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ABSTRACT

Tracking individual workers across jobs after Brazil's trade liberalization in the 1990s shows that tariff cuts trigger worker displacements, but neither exporters nor comparative-advantage sectors absorb trade-displaced labor. On the contrary, exporters separate from significantly more and hire fewer workers than the average employer. Trade liberalization increases transitions to services, unemployment, and out of the labor force. Results are consistent with faster labor productivity growth than sales expansions so that output shifts to more productive firms while labor does not. Higher rates of failed reallocations and longer durations of complete reallocations result, associated with a costly incidence of idle resources.

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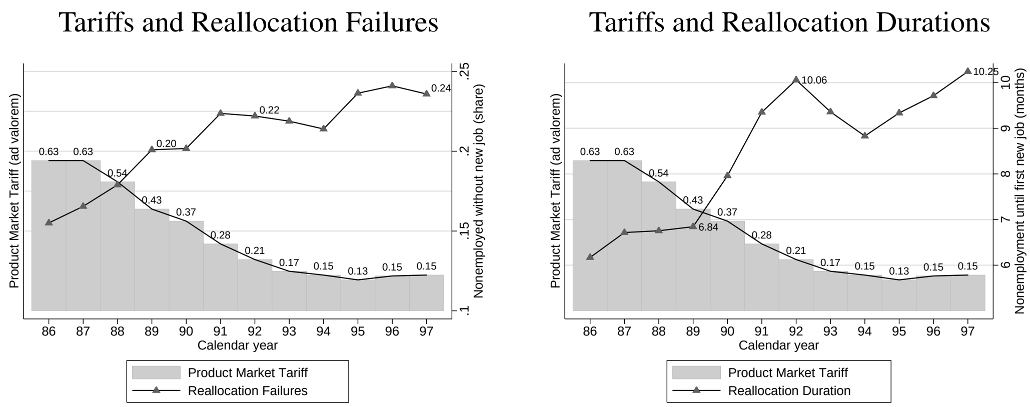
1 Introduction

Economists have long studied the consequences of foreign trade for domestic markets. At the heart of welfare gains from trade is the expansion of consumption possibilities and the reallocation of production factors. Yet research to examine the impact of trade liberalization on workers' individual employment trajectories across employers over time is scant. We use economy-wide linked employer-employee data and investigate resource reallocation directly, by following workers across employers and industries before and after a major trade reform in Brazil.

We find that Brazil's trade liberalization triggers worker displacements, particularly from more protected industries, as trade theory predicts and welcomes. But neither exporting firms nor comparative-advantage industries absorb trade-displaced workers for several years. In fact, exporters separate from significantly more and hire significantly fewer workers than the average employer after trade liberalization. Trade also heightens transitions out of the formal manufacturing sector and into services, unemployment, or out of the labor force. Idle resources result, which we observe in higher rates of failed reallocations and longer durations of complete reallocations.

Theory predicts that more efficient producers gain product-market shares as trade barriers fall (Melitz 2003). Evidence on trade liberalization episodes confirms this output reallocation (e.g. Pavcnik 2002). Responding to trade opportunities, employers upgrade skills and technology (Verhoogen 2008, Bustos 2011). In contrast, evidence on trade-induced labor reallocation is weak: there is a lack of reallocation evidence in developing countries (for a survey see Goldberg and Pavcnik 2007) and there is no detectable sectoral difference of trade exposure on displaced workers in industrialized countries (see e.g. Kruse 1988, Kletzer 2001, for the United States). We find clear trade-related worker flows. But more productive firms fail to expand employment after trade liberalization so that sales shift to more productive firms while labor moves in the reverse direction or remains unallocated.[1] Our results are consistent with rising efficiency at surviving firms under heightened competition (as in Raith 2003, for instance) and with more productive firms that

[1] During the 1990s, labor reallocation and productivity change are negatively related throughout Latin America (Pagés, Pierre and Scarpetta 2009). Brazil ranks around the world's median in exports per capita: according to WTF and WDI data for 2000, Brazil takes the 48th percentile in per-capita exports. As regards Brazil's labor cost, Heckman and Pagés (2004) estimate the expected value of total labor costs in Brazil to equal 50 months of pay, compared to an OECD average of 42 months, and assess Brazilian social security contributions at 75 percent of total labor costs (40 months), compared to an average of 96 percent in OECD countries (38 months).



Sources: RAIS 1986-2001 (1-percent random sample), workers nationwide of any gender or age, separated from a formal-sector job; not re-acceding into a formal-sector job within 48 months (*left graph*) or re-acceding into a formal-sector job within 48 months (*right graph*). Product tariffs from Kume, Piani and Souza (2003), employment weighted at *Nível 50* sector level in 1988.

Figure 1: **Tariffs and labor-market performance**

expand output while employing fewer workers under Brazil's below-unity elasticities of demand (Asano and Fiuza 2003).[2] Our estimates and simulations confirm that a large part of the increase in nonemployment during the early 1990s is attributable to trade liberalization.

This direct labor-market evidence offers a novel explanation why pro-competitive reforms can be associated with strong efficiency gains at the employer and industry level but not in the aggregate, where idle resources result. Figure 1 shows our two main measures of idleness in Brazil during the 1990s and plots them against Brazil's falling trade barriers. The share of displaced workers with no reallocation for four years rises from below 18 percent before 1990 to 22 percent by 1993. The duration of complete reallocations lasts nine months by 1993, up from six months and less before 1990. Conservatively measured, the foregone annual wage bill from the increase in reallocation durations and failures after 1990 amounts to between one and four percent of GDP.

To measure the effect of trade on employment trajectories, we combine administrative employer-employee records that provide detailed demographic information on the universe of formally employed workers with employer-level data on export status and labor productivity. The time horizon of sixteen years from 1986 to 2001 allows us to measure idle resources for a long period. Use

[2] For a common class of open-economy models, it depends on the elasticity of demand whether a sector's productivity increase results in an employment expansion (see e.g. Obstfeld and Rogoff 1996, ch. 4.3.2).

of worker panel data is crucial. Our findings show that otherwise unobserved worker characteristics are important determinants of transitions and obscure the estimated impact of trade on labor market transitions, if not controlled for. The link to individual employer data is necessary to document the direction of worker flows not just across sectors but within sectors, where most reallocations occur (see also Levinsohn 1999). Our findings confirm that employer characteristics interact significantly with trade exposure. The results are robust to sector effects, year effects and sector-specific trends, and standard errors are estimated under simultaneous clustering at the worker and sector level. We control for other concomitant economic changes, including macroeconomic stabilization, foreign direct investment, privatization, service-job outsourcing, and a pre-trade reform of labor-market regulations. We construct instrumental variables for tariffs and exports, using sectoral imports from other source countries than Brazil in foreign destinations and foreign price components of the sectoral real exchange rate. The instruments are strong predictors of export behavior and trade policy, and address potential simultaneity between labor-market changes and trade.

Our paper is related to several strands of the literature. A number of industry-level studies use measures of job creation, destruction, and churning (excess turnover beyond net change). Haltiwanger, Kugler, Kugler, Micco and Pagés (2004), for instance, show for six Latin American countries that tariff reductions are associated with heightened within-sector churning and net employment reductions at the sector level. For trade liberalization episodes in a large sample of countries, Wacziarg and Wallack (2004) fail to detect significant employment shifts. Employer-level studies show for several countries that trade reforms are associated with product market reallocation towards more efficient producers and employment reductions at producers that are adversely affected by trade (see e.g. Trefler 2004, Verhoogen 2008).[3] After trade reform in Brazil, employment contracts at highly productive firms. Linked employer-employee data allow us to observe the resulting incidence of idle resources and to estimate hirings separately from separations. The hiring margin is critical because it determines the job finding rate, which regulates nonemployment.

Using labor surveys aggregated to the sector level, Goldberg and Pavcnik (2003) report no

[3] In contrast, Currie and Harrison (1997) do not find Moroccan firms to adjust employment after trade reform, and Revenga (1997) reports that trade reform reduces employment at Mexican firms with no detectable cross-sectoral reallocation. Using import penetration as predictor, Revenga (1992) detects significant employment effects in U.S. industries. At the individual worker level, however, Kruse (1988) and Kletzer (2001) show that trajectories of displaced U.S. workers are largely explained by differences in workforce characteristics across sectors and not by the sector's trade exposure.

statistically significant relation between informal work and trade in Brazil. Our estimates confirm this finding at the individual level, controlling for the worker's employment history. We do find, however, that tariff reductions raise transitions to unemployment and out of the labor force.[4] Amiti and Konings (2007) and Goldberg, Khandelwal, Pavcnik and Topalova (2010) stress the role of intermediate inputs for productivity change and the introduction of new products at the firm level. In line with those results, we find in several specifications that lower input tariffs raise worker retentions—either because lower input costs alleviate competitive pressure or because they permit the adoption of more efficient production and additional products.

A number of theoretical papers examine implications of trade for labor market reallocation under institutional frictions, extending the work of Davidson, Martin and Matusz (1988). In models by Kambourov (2009) and Helpman, Itskhoki and Redding (2010), for instance, the reallocation of workers following trade liberalization depends on the country's labor market institutions, such as firing costs and search frictions.[5] For the United States, Artuç, Chaudhuri and McLaren (2010) show in structural estimation that sizable switching costs for individual workers between job locations dampen mobility after trade shocks.[6] Except for a differential impact of high-school education on hirings, we do not find Brazil's trade reform to exert a significant heterogeneous effect on workers after controlling for employer characteristics. This underlines the importance of firm-level responses. Coşar, Guner and Tybout (2011) model hiring and firing frictions for heterogeneous firms and structurally estimate an extended Melitz (2003) model for worker transitions using Colombian firm data. Their simulations suggest only a minor effect of Colombia's import tariff reduction on labor markets, however. This paper aims to complement those structural approaches by estimating the impact of trade liberalization on workers' observed employment trajectories under few assumptions. We document salient employment responses to trade reform and spells of nonemployment as a source of adjustment cost to trade reform.

[4] A related strand of the literature uses the spatial distribution of industries to study effects of trade liberalizations on regional outcomes (Topalova 2010, Hasan, Mitra, Ranjan and Ahsand 2011) and cross-regional mobility (Aguayo-Tellez, Muendler and Poole 2010, Kovak 2011).

[5] Davis and Harrigan (2011) and Egger and Kreickemeier (2009) derive related implications in trade models under efficiency-wage and fair-wage compensation, respectively.

[6] Dix-Carneiro (2010) introduces transferrable human capital with differential returns across sectors and affirms, in structural estimation using Brazilian worker panel data, that individual switching costs matter more for labor-market outcomes than foregone human capital returns. Coşar (2010), in contrast, calibrates a trade model to Brazilian data under sector-specific human capital and search frictions and finds a larger role of human capital.

The paper is organized in five more sections. The next Section 2 presents our data and reports descriptive evidence on trade and labor reallocation. Section 3 analyzes worker separations and accessions to identify sector and firm predictors that explain labor-market outcomes. Section 4 reports estimation of trade-related worker reallocations across sectors and work status transitions from a household perspective. Section 5 conducts counterfactual simulations based on a law of motion that generates the relevant labor reallocation. Section 6 concludes.

2 Labor Reallocation, Productivity and Trade

Labor reallocation is the reassignment of workers to jobs across employers and sectors. Our concern is with potentially idle labor: displaced workers who await formal-sector reallocation. We track Brazil's labor reallocation with two main data sets for workers of any age and gender. First, we construct data on the basis of Brazil's comprehensive linked employer-employee records for the 16-year time span from 1986 through 2001. Linked employer-employee records document two margins that change the pool of workers to be reallocated: separations from formal jobs that fill the nonemployment pool, and accessions into formal jobs that empty the nonemployment pool. Second, we use household data (without employer information) to obtain information on transitions into detailed types of work status outside formal employment.

Data. The linked employer-employee records RAIS (*Relação Anual de Informações Sociais*) document all formally employed workers and their individual employers over time so that we can cover national formal-sector migration and condition on worker-fixed effects in estimation. Every job observation is identified by the worker ID (PIS), the plant ID (of which the firm ID is a systematic part), the month of accession, and the month of separation. To construct the worker sample, we take the list of all proper worker IDs (11-digit PIS) that ever appear in RAIS, draw a ten-percent random sample of workers of any age and gender, and then trace the selected workers through their formal jobs. For most statistics, we remove multiple jobs and only retain a worker's highest paying job at a given moment.[7]

[7]We use a subsample of prime-age males in our working paper (Menezes-Filho and Muendler 2007) and find largely similar results.

We define a worker's separation as a quit or layoff from the last formal employment in the calendar year. Conversely, we define an accession as a worker's hiring into the first formal employment in the calendar year. When we infer separations and accessions, we exclude transfers across plants within the same firm, as well as retirements and reported deaths on the job. Among the separations, reported quits are infrequent compared to layoffs (see Table E.1 in the Appendix). We consider separations as a single category in reported regressions. There is no marked difference between quits and layoffs in separate regressions. Worker information covers education, tenure, age, and gender; job information includes an occupation classification comparable to ISCO-88 (four-digit level) and the wage; plant information covers sector, municipality, and public-private ownership categories (see Appendix A for details). In RAIS, we base our sector information on a mix of the subsector *IBGE* classification (roughly comparable to the *NAICS* three-digit level) for the years 1986 through 1993 and the more detailed *CNAE* classification (close to Europe's *NACE* four-digit level) for the later sample years 1994 through 2001.

To RAIS, we link information on the employer's export status from national customs records. Annual customs office records on exports are available to us from SECEX (*Secretaria de Comércio Exterior*) for 1990 through 1998. We set the indicator variable for a firm's export status to one if SECEX records show exports of any product from the firm in a given year.[8] We link the export-status indicator to RAIS at the firm level. To obtain direct measures of productivity, we use the manufacturing survey PIA from the Brazilian census bureau *IBGE* for the period 1990 to 1998. PIA is a random sample of all but the smallest manufacturing firms and offers output, labor and capital stock information (for details see Appendix B). In order to withdraw PIA data from *IBGE*, we obtain randomly tabulated cells of three (to five) firms.[9] We use the individual tax identifier of each firm within a random cell to link PIA to RAIS. We compute labor productivity for every firm in a random cell as the cell mean of total factor productivity plus the estimated contributions of the capital stock and intermediate inputs. For this purpose, we obtain total factor productivity estimates from an Olley and Pakes (1996) method with Cobb-Douglas production function coefficients for 27 manufacturing sectors at *Nível 50* (comparable to the ISIC3 two-digit level, see Muendler 2004).

[8] We do not use a minimum exports per sales ratio to define the export indicator because sales information is only available for a small subsample of PIA firms.

[9] This random aggregation is necessary under confidentiality requirements and has been used before in Menezes-Filho, Muendler and Ramey (2008), where sample properties are reported.

Second, we use the metropolitan household survey PME. PME provides direct information on household members with or without formal-sector employment and covers one work status transition at the annual horizon for every household member (between the fourth and the eighth interview). We can control for the individual's work status during the three months prior to the fourth interview. PME distinguishes formal employment (with a labor ID card, *carteira*) and informal employment (without *carteira*), as well as self-employment, unemployment, and withdrawals from the labor force. The labor ID card criterion to define formality makes the PME classification equal to RAIS. There is a marked increase in informal work status over the 1990s across all sectors. By far the strongest relative increase in informality occurs in manufacturing, where the share of informal workers almost doubles from above 6 to 12 percent. Non-manufacturing sectors exhibit an average increase in informality of only around 50 percent. We map the PME sector information to twenty sectors comparable to subsector *IBGE*, close to the *atividade*-80 classification.

We combine sector-level variables from several sources with RAIS and PME at the finest possible level. We obtain *ad valorem* tariffs by sector and year and compute intermediate-input tariffs in addition to product-market tariffs using input-output matrices (see Appendix D). To relate our empirical analysis to classic industry-level trade theories, we compute revealed comparative-advantage measures for Brazil from UN Comtrade trade data for 1986-98 following Balassa (1965).

Idle labor. Individual worker panel data allow us to analyze the incidence of nonemployment in the reallocation process. We compute two main measures. We define the *rate of failed reallocations* as the share of displaced individuals who do not find a new formal job within 12 or 48 months, and we define the *duration of complete reallocations* as the average time that a displaced individual takes to find the first formal job if successfully reallocated within 12 or 48 months.

For comparisons to GDP, we revisit the evidence from Figure 1, which used the four-year horizon, with comparable statistics at the annual horizon in Table 1. The table shows that the share of displaced workers without reallocation for a year increases strongly from 29 to 47 percent between 1986 and 1998. There is some variation in the failure rate across skill groups and gender within any given year: male, young and college-educated workers' reallocations fail less frequently than average. Time variation, however, dwarfs the group differences. A similar pattern applies to

Table 1: LABOR MARKET PERFORMANCE AND ECONOMIC OUTCOMES

	1986	1990	1992	1994	1998
FAILED REALLOCATIONS WITHIN A YEAR					
Mean failure rate (share of displaced)	.285	.354	.441	.391	.474
female workers	.387	.427	.500	.451	.517
young workers	.297	.361	.445	.384	.446
high-school or college educ. workers	.305	.350	.416	.366	.435
Change over 1990		.000	.088	.037	.120
Idle labor (foregone share of GDP)		.000	.024	.009	.037
DURATIONS OF SUCCESSFUL REALLOCATIONS WITHIN A YEAR					
Mean duration (in months)	2.918	3.927	4.280	4.125	4.253
female workers	3.157	3.965	4.097	4.017	4.097
young workers	2.896	3.909	4.184	3.969	4.105
high-school or college educ. workers	2.558	3.397	3.622	3.458	3.633
Change over 1990 (one twelfth)		.000	.029	.017	.027
Idle labor (foregone share of GDP)		.000	.008	.004	.008

Sources: RAIS 1986-1999 (1-percent random sample), workers nationwide of any gender or age, displaced from a formal-sector job; not rehired into a formal-sector job within 12 months (*upper panel*) or rehired into a formal-sector job within 12 months (*lower panel*). PME 1986-1999, share of idle prime-age male metropolitan workers (unemployed or withdrawn from labor force) used for nationwide sample, and *Banco Central do Brasil*, GDP.
Notes: Young workers have ten or less years of potential labor force experience, high-school or college-educated workers have some high-school education. Foregone GDP is the unrealized wage bill, measured as the product of the observed change over 1990 times the number of newly displaced workers during the year times their wage upon displacement. Idle labor is defined as the share of displaced workers in PME with transitions to unemployment or out of the labor force.

durations of complete reallocations in the lower panel of Table 1. The relatively minor cross-sectional differences between gender and skill groups, compared to major time variation, suggests that studying macroeconomic sources of variation in labor-market outcomes promises to uncover first-order changes in labor-market outcomes.

These figures suggest that idle resources in the labor market are a crucial aspect of Brazil's aggregate performance. For reallocation failures in the upper panel of Table 1, we calculate the foregone share of GDP as the unrealized wage bill that the additional failures after 1990 imply, given a displaced worker's last wage. We only consider the share of displaced formal-sector workers as idle who typically become unemployed or move out of the labor force in metropolitan labor-markets—a 36 percent share on average in the metropolitan household data 1990-98. We assume that the remaining 64 percent of displaced workers immediately take up an informal job or self-employment and fully retain their pre-displacement earnings. This makes our estimates of foregone GDP conservative. The magnitudes are nevertheless striking. The unrealized wages implied by additional reallocation failures after 1990 amount to 2.4 percent of foregone GDP in 1992 and 3.7 percent in 1998. The increased duration of complete reallocations in the lower panel of Table 1 implies another 0.8 percent of foregone GDP in 1992 and 1998. This brings the total foregone wage bill to more than 3 percent of GDP in 1992, to more than 1 percent in 1994 (a year with strong GDP growth), and to 4.5 percent in 1998.

Labor versus product-market reallocation. Labor reallocation is distinct from the reallocation of product market shares. If a firm's labor productivity $\phi \equiv y/\ell$ rises faster than its output y, then the output expansion $\hat{y}$ is associated with an employment reduction $\hat{\ell} = \hat{y} - \hat{\phi} < 0$. A productivity increase typically results in a less than proportional output increase $\hat{y} < \hat{\phi}$ if the price elasticity of demand is below unity, as observed across most Brazilian industries (Asano and Fiuza 2003).[10] Similarly, if firms exit but survivors and entrants raise labor productivity faster than their output, output shares are being reallocated to more productive survivors while workers shift in the opposite direction. Product-market reallocations to more productive firms and simultaneous workforce shifts away from more productive firms are thus a theoretical possibility.

Reverse resource and product-market reallocations are Brazil's reality during the 1990s, as Table 2 documents. The table decomposes revenue-based total factor productivity (columns 1-4) and labor productivity (columns 5-8) into the contributions of firm-level productivity and firm-level weights, where the weights are output in the case of total factor productivity and employment in the case of labor productivity (alternative weights preserve the patterns). Following Olley and Pakes (1996), aggregate productivity in the cross section of firms (columns 1 and 5) is split into the unweighted mean productivity level (columns 2 and 6) and the covariance between deviations of the weights and productivities from annual means (columns 3 and 7). The relative log TFP change of 3.5 percent between 1990 and 1998 is modest (column 1).[11] Substantial capital accumulation contributes to the faster increase in log labor productivity by 7.3 percent between 1990 and 1998

[10]Under a price elasticity of demand $\varepsilon_{y,p}$ and a productivity elasticity of price $\varepsilon_{p,\phi}$, we have $\hat{y} = \varepsilon_{y,p}\,\varepsilon_{p,\phi}\,\hat{\phi}$ so that $\hat{y} < \hat{\phi}$ if and only if $\varepsilon_{p,\phi} < 1/\varepsilon_{y,p}$. The condition $\varepsilon_{p,\phi} < 1/\varepsilon_{y,p}$ can only be violated if productivity-induced price drops are considerably faster than productivity increases, at a rate above the inverse price elasticity of demand. In monopolistically competitive markets with Dixit-Stiglitz demand, or perfectly competitive markets, prices drop at the rate of productivity increase ($\varepsilon_{p,\phi} = 1$).

[11]In Table 2, we divide aggregate log productivity levels by the aggregate 1990 log level. Rebasing to 1986 at the firm level in Muendler (2004) yields a 4.7 percent increase between 1990 and 1998.

Table 2: PRODUCTIVITY VARIATION ACROSS FIRMS AND OVER TIME

	TFP & Output shares				Labor Prod. & Employment shares				Outp. & Empl.
	Cross section			Ann. chg.	Cross section			Ann. chg.	Ann. chg.
	wgtd.	unwgtd.	cov.	avg. corr.[a]	wgtd.	unwgtd.	cov.	avg. corr.[a]	avg. corr.[a]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1986	1.018	.924	.095		1.011	1.019	-.008		
1990	1.000	.899	.101	.165	1.000	.997	.003	-.164	.182
1992	1.017	.911	.105	.142	1.015	1.008	.007	-.198	-.093
1994	1.013	.918	.096	.135	1.023	1.019	.005	-.183	.166
1998	1.035	.910	.125	.148	1.073	1.043	.030	-.170	.367

[a]Period averages of correlation coefficients (periods 1986-90, 1990-92, 1992-94, 1994-98).

Source: *PIA* firms 1986-98 (1991 missing); log total factor productivity from Muendler (2004) based on Olley and Pakes (1996) estimation (at *Nível 50*), inferring labor productivity under changing capital stocks and intermediate-input uses.
Note: Cross-sectional productivity decomposition as in Olley and Pakes (1996): $y_t = \bar{y}_t + \sum_i \overline{\Delta}\theta_{it}\overline{\Delta}y_{it}$, where y_t is weighted and $\bar{y}_t$ is unweighted mean log productivity, θ denotes the weights and $\overline{\Delta}$ deviations from cross-section means (rebased to unity in 1990). Annual change correlations (correlation coefficients) relate $\Delta_{t-1}\theta_{i,t}$ and $\Delta_{t-1}y_{i,t}$ as well as employment changes and output changes, where Δ_{t-1} denotes the first difference between t and $t-1$.

(column 5). Alongside, Table 2 reports the raw covariance of year-over-year productivity changes at surviving firms (columns 4 and 8)—a term in the Haltiwanger (1997) decomposition over time.[12]

The decompositions in Table 2 show for the cross section of Brazilian manufacturers that firms with higher total factor productivity (TFP) command larger product-market shares (column 3).[13] Over time, TFP improvements among survivors are associated with gains in product-market shares (column 4). These facts are well known for Brazil and similar countries, but sometimes confounded with resource allocation. The cross-sectional covariance between labor productivity and employment shares, in fact, is considerably weaker (column 7) than between TFP and product-market shares (column 3). Most strikingly, firm-level labor productivity advances are associated with reductions in employment shares (column 8).[14] These correlation patterns are not specific to Brazil. Pagés et al. (2009) show for thirteen Latin American economies that labor reallocation and

[12]Centered covariances exhibit a similar pattern as the raw covariances, with always positive TFP and always negative labor productivity covariations. To facilitate comparisons to other research, we report the raw covariance from the Haltiwanger decomposition.

[13]This cross-sectional covariation is not necessarily reflective of an efficient product-market share allocation. Under a Cobb-Douglas production function and revenue-based TFP measures, revenue per input composite is theoretically a constant across firms, irrespective of potentially different quantity-based TFP levels (Hsieh and Klenow 2009). By construction, a part of the positive covariation is due to output entering the TFP numerator.

[14]Mostly firm exits raise the covariance between labor productivity and employment in the cross section over time (column 7).

employer-level productivity growth are negatively related between the early 1990s and the early 2000s. Foster, Haltiwanger and Krizan (2001) document that changes in manhours are negatively associated with productivity change among continuing U.S. plants for 1977-1992. Employment enters the denominator of labor productivity, however, so employment is negatively related to labor productivity by construction. Inference on labor shifts therefore requires more direct evidence. For this purpose, column 9 shows the correlation between sales and employment changes. As expected, sales and employment tend to move in the same direction during most periods, with the important exception of the trade liberalization episode 1990-92, when firms that expand sales contract employment. This is consistent with the hypothesis that labor moves in the opposite direction of product-market reallocation during Brazil's trade reform while firm-level productivity increases.[15]

Economic reforms. In 1990, the Brazilian government breaks with the country's decade-old import substitution policy, which provided more protection to sectors with low comparative advantage (Pavcnik, Blom, Goldberg and Schady 2004), and embarks on a substantial trade liberalization. Earlier *ad valorem* tariff reductions in the late 1980s were less effective because of binding non-tariff barriers (Kume, Piani and Souza 2003). The far-reaching trade reform under the Collor de Melo administration in 1990 involves both the removal of non-tariff barriers and the adoption of a new tariff structure. Collor abolishes all non-tariff barriers by presidential decree on his first day in office and implements a tariff schedule with lower levels and less cross-sectoral dispersion, to be completed by 1993. As shown in Figure 1 above, product tariffs drop from an average level of 63 percent in 1987 to 15 percent by 1997. While product tariffs range between 21 (metallic products) and 63 percent (apparel and textiles) in 1990, they drop to a range between 9 percent (chemicals) and 34 percent (transport equipment) in 1997. Brazil's tariff structure continues to afford effective protection to manufacturing industries. In 1990, product tariffs are around 45 percent above intermediate-input tariffs in value-added terms. By 1997 the reduced cross-sector dispersion of tariffs results in a smaller rate of effective protection of about 20 percent on average.

[15]The descriptive evidence so far is also consistent with the hypothesis by Nishida and Petrin (2009) for Chile and Colombia that deregulation erodes markups faster at employment-expanding firms. While markups may also drop in Brazil, our direct evidence on worker flows below firmly establishes that employment falls at exporters and in comparative-advantage industries as product tariffs are reduced.

Additional reforms partly coincide with trade liberalization. Privatization efforts for public utilities begin in the early 1990s and accelerate by the mid 1990s, while Brazil simultaneously removes capital-account restrictions. In 1994, drastic anti-inflation measures succeed for the first time in decades. These reforms are accompanied by a surge of foreign direct investment inflows during the mid 1990s and advances in outsourcing of service jobs across domestic employers.

These pro-competitive product-market reforms of the 1990s were preceded by a labor-market reform in 1988: Brazil's new constitution introduced a series of changes that reduced the work week and increased overtime premia and benefits—significantly raising labor costs (Paes de Barros and Corseuil 2004). Concomitant reforms not withstanding, its scope and pace make trade liberalization a focal candidate to explain employment shifts and work status transitions. In the empirical analysis that follows we apply statistical treatments to control for concomitant and preceding economic changes.

Worker reallocation. Worker panels allow us to observe the direction of labor flows. We use the four-year horizon to infer long-term reallocation. Within fifty months, 95 percent of complete reallocations are complete in 1986-2001 (median duration of six months). Table 3 reports transitions of displaced workers from formal-sector jobs to other formal-sector jobs (columns 1-6) and the share of displaced workers with no observed formal-sector rehiring at the four-year horizon (column 7). Retained workers do not enter the statistics. Agricultural, mining and manufacturing plants are grouped into their sector's comparative advantage quintiles at the subsector *IBGE* level. All other sectors—commerce, services, construction, utilities, and public administration—are considered nontraded in the table.

The majority of successful worker reallocations within traded-goods sectors is to employers in the same comparative-advantage quintile: transition rates along the diagonal in the five traded-merchandise sectors far exceed those off the diagonal (column 1-5). These worker flows within sectors are consistent with the idea that reallocations between employers in an industry are dominant, as new trade theory posits, and warrant a link of worker panel data to employer characteristics.

The largest fraction of workers with displacement from a traded-goods industry, about a two-fifths, finds employment in nontraded sectors (column 6). And almost as many workers with displacements from a traded-goods sector, roughly another two-fifths, are not rehired into any for-

Table 3: FOUR-YEAR SECTOR TRANSITIONS AND FAILURES

To:	Traded: Comp. adv. quintile[a]					Non-traded	*Failure*	*Total*
	1st	2nd	3rd	4th	5th			
From: (in %)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Traded: Comp. adv.[a]								
1st quintile	28.0	6.8	2.4	5.7	3.0	30.1	24.0	100.0
2nd quintile	9.2	17.9	3.1	5.8	4.7	35.5	23.9	100.0
3rd quintile	5.3	4.9	15.4	13.0	3.2	32.7	25.6	100.0
4th quintile	4.5	4.2	8.3	23.3	5.8	30.4	23.6	100.0
5th quintile	3.9	4.0	2.3	9.9	24.7	32.8	22.4	100.0
Nontraded	2.6	2.2	1.6	3.8	2.8	58.5	28.5	100.0
Failure	5.7	3.0	4.1	11.5	7.3	68.4	.0	100.0
Implied stationary distrib.[b]	3.4	2.6	2.0	4.5	3.3	41.3	42.8	100.0
Impl. stat. distrib. 1990-94[b]	3.7	2.7	1.8	6.5	4.4	40.9	40.0	100.0
Impl. stat. distrib. 1994-98[b]	2.2	1.7	1.8	3.5	2.3	35.6	53.0	100.0

[a]Balassa (1965) comparative advantage, transition year quintile (5th: strongest advantage).

[b]Failure adjustment of stationary distribution based on estimate of 4-year nonformal-to-nonformal transitions from PME (for 1986-98 64.9% of nonformal PME workers are in nonformal work status after three annual transitions, replacing the zero from RAIS, 65.3% for 1990-94, and 71.7% for 1994-98).

Sources: RAIS 1986, 1990, 1994 and 1998 (1-percent random sample), workers nationwide of any gender or age; and PME 1986-1999. UN Comtrade 1986-98 for Balassa comparative advantage at subsector *IBGE* level.
Note: Transition frequencies refer to employments in Brazil four years after separation, based on last employment of year (highest paying job if many). Failed accessions are separations followed by no formal-sector employment anywhere in Brazil within four years, excluding workers with retirement or death, or age 65 or above in past job. The stationary distribution is the normalized left eigenvector of the PME-corrected RAIS transition matrix associated with the eigenvalue of one.

mal job within four years (column 7). These patterns are broadly consistent with the idea that work status changes out of formality (recorded as failures here), and jobs in nontraded sectors, provide a buffer for labor reallocation after trade reform. Repeating the statistical exercise for various subperiods shows that reallocation patterns in traded-goods industries change over time, as summarized by the implied stationary distributions for subperiods, with absorption in nontraded sectors declining over time while the share of failed reallocations increases. We now turn to regression predictions of these worker flows to discern the extent to which trade or other variables affect the worker transitions.

3 Separations and Accessions

Estimation. Employers adjust workforces through separations and accessions. Separations in turn burden, and accessions unburden, the pool of workers to be reallocated. We consider the probability that an employer-employee match is terminated (a separation) or is formed (an accession), conditional on a worker-fixed component α_i that is observable to the employer and the worker:

$$\mathbb{E}\left[\sigma_{i,t+1} \middle| \mathbf{x}_{it}, \mathbf{y}_{J(it)t}, \mathbf{z}_{S(J(it))t}\right] = \mathbf{z}_{S(J(it))t}\beta_z + \mathbf{y}_{J(it)t}\beta_y + \mathbf{x}_{it}\beta_x + \alpha_i + \alpha_{S(J(it))t} + \alpha_t, \qquad (1)$$

where σ_{it} denotes the binary outcome ($\sigma_{it} = A_{it}$ accession or not, $\sigma_{it} = S_{it}$ separation or not) for worker i at time t. $\mathbf{z}_{S(J(it))t}$ is a vector of sector-level covariates of the worker's displacing or hiring sector $S(J(it))$ including a sector-fixed effect and, in some specifications, a sector-year trend; $\mathbf{y}_{J(it)t}$ is a vector of plant-level covariates of worker i's displacing or hiring plant $J(it)$; $\mathbf{x}_{it}$ is a vector of covariates that are worker, job or match specific; β_z, β_y, β_x are coefficient vectors; α_i is the worker-fixed effect, $\alpha_{J(it)t}$ a sector-fixed effect, and α_t a year effect. There is an unobserved error to terminations and formations of employer-employee matches, which we assume to be mean independent in the above linear probability model.[16] Coefficients on worker and job covariates are identified from time variation within and across employers. Educational attainment changes little for individuals in the labor force, however. We consequently drop education categories from the worker characteristics vector but keep educational workforce composition shares among the plant-level regressors.

We cluster standard errors at the worker and at the sector level. Workers switch sector so that non-nested clustering requires a two-way correction (Cameron, Gelbach and Miller 2011). For this purpose, we first obtain the variance-covariance matrix from a worker-fixed effects regression clustering on sectors as if switching workers did not cross clusters, then obtain the variance-covariance

[16]For theoretical consistency with random shocks to employer-employee matches, we alternatively assume the disturbance to be doubly exponentially distributed and independent across matches. We fit the according conditional logit model

$$Pr\left(\sigma_{it} \middle| \mathbf{x}_{it}, \mathbf{y}_{J(it)t}, \mathbf{z}_{S(J(it))t}\right) = \frac{\exp\{\mathbf{z}_{S(J(it))t}\beta_z + \mathbf{y}_{J(it)t}\beta_y + \mathbf{x}_{it}\beta_x + \alpha_i + \alpha_{S(J(it))t} + \alpha_t\}}{1 + \exp\{\mathbf{z}_{S(J(it))t}\beta_z + \mathbf{y}_{J(it)t}\beta_y + \mathbf{x}_{it}\beta_x + \alpha_i + \alpha_{S(J(it))t} + \alpha_t\}}$$

using conditional maximum likelihood estimation (the full maximum likelihood estimator is inconsistent). The conditional maximum likelihood function for individual workers does not permit (non-nested) clustering of standard errors at the sector level.

matrix from a worker-fixed effects regression clustering at the worker level, and finally we obtain a Huber-White robust variance-covariance matrix to arrive at our estimated variance-covariance matrix as the sum of the former two matrices less the third matrix. We use a mix of twelve manufacturing sectors at the subsector *IBGE* level in 1990-1993 and 266 manufacturing industries at the *CNAE* level in 1994-98 to obtain a large set of clusters and plausibly achieve consistency of the standard errors.

Benchmark estimates on the full sample. Table 4 reports estimates of the linear probability model (1) for separations from and accessions to formal manufacturing jobs. Specifications in columns 1 and 4 do not condition on worker-fixed effects (only on sector and year effects). In the separation regressions this lacking conditioning results in statistically insignificant trade-related predictors (sector-level tariffs and firm-level export status). In contrast, once we condition on worker-fixed effects in columns 2 and 5, export status and the product market tariff become statistically significant in the separation regression and, in the accession regression, the product-market tariff coefficient more than doubles. This is consistent with the idea that high- and low-turnover workers match with certain sectors and firm types, confounding estimates, and underscores the importance of linked employer-employee data for estimation.[17]

We consider the estimates in columns 2 and 5 of Table 4 our benchmark. An elevated product tariff predicts lower separation rates from formal jobs (with a p-value of .057 while subsequent specifications robustly show the effect at p-values below .05). So Brazil's trade reform triggers additional separations through reduced product-market tariffs. Input tariffs in the regression capture the effective rate of protection but are not significantly associated with either job separations or accessions. Separations are significantly more frequent at exporters, contrary to predictions of classic and new trade theory. Among the sector-level controls, FDI inflows into the sector predict a statistically significant reduction in job separations. The sectoral real exchange and the Herfindahl concentration index have no significant predictive power after conditioning on year effects.

At the opposite margin of job accessions (column 5), elevated product tariffs predict signifi-

[17]The worker-fixed component may capture worker motivation and persistence, and similar non-cognitive abilities (on their early formation see e.g. Cunha, Heckman and Schennach 2010), differentially affecting retentions and tenure by activity.

Table 4: SEPARATIONS AND ACCESSIONS

	Separations			Accessions		
Sample	RAIS	RAIS	RAIS	RAIS	RAIS	RAIS
Estimator	OLS	FE	FE-IV	OLS	FE	FE-IV
	(1)	(2)	(3)	(4)	(5)	(6)
Product Market Tariff	-.091	-.187	-.263	.102	.243	.309
	(.098)	(.098)*	(.021)***	(.052)**	(.122)**	(.021)***
Intm. Input Tariff	.253	.297	.192	-.197	-.430	-.328
	(.195)	(.289)	(.032)***	(.092)**	(.352)	(.032)***
Exporter Status	.006	.037	.067	-.056	-.048	-.211
	(.005)	(.003)***	(.032)**	(.004)***	(.002)***	(.031)***
Sector-level covariates						
FDI Flow (USD billion)	-.011	-.014	-.012	.007	.009	.009
	(.004)***	(.005)***	(.0006)***	(.003)**	(.004)**	(.0006)***
Sector real exch. rate	-.109	-.116	-.151	.228	.122	.066
	(.140)	(.220)	(.018)***	(.102)**	(.284)	(.018)***
Herfindahl Index (sales)	-.163	-.158	-.018	.150	.127	-.015
	(.059)***	(.097)	(.005)***	(.075)**	(.095)	(.005)***
Plant-level covariates						
Log Employment	-.020	-.060	-.063	-.021	-.015	.001
	(.002)***	(.002)***	(.003)***	(.002)***	(.001)***	(.003)
Worker-level covariates						
Tenure at plant (in years)	-.139	.140	.139			
	(.007)***	(.005)***	(.001)***			
Sqrd. Tenure at plant (sq. yrs.)	.020	-.016	-.017			
	(.0008)***	(.0009)***	(.0002)***			
Pot. labor force experience	.00009	.001	.001	-.006	-.001	-.001
	(.0001)	(.00008)***	(.00006)***	(.0001)***	(.0002)***	(.00006)***
Prof. or Manag'l. Occ.	-.084	-.037	-.038	-.154	-.067	-.070
	(.005)***	(.003)***	(.002)***	(.007)***	(.004)***	(.002)***
Tech'l. or Superv. Occ.	-.076	-.034	-.034	-.142	-.073	-.076
	(.005)***	(.004)***	(.002)***	(.006)***	(.004)***	(.002)***
Unskilled Wh. Collar Occ.	-.064	-.035	-.036	-.115	-.066	-.069
	(.005)***	(.003)***	(.002)***	(.006)***	(.004)***	(.002)***
Skilled Bl. Collar Occ.	-.024	-.003	-.004	-.072	-.064	-.064
	(.005)***	(.002)**	(.0009)***	(.007)***	(.004)***	(.0009)***
Worker effects		yes	yes		yes	yes
Sector effects	yes	yes	yes	yes	yes	yes
Year effects	yes	yes	yes	yes	yes	yes
Observations	5,338,164	5,338,164	5,326,737	5,303,710	5,303,710	5,292,404
R^2 (within)	.068	.056		.097	.033	

Sources: RAIS 1990-98 (10-percent random sample), workers nationwide of any gender or age, separated from or acceding into manufacturing job; SECEX 1990-98; and complementary sector data.
Note: Separations exclude transfers, deaths, and retirements; accessions exclude transfers. Reference observations are employments with no reported separation or accession in a given year. Plant-level controls (share of some college, some high school and white-collar occupations) not reported. Sector information at subsector *IBGE* level 1990-93 and CNAE 4-digit level 1994-98. Instruments for the three endogenous variables Product-market tariffs, Intermediate input tariffs and Export Status are PPI in Europe, PPI in North America, non-Brazilian imports to Asia-Pacific, Central and Eastern European, North American, Other Industrialized and Western European countries (at subsector *IBGE* level 1990-98). See Table G.1 for the first stage. Standard errors in parentheses (two-way clustering at worker and sector level following Cameron et al. 2011, except non-clustered IV): * significance at ten, ** five, *** one percent.

cantly more accessions, mirroring the sign from the separation regression (column 2). Exporters exhibit significantly lower accession rates, also mirroring their higher separation rates and contrary to predictions of classic and new trade theory. FDI inflows are associated with significantly more accessions.

Larger manufacturing plants exhibit less turnover: they displace significantly fewer (column 2) and hire significantly fewer workers (column 5). The separation rate convexly drops with tenure in the absence of worker-fixed effects (column 1), but concavely increases with tenure when we condition on unobserved worker effects (column 2). This sign reversal is consistent with the conventional explanation that over time the employer learns about the worker's initially unobserved ability so that the separation rate drops as more workers' abilities in a cohort become known. The worker effect in the regression is a measure of worker ability over the full sample period so that, conditional on this ability measure, the employer's heightened incentive to dismiss workers with long tenure prevails under Brazil's severance pay law, which make retentions increasingly costly as wages increase with tenure (see Appendix A on incentives for dismissals and rehires). Workers in occupations of intermediate skill intensity experience relatively fewer separations, and workers are relatively less likely to be hired into high-skill intensive manufacturing occupations.

Instrumental-variable regressions. A concern with evidence so far is that Brazil's preceding reform of labor institutions in 1988 may relate to falling trade barriers through sector attributes, including labor turnover characteristics over time. Moreover, the reduction in tariff dispersion gives rise to a simultaneity concern despite the apparently exogenous nature of trade reform for individual employers (the enactment by decree on president Collor's first day in office surprises politicians and businesses alike). By design, initially highly protected sectors face the largest product tariff declines, which are in turn spread over longer phase-out periods. We therefore predict tariffs at the sector level with instrumental variables. At the firm-level, employers decide exporting status and labor turnover simultaneously. We therefore also predict export status with instruments.

Our main instrumental variables are imports into Brazil's export destinations from countries other than Brazil, weighted with Brazil's sectoral export volumes in the base year 1990. We use *WTF* (NBER) data on bilateral trade 1990-98 to construct the instruments by subsector *IBGE* and

CNAE for seven world regions by year.[18] To avoid spurious correlations between Brazilian labor-market outcomes and neighboring countries' imports, we omit Latin America from the export destinations. As additional sector-level instruments we use two important components of Brazil's sectoral real exchange rates: the sector price levels in the United States/Canada and the EU. The identifying assumption for these instruments is that changes in foreign competitive conditions cannot affect the termination and formation of job matches in Brazil other than through trade-related variables.[19]

The foreign-competition instruments prove to be significant covariates of sector tariffs and firm-level export status. On the first stage, we regress product tariffs, input tariffs and export status on the instrumental variables, weighting the regressions by the employment observations in the separation and accession samples. (Table G.1 in the Appendix shows results by sample.) There is no evidence of weak instruments: F statistics from joint significance tests on the instruments vary between 100 and more than 80,000. The instruments are statistically significant predictors at the one-percent level. Brazil's tariff schedule is arguably related to the foreign-competition instruments by policy design, whereas firm-level exports expectedly respond to foreign competitive conditions. Non-Brazilian imports into Brazil's export destinations can either be positively related to Brazilian exports, if they reflect strong foreign demand shocks, or negatively, if Brazilian firms face tougher competition from non-Brazilian suppliers. Producer prices in the United States/Canada and the EU are significantly associated with Brazilian tariffs and exports, with their sign depending on the inclusion of the remaining instruments.

Using instrumented tariffs and export status in estimation equation (1), there is not a single sign reversal in the potentially simultaneity-afflicted coefficients—on tariffs and export status—as columns 3 and 6 of Table 4 show. In summary, instrumentation corroborates our main explanation

[18] We calculate sector-specific weights for each foreign destination country in 1990 using SECEX exports data for Brazil (based on RAIS sector information for the SECEX exporters). We then calculate aggregate imports into each foreign country, excepting imports from Brazil, and weight the country aggregates with Brazilian export volumes by sector and destination in 1990. We finally aggregate the sector-weighted country totals to seven world regions and obtain seven foreign import-demand instruments that vary by sector and year. The seven world regions are Asia-Pacific Developing countries (APD), Central and Eastern European countries (CEE), Latin American and Caribbean countries (LAC), North American countries (NAM excluding Mexico), Other Developing countries (ODV), Other Industrialized countries (OIN), and Western European countries (WEU).

[19] Import penetration may also be a function of foreign competitive conditions and affect exporters and non-exporters alike. The inclusion of import penetration does not alter results but thwarts interpretation (we report according results in Menezes-Filho and Muendler 2007).

for lacking labor reallocation: reduced product tariffs raise separations and reduce accessions. Exporters separate from their workers significantly more frequently than the average employer and hire less frequently.

We perform several additional robustness checks. Results for three such checks are reported in Table G.2 (Appendix G). The privatization of state-owned businesses and the progressing outsourcing of service jobs (*tercerização*) to specialized suppliers partly coincide with trade reform, so we address the omitted variable concern for a subsample with according information (privatization in columns 1 and 4, outsourcing in 2 and 5). The signs of our main coefficient estimates on tariffs and export status, and their statistical significance, remain unaltered. Linear probability models raise the concern that mass points in the distribution, such as from unobserved worker characteristics, can bias results, so we also run a conditional (worker-fixed effects) logit regression. Sign patters of the main coefficients are again unaltered.

Classic and new trade theories. Having established that exporting firms behave differently in periods of trade reform, we now investigate interactions with main sector and employer characteristics. This allows us to examine whether sectors and firms react as predicted by classic trade theory and new trade theories of heterogeneous firms. The sector-level measure of revealed comparative advantage turns out to vary little over time for Brazil in 1990-98 so we interact it with tariffs in the presence of sector fixed effects. Columns 1 and 4 of Table 5 show that, contrary to standard trade theory, the negative association of product tariffs with separations is stronger in comparative advantage sectors and so is their positive association with hirings. So comparative-advantage sectors displace significantly more and hire fewer workers than the average sector when product tariffs fall. Input tariffs exhibit the opposite signs, as they generally do in our regressions. A consistent explanation is that effective rates of protection increase in product tariffs but fall in input tariffs.

In columns 2 and 5 we interact the tariff variables with export status. Estimates show that the effect of trade liberalization is stronger for exporting firms, which displace more than the average firm as a result of trade reform, contrary to results of static new trade theory with exogenous firm productivity. Coefficients at the hirings margin are not statistically significant. Columns 3 and 6 include interactions of tariffs with both export status and comparative advantage measures, and

Table 5: SEPARATIONS AND ACCESSIONS: ADDITIONAL SPECIFICATIONS

	Separations			Accessions		
Sample	RAIS	RAIS	RAIS	RAIS	RAIS	RAIS
Estimator	FE	FE	FE	FE	FE	FE
	(1)	(2)	(3)	(4)	(5)	(6)
Prd. Trff. $\times$ Comp. Adv.	-.270		-.288	.332		.317
	(.117)**		(.118)**	(.161)**		(.153)**
Intm. Trff. $\times$ Comp. Adv.	.400		.415	-.424		-.397
	(.118)***		(.117)***	(.188)**		(.183)**
Prd. Trff. $\times$ Exporter		-.090	-.146		-.098	-.033
		(.047)*	(.042)***		(.101)	(.086)
Intm. Trff. $\times$ Exporter		.209	.273		-.060	-.140
		(.063)***	(.057)***		(.132)	(.112)
Product Market Tariff	.201	-.118	.335	-.236	.301	-.201
	(.137)	(.107)	(.156)**	(.164)	(.179)*	(.180)
Intm. Input Tariff	-.294	.176	-.501	.306	-.437	.321
	(.288)	(.288)	(.289)*	(.287)	(.391)	(.299)
Exporter Status	.038	.023	.025	-.047	-.017	-.017
	(.003)***	(.005)***	(.005)***	(.003)***	(.007)**	(.007)**
Worker effects	yes	yes	yes	yes	yes	yes
Sector effects	yes	yes	yes	yes	yes	yes
Year effects	yes	yes	yes	yes	yes	yes
Observations	5,195,376	5,338,164	5,195,376	5,164,959	5,303,710	5,164,959
R^2	.057	.056	.057	.033	.034	.033

Sources: RAIS 1990-98 (10-percent random sample), workers nationwide of any gender or age, separated from or acceding into manufacturing job; SECEX 1990-98; and complementary sector data.
Note: Balassa (1965) revealed comparative advantage measure for the initial year 1990. Separations exclude transfers, deaths, and retirements; accessions exclude transfers. Reference observations are employments with no reported separation or accession in a given year. Additional regressors (not reported) as in Table 4. Sector information at subsector *IBGE* level 1990-93 and CNAE 4-digit level 1994-98. Standard errors in parentheses (two-way clustering at worker and sector level following Cameron et al. 2011): * significance at ten, ** five, *** one percent.

their association with separations remains statistically significant and at odds with classic and new trade theories. At the accessions margin, only comparative advantage interactions stay significant.

We turn to interactions of tariffs with main worker characteristics. In contrast to the importance of sector and employer characteristics, the interaction between trade variables and worker attributes is generally not significantly related to separations and accessions. As reported in Table G.3 in the Appendix, the only significant differential effect is that high-school educated workers experience less frequent hirings when input tariffs are low. The generally lacking difference between worker groups reconfirms our descriptive evidence in Table 1, where cross-group differences in reallocation outcomes were minor compared to time variation. Most importantly, young workers (with ten or less years of potential labor force experience) exhibit no differential response. This finding is consistent with our maintained assumption that cohort entry and exit do not significantly interact

Table 6: SEPARATIONS, ACCESSIONS AND PRODUCTIVITY

	Separations			Accessions		
Sample	RAIS-PIA	RAIS-PIA	RAIS-PIA	RAIS-PIA	RAIS-PIA	RAIS-PIA
Estimator	FE	FE	FE	FE	FE	FE
	(1)	(2)	(3)	(4)	(5)	(6)
Product Market Tariff	-.174	-.144	-.148	.168	.079	.082
	(.057)***	(.057)**	(.057)***	(.053)***	(.046)*	(.046)*
Intm. Input Tariff	.260	.266	.287	-.291	-.116	-.119
	(.175)	(.230)	(.233)	(.189)	(.152)	(.154)
Exporter Status	.010	.009		-.022	-.021	
	(.004)**	(.004)**		(.003)***	(.003)***	
Log LP			.014			-.004
			(.004)***			(.003)
Worker effects	yes	yes	yes	yes	yes	yes
Sector effects	yes	yes	yes	yes	yes	yes
Year effects	yes	yes	yes	yes	yes	yes
Sector-year trend		yes	yes		yes	yes
Observations	1,860,763	1,860,763	1,860,763	1,845,911	1,845,911	1,845,911
R^2	.079	.079	.079	.037	.039	.039

Sources: RAIS 1990-98 (10-percent random sample), workers nationwide of any gender or age, separated from or acceding into manufacturing job; PIA 1990-98 random three-firm aggregates; SECEX 1990-98; and complementary sector data.
Note: Balassa (1965) revealed comparative advantage measure for the initial year 1990. Separations exclude transfers, deaths, and retirements; accessions exclude transfers. Reference observations are employments with no reported separation or accession in a given year. Additional regressors (not reported) as in Table 4. Sector information at subsector *IBGE* level 1990-93 and CNAE 4-digit level 1994-98. Standard errors in parentheses (two-way clustering at worker and sector level following Cameron et al. 2011): * significance at ten, ** five, *** one percent.

with the timing of trade liberalization.[20]

Overall, estimates so far show that drops in Brazil's product tariffs predict more worker separations and fewer accessions, while exporters separate from their workers significantly more frequently and hire significantly less frequently than other firms. These findings are hard to reconcile with conventional trade and multi-sector macroeonomic theories that predict that inputs shift in lock-step with product-market reallocation. However, our earlier descriptive evidence, by which firms that raise productivity gain product-market shares but lose employment shares (Table 2), suggests an explanation.

[20]We also perform a cohort decomposition for Brazil in 1986, 1990, 1994 and 1998, similar to that in Kim and Topel (1995) for South Korea, and do not find important changes in cohort composition by experience (four or less years of potential labor force experience) over time or across sector quintiles of comparative advantage (see our online Data Supplement).

Labor productivity. We hypothesize that heightened foreign competition in Brazilian product markets induces endogenous productivity growth at continuing firms, while uncompetitive firms exit. Among the possible sources of endogenous productivity change at survivors are reorganizations with more efficient principal-agent relationships (Raith 2003), innovations of production processes (Aghion, Blundell, Griffith, Howitt and Prantl 2009), and the introduction of products (Atkeson and Burstein 2010). Under changing productivity, as argued above, a below-unity price elasticity of demand typically implies that firms and sectors that expand their product market share shed employment. Incentives for innovation differ across sectors. In a Schumpeterian growth model, foreign entry spurs innovation only in sectors close to the technology frontier (Aghion et al. 2009). This insight can explain why comparative-advantage sectors reduce employment. Innovation incentives also differ across firms. The rents to innovation are larger for exporters with access to foreign markets (Yeaple 2005, Ederington and McCalman 2008, Costantini and Melitz 2008). This prediction can explain why exporters reduce employment.

To investigate the role of labor productivity, and its possible association with export status more closely, we bring information from the manufacturing survey PIA to our regressions. PIA covers a sample of all but the smallest manufacturing firms. For comparison we first repeat our benchmark regression (columns 2 and 5 in Table 4) with the combined RAIS-PIA sample. Columns 1 and 4 of Table 6 show estimates of estimation equation (1) for the subsample of RAIS plants that belong to a PIA firm. Compared to estimates for the RAIS universe in prior tables, basic sign patterns are unaltered while magnitudes drop somewhat, suggesting that medium-sized to large firms react less responsively to trade shocks at impact. Lacking instruments for firm-level changes other than export status, we include sector-year trends in the regressions in columns 2 and 5 and find basic patterns unaltered. This result, together with the earlier corroboration from instrumental-variable regressions, renders it little plausible that changes to labor institutions under the constitutional labor-market reforms in 1988, preceding trade liberalization in 1990, lead to erroneous attribution of labor-market effects to trade.

To assess whether labor productivity is a similarly relevant predictor of separations and accessions as export status, we include the firms' labor productivity in the regression. As columns 3 and 6 in Table 6 show, more productive firms separate from more workers and hire fewer workers.

The lower accession probability at more productive firms is not statistically significant, perhaps because random three-firm aggregation harms precision. The evidence nevertheless suggests that firms with higher productivity exhibit similar employment choices as do exporters.[21]

It remains to be seen whether trade liberalization is associated with labor productivity increases. Table 7 investigates just that, by regressing labor productivity and capital per worker on our trade variables. Column 1 reports the results from a firm-level fixed-effects regression and shows no significant effect of the product tariff. As argued before, the tariff variable is subject to simultaneity concerns because initially highly protected sectors with less productive firms face stronger tariff drops and longer phase-out periods by design. We therefore instrument the trade regressors with the foreign-competition variables as described above. Results in column 2 show that reduced product tariffs raise firm-level labor productivity, as in Muendler (2004), and confirm that exporters raise labor productivity faster than the average firm, as theory predicts. Column 3 shows that these productivity improvements come through capital deepening: product tariff reductions and export status lead to more intensive capital use per worker.

In summary, an interpretation consistent with our descriptive and regression evidence so far is that heightened product-market competition after unilateral trade reform induces productivity growth among surviving firms, resulting in lower employment especially at exporters who raise labor productivity faster. This process is consistent with productivity shocks in dynamic trade theories (e.g. Ederington and McCalman 2008, Costantini and Melitz 2008) and does not necessarily have adverse welfare consequences. At the same time, however, the Brazilian economy suffers an increasing incidence of idle resources, so we now ask to what extent trade affects labor-market adjustment.

4 Reallocation

We turn to worker reallocation and investigate trade-related transitions between sectors as well as work status types. Denote the set of employment outcomes (sectors of employment or work status types) with $\mathbb{S}$. An individual's probability to move to outcome $\sigma_{i,t+1}$, conditional on present

[21]Trade-induced technical change can also lead to differential earnings responses (see e.g. Neary 2002) and alter the wage distribution under assortative matching of workers to employers (e.g. Yeaple 2005).

Table 7: PRODUCTIVITY

Dependent variable	Log Labor Prod.	Log Labor Prod.	Log Capital/Empl.
Estimator	FE	FE-IV	FE-IV
	(1)	(2)	(3)
Product Market Tariff	.084	-1.446	-3.160
	(.170)	(.709)**	(1.345)**
Intm. Input Tariff	-1.031	1.590	2.406
	(.239)***	(1.104)	(2.128)
Exporter Status	.044	.045	.078
	(.012)***	(.012)***	(.021)***
Sector-level covariates			
FDI Flow (USD billion)	.011	.001	.021
	(.009)	(.010)	(.019)
Sector real exch. rate	.249	-.046	-1.700
	(.288)	(.321)	(.586)***
Herfindahl Index (sales)	.099	.272	-.026
	(.117)	(.141)*	(.256)
Firm aggregates of plant-level covariates			
Log Employment	-.133	-.134	-.295
	(.008)***	(.008)***	(.014)***
Firm aggregates of worker-level covariates			
Pot. labor force experience	-.002	-.001	.010
	(.001)	(.001)	(.002)***
Prof. or Manag'l. Occ.	-.064	-.059	-.030
	(.052)	(.053)	(.094)
Tech'l. or Superv. Occ.	.015	.022	-.008
	(.043)	(.044)	(.078)
Unskilled Wh. Collar Occ.	-.024	-.022	.052
	(.048)	(.048)	(.087)
Skilled Bl. Collar Occ.	.031	.030	.080
	(.025)	(.025)	(.045)*
Firm effects	yes	yes	yes
Year effects	yes	yes	yes
Observations	23,268	23,251	25,574

Sources: PIA 1990-98 firm sample linked to RAIS 1990-98 firm sample (based on 10-percent random worker sample). *Note*: Additional regressors (not reported) as in Table 4. Sector information at subsector *IBGE* level 1990-93 and CNAE 4-digit level 1994-98. Instruments for the three endogenous variables Product-market tariffs, Intermediate input tariffs and Export Status are PPI in Europe, PPI in North America, non-Brazilian imports to Asia-Pacific, Central and Eastern European, North American, Other Industrialized and Western European countries (at subsector *IBGE* level 1990-98). Robust standard errors in parentheses: * significance at ten, ** five, *** one percent.

outcome $\sigma_{it} = \sigma$, is specified as

$$Pr(\sigma_{i,t+1} | \, \sigma_{it} = \sigma; \mathbf{x}, \mathbf{z}) = \frac{\exp\{\mathbf{z}_{S(i)t}\beta_z^{\sigma} + \mathbf{x}_{it}\beta_x^{\sigma} + \alpha_t^{\sigma} + \alpha_{c(i)t}^{\sigma}\}}{\sum_{\varsigma \in \mathbb{S}} \exp\{\mathbf{z}_{S(i)t}\beta_z^{\varsigma} + \mathbf{x}_{it}\beta_x^{\varsigma} + \alpha_t^{\varsigma} + \alpha_{c(i)t}^{\varsigma}\}}, \tag{2}$$

where $\mathbf{z}_{S(i)t}$ is a vector of sector-level covariates of the individual's initial sector $S(i)$, including a sector-fixed effect; $\mathbf{x}_{it}$ is a vector of covariates that are job and worker specific; β_x^{ς} and β_z^{ς} are coefficient vectors for the future work status $\varsigma \in \mathbb{S}$; and α_t^{ς} and $\alpha_{c(i)t}^{\varsigma}$ are year and regional effects. Coefficients are identified relative to a baseline outcome at $t+1$. For sector reallocation regressions, we use continuous employment with no reported separation (no transition) as the baseline outcome. For work status regressions, we use as the baseline work status a household member's continuation in the present work status, $\sigma_{i,t+1} = \sigma_{it} = \sigma$ (at the same or a different employer). The employer-employee specific errors of outcomes are assumed to be doubly exponentially distributed and independent across employer-employee matches. We fit model (2) with maximum likelihood and restrict the estimation sample to manufacturing jobs at t, for which trade-related covariates $\mathbf{z}_{S(i)t}$ are well defined, but do not impose a sector restriction on job observations at $t+1$. We compute Huber-White robust standard errors.

Trade-related sector transitions. We define the set of possible future sectors of employment for a worker with a formal manufacturing job: (1) the worker retains the present formal job with no reported separation from year to year (the baseline category of no employer transition); (2) the worker moves to another employer within the same subsector *IBGE*; (3) the worker moves to an employer in another manufacturing subsector *IBGE*; (4) the worker moves to an employer outside manufacturing (including services but also agriculture and mining); or (5) the worker fails to have formal employment in the following calender year.

Table 8 shows the results from a single estimation of equation (2), with estimates for outcomes (2) through (5) reported in columns 1 through 4. A lower product-market tariff in the worker's manufacturing sector at t makes a reallocation to another job more likely across all future outcomes, relative to the baseline category of retained employment. As in the separation and accession regressions before, the intermediate-input tariff consistently exhibits the reverse signs of the product tariff, either because lower input costs alleviate competition or because they lead to more efficient production and gains in product market shares. A product tariff reduction raises most the relative

Table 8: MULTINOMIAL LOGIT ESTIMATION: REALLOCATION

Transition to:	Manufacturing: Same sector	Manufacturing: Other sector	Non-manufacturing	Failure
	(1)	(2)	(3)	(4)
Product Market Tariff	-1.653	-3.065	-.478	-.587
	(.050)***	(.060)***	(.043)***	(.042)***
Intm. Input Tariff	2.348	5.458	.586	1.333
	(.070)***	(.085)***	(.062)***	(.060)***
Exporter Status	-.014	.105	.010	.016
	(.002)***	(.003)***	(.002)***	(.002)***
Sector-level covariates				
FDI Flow (USD billion)	-.048	-.033	-.051	-.030
	(.003)***	(.004)***	(.003)***	(.002)***
Herfindahl Index (sales)	-1.606	1.234	-.225	.031
	(.079)***	(.086)***	(.063)***	(.063)
Plant-level covariates				
Log Employment	-.196	-.113	-.135	-.126
	(.0007)***	(.0008)***	(.0006)***	(.0006)***
Share: White-collar occ.	.305	.167	.650	.288
	(.006)***	(.008)***	(.005)***	(.005)***
Worker-level covariates				
Prof. or Manag'l. Occ.	-.271	-.540	-.469	-.220
	(.006)***	(.007)***	(.005)***	(.004)***
Tech'l. or Superv. Occ.	-.432	-.524	-.265	-.311
	(.005)***	(.006)***	(.003)***	(.004)***
Unskilled Wh. Collar Occ.	-.701	-.475	-.129	-.384
	(.005)***	(.006)***	(.003)***	(.004)***
Skilled Bl. Collar Occ.	.151	-.033	-.255	-.172
	(.003)***	(.003)***	(.002)***	(.002)***
Year effects		yes		
Sector effects		yes		
Obs.		25,435,160		
Pseudo R^2		.057		

Sources: RAIS 1990-98 (10-percent random sample), workers nationwide of any gender or age, separated from or remaining in manufacturing job; SECEX 1990-98; and complementary sector data.
Note: Baseline category is no transition (continuous employment with no reported separation in a given year). Multinomial logit estimates of employment transitions. Separations exclude transfers, deaths, and retirements; accessions exclude transfers. Additional regressors (not reported): worker and plant-level workforce education. Sector information at subsector *IBGE* level. Robust standard errors in parentheses: * significance at ten, ** five, *** one percent.

odds that the worker moves to employment in another manufacturing sector, but an import tariff reduction simultaneously diminishes most the relative odds of a transition to another manufacturing sector. The resulting small net effect is consistent with our descriptive evidence that most reallocations occur within industries. Exporter workers are less likely to switch employer within the same industry than they are to retain their present job, but exporter workers are more likely to move to employment in another industry or to drop out of the formal sector. This evidence is consistent with our prior finding that exporters displace more workers than nonexporters during the sample period.

Work status transitions. PME household data allow us to discern work-status transitions out of formal employment. We estimate a multinomial logit model of a single work status transition for every PME household member at the annual horizon.[22] The set of work status outcomes for a worker with a formal manufacturing job contains five alternatives: (1) the worker retains the formal manufacturing job or switches to a new formal job (not necessarily in manufacturing, the baseline category); (2) the worker moves to an informal job (not necessarily in manufacturing); (3) the worker moves to self-employment; (4) the worker moves to unemployment; and (5) the worker withdraws from the labor force. As a proxy for the worker-fixed effect, we include among the job-worker covariates an indicator whether the household member had formal work during the preceding four months. The employer is not identified in household data, so export status is unknown to us.

Table 9 presents the results. Lower product-market tariffs are associated with significantly higher odds of transitions into unemployment and out of the labor force, resulting in significantly more transitions out of formality. Intermediate-input tariffs show converse signs as expected. Workers with stable formal-sector employment for four months are significantly less likely to lose formality status over the following year. Higher educational attainment, from some high-school attainment through college education, predicts significantly fewer transitions into informality, self-employment and unemployment. Education groups with less than a college degree are more likely to transition out of the labor force.

[22] We choose a multinomial over an ordered logit model because, conditional on a set of individual job and worker characteristics, work status types such as informal or self-employment have no intrinsic ordering.

Table 9: WORK STATUS TRANSITIONS FROM FORMAL EMPLOYMENT

	From formal manufacturing employment in t to:			
(in $t+1$)	Informal	Self employed	Unemployed	Withdrawn
Covariate (in t)	(1)	(2)	(3)	(4)
Product Market Tariff	.646	.319	-2.035	-1.929
	(.870)	(.474)	(.788)***	(.721)***
Intm. Input Tariff	-1.417	.835	2.403	2.761
	(1.056)	(.632)	(.707)***	(.796)***
Formal empl. for four months	-1.299	-1.190	-.610	-.882
	(.040)***	(.067)***	(.077)***	(.035)***
Age	-.100	.152	-.027	-.171
	(.013)***	(.018)***	(.015)*	(.015)***
Sqrd. age	.001	-.002	-.0001	.003
	(.0002)***	(.0002)***	(.0002)	(.0002)***
Indic.: Male	.263	.578	.098	-1.115
	(.064)***	(.106)***	(.080)	(.055)***
Some High School	-.065	-.195	.025	.008
	(.064)	(.071)***	(.051)	(.081)
Some College	-.199	-.432	-.064	-.342
	(.080)**	(.089)***	(.081)	(.080)***
College Degree	-.292	-.500	-.361	-.521
	(.081)***	(.088)***	(.128)***	(.096)***
Year effects	yes			
Sector effects	yes			
Metro area effects	yes			
Obs.	48,353			
Pseudo R^2	.066			

Source: PME 1986-99, household members of any gender and age in metropolitan area, with initial formal manufacturing employment (annual transitions between 4th and 8th interview).
Note: Baseline category is continuation in formal work status. Sector-level variables at level similar to *atividade*-80 classification. Further regressors (not reported): Sector real exchange rate, FDI flow, Herfindahl index. Standard errors in parentheses (clustering at sector level): * significance at ten, ** five, *** one percent.

In related research, Goldberg and Pavcnik (2003) detect no significant effect of trade liberalization on the incidence of informality in sector data for Brazil. Our estimates for trade-related informality transitions also lack statistical significance after inclusion of sector-fixed effects. The evidence from PME is nevertheless consistent with the hypothesis that falling product tariffs are associated with significantly more transitions out of formality and into unemployment.

In conclusion, our estimates of reallocations from RAIS and PME data suggest that transitions out of formality and into nonemployment are trade related. To assess the quantitative importance of trade for idle resources, we conduct counterfactual simulations of alternative trade policies.

5 Counterfactual Simulations

We characterize nonemployment in the reallocation process with two main measures: the rate of failed reallocations and the duration of complete reallocations, as reported in the introductory Figure 1 and in Table 1. These measures of idle labor are driven by separations and accessions under a canonical law of motion.

Law of motion. In worker panel and linked employer-employee data like ours, there are two chief states of work status for a worker in period t: formal employment e_t and nonemployment n_t (including informal employment, self-employment, unemployment, and withdrawal from the labor force). Denote the gross worker flow from employment into nonemployment with en_t, and the converse gross worker flow from nonemployment into employment with ne_t. A canonical law of motion describes this labor market:

$$n_{t+1} - n_t = S_t \, e_t - F_t \, n_t = -(e_{t+1} - e_t), \tag{3}$$

where $S_t \equiv en_t/e_t$ is the separation rate from employment and $F_t \equiv ne_t/n_t$ is the job finding rate out of nonemployment. This law of motion is the discrete time version of nonemployment evolution in Pissarides (2000, ch. 1.1), similar to Mortensen (1994). Instead of the job finding rate F_t, in RAIS we observe the accession rate into employment $A_t \equiv ne_t/e_t = (n_t/e_t)F_t$. We rewrite the law of motion in terms of the accession rate:

$$n_{t+1} - n_t = (S_t - A_t) \, e_t. \tag{4}$$

As a result, the nonemployment pool (the pool of workers to be reallocated) evolves with

$$n_{t+1} = S_t \, e_t + (1 - F_t) \, n_t.$$

Two important measures characterize the reallocation process. First, there is the failure rate of an individual entering nonemployment at t to find a new formal job within T periods (*rate of failed reallocations*), which is

$$\phi_{t,t+T} \equiv \prod_{\tau=t}^{t+T} (1 - F_\tau). \tag{5}$$

Second, for the complementary group of workers with complete reallocations within T periods there is the average duration that an individual entering nonemployment at t takes to find the first new formal job (*duration of complete reallocations*). To compute that duration, first observe that the probability that a nonemployed individual finds at least one job within T periods is $1-\phi_{t,t+T} = 1-\prod_{\tau=t}^{t+T}(1-F_\tau)$ and the probability to find the first new job exactly in period m is $\prod_{\tau=t}^{m-1}(1-F_\tau)F_m$. Then the duration of complete reallocations is

$$d_{t,t+T} \equiv \sum_{m=t}^{t+T} m \, \frac{\prod_{\tau=t}^{m-1}(1-F_\tau)\, F_m}{1-\phi_{t,T}}. \tag{6}$$

Both statistics (5) and (6) only depend on $F_t = (e_t/n_t)\, A_t$. However, (e_t/n_t) depends on separations S_t, so F_t does depend on both S_t and A_t.

Simulation. Using our estimates of separation and accession rates S_t and A_t, we can infer the counterfactual separations and accessions that would have occurred had tariffs followed a different time path.[23]

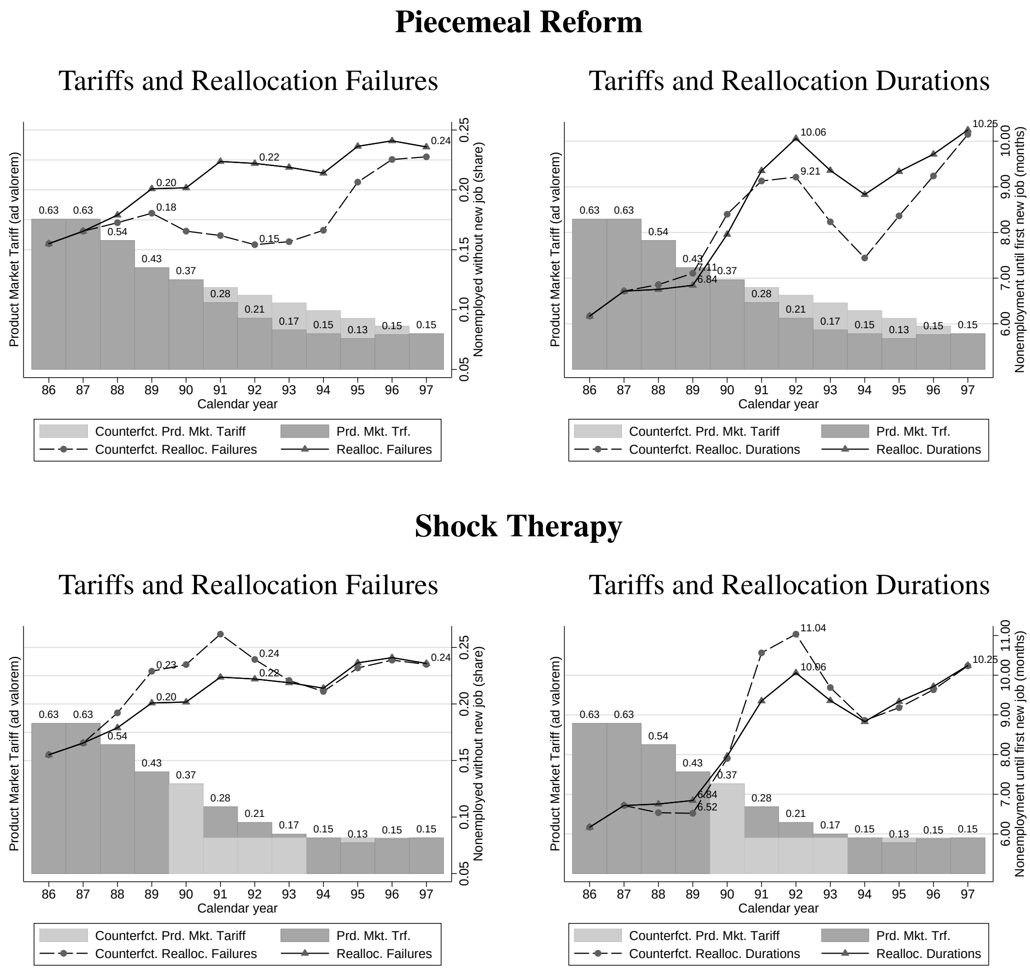
Denote the counterfactual accession and separation rates with $\hat{A}_t$ and $\hat{S}_t$. We predict $\hat{A}_t = A_t + \beta^A_\tau(\hat{\tau}^{\text{cntrfct}}_t - \tau_t)$ and $\hat{S}_t = S_t + \beta^S_\tau(\hat{\tau}^{\text{cntrfct}}_t - \tau_t)$ from the estimated effects, where $\hat{\tau}^{\text{cntrfct}}_t$ is the counterfactual product tariff level and β^A_τ and β^S_τ are the coefficient estimates on product-market tariffs form our benchmark specification using the full RAIS random sample (Table 4, columns 2 and 5). We do not use intermediate-input tariffs for simulation because their coefficients are statistically not significant in the benchmark specifications.

We first compute the job finding rates F_s that are consistent with the observed reallocation failure rates $\phi_{t,t+T}$ (Figure 1), anchoring inference at the end of the sample period in 1997-2001. From the data, we recursively back out actual $F_t = 1 - \phi_{t,t+T}/[(1-F_{t+1})\cdots(1-F_{t+T})]$ for all years prior to 1997. Then we simulate the counterfactual job-finding rate with

$$\hat{F}_s = \hat{A}_s / \widehat{\left(\frac{n_s}{e_s}\right)},$$

where we adjust the nonemployment-to-employment ratio from PNAD household data by year-over-year changes to numerator and denominator under the law of motion. The counterfactual accession rate $\hat{A}_s$ enters into $\hat{F}_s$ both directly and through the counterfactual nonemployment-to-

[23] Lacking monthly variation in industry-level variables and firm export status, our choice of period for estimation in Section 3 has been the calendar year. The regression sample data relate the first separation (per worker and year) to final employment, and they relate the last accession (per worker and year) to first employment.



Sources: RAIS 1986-2001 (10-percent random sample for estimates), workers nationwide of any gender or age, separated from a formal-sector job; not re-acceding into a formal-sector job within 48 months (*left graphs*) or re-acceding into a formal-sector job within 48 months (*right graphs*). PNAD 1988-1998, household members nationwide age 25 through 64, with or without formal-sector job. Product tariffs from Kume et al. (2003), employment weighted at *Nível 50* sector level in 1988.

Note: Simulated job-finding rates $\hat{F}_s = \hat{A}_s/(\widehat{n_s/e_s})$ from $\hat{A}_t = A_t + \beta^A_\tau(\hat{\tau}^{\text{cntrfct}}_t - \tau_t)$ and $\widehat{n_s/e_s}$ from ratio of $\hat{n}_s = (\hat{S}_{s-1} - \hat{A}_{s-1})\, e_{s-1} + n_{s-1}$ and $\hat{e}_s = (\hat{A}_{s-1} - \hat{S}_{s-1})\, e_{s-1} + e_{s-1}$ given simulated relative changes to PNAD nonemployment and employment counts. Simulated finding rate $\hat{F}_s$ then used in the reallocation failure rate $\hat{\phi}_{t,t+T} \equiv \prod_{s=t}^{t+T}(1-\hat{F}_s)$ and the reallocation duration $\hat{d}_{t,t+T} \equiv \sum_{p=t}^{t+T} p \prod_{s=t}^{p-1}(1-\hat{F}_s)\, \hat{F}_p/(1-\hat{\phi}_{t,T})$.

Figure 2: **Counterfactual tariffs and simulated labor-market performance**

employment ratio, whereas the counterfactual separation rate $\hat{S}_t$ only enters through the nonemployment-to-employment ratio. We feed the counterfactual $\hat{F}_s$ into (5) and (6) (for additional details see Appendix F), and simulate the reallocation failure rate $\hat{\phi}_{t,t+T}$ and the average reallocation duration $\hat{d}_{t,t+T}$.

Scenarios. We consider two politically realistic counterfactual scenarios for tariffs over the period 1991-1996. First, we ponder a piecemeal tariff reduction that is spread over more than one election cycle, slower than the actual reform and without the observed tariff reversal after the mid 1990s. This piecemeal tariff path is depicted in the upper panel of Figure 2. Second, we conceive a radical immediate tariff drop in 1991 to the 1997 level, but without the tariff undershooting before the reversal in the mid 1990s. This shock-therapy tariff path is depicted in the lower panel of Figure 2.

The simulation shows for the piecemeal scenario that the rate of failed reallocations would have been considerably lower, remaining roughly at its 1990 level, until it increases in the late 1990s for reasons apparently unrelated to trade reform. Durations of complete reallocations would have temporarily gotten longer, peaking in the early 1990s, but dropped back to their initial 1990 levels by the mid 1990s, until durations also rise in the late 1990s for reasons apparently unrelated to trade reform.[24] The simulation result suggests that phasing tariffs down at a slower pace than within the first three years of the federal five-year election cycle would have considerably alleviated the adjustment burden on the Brazilian labor market.

A simulated shock therapy raises the rate of failed reallocations and extends the durations of complete reallocations in the early 1990s. By the mid 1990s, however, the simulated labor-market effects from shock therapy are hardly distinguishable from actual reallocation failures and durations. Under the shock therapy scenario, reallocation failures and durations worsen less than they seem to improve under the piecemeal scenario. The labor-market effects of Brazil's trade reform therefore appear to be closer to those of a shock therapy scenario than piecemeal reform. Simulation results for both scenarios support the hypothesis that trade reform contributes significantly to

[24]The simulation shows a faster increase in average reallocation durations prior to 1990 because a relatively smaller mass of workers will exhibit long reallocations in the early 1990s so that the weighted average reallocation duration mechanically rises prior to 1990 by equation (6).

Brazil's labor-market outcomes during the early 1990s.

6 Concluding Remarks

This paper contrasts the common finding that output shares are reallocated to more productive firms after trade reform with direct evidence on individual worker trajectories in the labor market. Comprehensive linked employer-employee data for Brazil reveal that product tariff reductions induce displacements especially from exporters and employers in comparative-advantage sectors, and product tariff cuts substantively depress hiring rates. The ensuring slow reallocation process results in substantial idle resources, which we observe through more frequently failing reallocations and longer durations of complete reallocations. A consistent explanation of these observed economic changes is that trade reform instills productivity growth at surviving firms, especially at exporters. Their product-market shares expand but their employment shares shrink under below-unity elasticities of demand.

Gains from trade accrue even in the absence of factor reallocation, through access to more varieties at global prices. Some of these gains dissipate, however, through adjustment costs from resulting idle resources for extended periods of time. In Brazil, more frequent failures of worker reallocations in the formal sector and more frequent transitions out of formality after trade reform substantively contribute to the slowdown in aggregate economic activity. Much research studies trade-related earnings inequality, conditional on employment. Our findings caution that earnings inequality also responds to trade-related spells of unemployment, self employment and informality, which are typically associated with foregone earnings. Brazil's evidence suggests that the labor-market response to trade reform depends on several concomitant factors, among them the strength of firm-level productivity change in response to foreign competition and access to foreign inputs, demand elasticities in the product market, and labor-market institutions. Their interactions cross the boundaries of multiple fields of economics but promise to illuminate important consequences of trade for welfare and earnings inequality.

Appendix

A Linked employer-employee data

Brazilian law requires every Brazilian plant to submit detailed annual reports with individual information on its workers and employees to the ministry of labor (*Ministério de Trabalho*, MTE). The collection of the reports is called *Relação Anual de Informações Sociais*, or RAIS, and typically concluded at the parent firm by late February or early March for the preceding year of observation. RAIS primarily provides information to a federal wage supplement program (*Abono Salarial*), by which every worker with formal employment during the calendar year receives the equivalent of a monthly minimum wage. RAIS records are then shared across government agencies. An employer's failure to report complete workforce information can result in fines proportional to the workforce size; but fines are seldom issued. A strong incentive for compliance is that the worker's annual public wage supplement is exclusively based on RAIS so that workers follow up on their records, whereas employer contributions are not based on RAIS. The ministry of labor estimates that currently 97 percent of all formally employed workers in Brazil are covered in RAIS, and that coverage exceeded 90 percent throughout the 1990s.

RAIS includes workers formally employed in any sector (including the public sector). Our full data have 71.1 million workers with 556.3 million job spells at 5.52 million plants in 3.75 million firms between 1986 and 2001. Overall, formal-sector employment increases from 23.2 million to 24.5 million between 1990 and 1998 in RAIS. We infer a plant's workforce composition by aggregating RAIS to the plant level.

Incentives for dismissal. By Brazilian law, the employer is required to make monthly contributions to a severance pay account for every worker (*Fundo de Garantia por Tempo de Serviço* or *FGTS*, managed by the Brazilian state bank Caixa Econômica Federal). Contributions to this account are akin to mandatory worker savings for precaution against possible future separation. During the 1990s, monthly contributions amount to a fraction $\alpha = .08$ of the worker's monthly wage. In the case of an unjustified dismissal (*sem justa causa*), the employer must make a severance pay that amounts to a fraction $\beta = .40$ of the then accumulated *FGTS* balance. The worker can withdraw from the *FGTS* savings only if the worker suffers an employer-initiated dismissal (not if there is a worker-initiated quit), or can withdraw for a bank-sanctioned investment into domestic real estate, sanitation or infrastructure. See Heckman and Pagés (2004) and Gonzaga (2003) for further details.

Denote a worker's monthly earnings with $w(t)$. Then the accumulated severance pay account balance of a worker with tenure T is $\sum_{t=0}^{T} \alpha\, w(t)$. If a dismissal occurs at time T, then the employer's severance costs are $\beta \sum_{t=0}^{T} \alpha\, w(t)$. So the incremental severance cost for an additional period of employment from T until $T+1$ is $\beta\, \alpha\, w(T+1)$. This incremental severance cost strictly increases over time if and only if the wage increases with tenure. For the employer can reset the wage to a lower level upon dismissal and re-hiring, the Brazilian severance pay mandate creates an incentive for spurious dismissals with subsequent re-hiring.

Observation screening. In RAIS, workers are identified by an individual-specific PIS (*Programa de Integração Social)* number that is similar to a social security number in the United States (but the PIS number is not used for identification purposes other than the administration of the wage supplement program *Abono Salarial*). A given plant may report the same PIS number multiple times within a single year in order to help the worker withdraw deposits from the worker's severance pay savings account (*Fundo de Garantia do Tempo de Serviço*, *FGTS*) through spurious layoffs and rehires. Bad compliance may cause certain PIS numbers to be recorded incorrectly or repeatedly. To handle these issues, we screen RAIS in two steps. (1) Observations with PIS numbers shorter than 11 digits are removed. These may correspond to informal (undocumented) workers or measurement error from faulty bookkeeping. (2) For several separation statistics, we remove multiple jobs from the sample if a worker's duplicate jobs have identical accession and separation dates at the same plant. For a worker with such multiple employments, we only keep the observation with the highest average monthly wage level (in cases of wage ties, we drop duplicate observations randomly).

Experience, education and occupation categories. During the period 1986-93, RAIS reports a worker's age in terms of eight age ranges. For consistency, we categorize the age in years into those eight age ranges also for 1994-2001. We construct a proxy for potential workforce experience from the nine education categories and the mean age within a worker's age range. For example, a typical Early Career worker (34.5 years of age) who is also a Middle School Dropout (left school at 11 years of age) is assigned 23.5 years of potential workforce experience. We adopt the same conventions as Menezes-Filho et al. (2008) when we infer the "typical" age of a worker in the reported age ranges, when we regroup the nine RAIS education categories into four categories, and when we reclassify the 3-digit CBO occupation indicators into single-digit ISCO-88 categories.

B Manufacturing firm data

We use productivity measures from Brazil's annual manufacturing firm survey PIA (*Pesquisa Industrial Anual*) for 1986-98. PIA is a representative sample of all but the smallest manufacturing firms, collected by Brazil's statistical bureau *IBGE*. We first obtain log TFP measures from Olley and Pakes (1996) estimation at the *Nível 50* sector level under a Cobb-Douglas specification (Muendler 2004). We then convert log TFP to log labor productivity by adding the production-coefficient weighted effects of capital accumulation and intermediate input use. Labor productivity is denominated in BRL-deflated USD-1994 revenue equivalents per worker.

IBGE's publication rules allow data from PIA to be withdrawn in the form of tabulations with at least three firms per entry. We construct random combinations of three firms by drawing from sector-location-year cells. A cell is defined by the firm's *Nível 50* sector, headquarters location, and pattern of observation years. We assign every PIA firm to one and only one multi-firm combination. Per cell, one four- or five-firm combination is defined when the number of firms in the sector-location-year cell is not divisible by three. For each three-to-five-firm combination, we calculate mean log productivity but retain the firm identifiers behind the combination—permitting the linking to RAIS.

C Metropolitan household data

The Brazilian monthly employment survey PME (*Pesquisa Mensal de Emprego*) is conducted by Brazil's statistical bureau *IBGE*, using a rotating panel similar to the U.S. *CPS* and the British

BHPS. PME follows a random sample of households for 16 months in six metropolitan areas (São Paulo, Rio de Janeiro, Belo Horizonte, Porto Alegre, Salvador, Recife), with an eight-month interval after the fourth interview.[25] Changes to the sample design adversely affect worker panels starting in odd years. So, we use only individuals whose first survey occurs in 1986, 1988, 1990, 1992, 1994, 1996 or 1998.

In the survey, individuals without employment are considered unemployed if they report active search for work during the week prior to the interview, and are considered out of the workforce otherwise. Household members who work for their own account but do not employ others are considered self-employed. We exclude transitions of individuals who become employers.

D Additional sector data

We use data on *ad valorem* tariffs by sector and year from Kume, Piani and Souza (2003). The tariffs are the legally stipulated nominal rates for Brazil's trade partners with no preferential trade agreement, and not weighted by source country. We combine these tariff series with economy-wide input-output matrices from *IBGE* to arrive at intermediate input tariff measures by sector and year. We calculate the intermediate-input tariff as the weighted arithmetic average of the product-market tariffs, using sector-specific shares of inputs for the input-output matrix as weights. Foreign direct investment (FDI) and annual GDP data are from the Brazilian central bank.

We construct sector-specific real exchange rates from the nominal exchange rate to the U.S. dollar E, Brazilian wholesale price indices P_i, and average foreign price series for groups of Brazil's main trading partners P_i^* by sector i, and define the real exchange rate as $q_i \equiv EP_i^*/P_i$ so that a high value means a depreciated real sector exchange rate. We rebase the underlying price series to a value of 1 in 1995. We use Brazil's import shares from its major 25 trading partners in 1995 as weights for P_i^*. We obtain sector-specific annual series from producer price indices for the 12 OECD countries among Brazil's main 25 trading partners (sector-specific PPI series from *SourceOECD*; U.S. PPI series from *Bureau of Labor Statistics*). We combine these sector-specific price indices with the 13 annual aggregate producer (wholesale if producer unavailable) price in-

[25] Individuals within households are surveyed for a total of eight interviews over a 16-months period. Denoting the initial month with m, interviews are at m, $m+1$, $m+2$, $m+3$, $m+12$, $m+13$, $m+14$, and $m+15$.

Table E.1: SUMMARY STATISTICS

	All sectors and firms		5th comp. adv. quintile	Exporter
	Mean	Std.Dev.	Mean	Mean
	(1)	(2)	(3)	(4)
Outcomes				
Indic.: Separation	.237	.425	.278	.197
Quit	.038	.191	.048	.028
Indic.: Accession	.223	.416	.282	.156
Main covariates				
Exporter Status	.491	.500	.472	1.000
Product Market Tariff	.206	.113	.181	.206
Intm. Input Tariff	.155	.085	.114	.153
Balassa Comp. Adv. 1990	1.424	1.062	3.189	1.404
Plant-level covariates				
Log Employment	5.129	1.970	5.546	6.238
Log Employment 1998/90	.939		.956	.924
Log Labor Productivity	11.202	.752	11.063	11.260
Log Labor Productivity 1998/90	1.051		1.021	1.052

Sources: RAIS 1990-98 (10-percent random sample), workers nationwide of any gender or age, with manufacturing job. Statistics based on separation sample, except for accession indicator (5,338,164 observations in separation, 5,303,710 in accession sample). Sector information at subsector *IBGE* level. *PIA* 1986-98 for labor productivity information.

dex series for Brazil's remaining major trading partners (from *Global Financial Data*), for whom sector-specific PPI are not available.

E Workforce characteristics and trade exposure

Table E.1 provides a summary comparison of variables for workers in manufacturing industries with different quintiles of comparative advantage, and between exporters and the average employer. Top comparative-advantage industries (in the highest quintile) show a higher labor turnover than the average sector with both more worker separations and more accessions, whereas exporting firms exhibit below-average turnover with both fewer worker separations and fewer accessions than average.[26] Among the separations, reported quits play a minor role.

Exporters employ 49.1 percent of Brazil's labor force in 1990-98 (but account for only around

[26]Cuñat and Melitz (2010) develop a Ricardian model in which countries with more flexible labor markets specialize in more volatile industries. Evidence in Table E.1 suggests that Brazil, a country with relatively rigid labor markets compared to its main trade partners by World Bank measures (Botero, Djankov, La Porta, Lopez de Silanes and Shleifer 2004), exhibits more labor volatility in its comparative-advantage industries.

five percent of firms). The share of exporter employment is slightly lower in manufacturing industries in the top quintile, suggesting that exporters are relatively larger employers in sectors with below-top comparative advantage. Expectedly for a country with a history of import-substitution industrialization, Brazil's top comparative-advantage industries have below average product market tariffs. Interestingly, however, workers at exporters face similar tariff levels to workers at the average employer and, consistent with this pattern, face similar levels of comparative advantage. Firms in top comparative-advantage industries and exporters have larger workforces than average (91 and 319 workers more, respectively, than the average formal-sector manufacturing plant with 141 workers). Manufacturing employment drops between 1990 and 1998, and drops faster than average at exporters.

Labor productivity is higher than average at exporters over the whole sample period, but lower than average at firms in comparative-advantage industries. Log labor productivity in 1998 exceeds log labor productivity in 1990 by roughly 5.1 percent in the estimation sample, and by roughly 5.2 percent at exporters.

F Simulations

We first infer the job finding rates F_s that are consistent with the observed reallocation failure rates $\phi_{t,t+T}$ in Figure 1 at the four-year horizon. For this purpose, we anchor inference at the end of the sample period and assume that the economy reaches its four-year average job finding rate in the period 1997-2000 so that, using (5), $F_{1997} = 1 - (\phi_{1997,2000})^{1/4} = F_{1998} = \ldots = F_{2000}$. This is a relatively mild assumption because we restrict our counterfactual differences in tariffs to the period 1991-1996 and do not use later years for counterfactual comparisons. We recursively infer $F_t = 1 - \phi_{t,t+T}/[(1 - F_{t+1}) \cdots (1 - F_{t+T})]$ for all years prior to 1997.

We simulate the counterfactual job-finding rate $\hat{F}_s$ as described in the text. To simulate the reallocation failure rate $\hat{\phi}_{t,t+T}$ we use $\hat{F}_s$ in (5). The left-hand side graphs in Figure 2 show simulated reallocation failures. To simulate the average reallocation duration $\hat{d}_{t,t+T}$, we face a short-coming of the canonical law of motion (3). The job finding rates F_s that are consistent with the observed reallocation failure rates $\phi_{t,t+T}$ in Figure 1 at the four-year horizon are inconsistent with the observed reallocation durations. The failure-inferred job finding rates imply more than one-third longer reallocation durations than are actually observed. A likely reason is unaccounted heterogeneity of high-turnover and low-turnover workers, with a larger mass of rapidly reallocated nonemployed workers at shorter horizons than a constant job-finding rate implies. So for our simulated average reallocation duration $\hat{d}_{t,t+T}$, we use the observed annual adjustment factors that translate the durations from failure-implied finding rates into accordingly shorter usually observed durations. The right-hand side graphs in Figure 2 show simulated reallocation durations.

G Additional Regressions

We report in this appendix in Table G.1 the first-stage results that underly the IV regressions in columns 3 and 6 of Table 4. We report additional robustness results in Tables G.2 and G.3.

Ownership of a plant is observable in RAIS since 1995, when privatization of state-owned services firms began. We impute a plant's ownership status in 1990-94 as the ownership status in 1995 and include the private-ownership indicator at the plant-level in regression (1). As columns 1 and 4 show, coefficient estimates on the trade-related variables exhibit no statistically significant change, and the ownership-status itself is not a statistically significant predictor of separations. Accessions are significantly lower in sectors with many private firms. We infer the susceptibility of a job to outsourcing (*terceirização*) if it is a service occupation at the CBO three-digit level that can be performed in-house or be provided by a specialized subcontractor. Jobs susceptible to outsourcing exhibit statistically significantly more separations and fewer accessions, but there are no statistically significant coefficient changes in trade variables. Conditional logit estimates, reported in columns 3 and 6 of Table G.2, corroborate our linear model estimates.

Table G.1: FIRST-STAGE PREDICTIONS

	Separations			Accessions		
Sample	RAIS	RAIS	RAIS	RAIS	RAIS	RAIS
Dependent variable	Prd. Mkt. Tariff	Intm. Inp. Tariff	Exp. Status	Prd. Mkt. Tariff	Intm. Inp. Tariff	Exp. Status
	(1)	(2)	(3)	(4)	(5)	(6)
Instruments						
World imports APD	-28.577	-50.974	.458	-28.002	-50.841	1.412
	(.211)***	(.130)***	(1.501)	(.211)***	(.130)***	(1.514)
World imports CEE	-488.715	-326.813	-60.492	-488.616	-326.072	-63.142
	(.542)***	(.333)***	(3.850)***	(.542)***	(.333)***	(3.880)***
World imports NAM	-59.363	-9.996	35.198	-58.448	-10.016	40.100
	(.179)***	(.110)***	(1.270)***	(.179)***	(.110)***	(1.282)***
World imports OIN	94.904	113.853	68.189	94.310	114.633	65.991
	(.340)***	(.209)***	(2.416)***	(.341)***	(.210)***	(2.443)***
World imports WEU	-100.136	-88.568	39.455	-100.563	-88.352	39.466
	(.243)***	(.149)***	(1.728)***	(.244)***	(.150)***	(1.746)***
PPI Idx. EU, import-weight 95	-.429	-.171	-.185	-.438	-.169	-.209
	(.002)***	(.001)***	(.016)***	(.002)***	(.001)***	(.016)***
PPI Idx. NAM, import-weight 95	.494	.043	-.204	.498	.049	-.216
	(.002)***	(.001)***	(.016)***	(.002)***	(.001)***	(.016)***
Exogenous covariates						
FDI Flow (USD billion)	.001	.0001	-.005	.001	.0002	-.005
	(.00006)***	(.00004)***	(.0004)***	(.00006)***	(.00004)***	(.0005)***
Herfindahl Index (sales)	-.027	-.059	.007	-.028	-.060	.014
	(.0005)***	(.0003)***	(.003)**	(.0005)***	(.0003)***	(.003)***
Log Employment	.0008	-.00008	.110	.0008	-.00008	.105
	(.00002)***	(.00002)***	(.0002)***	(.00002)***	(1.00e-05)***	(.0002)***
Share: Some High School	-.0003	-.0007	.073	1.73e-06	-.0007	.074
	(.0002)	(.0001)***	(.001)***	(.0002)	(.0001)***	(.001)***
Share: Some College	.001	-.006	.214	.0006	-.006	.228
	(.0003)***	(.0002)***	(.002)***	(.0003)*	(.0002)***	(.002)***
Share: White-collar occ.	.0002	.002	.148	-.0005	.002	.152
	(.0002)	(.0001)***	(.001)***	(.0002)***	(.0001)***	(.001)***
Worker effects	yes	yes	yes	yes	yes	yes
Sector effects	yes	yes	yes	yes	yes	yes
Year effects	yes	yes	yes	yes	yes	yes
Observations	5,326,737	5,326,737	5,326,737	5,292,404	5,292,404	5,292,404
R^2 (within)	.823	.883	.123	.824	.883	.120
F statistic (joint IVs)	79477.27	13104.83	102.66	81178.72	13114.04	119.581

Sources: WTF (NBER) bilateral import data 1990-98 at subsector *IBGE* level; sector data from various sources at subsector *IBGE* level 1990-93 and CNAE 4-digit level 1994-98; RAIS 1990-98 labor force information; SECEX exporter information 1990-98.

Note: First-stage estimates for column 3 and 6 in Table 4 weighted by worker-sample observations. Imports to foreign destinations are annual sector-weighted shipments from source countries other than Brazil, coefficients rescaled to imports in USD trillion. Additional regressors (not reported) as in Table 4. Robust standard errors in parentheses: * significance at ten, ** five, *** one percent.

Table G.2: SEPARATIONS AND ACCESSIONS: ROBUSTNESS

	Separations			Accessions		
Sample	RAIS-PIA	RAIS-PIA	RAIS-PIA	RAIS-PIA	RAIS-PIA	RAIS-PIA
Estimator	FE	FE	cLogit	FE	FE	cLogit
	(1)	(2)	(3)	(4)	(5)	(6)
Product Market Tariff	-.264	-.183	-1.161	.244	.235	1.776
	(.119)**	(.097)*	(.095)***	(.141)*	(.119)**	(.104)***
Intm. Input Tariff	.415	.289	2.556	-.420	-.420	-3.307
	(.317)	(.288)	(.149)***	(.369)	(.349)	(.155)***
Exporter Status	.037	.037	.234	-.046	-.048	-.358
	(.003)***	(.003)***	(.006)***	(.002)***	(.002)***	(.006)***
Share: Jobs at private firms	-.014			-.086		
	(.047)			(.034)**		
Indic.: Outsourceable job		.006			-.016	
		(.002)***			(.002)***	
Worker effects	yes	yes	yes	yes	yes	yes
Sector effects	yes	yes	yes	yes	yes	yes
Year effects	yes	yes	yes	yes	yes	yes
Observations	4,747,727	5,281,036	2,846,694	4,725,103	5,248,748	2,576,206
(Pseudo) R^2	.056	.056	.145	.033	.033	.076

Sources: RAIS 1990-98 (10-percent random sample), workers nationwide of any gender or age, separated from or acceding into manufacturing job; PIA 1990-98 random three-firm aggregates; SECEX 1990-98; and complementary sector data.
Note: Separations exclude transfers, deaths, and retirements; accessions exclude transfers. Reference observations are employments with no reported separation or accession in a given year. Additional regressors (not reported) as in Table 4. Sector information at subsector *IBGE* level 1990-93 and CNAE 4-digit level 1994-98. Standard errors in parentheses (two-way clustering at worker and sector level following Cameron et al. 2011): * significance at ten, ** five, *** one percent.

Table G.3: SEPARATIONS AND ACCESSIONS: WORKER INTERACTIONS

	Separations			Accessions		
Sample	RAIS	RAIS	RAIS	RAIS	RAIS	RAIS
Estimator	FE	FE	FE	FE	FE	FE
	(1)	(2)	(3)	(4)	(5)	(6)
Product Market Tariff	-.178	-.173	-.173	.252	.232	.256
	(.106)*	(.094)*	(.096)*	(.133)*	(.099)**	(.122)**
Intm. Input Tariff	.296	.288	.304	-.454	-.507	-.448
	(.297)	(.284)	(.288)	(.362)	(.332)	(.350)
Exporter Status	.037	.037	.037	-.048	-.047	-.048
	(.003)***	(.003)***	(.003)***	(.002)***	(.002)***	(.002)***
Prd. Trff. × High-sch. or coll. ed.	-.034			-.021		
	(.034)			(.041)		
Intm. Trff. × High-sch. or coll. ed.	-.029			.118		
	(.045)			(.051)**		
Prd. Trff. × Young		-.100			.142	
		(.060)*			(.200)	
Intm. Trff. × Young		.096			.162	
		(.073)			(.250)	
Prd. Trff. × White collar			-.058			-.052
			(.043)			(.040)
Intm. Trff. × White collar			-.082			.072
			(.060)			(.063)
Worker effects	yes	yes	yes	yes	yes	yes
Sector effects	yes	yes	yes	yes	yes	yes
Year effects	yes	yes	yes	yes	yes	yes
Observations	5,338,164	5,338,164	5,338,164	5,303,710	5,303,710	5,303,710
R^2	.056	.056	.056	.033	.035	.033

Sources: RAIS 1990-98 (10-percent random sample), workers nationwide of any gender or age, separated from or acceding into manufacturing job; PIA 1990-98 random three-firm aggregates; SECEX 1990-98; and complementary sector data.
Note: Separations exclude transfers, deaths, and retirements; accessions exclude transfers. Young workers have ten or less years of potential labor force experience, high-school or college-educated workers have some high-school education. Reference observations are employments with no reported separation or accession in a given year. Additional regressors (not reported) as in Table 4. Sector information at subsector IBGE level 1990-93 and CNAE 4-digit level 1994-98. Standard errors in parentheses (two-way clustering at worker and sector level following Cameron et al. 2011): * significance at ten, ** five, *** one percent.

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