in exporting firms relative to domestic firms. However, as we have already discussed, this analysis is potentially problematic as it ignores the endogenous sorting of workers based on unobservables. We test for this possibility and find that the data indeed decisively reject the assumption of exogenous worker mobility.[3] We then evaluate the wage effects of trade reform by allowing for the non-random matching of workers with firms based on time-invariant, worker–firm-specific productivity effects.

Our main finding is that, once we use detailed information on worker and firm characteristics to control for compositional effects and allow for the endogenous assignment of workers to firms which may arise due to unobserved (time-invariant) firm–worker match-specific productivity, the data indicate an economically and statistically insignificant differential effect of trade openness on wages at exporting firms relative to domestic firms. Moreover, we find that once we allow for match-specific productivity, the premium paid to workers at exporting firms is also economically and statistically insignificant.[4] In addition, consistent with the models of Yeaple (2005), Davidson et al. (2008), and Helpman et al. (2010), we find that workforce composition, in terms of innate worker ability and the quality of worker–firm matches, improves systematically in exporting firms relative to domestic firms following liberalization.[5] This finding 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 fixed worker–firm match-specific ability) 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*, a worker who transitions to a firm with which she is better matched will, however, earn a higher wage because of her higher productivity there. Nevertheless, exporting firms will pay a differentially higher *average* wage post-liberalization because of the improvement in the composition of the workforce in terms of innate worker ability and worker–firm match quality.

In sum, our main result of an insignificant differential effect of trade on the wages of workers employed at exporting and non-exporting firms suggests a different picture of the links between trade liberalization and wages than that obtained by analyzing the data at a more aggregate (firm) level and underscores the importance of allowing for labor market frictions and endogenous matching in studying the effects of trade policy changes on wages. To our knowledge, this paper is the first in the trade literature to highlight the problematic issue of the endogenous mobility of workers across firms and its potential to lead to biased parameter estimates regarding the link between trade and wages.[6] We believe this to be the core contribution of our paper.

The remainder of this paper is organized as follows. Section 2 presents a background discussion on Brazil's trade policy reforms and describes the data we use. We begin our analysis of the data by presenting, in Section 3, the empirical methodology and estimation results for the aggregate (firm-level) analysis. In Section 4, we discuss endogenous worker mobility and its relevance in our empirical context. Section 5 describes the results we obtain using matched employer–employee data, and Section 6 concludes.

[2] There is a small but growing literature exploring the links between trade openness and wages using matched employer–employee data. See for instance, Frías et al. (2009) on wage differentials between exporting and non-exporting firms following a depreciation in Mexico; Hummels et al. (forthcoming) on the impact of outsourcing on wages in Denmark; Davidson et al. (2010) on the impact of globalization on efficiency of labor market sorting in Sweden and, Helpman et al. (2013) on the effect of trade on inequality in Brazil.

[3] Specifically, as we discuss in Section 4, we test whether wage behavior at the worker level confirms the maintained assumption of conditionally random worker–firm assignment (conditional on observable characteristics of workers and firms) using a test statistic introduced in Abowd et al. (2010).

[4] This finding is consistent with Schank et al. (2007) who report an insignificant exporter premium for Germany using matched worker–firm data.

[5] Though our findings are consistent with the predictions of Yeaple (2005), Davidson et al. (2008), and Helpman et al. (2010), we do not attempt to distinguish between the specific channels highlighted in these various models.

[6] As we will discuss in greater detail later in the paper, our estimation methodology, which allows for time-invariant worker–firm match effects in the specification, does not "solve" the problem of endogenous mobility (as worker mobility may also depend upon time-varying match effects). However, by taking time-invariant match effects into account, our specification corrects for endogenous mobility that is based on time-invariant match productivity and thus proceeds under weaker assumptions than much of the previous literature that has investigated these questions. Furthermore, in an additional test, we allow for a particular form of time variation in the worker–firm effects, by allowing for the magnitude of the worker–firm match effect to change when firms change export status, and find that our results are not affected by this modification.

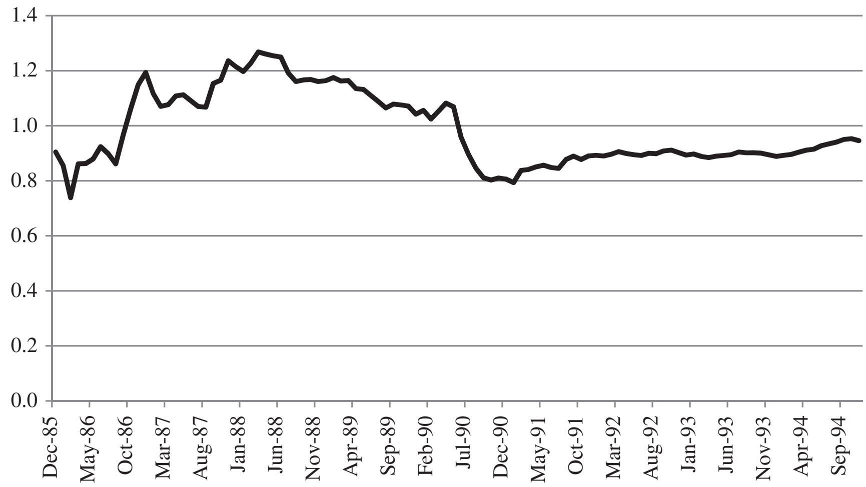

Fig. 1. Time variation in real exchange rates (R/$), 1990–1998.
Source: Muendler (2003).

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. This section begins with a review of the country's main policy reforms of the 1990s, and then describes the data we use in the analysis that follows.

2.1. Brazil's policy reforms

The 1990s were a period of dramatic policy reform in Brazil, providing a particularly appropriate setting in which to study the impact of trade liberalization on wages. As compared to the gradual process of globalization in many developed countries, Brazil's trade reform occurred over a relatively short period of time, and with substantial cross-industry variation. Furthermore, many of the policy reforms were arguably unanticipated by firms and workers and can 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)). These reforms had little impact on import competition, 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 (Moreira and Correa (1998)). Final goods tariffs across all sectors fell by over 50% in just five years according to Table 1 in Kume et al. (2003), from 30.5%, on average, in 1990 to 11.2%, on average, in 1994.

In 1994, after decades of high inflation and several unsuccessful stabilization attempts, the Brazilian government succeeded with its macroeconomic stabilization plan (*Plano Real*), 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, the real exchange rate actually appreciated in the first months (see Fig. 1). In response, the government partially reversed trade reforms in 1995 after manufacturing industries lost competitiveness due to the *real*'s appreciation.[8] Final goods tariffs climbed slightly in subsequent years from an average of 12.8% in 1995 to an average of 15.5% in 1998 (see Table 1 in Kume et al. (2003)).[9]

2.2. Data sources

2.2.1. 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

[7] A detailed discussion of the exogeneity of Brazil's tariff reforms is provided in Appendix A.1 of the updated working paper version of this paper, Krishna et al. (2011).

[8] Prior to 1994 and the implementation of the new currency, controls on Brazil's former currency, the *cruzado*, 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.

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

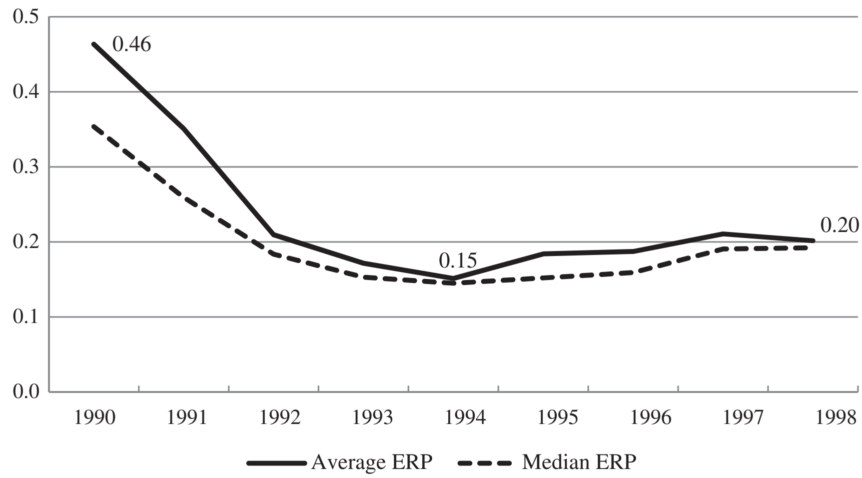

Fig. 2. Time variation in effective rates of protection, 1990–1998.
Source: Kume et al. (2003).

remains with the worker throughout her 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 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.

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 et al. (2008) and Poole (2013)), 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.

2.2.2. 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 et al. (2003), who report monthly protection rates at the *Nível 80* Brazilian industrial classification level. We match the December levels of final goods 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.[10]

Fig. 2 displays both the mean and median values of the effective rate of protection across manufacturing industries for our sample period. The early 1990s experienced sharp declines in the effective rate of protection. Mean rates fell from around 46% in 1990 to approximately 15% 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 Fig. 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 of 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, average ERP declined 27 percentage points between 1990 and 1998, some sectors liberalized more than others.[11]

2.2.3. 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

[10] A detailed discussion of the construction of our tariff measures is provided in Appendix A.2 of the updated working paper version of this paper, Krishna et al. (2011).

[11] Specifically, as we document in Appendix Table A.1 of Krishna et al. (2011), 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.

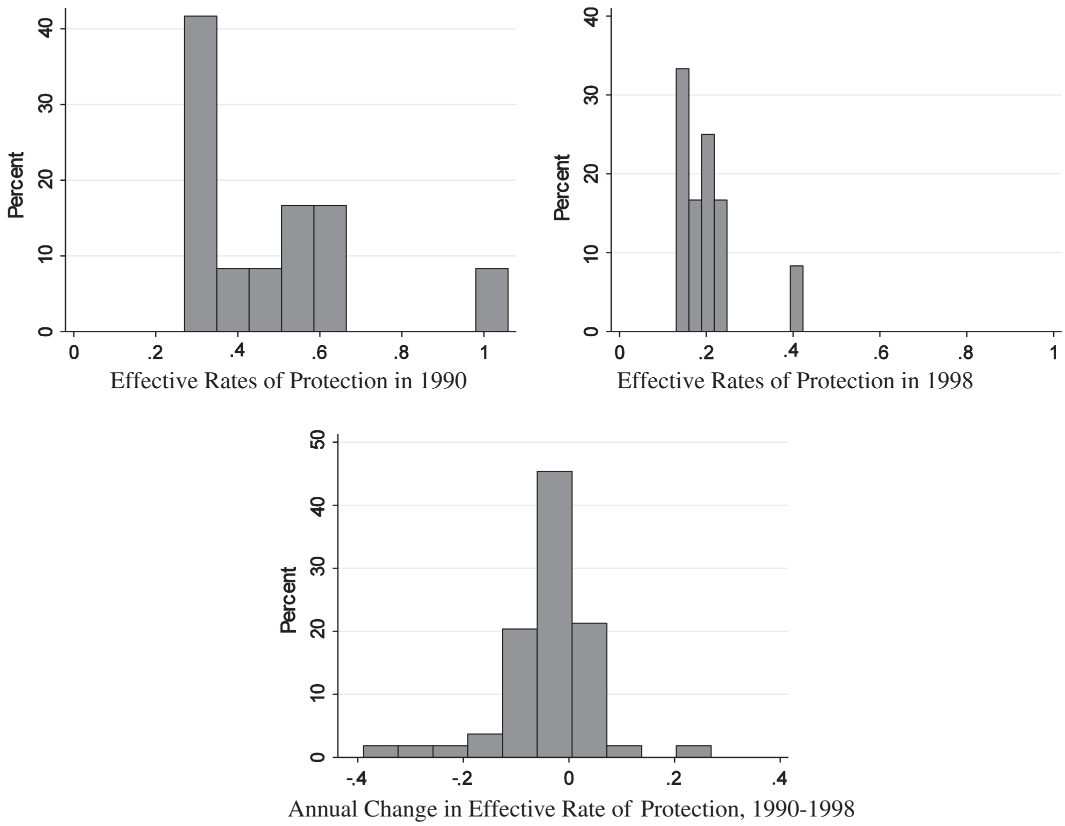

Fig. 3. Cross-industry variation in effective rates of protection.
Source: Kume et al. (2003).

dollar value of free-on-board exports in a given year and zero otherwise. We also test for the consistency of our results to alternative ways of classifying exporters.

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% of firms begin exporting, while another 6.3% of firms switch out of exporting.

2.3. Descriptive statistics

2.3.1. Worker data

The base sample described in Section 2.2.1 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 of the wage impacts of trade reform, we further restrict the data to the manufacturing sector for which we have information on trade protection levels.[12] 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, for our worker-level analyses in Sections 4 and 5, we group small firms with few movers together for a more precise estimation of the firm fixed effects, following Abowd et al. (1999). 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.[13]

Generated in this manner, the sample for our worker-level estimations (the "worker sample") consists of 504,424 worker–firm-year observations, characterizing 114,026 workers. We report detailed descriptive statistics (on wages, educational attainment and occupational composition) for the worker sample across firm-types in Panel A of Table 1. The average worker–firm match is represented in the data for 3 years, while the average worker is employed in approximately 1.5 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% of workers switch into exporting firms and 12.3% switch out of exporting firms.

2.3.2. Firm data

Again, the base sample described in Section 2.2.1 includes 2,173,888 worker–firm-year observations, 494,229 workers employed in 321,427 firms across 26 broad industries of the economy. As in the worker-level analysis, we further restrict the data to the manufacturing sector for which we have information on trade protection levels, and aggregate the data to the firm-year level.

[12] In consistency checks, we test how our restriction to manufacturing influences our main results.

[13] 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.

Table 1
Descriptive statistics.

	Full sample	Exporters	Domestic
Panel A: Worker sample			
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
Panel B: Firm sample			
Average employment	73	346	37
Average log (employment)	2.82	4.75	2.57

Sources: RAIS (1990–1998) and SECEX (1990–1998).
Note: Panel A reports descriptive averages for the worker sample used in our main worker-level analysis. Panel B reports descriptive averages for the firm sample used in our firm-level analysis.

Generated in this manner, the sample for our firm-level estimations (the "firm sample") consists of 505,369 firm-year observations.[14] We report detailed descriptive statistics for the firm sample across firm-types in Panel B of Table 1. The average firm is represented in the sample for approximately 5 years. 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 endogenous match heterogeneity into the analysis. Our firm-level specification is closely linked to that of Amiti and Davis (2012), who model the relationship between unilateral tariff changes, modes of globalization of the firm, and average firm-level wages. As we discuss in Section 2.3.2 we form a firm-level panel of population aggregates from RAIS and estimate the following specification:

$$\overline{y_{jt}} = \gamma_1 Protect_{kt} + \gamma_2 Protect_{kt} * Exp_{jt} + \gamma_3 RER_t * Exp_{jt} + \gamma_4 Exp_{jt} + \delta_{tr} + \beta Z_{jt} + \psi_j + \varepsilon_{jt} \tag{1}$$

where the dependent variable, $\overline{y_{jt}}$, is the logarithm of average wages at the firm level for firm j at time t, $Protect_{kt}$ 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 both final goods 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 inputs tariffs, the ERP is a more appropriate measure of protection faced by firms.[15] In each specification, we include an interaction term between $Protect_{kt}$ and Exp_{jt} to allow for changes in protection to have different 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.[16] The time-varying, firm-level controls, Z_{jt}, include variables available in standard firm-level datasets such as the logarithm of employment and the occupational skill composition[17] of the firm, in addition to average worker tenure at the firm, and controls for the age,[18] gender, and educational skill composition[19] of the firm. 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 policy changes that may differentially impact wages of firms in different regions of Brazil.[20] 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).[21]

In interpreting our estimates from specification (1), we focus specifically on the magnitude of the differential effect of trade policy changes on average firm-level wages at exporters relative to non-exporters (γ_2). If a decline in protection results in a differential increase in firm-level average wages in exporting firms, then γ_2 will take a value that is less than zero. The coefficient γ_1 reflects the responsiveness of average wages in firms serving only the domestic market to changes in

[14] We note that this sampling strategy produces a firm sample that may be biased towards larger employers (i.e., those employers with sampled workers). In unreported results, available by request, we 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. Moreover, recall that we restrict the worker sample to workers with at least two years of data. By contrast, the firms associated with the single observation workers remain in the firm sample, so as to minimize any potential bias from larger firms. This is the major difference between the worker sample and the firm sample. This difference, however, does not affect the conclusions from our firm-level analysis. These unreported results are available upon request.

[15] Ideally, our analysis would allow for final goods tariffs and intermediate inputs tariffs to be included separately. However, as is highlighted in Amiti and Davis (2012), a proper identification of the individual tariff effects requires a sufficient level of disaggregation, which our data do not allow giving rise to a high level of collinearity. In our preferred specifications, we follow standard econometric procedure and focus on the combined effect of the two tariffs using the effective rate of protection.

[16] We also check the consistency of our results using industry-specific real exchange rates.

[17] 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.

[18] 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.

[19] 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.

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

[21] Bertrand et al. (2004) remark that standard errors may be serially correlated in difference-in-differences estimations like ours. In unreported results, we show that our findings are robust to clustering the standard errors separately at the industry level and at the firm level.

Table 2
Trade protection and average wages.

	Tariff	ERP
Tariff	0.170**	
	(0.079)	
Export*Tariff	−0.248***	
	(0.074)	
ERP		−0.011
		(0.033)
Export*ERP		−0.103***
		(0.037)
Export*RER	−0.258***	−0.233***
	(0.058)	(0.055)
Export	0.328***	0.279***
	(0.074)	(0.067)
N	505,369	505,369
Detailed firm-level controls	YES	YES
Region specific year dummies	YES	YES
Firm fixed effects	YES	YES

Sources: RAIS (1990–1998), SECEX (1990–1998), Kume, Piani and Souza (2003), and Muendler (2003).
Note: This table reports coefficients from the ordinary least squares estimation of eq. (1) in the paper, where the dependent variable is the log of firm-level average wages for all firms. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level. Robust standard errors, clustered at the industry-year level, are reported in parentheses. Unreported covariates at the firm level include the log of employment, average worker tenure, and the age, gender, educational, and occupational composition of the firm.

protection. 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.[22]

Estimation results from Eq. (1) with tariffs as the measure of protection are reported in the first column of Table 2. 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 wages in exporting firms relative to non-exporting firms, as expected. Our data also report a strong, positive exporter dummy, consistent with previous studies at the firm level. 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.

In the second column of Table 2, we report estimation results for Eq. (1) 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 column of Table 2 for output tariffs. 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 ERP. A ten percentage point decrease in ERP increases average wages by 1.1% at exporting firms.[23]

4. 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 different degrees of trade exposure. 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 interrelated 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 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,[24] analysis conducted at the firm level will be biased.

Recent contributions to the theoretical literature on trade and labor markets emphasize the role played by observable and *ex-ante* unobservable worker characteristics in determining job assignment and wages (e.g., Yeaple (2005), Davidson et al. (2008), and Helpman et al. (2010)). In this setting, 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. Therefore, when the job mobility of workers is at least partly determined by unobservable worker–firm match quality (endogenous mobility) and thus non-random, estimates of the differential effect of trade on wages in exporting firms in Eq. (1) will be 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 right hand side variables, and thus a failure of the maintained assumption underlying the estimation. In other words, 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.

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 should note here that our work does not attempt to discern between these different models, but aims to emphasize the relevance of endogenous worker–firm matching in the context of international trade. We now proceed to explicitly test for the presence of endogenous worker mobility in our data. As we describe in the

[22] Note that when ERP is the measure of protection (instead of tariffs), γ_1 reflects the combined effect of a positive impact of a reduction in input tariffs (through prices and access to enhanced variety and quality of inputs), as well as any negative impact of increased import competition due to a decline in output tariffs. All else equal, if the industries that experience a decline in final goods tariffs also experience a decline in input tariffs, the output tariff is likely to overestimate the actual decrease in protection facing the industry. Hence, we expect the coefficient to be smaller in magnitude when the measure of protection is the ERP compared to the estimated coefficient on (output) tariffs.

[23] A wide array of additional consistency checks are provided in Appendix B of the updated working paper version of this paper, Krishna et al. (2011).

[24] 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 Eq. (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.

following subsection, we use matched employer–employee data and closely follow the recent work of Abowd et al. (2010) in constructing suitable tests of endogenous worker mobility.

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:

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

where i indexes the individual, j indexes the firm, t indexes time, and 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 Eq. (2). It is important to note, however, that the classic identifying assumption for Eq. (2) is that the idiosyncratic disturbance term in each period is mean independent of observable worker and firm characteristics as well as firm and worker fixed effects, as follows:

$$E\left(\varepsilon_{ijt}\middle|\alpha_i, \psi_{\mathrm{j(i,t)}}, X_{it}, Z_{jt}\right) = 0.$$

Often referred to in the literature as the assumption of "conditional exogenous mobility" (see, for instance, Abowd et al. (1999), Woodcock (2011), Sørensen and Vejlin (2011), and Abowd and Schmutte (2012)), it 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 labor markets with frictions. Importantly, in the context of international trade, for example, 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*. 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 at exporters. In this case, the estimates of Eq. (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 Eq. (2).

We test the validity of the exogenous mobility assumption using the "match effects test" introduced by Abowd et al. (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, $(\bar{\varepsilon}_{ijt-1})$, (within quintiles of the residual distribution) should not predict the transition across heterogeneous firms (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 Eq. (2) for the worker sample described in Section 2.3.1. Then, for workers who switched employers between time t and $t-1$, the average residual within worker and firm $(\bar{\varepsilon}_{ijt-1})$ is calculated for the complete duration of the match (i.e., until $t-1$). $(\bar{\varepsilon}_{ijt-1})$ 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 $\psi_{j(i,t-1)}$ quintile to the current employer's $\psi_{j(i,t)}$ quintile, should be independent of $(\bar{\varepsilon}_{ijt-1})$. Importantly, if the null hypothesis is rejected for our data, this would suggest that the estimation results from Eq. (2), and by extension Eq. (1), are biased. We calculate the test statistic both by grouping small firms with less than two movers into one firm (as we describe in Section 2.3.1), 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.

Our data strongly reject the null hypothesis of exogenous mobility for the sample of job switchers in our data. The match effects test statistic, distributed chi-squared with 496 degrees of freedom,[25] has a value $\chi^2 = 8600$ when we group small firms and $\chi^2 = 19{,}000$ when we omit these firms from the sample (and thus statistically significant at the 1% level of significance). 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.

To further emphasize this point, following Abowd, McKinney, and Schmutte (2010) and Schmutte (forthcoming), we illustrate worker employment transitions in a series of plots. Fig. 4 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 $(\bar{\varepsilon}_{ijt-1})$, 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. Fig. 4 demonstrates that this is not the case in our data. For example, while job changers in the extremes of the match distribution ($\mathrm{Q}(\bar{\varepsilon}_{ijt-1})$ and $\mathrm{Q}(\bar{\varepsilon}_{ijt-1}) = 5$) are most likely to originate from the lower-middle of the $\psi_{j(i,t-1)}$ distribution ($\mathrm{Q}(\psi_{j(i,t-1)}) = 2$), job changers in the middle of the match effect distribution $\mathrm{Q}(\bar{\varepsilon}_{ijt-1}) = 3$ most often originate from the first quintile of the $\psi_{j(i,t-1)}$ distribution.

In Fig. 5, 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, Figs. 6a–c illustrate that these transition probabilities vary across quintiles of the match effect distribution. Figs. 6a,6b, and 6c plot the transition probabilities for the first, third, and fifth quintiles of the match effects distribution, respectively. The figures show 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 quintile than job changers in the middle of the match effects distribution. This is most notable for $\mathrm{Q}\ (\bar{\varepsilon}_{ijt-1}) = 5$ in Fig. 6c. 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.

[25] The degrees of freedom are calculated as $\left(\#\mathrm{Q}(\alpha_i) * \#\mathrm{Q}\left(\psi_{\mathrm{j(i,t-1)}}\right)\right) * \#\mathrm{Q}\left(\psi_{\mathrm{j(i,t)}}\right) - 1 * \#\mathrm{Q}(\bar{\varepsilon}_{ijt-1}) - 1) = (5*5*5-1)*(5-1) = 496$ where #Q denotes the number of quintiles.

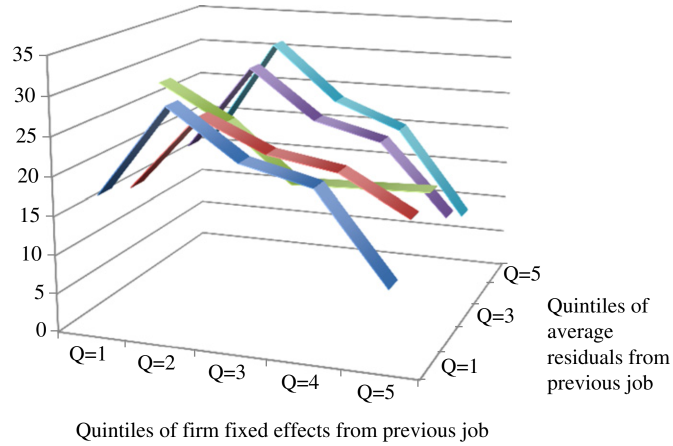

Fig. 4. Distribution of quintiles of the firm-fixed effects for the previous job ($\psi_{j(i,t\text{ - }1)}$) by quintiles of average residuals ($\bar{\varepsilon}_{ijt-1}$).

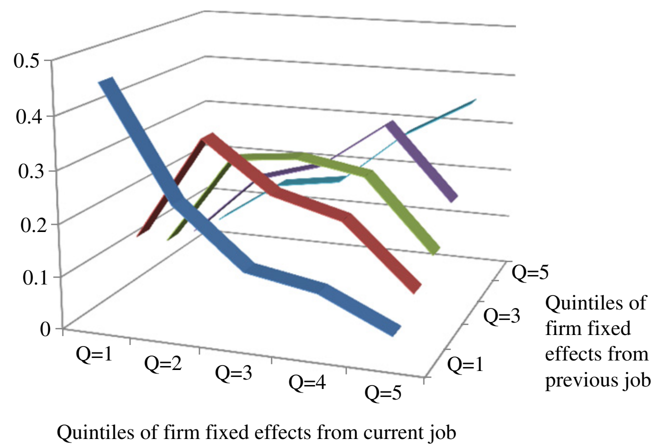

Fig. 5. Transition probabilities between quintiles of firm-fixed effects in previous job ($\psi_{j(i,t\text{ - }1)}$) and current job ($\psi_{j(i,t)}$).

This is evidenced, for instance, by the relatively flat surface in lower-right quadrant of Fig. 6b,[26] where few transitions from low effect to high effect employers are seen. Importantly, the differences between 6a, 6b and 6c indicate that there is considerable variation in transition probabilities across the different quintiles of the match effects distribution. Taken together, Figs. 4, 5 and 6a–c clearly illustrate the failure of the exogenous mobility assumption, as the estimated match effects contain information on job-to-job transitions, confirming the conclusions reflected in the value of the test statistic for exogenous mobility that we report.[27]

5. Worker-level analysis with match fixed effects

When match-specific productivity is important in wage determination, the Abowd et al. (1999) specification in Eq. (2) which includes only worker and firm fixed effects will result in 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 et al. (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, such as the one in Eq. (1), 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 Eq. (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 and augment Eq. (1) as follows:

$$\begin{aligned} y_{ijt} = {} & \gamma_1{}'Protect_{kt} + \gamma_2{}'Protect_{kt} * Exp_{jt} + \gamma_3{}'RER_t * Exp_{jt} + \gamma_4{}'Exp_{jt} \\ & + M_{ij} + \delta_{tr} + \phi X_{it} + \beta Z_{jt} + \varepsilon_{ijt} \end{aligned} \tag{3}$$

where M_{ij}, a given worker i's employment at a given firm j, denotes worker–firm match fixed effects (or job-spell fixed effects). All other variables are as previously defined.

[26] This is consistent with the finding in Abowd et al. (2010) for the United States that the likelihood for upgrading the firm effect is strongest for workers at the median match effect.

[27] In results available by request, we compute the Abowd et al. (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.

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 ($\gamma_2{}'$) controlling for worker, firm, and match effects rather than in estimating separately the worker and firm effects.[28] 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 $\gamma_2{}'$ is different than the analogous coefficient in Eq. (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 note that the inclusion of time-invariant match effects, M_{ij}, on the right hand side of Eq. (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} ($E(\varepsilon_{ijt}|\alpha_i, \psi_{j(i,t)}, X_{it}, Z_{jt}, M_{ij}) = 0$). As discussed in Abowd et al. (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 with match fixed effects allows for an important source of endogenous worker mobility, which may arise due to time-invariant match-specific productivity, but clearly 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.[29]

5.1. Worker-level estimation results

Table 3 reports estimation results from Eq. (3) with match fixed effects for both tariffs (in the left panel) and ERP (in the right panel) as the measure of protection, using the worker sample described in Section 2.3.1.[30] In our estimation of Eq. (3), we cluster standard errors at the industry-year level.[31]

Our estimates suggest that the inclusion of match-specific productivity effects results in insignificant estimates of both the coefficient on the trade protection measure ($\gamma_1{}'$) and the coefficient measuring the differential impact of trade reform on workers employed in exporting firms ($\gamma_2{}'$). Note that the point estimates of the differential effect of the change in protection on exporters reported in Table 3 are also much lower (by more than half) than the corresponding estimates based on firm-level data, reported in Table 2.[32] Further, Table 3 also indicates that once we allow for match-specific productivity, as well as a wide array of worker and firm level controls in explaining individual wages, the premium paid to workers at exporting firms (evaluated at mean values of ERP and RER as the coefficient on export status, plus the coefficient on the export–ERP interaction times the mean value of the ERP, plus the coefficient on the export–RER interaction times the mean value of the RER) is economically and statistically insignificant.[33]

As we have previously discussed, our analysis cannot account for endogenous mobility related to time-varying match-specific productivity. Indeed, while the theory on trade and labor markets has considered that worker composition may change with export status, it has not, to our knowledge, actually allowed for time-varying match-specific effects between a given worker and a given firm. Nevertheless, time-varying match effects remain an empirical possibility. To explore the hypothesis that changes in export status may result in a change in match-specific effects, we re-estimate our main worker-level regression from Eq. (3) with an interactive exporter-match fixed effect. That is, we allow the match effect to vary by the export status of the firm; a new match is assigned for any worker–firm match in which the firm switches export status.[34] Our main parameter of interest on the interaction term between protection and export status continues to be statistically insignificant (see columns (2) and (4) for tariffs and ERP, respectively) when we incorporate interactive exporter-match fixed effects,[35] that is, even when we allow for match effects that vary over time (with the export status of the firm), the differential impact of trade on the wages of workers at exporting firms relative to otherwise identical workers at domestic firms appears to be insignificantly different from zero.

In Table 4, we conduct a wide array of tests to confirm that our main results hold across a number of specifications. The first two columns report estimation results from specification (3) for alternative samples. As we describe in the data section, the regressions in Table 3 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. Our main interaction coefficient of interest remains statistically insignificant.

Menezes-Filho and Muendler (2011) document that Brazil's trade liberalization increased transitions from formal manufacturing to the service sector, unemployment, and out of the labor force. In the next two columns, we test the importance of our restriction to the formal manufacturing sector on the results. First, we include the agriculture and mining sectors, as important tradable sectors for Brazil for which we also have information on tariff rates and export status, into our main analysis. Second, in a similar fashion, we extend the sample further to include traditionally non-traded sectors, like services, which

[28] 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 et al. (2010)) or decomposing wage variation into variation arising from firm heterogeneity and worker heterogeneity (Frías et al. (2009)) *inter alia*, match effects should be included in addition to firm and worker fixed effects. See Woodcock (2011) for details.

[29] Furthermore, as we will discuss shortly, in a modified specification, we allow for a particular form of time variation in the worker–firm effects, by allowing the magnitude of the worker–firm match effect to change when firms change export status.

[30] As we discuss in Section 2.3.2, given our computing capacity, the data available to us, and the questions we ask, we consider slightly different samples between Sections 3 and 5. Footnote 14 offers further details on these sample differences. We note, however, that our main result from the firm-level analysis (that exporters differentially increase wages in response to trade reform) continues to hold when we only consider the set of firms in our worker sample.

[31] However, as in the firm-level analysis, our main results hold when we cluster the standard errors at both the industry level and the firm level to account for the possibility of serially-correlated errors.

[32] Note that our estimates in the baseline specification reported in Table 3 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 4). We thank Rodney Ludema for a very helpful discussion on this point.

[33] This finding of an insignificant exporter premium is consistent with the results of Schank et al. (2007) for Germany.

[34] Identification in this model is a result of within-exporter-match changes over time in tariffs across firm-types. As such, we can no longer identify the exporter dummy, which is constant within a firm-type-match.

[35] We thank an anonymous referee for this valuable suggestion.

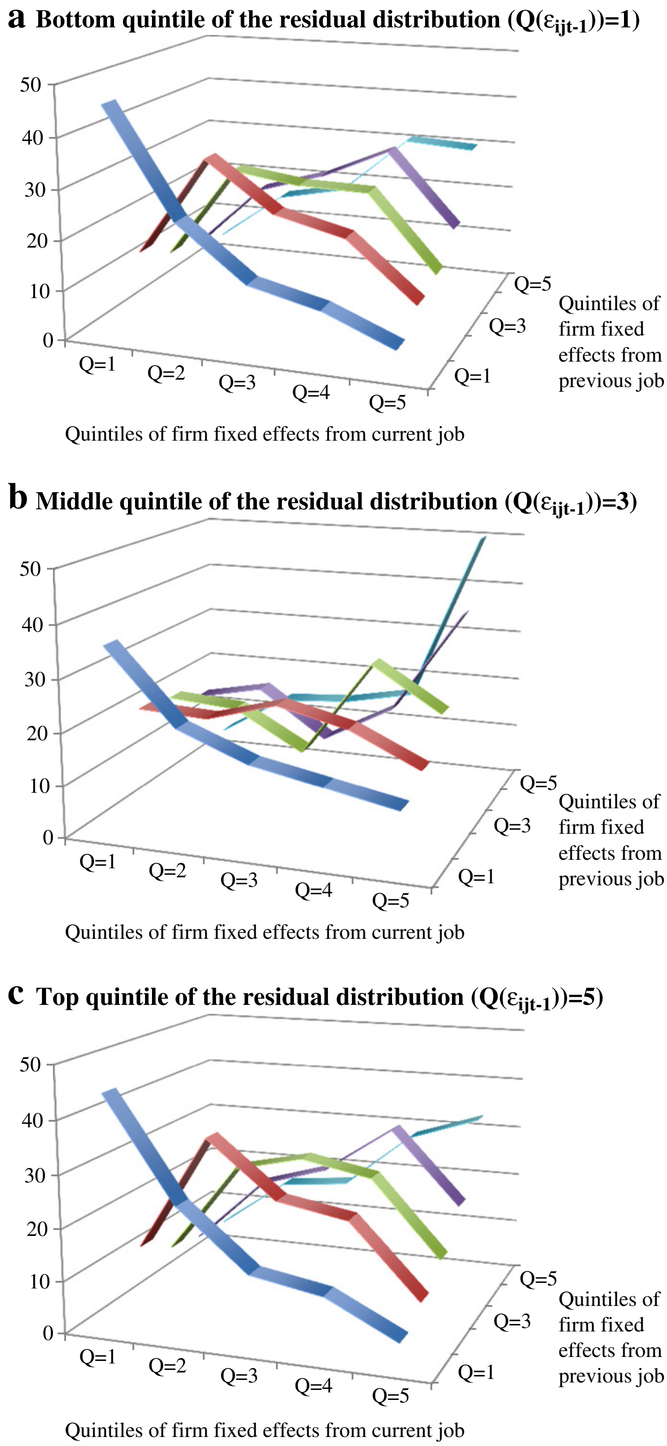

Fig. 6. Transition probabilities between quintiles of firm-fixed effects in previous job ($\psi_{j(i,t-1)}$) and current job ($\psi_{j(i,t-1)}$), by quintiles of average residuals. ($\bar{\varepsilon}_{ijt-1}$).

employ a large portion of the Brazilian population.[36] The results are consistent with our main analysis for the manufacturing sector only.

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.[37] 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, returns to skill) that might exhibit sector-level variation, and that

[36] We do not, however, have export status or tariff information for these sectors. Therefore, in order to understand the implications of including all sectors of the economy in our main estimation, we follow recent work (e.g., Topalova (2010), McCaig and Pavcnik (2012), and Kovak (2013)) and denote tariff rates for all non-traded industries to be zero and denote all firms in these industries to be non-exporters.

[37] We construct industry-specific real exchange rates using time-varying trade weights, as in Goldberg (2004). More specifically, we calculate $RER_{ind_t^k} = \sum_c \left(0.5\frac{X_{t-1}^{kc}}{\sum_c X_{t-1}^{kc}} + 0.5\frac{M_{t-1}^{kc}}{\sum_c M_{t-1}^{kc}}\right) RER_t^c$ 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 and from country c at time $t-1$.

Table 3
Trade protection and individual wages.

	Tariff		ERP	
Tariff	0.133	0.101		
	(0.177)	(0.176)		
Export*Tariff	−0.110	−0.083		
	(0.090)	(0.107)		
ERP			−0.020	−0.035
			(0.073)	(0.073)
Export*ERP			−0.045	−0.028
			(0.045)	(0.054)
Export*RER	−0.094*	−0.118*	−0.085*	−0.110*
	(0.056)	(0.062)	(0.051)	(0.057)
Export	0.133*		0.113*	
	(0.072)		(0.061)	
N	504,424	504,424	504,424	504,424
Detailed firm-level controls	YES	YES	YES	YES
Detailed worker-level controls	YES	YES	YES	YES
Region-specific year dummies	YES	YES	YES	YES
Match fixed effects	YES	NO	YES	NO
Exporter-match fixed effects	NO	YES	NO	YES

Sources: RAIS (1990–1998), SECEX (1990–1998), Kume et al. (2003), and Muendler (2003).
Note: This table reports coefficients from the ordinary least squares estimation of eq. (3) in the paper, where the dependent variable is the log annual individual real wage for all worker–firm matches. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level. Robust standard errors, clustered at the industry-year level, are reported in parentheses. Unreported covariates at the firm level include the log of employment, average worker tenure, and the age, gender, educational, and occupational composition of the firm. Unreported covariates at the worker level include indicators for the worker's age, occupation, and education, as well as the worker's current tenure at the firm.

may not be fully captured by firm fixed effects or region-specific year dummies.[38] Our results continue to hold when using an industry-specific real exchange rate measure and sector-year indicator variables in the 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 specification, 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.[39] 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 et al. (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, biasing coefficients to zero. Therefore, in the next two columns, we consider only firms which exported for three consecutive years and firms with worldwide export sales greater than the 10th percentile[40] as exporting firms, respectively. That is, firms which switch in and out of exporting over a short horizon or export very small quantities are considered non-exporters. Our results continue to be statistically insignificant when we consider these changes in the exporting cutoff. In addition, to alleviate potential concerns regarding endogeneity of the export decision, we follow Amiti and Davis (2012) and consider a time-invariant measure of export status. We define a firm as an exporter if it exported a positive dollar value at the beginning of our sample (1990) and zero otherwise. Our main coefficient of interest remains insignificant, and the point estimate is similar in magnitude.

Until now, all of our tests have relied on the extensive margin of exporting. In the final two columns, we exploit information on the intensive export margin, as measured by the logarithm of exports (plus one) and the logarithm of exports (plus one) per worker. When we use the magnitude of exports 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.[41]

5.2. Discussion of worker-level results

Our findings 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 at the firm level using average firm-level wages. 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 coefficient on the differential impact of trade reform on average firm-level wages at exporters from Eq. (1), without controlling for match effects, would mistakenly be estimated as significant even in the absence of any true effect.

Key to this argument is that exporters differentially increase workforce quality (in terms of innate worker ability and/or in terms of match-specific productivity) post-trade reform. In Table 5, we use the match effects estimated from Eq. (3) to confirm this point. Specifically, we calculate the average match effect for all firms in a pre-liberalization year (1990) and in a post-liberalization year (1998).[42] We then report the average (and median) match effect across all firms, all exporting firms, and all non-exporting firms in the pre- and post-trade reform years. The data document that average match quality at exporters increased between 1990 and 1998 (from 0.136 to 0.160), meanwhile average match quality at non-exporters decreased from −0.019 to −0.069.[43] Note that the magnitude of the estimated match effect is also larger for exporters than for non-exporters in both 1990 and 1998. Recall that the match fixed effects in Eq. (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] The relative improvement in the distribution of match-specific ability in exporting firms is consistent with models

[38] 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.

[39] This is consistent with evidence presented from a wide range of countries, including France (Eaton et al. (2004)), the United States (Bernard and Jensen (1995)), Chile (Blum et al. (2012)) and Colombia (Eaton et al. (2008)).

[40] Over our sample period, the 10th percentile of the value of exports is approximately $5700.

[41] Even further tests are reported in the working paper version (Krishna et al. (2011)).

[42] To be clear, though estimated match effects from Eq. (3) are time-invariant, the firm's average match quality changes over time as the firm's workforce changes.

[43] More formally, when we regress estimated average match quality in a firm on the trade and export status variables, while controlling for firm fixed effects and observable firm characteristics as in Eq. (1), we find that average match quality increases at exporters relative to non-exporters with a decline in ERP, supporting the notion that exporters differentially increase workforce quality post-trade reform. We thank an anonymous referee for this valuable suggestion.

[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.

Table 4
ERP and individual wages.

	Alternative samples						Export status			Intensive margin	
	5% metro male sample	Lib. period 1990–1994	With agriculture and mining	With all sectors	Industry-specific RER	Sector-year dummies	Three consecutive years	Value cutoff 10th percentile	Exporter in 1990	Log (value of exports)	Log (exports per worker)
ERP	−0.044	−0.040	−0.067	−0.031	−0.025		−0.026	−0.012	−0.015	−0.023	−0.039
	(0.059)	(0.092)	(0.064)	(0.055)	(0.074)		(0.072)	(0.073)	(0.074)	(0.072)	(0.070)
Export*ERP	−0.031	−0.046	−0.018	−0.049	−0.035	−0.012	−0.037	−0.058	−0.057	−0.003	−0.003
	(0.050)	(0.045)	(0.046)	(0.047)	(0.044)	(0.036)	(0.042)	(0.044)	(0.050)	(0.003)	(0.003)
Export*RER	−0.019	−0.053	−0.065	−0.107**	−0.003***	−0.076	−0.116**	−0.089*	−0.124**	−0.006*	−0.006
	(0.047)	(0.051)	(0.049)	(0.051)	(0.001)	(0.050)	(0.053)	(0.051)	(0.053)	(0.004)	(0.004)
Export	0.054	0.076	0.085	0.150**	0.252***	0.096	0.130**	0.122**	0.129	0.009**	0.009*
	(0.055)	(0.058)	(0.059)	(0.065)	(0.001)	(0.060)	(0.064)	(0.062)	(0.083)	(0.004)	(0.005)
N	447,957	269,951	607,648	2,028,894	504,424	504,424	504,424	504,424	504,424	504,424	504,424
Detailed firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Detailed worker-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-specific year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Match fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Sources: RAIS (1990–1998), SECEX (1990–1998), Kume et al. (2003), Muendler (2003), IMF bilateral real exchange rates, and NBER trade flows.
Note: This table reports coefficients from ordinary least squares estimations, where the dependent variable is the log annual individual real wage for all worker–firm matches. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level. Robust standard errors, clustered at the industry-year level, are reported in parentheses. Unreported covariates at the firm level include the log of employment, average worker tenure, and the age, gender, educational, and occupational composition of the firm. Unreported covariates at the worker level include indicators for the worker's age, occupation, and education, as well as the worker's current tenure at the firm.

Table 5
Estimated match effects over time.

	Average match effect			
	1990		1998	
	Median	Mean	Median	Mean
Exporters	0.145	0.136	0.169	0.160
Non-exporters	0.017	−0.019	−0.055	−0.069
All firms	0.065	0.031	0.059	0.034

Sources: RAIS (1990–1998), SECEX (1990–1998), Kume et al. (2003), and Muendler (2003).
Note: This table reports average match effects across all workers within a firm, by firm-type, at the beginning (1990) and the end (1998) of the sample.

emphasizing non-random allocation of workers across production activities.[45]

This central 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, 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.

A comparison of estimates obtained from specifications with and without match fixed effects suggests that match effects matter both qualitatively and quantitatively. Table 6 compares estimates obtained from alternate specifications with only firm fixed effects included, 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. Consequently, for the purposes of Table 6, we further restrict the worker sample defined in Section 2.3.1, to workers who switch employers. As expected, the inclusion of worker fixed effects lowers (in absolute value) the magnitude of the point estimate on γ_2' from 0.076 to 0.050. The inclusion of match fixed effects lowers the coefficient further from 0.050 to 0.017.

6. Conclusion

This paper, which explores the role of endogenous worker mobility for the analysis of the effect of trade liberalization on labor markets using matched employer-employee data from Brazil, provides striking results. Once we allow for endogenous mobility due to worker–firm specific matches, we find, in contrast to most previous studies, an insignificant exporter premium and also an insignificant differential effect of trade liberalization on the wages of workers at exporting firms relative to identical workers at non-exporting firms. We also find that workforce composition post-liberalization improves systematically in exporting firms in terms of the combination of innate worker ability and the quality of worker–firm

[45] 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 Yeaple (2005), Davidson et al. (2008), and Helpman et al. (2010), we should note that there are other dimensions along which the data are not entirely in line with the predictions of these theories. For instance, complementarities between workers' abilities and firm productivity in Helpman et al. (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 et al. (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').

Table 6
Alternative specifications for switchers.

	Only firm effects	Both firm and worker effects	Match effects
ERP	0.060	0.020	−0.011
	(0.081)	(0.063)	(0.095)
Export*ERP	−0.076	−0.050	−0.017
	(0.069)	(0.050)	(0.058)
Export*RER	−0.102	−0.097*	−0.071
	(0.070)	(0.054)	(0.063)
Export	0.188**	0.144**	0.098
	(0.087)	(0.066)	(0.076)
N	226,193	226,193	226,193
Detailed firm-level controls	Yes	Yes	Yes
Detailed worker-level controls	Yes	Yes	Yes
Region-specific year dummies	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	No
Worker fixed effects	No	Yes	No
Match fixed effects	No	No	Yes

Sources: RAIS (1990–1998), SECEX (1990–1998), Kume et al. (2003), and Muendler (2003).
Note: This table reports coefficients from ordinary least squares estimations, where the dependent variable is the log annual individual real wage for all worker–firm matches. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level. Robust standard errors, clustered at the industry-year level, are reported in parentheses. Unreported covariates at the firm level include the log of employment, average worker tenure, and the age, gender, educational, and occupational composition of the firm. Unreported covariates at the worker level include indicators for the worker's age, occupation, and education, as well as the worker's current tenure at the firm.

matches. Endogenous matching of workers with firms is thus crucial in determining wage outcomes for workers in open economies.

Future work in this area will perhaps focus on these allocation mechanisms and the associated consequences for wage determination by exploring in greater detail mobility patterns of workers between exporting and non-exporting firms and changes in these transition probabilities in order to better understand the mechanisms by which exporters upgrade match quality after liberalization. We note also that the conceptual issues engaged by this paper are quite general ones; the greater availability of matched worker–firm data sets from other countries in recent years should allow for similar analytical explorations in other empirical settings.

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