# Trade and Labor Reallocation with Heterogeneous Enforcement of Labor Regulations*

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**Abstract:** This paper revisits the question of how trade openness affects labor market outcomes in a developing country setting. We explore the fact that plants face varying degrees of exposure to global markets and to the enforcement of labor market regulations, and rely on Brazil's currency crisis in 1999 as an exogenous source of variation in industry-specific real exchange rates and hence, access to foreign markets. Using administrative data on employers matched to their employees and on the enforcement of labor regulations at the city level over Brazil's main crisis period, we document that the way trade openness affects labor market outcomes for plants and workers depends on the stringency of *de facto* labor market regulations. In particular, we show that after a trade shock plants facing stricter enforcement of the labor law decrease job creation and increase job destruction by more than plants facing looser enforcement. Consistent with our predictions, this effect is strongest among small, labor-intensive, non-exporting plants, for which labor regulations are most binding. We also note a stronger impact of enforcement on younger workers. Finally, our findings are consistent with the hypothesis that increased regulatory enforcement limits the plant-level productivity gains associated with trade openness. The latter implies that increasing the flexibility of *de jure* regulations will allow for broader access to the gains from trade.

Keywords: Globalization; Enforcement; Labor Market Regulations; Linked Employer-Employee Data.
JEL: F16; J6; J8.

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## 1. Introduction

A key argument in favor of liberalizing trade relations is that factors can reallocate to more efficient uses, allowing for enhanced productivity, income growth, and welfare (Pavcnik 2002; Feyrer 2009; Broda and Weinstein 2006). Early studies in many developing countries, however, found little impact of trade liberalization on plant-level employment and wages (Currie and Harrison 1997; Feliciano 2001). More recent work offers mixed results on the causal link from exporting to productivity and evidence of slow labor market adjustment to trade reform. A potential explanation for these various findings are restrictive labor market regulations, which inhibit the reallocation of workers, limiting the extent to which plants can benefit from increased openness (Freund and Bolaky 2008; Kaplan 2009).

In this paper, we revisit the question of the impact of trade liberalization on labor reallocation in a developing country by exploring the fact that plants vary in the degree of exposure to global markets and that *de facto* labor regulations are heterogeneous within countries. We rely on detailed administrative data from Brazil covering the currency's devaluation episode. Our main reduced-form specification relates exogenous industry-specific exchange rate shocks to plant and worker outcomes over time, differentially for plants located in distinct labor market regulatory environments. Our findings show that more stringent *de facto* regulations reinforce the contractionary labor market effects of trade openness for small, labor-intensive, non-exporting plants. Overall, domestic plants in strictly-enforced areas increase job destruction and decrease job creation by more than otherwise identical domestic plants in weakly-enforced areas. From a policy standpoint, our work therefore contributes to an understanding of job security in an increasingly globalized world. Increasing the flexibility of *de jure* regulations will stimulate job creation and offer broader access to the gains from trade.

We contribute to a growing body of work in several ways. First, the micro-data available for Brazil are rich and appropriate to study the effects of trade liberalization on labor turnover. We exploit a matched employer-employee database covering the formal-sector labor force, in combination with information on the plant's exposure to global markets. The data allow us to analyze total employment at the plant level, and also to trace the movement of workers across different employers in response to the trade shock. Furthermore, it permits the decomposition of labor turnover into changes along the extensive margin (the accession and separation of workers) and along the intensive margin (hours worked and temporary contracts). The ability to match workers

to their employers is an integral part of our analysis, as recent evidence points to a sorting effect of globalization.[1] For instance, as in the model described in Helpman, Itskhoki, and Redding (2010), when there are complementarities between plant productivity and worker ability, plants have an incentive to screen for workers below a given ability level. Higher productivity exporters screen to a higher ability threshold and will thus have a workforce of higher average ability than non-exporters.[2] Because globalization increases plant selection into exporting as in Melitz (2003) and the incentives to screen for high-quality matches, it is important to account for heterogeneity in the quality of the worker-plant match in determining the effects of globalization on labor market outcomes (Woodcock 2011; Krishna, Poole, and Senses 2011). In our setting, an otherwise identical worker may have a higher probability of separation from (or a lower probability of accession to) a high productivity plant than from (and to) a low productivity plant, when such worker-plant production complementarities exist. This diversity offers disparate predictions for the effects of trade liberalization on worker turnover at exporting (high productivity) and non-exporting (low productivity) plants. Our reduced-form specification builds on the existing literature in this dimension. Notably, our preferred specification uses information on worker-level labor market outcomes, separately for exporting and domestic plants, and includes plant-worker match-specific effects to allow for the possibility of worker sorting and unobservable plant, worker, and match heterogeneity.

While we are not the first to investigate the impact of trade by the plant's mode of globalization (e.g., Amiti and Davis (2012)), we are not aware of any paper allowing for globalization to impact plants differently depending on their exposure to labor market regulatory enforcement.[3] Brazil has one of the most restrictive labor market regulatory frameworks in the world (Botero, Djankov, La

[1] Verhoogen (2008) documents a skill-upgrading in Mexican exporting plants after the 1994 peso devaluation. Bustos (2011) looks at the Brazilian reduction in tariffs as part of the Mercosur regional free trade agreement and finds that Argentinean plants above the median size upgrade skills, while plants below the median size downgrade skills.

[2] Changes in the skill composition of the workforce at exporters relative to non-exporters are also present in a number of other recent trade models (see for example, Yeaple (2005) and Davidson, Matusz, and Shevchenko (2008)).

[3] Allowing for globalization to impact plants differently depending on exposure to regulatory enforcement may help to provide an explanation for the mixed results in the literature on the causal impact of exporting on plant-level productivity. Clerides, Lach, and Tybout (1998) argue that the positive relationship between exporting and productivity is a result of the self-selection of plants into exports. In most cases, plants do not become more productive by exporting. By contrast, using plant-level data from Slovenia, De Loecker (2007), controlling for self-selection, finds evidence that exporters become more productive.

Porta, and Lopez de Silanes 2004; Almeida and Carneiro 2012).[4] However, the size of the informal labor force suggests that enforcement is weak in some areas, hinting at a gap between the laws stated on the books (*de jure* regulations) and their effective implementation (*de facto* regulations). Therefore, contrary to previous studies which rely on cross-country or across-state variation in existing *de jure* labor regulations (e.g., Besley and Burgess (2004) and Autor, Kerr, and Kugler (2007)), we explore the fact that Brazilian employers are exposed to varying degrees of *de facto* labor regulations, via Ministry of Labor inspections. Especially in a developing country context where enforcement is not homogeneous, we argue exploring time series and within-country variation in regulatory enforcement offers a better measure of a plant's true flexibility in adjusting labor to shocks than looking at variations in *de jure* regulations.[5] We thus investigate the differential impact of globalization on worker turnover among otherwise identical plants and workers facing different *de facto* enforcement of the labor law.[6]

Finally, in contrast to most of the literature investigating the impact of trade liberalization on the real economy using potentially endogenous tariff changes[7], we explore the Brazilian currency's strong devaluation in January 1999 as a large and unanticipated exogenous shock to both employers and workers.[8] Following Goldberg (2004), we construct trade-weighted industry-specific real exchange rates in order to capture changes in industry competitiveness over time. The economy-wide real exchange rate depreciated 32% from 1996 to 2001, with a 23% drop occurring between December 1998 and January 1999 alone (see Figure 3.1; Muendler 2003). However, though all industries suffered exchange rate declines over this time period, some endured more severe shocks than others, as measured by trade-weighted real exchange rates. We rely on this

[4] There is an extensive literature for developing countries analyzing the relationship between labor market regulations and labor market outcomes (e.g., Kugler (1999), Kugler and Kugler (2009), Ahsan and Pages (2009), Petrin and Sivadasan (2006), and several other studies cited in Heckman and Pages (2004)).

[5] To date, few papers have explored both within-country and time series measures of enforcement. Notable exceptions include Caballero, Cowan, Engel, and Micco (2004) and Almeida and Carneiro (2012).

[6] Currie and Harrison (1997) rule out labor market regulations as an explanation for their insignificant finding of the effects of trade reform on employment levels, and suggest that despite formal labor market barriers there is little enforcement which leaves regulations ineffective. Unlike Currie and Harrison (1997), our city-level data on Ministry of Labor inspections allow us to capture exactly this variation in within-country compliance with labor market regulations.

[7] Political economy factors in tariff formation and adjustment have been noted by a number of authors. See, for example, Olarreaga and Soloaga (1998) for the case of Brazil's regional free trade area, Mercosur. In fact, as protectionist pressures grew in the aftermath of the introduction of a new currency in 1994, average tariffs marginally *increase* beginning in 1995. See Section 2.2 for further discussion.

[8] Other papers using currency shocks as exogenous sources of variation to investigate international trade relationships include Verhoogen (2008), who uses Mexico's 1994 peso devaluation to explore the relationship between trade and inequality, and Brambilla, Lederman, and Porto (forthcoming), who use Brazil's currency crisis as a shock to Argentinean exporters.

industry-level variation in real exchange rates over time to exogenously identify the effect of Brazil's increased globalization on employment and labor turnover at the plant and worker level.

To summarize, we are interested in analyzing the effect of trade liberalization on labor reallocation within Brazil. We explore across industry and over time variation in real exchange rates in order to capture changes in industry competitiveness, in combination with city and time variation in the enforcement of labor market regulations, facing exporters and non-exporters. An important concern relates to the exogeneity of the variation in the enforcement of labor regulations across cities. At any given point in time, enforcement of labor regulations at the city level is not likely to be randomly distributed. On the one hand, enforcement may be stronger in cities with higher violations of the law. On the other hand, cities with better institutions could have stricter enforcement. Although it is unclear how these patterns may impact worker reallocation, a potential bias may still exist. To minimize this concern, we note that our empirical methodology follows the program evaluation literature and relates exogenous real exchange rate changes to plant and worker outcomes over time, differentially for plants located in variable labor market regulatory environments. Our main coefficient of interest is the differential effect of changing enforcement on plants that, all else constant, are exposed to different exogenous industry-specific trade shocks. Therefore, our reduced-form specification relates annual changes in the probability of inspection in a given city (which we proxy for using total labor inspections per 100 plants) and annual changes in industry-specific real exchange rates, with annual changes in labor market outcomes. We run this specification separately for exporting and non-exporting plants.

One could still question the exogeneity of *changes* in enforcement at the city level. To the extent that these changes correlate with changes in labor market outcomes, our estimates for the effects of enforcement may be biased. We emphasize, however, that our focus is on the interaction between exogenous *changes* in industry-specific real exchange rates and *changes* in the degree of regulatory enforcement, as is customary in the program evaluation literature. For our main coefficient of interest to be biased, it must be that plants in industries exposed to greater depreciations *and* in cities exposed to greater *de facto* enforcement also have systematically different labor turnover, for some unobserved reasons. One possibility is that industries are regionally-concentrated, such that the industries experiencing the most severe depreciations are located in the cities experiencing the greatest increases in enforcement (i.e., growing cities that may also have more dynamic labor markets). Our reduced-form estimation includes state-specific year dummies, which we argue helps

to correct for some of this bias. In ongoing work, we also include city-specific year dummies in order to control for the possibility that differences in labor flows over time in certain cities may be driving the differences in labor turnover we find.

We begin our analysis at the plant-level, investigating the differential impact of trade openness on plant size for plants located in heavily-inspected cities relative to plants located in weakly-inspected cities. We then consider a more disaggregated worker-level analysis. This allows us to decompose how plants adjust labor in response to currency devaluations and how enforcement influences these adjustments—along the extensive margin (hiring and firing) or along the intensive margin (changes in hours worked or between full-time and temporary contracts). Meanwhile, our worker-level analysis helps to address the possibility of worker sorting in the labor reallocation process.

We now briefly discuss the main empirical predictions in our reduced-form model. With a devaluation of the Brazilian currency (the *real*), imports into Brazil become more expensive, improving the competitiveness and enhancing the profitability of Brazilian plants selling in the domestic market. To the extent that profits and employment growth are correlated at the plant level, a currency depreciation is expected to increase employment for the average plant in the country.[9] The same depreciation differentially improves conditions for exporters, as Brazil's trading partners need fewer currency units to purchase Brazilian goods. We expect this enhanced foreign market access to differentially increase employment at Brazil's exporting plants relative to plants producing only for the domestic market.[10]

Our hypothesis relies on the extent to which labor regulations "bite"; that is, whether regulations are enforced. An increase in the enforcement of labor market regulations through more labor inspections is expected to directly impact the compliance with labor regulations through the hiring and firing of formal labor.[11] However, the direction of the effect of enforcement on employment is

[9] Revenga (1992) uses the sharp appreciation of the U.S. dollar during the early 1980s to demonstrate significant employment reductions for import-competing industries. Ribeiro, *et al* (2004) consider the case of Brazil, documenting the importance of the exchange rate for job creation. Interestingly, Burgess and Knetter (1998) evaluate employment responses to exchange rate shocks at the industry-level across the G-7 countries, and argue that country-level differences in the response to the exchange rate shock may be attributable to variation in labor market regulations.

[10] Goldberg and Tracy (2003) demonstrate that the employment declines associated with a U.S. dollar appreciation grow stronger as industries increase in export orientation.

[11] Cardoso and Lage (2007) show that inspections are primarily linked to stricter enforcement of mandatory severance payments, mandated health and safety regulations, and to the worker's formal registration. Bertola, Boeri, and Cazes (2000) suggest that differences in enforcement across countries, related for example to the

ambiguous. On the one hand, stricter enforcement of labor regulations raises the cost of formal workers. As such, plants facing stricter enforcement will have increased difficulties in adjusting labor. On the other hand, the stricter enforcement of labor regulations also increases job quality, in terms of compliance with mandated benefits for the worker. For this reason, we may find increases in employment in more heavily-enforced cities, as formal employment becomes a more attractive option and formal work registration increases.

In this paper, we focus on the differential impact of openness on labor market outcomes across plants located in cities with varying degrees of regulatory enforcement. Our main results are consistent with the view that the extent to which trade affects labor market outcomes depends on the *de facto* degree of stringency of the labor regulations faced by plants. In particular, plants facing stricter enforcement of the labor laws increase employment by less than plants facing fewer inspections with the expansionary trade shock. Moreover, conditional on the (unobservable and time-invariant) plant-worker match, and several time-varying worker, plant, and sector characteristics, we note that openness is associated with an increase in the probability of hiring at exporters and a decrease in the probability of hiring at domestically-oriented plants, as is predicted by new heterogeneous firm trade models. The results suggest that enforcement influences mainly non-exporting plants along the extensive margin, but has little effect on adjustment along the intensive margin (as captured by hours worked and workers with full-time contracts). We note also that our findings are mainly concentrated among small, labor-intensive, non-exporting plants for which labor regulations are likely to be most restrictive.

The magnitudes of our estimates seem to be plausible. Evaluating the effect on workers and plants located in municipalities at the mean level of inspections, a 10 percent depreciation of the *real* increases job match creation at exporting plants by 4.1% and decreases job creation at non-exporters by 7.3%. Meanwhile, the impact for domestic plants varies depending on the level of enforcement the plant faces—for domestic plants located in municipalities t the $10^{th}$ percentile of inspections, a 10 percent depreciation of the *real* decreases the probability of hire by only 6.0%,

efficiency of a country's legal system, are as or even more important, than differences in *de jure* regulations. For example, Caballero, Cowan, Engel, and Micco (2010) explore a panel of 60 countries around the world and find that labor regulations have adverse effects on job turnover and plants' speed of adjustment to shocks, but only in countries with a strong rule of law and government efficiency (taken as measures of enforcement of regulations). However, as with many cross-country studies, the limited time series variation in labor regulations and measures of enforcement poses challenges for identification.

while workers matched with domestic plants located in municipalities in the 90th percentile of inspections experience a decrease in job match creation of around 9.0%.

Our results strongly suggest that more stringent *de facto* regulations limit job creation with enhanced trade openness. Overall, small, labor-intensive, non-exporters separate from more workers and hire fewer with increased enforcement of regulations. We show that this increased enforcement of labor regulations is also associated with lower plant-level productivity, as proxied by plant-level average wages. It seems clear from our data that strict labor market institutions limit the possibility for plant-level productivity and profitability gains associated with trade openness. From a policy standpoint, our work therefore contributes to an understanding of job security in an increasingly globalized world. Increasing the flexibility of *de jure* regulations will offer broader access to the gains from trade and enhanced job creation.

In addition to the work cited above, our paper relates with a number of different literatures. First, our research is closely linked to a growing body of structural models linking trade and labor market policies, such as firing costs. In the model presented by Cosar, Guner, and Tybout (2011), tariff liberalizations increase firm-level job turnover, and reductions in firing costs reinforce the impact of globalization further increasing job turnover. Kambourov (2009) presents a model in which liberalizing trade in a restricted labor market environment is associated with slower inter-sectoral labor market reallocation, lower output, and reduced productivity. We see these structural papers as complementary to our reduced-form framework designed to identify the causal implications of trade openness on labor reallocation in the presence of a complete set of labor market regulations.

Second, our research is related to a set of empirical papers on product market liberalizations in different labor market environments. Aghion, Burgess, Redding, and Zilibotti (2008) show that India's deregulation of the *License Raj* (control over entry and production in the manufacturing industry) led to differential rates of growth across industries located in states with pro-employer labor market institutions relative to industries located in states with pro-worker labor institutions. Similarly, Hasan, Mitra, and Ramaswamy (2007) also distinguish India's states by the extent of labor market restrictions, and analyze the impact of India's 1991 trade reform on labor demand. The authors find supportive evidence for the interaction of trade reform and labor regulations; that is, the impact of trade reform on labor demand is larger in states with more flexible labor institutions. Using the same data, Topalova (2010) demonstrates that India's trade liberalization negatively

impacted poverty and per capita expenditures predominantly in states with less flexible labor markets. Also relevant to our study is Freund and Bolaky (2008) who argue that trade can only improve living standards in flexible economies. In particular, their findings on hiring and firing costs suggest that the positive effects of openness are reduced when labor regulations are excessive.[12] The benefit of linked employer-employee data allows us to move beyond the industry level and state level, to compute individual-level accessions and separations, as well as to incorporate plant, individual, and match heterogeneity. Moreover, as we previously mention, exploiting variation in *de facto* labor regulations offers a more complete measure of labor market flexibility than variation in *de jure* labor regulations alone.

The paper proceeds as follows. In the next section, we provide background information on the 1988 Constitutional reform, which established the current labor market regulatory framework in Brazil, the recent evolution in the enforcement of these labor laws conducted by the Ministry of Labor, and the main features of Brazil's recent globalization. In Section 3, we outline our main data sources and offer some simple descriptive statistics. Section 4 discusses the conceptual framework behind our main empirical strategy and proposes a simple difference-in-difference reduced-form specification for the empirical work. In Section 5, we present our main findings. Section 5 also offers evidence on the heterogeneity of our main findings, considering the industry's technological intensity, plant size, and the age of the worker, and aggregate implications of the enforcement of labor regulations on plant-level productivity. We conclude in Section 6 by highlighting the main policy implications.

## 2. Policy Background

The late 1980s to the early 2000s marked a period of substantial market-oriented reform in Brazil. Of particular relevance to our work are the establishment of a new Constitution in 1988, which offered increased employment protections for workers, the liberalizing trade policy reforms beginning in 1987, and the implementation of a new currency, the *real*, in 1994. Subsequently, the currency experienced a severe and unanticipated devaluation in January 1999. Each of these policy changes may have contributed to changing labor costs and labor reallocation.

### 2.1. Labor market regulations within Brazil

[12] Eslava, Haltiwanger, Kugler, and Kugler (2010) consider the case of Colombia's pro-market reforms of the 1990s. The authors find that allowing for frictionless factor adjustment would lead to substantial improvements in efficiency over the reform period.

**The 1988 Constitutional reform** The Brazilian Federal Constitution of 1988 imposed high labor costs to plants and was very favorable to workers. First, it reduced the maximum weekly working period from 48 to 44 hours. Second, it increased the overtime wage premium from 20% to 50% of the regular wage. Third, the maximum number of hours for a continuous work shift dropped from 8 to 6 hours. Fourth, maternity leave increased from 3 to 4 months. Finally, it increased one month's vacation time pay from 1 to 4/3 of a monthly wage.

Following the 1988 changes in the labor code, the cost of labor to employers increased. First, the employer's payroll contribution increased from 18% to 20%. Second, the penalty on the plant for dismissing the worker without cause increased from 10% to 40% of the total contributions to the severance fund, *Fundo de Garantia do Tempo de Serviço* (FGTS).[13] Employers in Brazil must also give advance notice to workers in order to terminate employment. During this interim period, workers are granted up to two hours per day (25% of a regular working day) to search for a new job.[14]

**Enforcement of labor regulations** These *de jure* labor regulations are effective throughout the country. However, as the Ministry of Labor is charged with enforcing compliance with the labor regulations, there is significant heterogeneity both within country and over time in terms of how binding is the law.[15] Given the geographic scope of the country, enforcement is first decentralized to the state level with the main labor offices (*delegacias*) located in the state capital. Enforcement is then further decentralized to the local level within each state, depending on the size of the state. For example, in 2001 the state of São Paulo had 24 local labor offices (*subdelegacias*) while smaller states like Acre had only the one office coinciding with the *delegacia* in the state capital.

Throughout the late 1990s and early 2000s, labor inspections became more frequent as the large public deficit led the Brazilian government to search for alternative ways to collect tax revenue.[16]

[13] If the worker is dismissed without justification (with the exception of workers on a probationary period), the plant is fined and has to pay the worker 40% of the FGTS contributions.
[14] Some plants voluntarily choose to grant workers the full monthly wage without requiring work. Barros and Corseuil (2004) find that there are large productivity losses during this period.
[15] A comprehensive explanation of the enforcement of the labor regulation system and its importance in Brazil is given in Cardoso and Lage (2007).
[16] An inspection can be triggered either by a random plant audit, or by a report (often anonymous) of non-compliance with the law. Workers, unions, the public prosecutor's office, or even the police can make reports. In practice, almost all of the targeted plants are formal plants because it is difficult to visit a plant that is not registered, since there are no records of its activity.

We note time variation in the location of local labor offices as new *subdelegacias* open over time. For instance, in 1996 in addition to the *delegacia* in the state capital, the state of Bahia had 6 *subdelegacias*. By 2001, Bahia had 8 *subdelegacias*. Because of this, the average distance to the nearest Ministry of Labor office decreased by about 5% between 1996 and 2001. In addition, the average number of inspections in the manufacturing sector per municipality increased from 13.2 in 1996 to 14.3 in 2001. As inspectors reached out with increased intensity, the median number of inspections also increased, suggesting a leftward shift of the distribution of inspections across municipalities.

Most of the inspections and subsequent fines for infractions in Brazil are to ensure plants' compliance with the worker's formal registration in the Ministry of Labor, contributions to the severance pay fund (FGTS), compliance with the minimum wage, and with the maximum working lengths. Evasion of one of these dimensions accounted for more than 40% of all fines issued in 2001. The monetary amount of the fines is economically significant and may be issued per worker or it may be indexed to the plant's size. For example, in 2001 values, a plant is fined 216 *reais* (or approximately USD 100) for each worker without a *carteira de trabalho*, formal work authorization. Considering that, at 2001 prices, the federal minimum wage was 222 *reais*, non-compliance with worker registration is non-trivial, implying a penalty of approximately one monthly wage per worker.

Plants weigh the costs and benefits of complying with this strict labor regulation. They decide whether to hire formally, informally, or formally but without fully complying with specific features of the labor code (e.g., avoiding the provision of specific mandated benefits, such as health and safety conditions, or avoiding payments to social security). The expected cost of evading the law is a function of the monetary value of the penalties (fines and loss of reputation) and of the probability of being caught. In turn, the probability of being caught depends on the plant's characteristics (such as size, globalization status, and legal status) and on the degree of enforcement of regulation in the city where the plant is located.[17]

### 2.2. Brazil's globalization

[17] As inspectors face a performance-based pay scheme, they often look for cases where the penalty is likely to be large. As such, there is a strong correlation between the size of the firm, as a proxy for the visibility of the firm, and the number of inspections (Cardoso and Lage 2007).

**Policy reforms** The second half of the 20th century in Brazil was characterized by tight import substitution industrialization policies designed to protect the domestic manufacturing sector from foreign competition. Beyond high tariff rates, substantial non-tariff barriers characterized Brazilian trade policy during this time period. The latter half of the 1980s and the beginning of the 1990s, however, witnessed sweeping changes in Brazilian trade policy. This occurred in two phases. First, average *ad valorem* final goods tariff rates fell from 58% in 1987 to 32% in 1989. These reforms had little impact on import competition however, as non-tariff barriers remained highly restrictive. Second, between 1990 and 1993, the federal government abolished all remaining non-tariff barriers inherited from the import substitution era and announced a schedule for the reduction of nominal tariffs over the next four years (Moreira and Correa 1998). Effective rates of protection fell by over 70% in just four years—from approximately 48%, on average, in 1990 to 14%, on average, in 1994 (Kume, Piani, and Souza 2003).

In 1994, after decades of high inflation and several unsuccessful stabilization attempts, the Brazilian government succeeded with a macroeconomic stabilization plan (*Plano Real*), designed to help correct a large fiscal deficit and lastingly end hyperinflation. The new currency, the *real*, was pegged to the U.S. dollar, and began at parity on July 1, 1994. Officially, the *real* was set to a crawling peg which permitted the currency to depreciate at a controlled rate against the U.S. dollar. However, as the country's persistent effort to control inflation paid off, the real exchange rate actually appreciated in its first months. In response, the government partially reversed trade reforms in 1995 after manufacturing industries lost competitiveness due to the *real*'s appreciation.[18]

**Currency crisis** Despite efforts to control public spending and raise tax revenues, Brazil's fiscal deficits remained high and continued to grow. Meanwhile, persistent current account deficits placed significant pressure on the pegged exchange rate and government reserves, leading investors to withdraw funds from Brazil. In response to the financial crises in Asia in 1997 and Russia in 1998, the authorities raised interest rates to encourage domestic savings and investment. However, as debt service obligations increased, investor panic persisted. Dollar reserves fell from approximately $58 billion in 1996 to $43 billion in 1998. In mid-January 1999, capital outflows accelerated further when the Governor of the State of Minas Gerais declared a moratorium on the state's debt payments to the national government, triggering the government's announcement of

[18] Average *ad valorem* tariffs climbed slightly in subsequent years—from an average of 12.2% in 1994 to an average of 14.4% in 2001.

the end of the crawling peg, allowing the *real* to float against the U.S. dollar (Gruben and Kiser 1999). Overnight, the nominal exchange rate devalued by 9% against the U.S. dollar, and by the end of the month, the *real* had depreciated by 25% (see Figure 3.1).

## 3. Data

Our main data are administrative records from Brazil for formal sector workers linked to their employers. We match these data by the plant's municipality with city-level information on the enforcement of labor market regulations, by the plant's sector with information on industry-specific real exchange rates, and by the employer tax identifier to information on exposure to global markets. The sample period for analysis covers Brazil's main currency crisis period, between 1996 and 2001. This exogenous shock to plant and worker outcomes allows us to uncover the differential impact of increased exposure to trade on labor reallocation depending on the degree to which plants face regulatory enforcement.

**Matched employer-employee administrative data** We use data collected by the Brazilian Labor Ministry, which requires by law that all registered establishments report on their formal workforce in each year.[19] This information has been collected in the administrative records *Relação Anual de Informações Sociais* (RAIS) since 1986. For our analysis, however, we use data from RAIS for the years 1996 through 2001, when we also have complementary information on regulatory enforcement, exchange rates, and the employer's globalization status.

The main benefit of the RAIS database is that both plants and workers are uniquely identified allowing us to trace workers over time and across different plants. The data also include the industry and municipality of each plant.[20] Other relevant variables of interest include the worker's month of accession to and the month of separation from the job, weekly hours worked, and the type

[19] Our analysis is restricted to the effect of trade liberalization on *formal* labor reallocation. It is plausible, however, that in cities with weaker enforcement and hence more flexible labor adjustment, along both the formal and informal margin, plants may be more likely to make adjustments along the informal margin. Therefore, our findings on formal labor adjustment do not capture total labor adjustment. It is not clear, however, how the results would change. Goldberg and Pavcnik (2003), Paz (2012), and Menezes-Filho and Muendler (2011) find mixed results on the impact of trade liberalization on the informal sector in Brazil. Also, to the extent that enforcement increases the cost of labor, we may note shifts away from labor (both formal and informal) towards capital.

[20] The industrial classification available in RAIS is the 4-digit National Classification of Economic Activities (CNAE).

of employment contract (temporary versus permanent), as well as detailed information on the worker's human capital, including her occupation, education, tenure at the plant, gender, and age. We define a worker as hired to employer *j* during year *t* if RAIS reports a non-missing value for the month of accession. We define a worker as fired from employer *j* during year *t* if RAIS reports the worker no longer employed at firm *j* on December 31 of year *t*.

We restrict observations in RAIS as follows. We draw a 1% random sample of the complete list of workers ever to appear in the national records and retrieve their complete formal sector employment history. We include only manufacturing sector[21] workers between the ages of 15 and 64 years, with a positive average monthly wage, and employed in private-sector jobs. To ensure a more precise identification of worker and match fixed effects, we further restrict the sample to those workers who are in the data for at least two years.

**Enforcement data** We also explore administrative city-level data on the enforcement of labor regulations, also collected by the Brazilian Ministry of Labor. Data for the number of inspector visits are available by city and 1-digit sector for the years 1996, 1998, 2000, and 2002. We use the information on visits by inspectors to manufacturing plants only. For our analysis, we interpolate average values for the missing years and match the enforcement data to the RAIS data by the plant's municipal location. This information identifies plants and workers facing varying degrees of regulatory enforcement.

We proxy the degree of regulatory enforcement with the intensity of labor inspections at the city level. In particular, our main measure of enforcement, designed to capture the probability of a visit by labor inspectors to plants within a city, is the logarithm of the number of labor inspections at the city level (plus one) per 100 plants in the city based on RAIS. This scaled measure of inspections helps to control for important differences across cities. Moreover, the impact of such a measure will reflect the direct effect of inspections, as well as plants' perceived threat of inspections (even in the absence of plant-level inspections) based on inspections at neighboring plants.

Table 3.1 reports the nationwide increase in enforcement of labor regulations between 1996 and 2001. The proportion of cities with at least 1 manufacturing inspection rose from 33% in 1996 to

[21] The manufacturing sector consists of CNAE 2-digit codes 15-37. In future drafts of the paper, we hope to incorporate other sectors for a comparative analysis, as well.

52% in 2001. This corresponds to an increase in the average number of inspections across cities, most notably between 1998 and 2000, when the average number of inspections increased by 15%. As the number of inspections at the city level is correlated with the size of the city (i.e., population, labor force, and number of plants), our preferred measure documents the number of inspections per 100 registered plants as is reported in column (3). The data report increases in the number of inspections per plant and per worker, as inspectors intensify the enforcement process to reach additional plants, workers, and cities.[22]

We also note significant heterogeneity in the within-country variation in the intensity of enforcement across cities in Brazil, as is depicted in Figure 3.2. The left panel illustrates the number of inspections per Brazilian city in 1998, with darker shades portraying higher numbers. The right panel depicts the same statistic two years later in the year 2000. We remark on the variation across municipalities and over time. First, we observe the darkest areas of the map in the high-income Southern and Southeastern regions of the country. We also notice a darkening of the map between 1998 and 2000 as enforcement spreads to further parts of the country.[23] In the analysis that follows, we include interactive state-year dummies into our difference-in-difference approach to account for the spatial pattern of inspections.

**Industry-specific exchange rates** We construct trade-weighted industry-specific real exchange rates based on bilateral real exchange rate data from the International Monetary Fund and bilateral trade flows by commodity made available by the National Bureau of Economic Research (Feenstra, Lipsey, Deng, Ma, and Mo 2004).[24] We match the industry-specific real exchange rates to the RAIS data by the plant's 4-digit industrial classification, in order to identify plants and workers in industries with differential globalization experiences.[25]

[22] In doing so, the distribution of inspections across cities shifts left. Most notably, the city of São Paulo reports the maximum number of inspections in every year, as would be expected given the city's large population and labor force size. However, São Paulo city also sees its number of inspections reduced by a third over the course of the five years.

[23] The same qualitative patterns hold for the number of inspections and the number of inspections per worker.

[24] Trade flows are organized by Standard Industrial Trade Classification (SITC) codes. We match the 4-digit SITC revision 2 codes to the 4-digit CNAE codes available in RAIS using publicly available concordances (http://www.econ.ucsd.edu/muendler/html/brazil.html#brazsec).

[25] As we discuss in Section 2.2, average tariff rates were relatively flat over our sample period. Variations in the real exchange rate, therefore, provide a more realistic measure of changes in trade openness during our sample period.

As was previously noted, Brazil's aggregate real exchange rate devalued in January 1999, increasing the relative price of Brazilian imports. However, the aggregate exchange rate may be less effective at capturing true changes in industry competitiveness, induced by changes in specific bilateral exchange rates, if particular trading partners are of particular importance for particular industries. That is, movements in the dollar/*real*, peso/*real*, and euro/*real* exchange rates may have different implications for different industries, depending on the industry's trade with the U.S., Argentina, and Europe, respectively. Therefore, following Goldberg (2004), we calculate the trade-weighted real exchange rate as follows:

$$trer_t^k = \sum_c \left( \left( .5 * \frac{X_t^{kc}}{\sum_c X_t^{kc}} + .5 * \frac{M_t^{kc}}{\sum_c M_t^{kc}} \right) * rer_t^c \right)$$

where *t* indexes time, *k* indexes industry, and *c* indexes country, such that the bilateral real exchange rate, $rer_t^c$, denoted in terms of foreign currency units per *real*, is weighted by industry-specific and time-varying export shares ($\frac{X_t^{kc}}{\sum_c X_t^{kc}}$) and import shares ($\frac{M_t^{kc}}{\sum_c M_t^{kc}}$).[26]

A decrease in the value of this index implies a real depreciation of the Brazilian *real* in trade-weighted terms for industry *k*. Across all industries, the average index decreased from 0.97 in 1996 to 0.62 in 2001, with the most dramatic drop of roughly 30% occurring between 1998 and 1999. As Figure 3.3 illustrates in the left and right panels, respectively, there is also substantial heterogeneity across industries in both the level of and annual changes in the real exchange rate. Though the mean exchange rate is valued at 0.66 in 1999 in the aftermath of the crisis, the manufacture of other food products has a substantially lower trade-weighted real exchange rate at 0.54, while the trade-weighted real exchange rate in the industry which manufactures strings, cables, and other cords is far higher at 0.80. Similarly, while all sectors experienced sharp exchange rate declines between 1998 and 1999, some suffered more than others. Non-ferrous metal manufacturing endured the steepest annual depreciation of 40 percentage points, while sugar manufacturing faced a mere 16 percentage point decline.

**Exposure to global markets** We also investigate information on the firm's degree of global engagement, as captured by total export sales. We rely on complementary data from the Brazilian

[26] Following Campa and Goldberg (2001), we lag the trade shares one period to avoid issues of endogeneity between trade and the exchange rate.

Customs Office (SECEX) to create a single indicator for the firm's globalization status. Information on firm-level export transactions is available from SECEX, which records all legally-registered firms in Brazil with at least one export transaction in a given year. We denote exporters to be those firms that exported any positive dollar amount at any point during the 1996 to 2001 time period. This time-invariant indicator is designed to minimize potential endogeneity concerns surrounding the export decision post-devaluation.

### 3.1. Descriptive statistics

We report detailed descriptive statistics in Table 3.2. Column (1) reports statistics for our final sample of formal-sector manufacturing workers, as well as the plants, cities, and industries in which they work. Column (2) reports summary statistics for the sample of exporting plants, while column (3) reports statistics for domestic plants. The final sample has 313,940 worker-plant-year observations, with 81,254 workers employed in 53,011 plants allowing for 122,017 worker-plant matches, covering 2,769 municipalities and 240 4-digit industries throughout the sample period of 1996 to 2001.

Approximately 29% of manufacturing workers were hired to a new employer during our sample period, while 25% were separated from their employer. The data report that only 2% of workers are employed with temporary contracts. Across employers, workers averaged 43.5 hours per week. The average age of a worker is 32 years. The majority of the manufacturing labor force has less than a high school education, while about 25% have at least a high school education, and only 7% have a tertiary education. Roughly a third of the manufacturing labor force is employed in skilled blue collar professions, such as machine operators. Another 25% are in white collar professions—7% in secretarial and sales positions and 17% in professional, managerial, and technical positions. Ten percent of the formal manufacturing sector is employed in unskilled blue collar jobs, most often found in the construction and service sectors. The average plant employs 59 workers, and pays an average annual wage of 2,970 *reais*. The average duration of a worker-plant match in the sample is 2 years and 9 months. The average municipality in our sample faces approximately 35 inspections during the 1996 to 2001 sample period.[27] The average industry has a trade-weighted real exchange rate index of 0.62 and employs over 18,000 workers, of which about 1 in every 5 are unionized.

[27] This number stands in contrast to the average number of inspections across all Brazilian municipalities (see Table 3.1). Our 1% random sample covers only registered firms which are, on average, larger. Therefore, this

Of the 53,011 plants, roughly a quarter are exporters. However, these 13,891 plants represent over half of the total number of observations and worker-plant matches, largely because exporting plants employ more workers on average (at 161 workers as compared to 27 workers for domestic plants) and because the average worker-plant match duration is roughly 4 months longer at exporting plants than at domestic plants. Similar to Menezes-Filho and Muendler (2011), our data report that accession rates are lower at exporters than at non-exporters—24% as compared to 34%, respectively. We also find that separation rates are lower at exporting plants for our sample of workers. Our RAIS matched data sample report the common finding in the literature that, on average, exporters are more skill-intensive and pay higher wages (e.g., Bernard and Jensen (1995)). Almost 40% of the manufacturing labor force at exporters is high-skilled, as defined by those workers with at least a high school education, while by comparison 25% of the workforce is high-skilled at plants serving the domestic market. The average annual wage paid by exporting plants is 4,907 *reais*, as compared to 2,541 *reais* at domestic plants. Exporters are only represented in about 53% of the 2,769 municipalities covered by our formal sector data.[28] Combined with their greater visibility due to the higher total employment numbers, the data indicate that on average the municipalities in which exporters are located are more heavily enforced than those in which domestic plants are located. On average, exporting plants face 61 manufacturing inspections while domestic plants face, on average, 39 inspections. By contrast, exporters and non-exporters are represented across almost all industries in Brazil. For this reason, we see little variation across plant-type in the average trade-weighted real exchange rate or in unionization rates.

## 4. Empirical Model

Our goal in this paper is to uncover how trade openness affects labor market reallocation. We consider the devaluation of the *real* in 1999 as the main exogenous trade shock and argue that a similar trade shock impacts plants differentially based on their exposure to the enforcement of labor regulations and their mode of globalization. We begin with the following framework in mind:

$$y_{jkt} = \beta_1 Z_{jt} + \beta_2 S_{kt} + \varphi_j + \delta_{st} + \varepsilon_{jt} \quad (1)$$

is to be expected, as these firms are naturally more exposed to enforcement than smaller firms (Cardoso and Lage 2007).

[28] See Aguayo-Tellez, Muendler, and Poole (2010) for further information on the spatial distribution of exporting plants.

where *j* indexes the plant, *k* indexes the plant's 4-digit industry, and *t* indexes time. We relate plant-level outcomes ($y_{jkt}$), such as total plant employment, to time-varying, plant characteristics ($Z_{jt}$) such as average worker tenure at the plant, and the age, gender, educational, and occupational composition of the plant, and time-varying, industry characteristics ($S_{kt}$) such as the unionization rate[29], industry employment, average worker tenure in the industry, and the age, gender, educational, and occupational composition of the industry. The specification also includes plant fixed effects ($\varphi_j$) to capture time-invariant factors, such as the plant's unobserved underlying productivity, technology, or management style, which may influence both a plant's selection into exporting and plant-level labor market adjustment, and state-specific year dummies ($\delta_{st}$) to control for the average effect on labor turnover of Brazil's many policy reforms over this time period.

Importantly, among the time-varying, industry-specific characteristics is the trade-weighted industry-specific real exchange rate ($TRER_{kt}$) which serves as an exogenous shock to trade openness. Our basic argument is based on the fact that when the *real* depreciates, the price of goods typically imported into Brazil will rise, improving the competitiveness and increasing profits of Brazilian plants. To the extent that plant profits and employment growth are correlated, we expect that a devaluation of the Brazilian *real* will increase employment for the average plant. The first column of Appendix Table A.1 reports results from the estimation of equation (1) where the dependent variable is the logarithm of plant employment. Consistent with the literature (e.g., Revenga (1992)), a depreciation of the trade-weighted real exchange rate is associated with increases in employment for the average plant.

Equation (1), however, considers only the industry-time shock of the exchange rate devaluation. Brazil's large informal sector suggests significant evasion of Ministry of Labor regulations. The implications of increased trade openness, via a real exchange rate depreciation, for formal labor turnover depend on the degree to which plants are exposed to labor market regulatory enforcement. We hypothesize that two identical plants will respond differently to changes in trade openness depending on the *de facto* regulations they face. For this reason, we adapt equation (1) as follows:

[29] As labor turnover may be restricted in heavily-unionized industries, we control for the time-varying unionization rate, based on Brazilian household surveys (PNAD).

$$y_{jmkt} = \gamma_1 TRER_{kt} * ENF_{mt} + \gamma_2 TRER_{kt} + \gamma_3 ENF_{mt} + \beta_1 Z_{jt} + \beta_2 S_{kt} + \varphi_j + \delta_{st} + \varepsilon_{jmt} \quad (2)$$

where $m$ now indexes the city (*munícipio*). $ENF_{mt}$ represents time-varying, municipality-level enforcement of labor regulations, as captured by Ministry of Labor inspections. We begin with a measure of enforcement as the logarithm of the number of inspections at the city level (plus one). $\gamma_1$, our main coefficient of interest, captures the differential impact of a trade shock on plants in strictly-enforced municipalities relative to weakly-enforced municipalities. In response to an expansionary trade shock, such as Brazil's currency devaluation, plants wish to expand employment ($\gamma_2 < 0$). However, plants in heavily-inspected cities may be differentially restricted from adjusting labor ($\gamma_1 > 0$)—as the cost of a formal worker increases, strictly-enforced plants will increase employment by less than weakly-enforced plants—or may adjust formal labor by relatively more ($\gamma_1 < 0$), as formal work registrations increase.

One could worry that the level of enforcement of labor market regulations is not exogenous to plant outcomes ($y_{jmkt}$). In particular, enforcement may be stricter in cities where violations of the labor laws are more frequent or in cities where institutions are more developed. Moreover, enforcement may be stronger for more visible (i.e., larger and more globalized) plants. As violations of labor laws, better institutions, and plant size and type are likely also correlated with labor market outcomes, to minimize this concern, we emphasize that equation (2) relates changes over time in the enforcement of labor market regulations to labor turnover. Importantly, as in much of the program evaluation literature, our identification strategy relies on how the exogenous trade shock impacts plant turnover differentially based on the plant's exposure to regulatory enforcement.

Column (2) of Appendix Table A.1 reports results from equation (2). Our data largely confirm these predictions. The exogenous real exchange rate depreciation increases plant-level employment. However, consistent with findings in Almeida and Carneiro (2009), increases in regulatory enforcement at the city level tend to decrease formal plant size, suggesting that increases in the cost of formal workers dominate any potential impact from increased compliance with mandated benefits and formal work registrations. In this paper, we are interested in the interaction term reflecting the differential impact of globalization on plants located in strictly-enforced municipalities ($\gamma_1$). Our results report that plants in heavily-inspected cities are restricted from expanding employment with a depreciation of the currency.

In the absence of plant-level information on inspections, our analysis intends to capture the probability that a plant is inspected. This allows for the direct effect of inspections, as well as the indirect effect of a neighboring plant's inspections. To the extent that large cities have many inspections, but also many plants to be inspected, in column (3) of Appendix Table A.1, we adjust our main enforcement variable to control for the size of the city. Specifically, our preferred enforcement variable characterizes inspections per 100 plants in the city. Evaluated at the 10th percentile of inspections, a 10 percent depreciation increases employment by 1.6%, while the same devaluation increases employment by only 1.0% at plants located in cities at the 90th percentile of inspections. These plant-level results highlight our main predictions—that strict labor market institutions limit plants' labor adjustment in response to shocks.

### 4.1. Worker-level employment transitions

When facing a trade shock, expanding plants can adjust along the extensive margin by increasing hiring, decreasing firing, or both, as well as along the intensive margin by increasing the hours worked for existing employees or switching from temporary to permanent contracts. Moreover, enforcement also likely influences adjustment along each of these margins. In order to better understand these mechanisms, our main reduced-form equation focuses on a worker-level analysis. In particular, we augment equation (2) as follows:

$$y_{ijt} = \gamma_1 TRER_{kt} * ENF_{mt} + \gamma_2 TRER_{kt} + \gamma_3 ENF_{mt} + \beta_1 Z_{jt} + \beta_2 S_{kt} + \beta_3 X_{it} + \pi_{ij} + \delta_{st} + \varepsilon_{ijt} \quad (3)$$

where $i$ indexes the worker and $y_{ijt}$ represents worker-level labor market outcomes, such as employment transitions and hours worked per week. We characterize employment transitions with three variables: an indicator variable that takes the value one if a match between worker *i* and plant *j* is created at time *t* (i.e., if there is a plant-year accession); an indicator variable that takes the value one if a match between worker $i$ and plant $j$ is destroyed at time $t$ (i.e., if there is a plant-year separation); and an indicator variable that takes the value one when worker *i* is employed with a full-time contract in plant *j* at time *t*. All other variables are defined as previously discussed. $X_{it}$ are time-varying worker-level characteristics (such as the worker's tenure at the plant, the worker's age (and age squared), education, and occupation) and $\pi_{ij}$ is a worker-plant (time-invariant) match effect.

We argue that time-invariant worker-plant match effects are important because when worker-plant production complementarities exist (as in new trade models), high productivity plants will screen for high ability workers. This may lead to the sorting of high ability workers into high productivity plants.[30] In our setting, this implies that following a trade liberalization, otherwise identical workers may have a higher probability of separation from (or a lower probability of accession to) a high productivity plant than from (and to) a low productivity plant. For this reason, we replace the plant fixed effects from equation (2) with worker-plant match-specific fixed effects ($\pi_{ij}$), which allow for time-invariant, unobservable match quality (associated with the potential for worker sorting) in the labor reallocation process.[31]

Our main specification in equation (3) relates exogenous changes in industry-specific real exchange rates with match-specific outcomes, between 1996 and 2001, differentially for worker-plant matches in strictly-enforced areas. In other words, we explore a difference-in-difference methodology to identify the effects of openness on labor turnover. The main coefficient of interest in equation (3) is $\gamma_1$, which captures the differential effect of stricter enforcement for workers employed in plants exposed to varying real exchange rate changes.

In addition, we argue that the implications of a real exchange rate devaluation are heterogeneous across plant types and therefore, consider equation (3) separately for exporting and domestically-oriented plants. Our prior is that openness allows plants best placed to compete abroad to expand and those in import-competing industries to relatively contract. We hypothesize that the expansionary effect (increase in hiring and decrease in firing) of the exchange rate shock ($\gamma_2$) will be larger for exporting plants than for plants serving only the domestic market, as foreign market access improves.

As in the plant-level analysis, the theoretical predictions for $\gamma_1$ are ambiguous. On the one hand, the stricter enforcement of labor regulations raises the cost of formal workers. As such, plants facing strict enforcement will have increased difficulties in adjusting labor. On the other hand, the stricter

[30] Krishna, Poole, and Senses (2011) document the importance of worker-firm complementarity in the labor reallocation process post-liberalization using matched employer-employee data for Brazil. Their results, controlling for the non-random assignment of workers to firms, suggests a strong bias in plant-level analyses, as exporters differentially increase match quality relative to non-exporters post-liberalization.

[31] As neither the plant nor the worker vary within a match, the match-specific effects also control for time-invariant, unobservable plant heterogeneity and time-invariant, unobservable worker heterogeneity.

enforcement of labor regulations also increases job quality, in terms of compliance with mandated benefits for the worker. For this reason, we might expect increases in employment in more heavily-enforced cities, as formal employment becomes a more attractive option and formal work registration increases. We empirically test this ambiguity given our strong predictions on the impact of trade openness on employment for exporters relative to non-exporters. Moreover, we further hypothesize that labor market regulations on formal employment are less binding for exporting firms, and thus expect the effects of regulatory enforcement to be less important for plants exposed to global markets.[32]

As many of the covariates in equation (3) are also dummy variables, we choose to estimate the equation using a linear probability model. Compared to a probit analysis, the linear probability model has the advantage of allowing for a straightforward interpretation of the regression coefficients.[33] To take into account the occurrence of repeated observations of individuals within city-sectors, we cluster the robust standard errors at the city-sector level, though our main results are robust to clustering at the match level, as well.

## 5. Main Results

Table 5.1 reports our main estimates where the dependent variable is the worker-plant-year accession. In columns (1) and (2) of Table 5.1, we estimate variants of equation (3) for all worker-plant matches in our sample. As discussed, the specification controls for unobservable, time-invariant worker-plant match quality, observable, time-varying worker, plant, and industry

[32] Cardoso and Lage (2007) argue that the integration of firms in international trade and the need to comply with international quality standards implicitly force firms to comply with labor regulations. This is reinforced in Harrison and Scorse (2003), who report that exporters and foreign firms in Indonesia are more likely to comply with labor regulations. In addition, Bloom and Van Reenen (2010) show that labor market regulations are negatively correlated with the quality of management practices across countries. At the same time, multinational and exporting firms tend to be better managed across all countries, suggesting the better institutional environment at exporting plants offers enhanced compliance.

[33] It is well-known that in the extreme case of a fully saturated model (i.e., one where all independent variables are discrete variables for mutually-exhaustive categories), the linear probability model is completely general and the fitted probabilities lie within the interval [0, 1]. When looking at accessions, separations, and full time contracts, our dependent variable will be a dummy variable and applying least squares will not yield the most efficient estimator. Therefore, in ongoing work, we are estimating our main models using a probit analysis for robustness.

characteristics, and state-year dummies.[34] The point estimates in columns (1) and (2) are never statistically significant, but the signs are informative. In particular, the coefficients suggest that for otherwise identical workers, plants, and matches, a depreciation of the real exchange rate increases a worker's probability of hire as the average plant expands.

We argue, however, that trade shocks and regulatory enforcement have different effects depending on the plant's mode of globalization. As real exchange rate depreciations increase the competitiveness of exporting plants in foreign markets, we anticipate an expansion of employment at exporters relative to non-exporters. In the final two columns of Table 5.1, we report coefficients for the set of exporting plants and non-exporting plants, respectively. We denote exporters as those firms that exported at any point during the period 1996 to 2001. This time-invariant indicator is designed to minimize potential endogeneity concerns surrounding the globalization decision post-devaluation.[35]

As predicted by new heterogeneous firm trade models, a depreciation of the *real* increases hiring at exporters, and decreases hiring at plants producing for the domestic market. Moreover, consistent with the literature, the way in which enforcement impacts hiring is different depending on the globalization status of the plant. Notably, enforcement has no statistical impact on exporting plants, in line with results in Harrison and Scorse (2003) for Indonesia that globalized firms are internally-enforced and more likely to follow existing labor regulations. By contrast, domestic plants in strictly-enforced municipalities decrease hiring by more than otherwise identical domestic plants in weakly-enforced municipalities, as the cost of formal workers increases for these plants. The impact of trade openness differs for plants with the same mode of globalization but varying degrees of

[34] In particular, we include at the worker level, the worker's age (and age squared), tenure at the plant in months, education (as two dummy variables—at least high school and more than high school where less than high school is the omitted category) and occupation (as three dummy variables—skilled blue collar worker, unskilled white collar worker, and professional/managerial worker where unskilled blue collar worker is the omitted category). At the plant level, we include average plant wages, plant employment, average worker tenure at the plant, and the age, gender, educational, and occupational composition of the plant. We also include the following industry characteristics: the industry unionization rate, industry employment, average worker tenure in the industry, and the age, gender, educational, and occupational composition of the industry.

[35] In unreported regressions, we test the robustness of our results to the endogeneity of the export status indicator. Specifically, we categorize plants based on the industry's import penetration. This industry-level categorization of the plant's globalization status largely confirms our main findings. Alternatively, we also classify plants based on the city's market access (in the form of three indicators: distance to the state capital, distance to the federal capital, and an index of transportation costs from the city to the nearest capital city regardless of state). Across all measures, our main findings are robust to a definition of globalization based on the city's market access.

exposure to *de facto* labor market regulations.

The magnitudes of our estimates seem to be plausible. Evaluating the effect on workers and plants located in municipalities at the mean level of inspections, a 10 percent depreciation of the *real* increases the hiring probability at exporting plants by 4.1% and decreases the hiring probability at domestic plants by 7.3%. The impact for domestic plants varies depending on the level of enforcement the plant faces—for domestic plants located in municipalities at the 10th percentile of inspections, a 10 percent depreciation of the *real* decreases the probability of hire by only 6.0%, while workers matched with domestic plants located in municipalities in the 90th percentile of inspections experience a decrease in the accession probability of around 9.0%.

In Table 5.2, we report estimates for equation (3), by the plant's mode of globalization, where the main dependent variable represents the worker's separation from the plant. We hypothesize that a depreciation of the real exchange rate decreases the probability of separation for the average plant, as competitiveness increases. This is confirmed in Table 5.2; the point estimate on $\gamma_2$ is positive, but statistically insignificant. We also note that, as predicted, the effect is driven by decreases in firing at expanding exporting plants. Though statistically insignificant, the results for non-exporting plants point to an increase in firing relative to exporting plants in response to a real exchange rate depreciation.

We remind the reader that increases in inspections ambiguously relate to separations. On the one hand, more enforcement increases the cost of firing and thus may decrease firings. On the other hand, with increased enforcement, existing labor becomes more expensive and the plant may resort to firing workers in the short run to overcome the increased cost. Once again, we anticipate the impact of regulatory enforcement to be stronger for non-exporting firms where regulations are most binding. Our data from Brazil support these claims. Exporters in strictly-enforced municipalities respond no differently to an expansionary trade shock than do identical exporters in weakly-enforced cities. However, non-exporting plants in strictly-enforced municipalities differentially increase firing as compared to similar non-exporters facing weaker enforcement. Domestic plants located in cities at the 10th percentile of inspections contract by increasing firing by approximately 2.0% in response to a 10 percent devaluation, while the probability that a match is destroyed at similar domestic plants located in cities at the 90th percentile of inspections increases by close to 5.0% in response to the same trade shock.

Taken together, Tables 5.1 and 5.2 suggest that stronger enforcement of labor regulations influence labor turnover along the extensive margin for non-exporting plants through increased firing and decreased hiring. That is, contracting non-exporters decrease job creation and increase job destruction even further due to increases in the cost of formal employment in strictly-regulated areas. These results offer important implications for policy. Job security in an increasingly globalized world receives considerable attention from academics, policymakers, and the media. Our results confirm recent trade models in which non-exporting plants contract in response to trade reform. More importantly, our data imply that labor market regulations reinforce these contractionary effects of trade reform for small, constrained non-exporting plants.

Expanding and contracting plants may also adjust along the intensive margin. For instance, when faced with a trade shock, employers may adjust the hours worked for existing employees or shift workers between full-time and temporary contracts. Although the labor law limits a continuous work shift to 6 hours, limits the weekly working period to 44 hours, and mandates increases in overtime pay, the effects of the labor laws on plants will likely differ depending on the degree of enforcement. Similarly, the increased cost of formal labor associated with stricter enforcement of labor regulations may lead plants to shift towards the use of temporary contracts over permanent contracts. The latter could help employers overcome the long-term relationships of more restrictive employment contracts. We next consider these adjustments for all plants and by the plant's mode of globalization.

Table 5.3 reports results from the estimation of equation (3) where the dependent variable is the logarithm of hours worked per week for all plants and by the plant's export status. Table 5.4 reports coefficients for the estimation of equation (3) where the dependent variable is an indicator variable if worker *i* is employed with a full-time contract in plant *j* in time *t*. Across Tables 5.3 and 5.4, our data suggest little variation in the intensive margin in response to exchange rate shocks and regulatory enforcement for both exporters and non-exporters. The point estimates in Table 5.3 provide some suggestive evidence for the idea that exporters differentially expand hours and non-exporters differentially contract hours. In Table 5.4, the evidence points to an increased use of temporary contracts to help smooth the shock from exchange rate changes, particularly at non-exporting plants. However, across all plant types, the degree of regulatory enforcement, contrary to predictions, does not influence hours worked or contract types.

### 5.1. Heterogeneity of the Results

Our main results provide suggestive evidence that non-exporters facing strict labor market regulations differentially decrease job creation and differentially increase job destruction in response to trade openness, as compared to similar plants located in areas with weak labor market regulatory enforcement. We do not find any significant impact of enforcement on adjustments along the intensive margin (hours and contract type) for either exporters or non-exporters. In this section, we consider the heterogeneity of these results based on industry-level differences such as the sector's technological intensity, based on plant-level differences such as employment, and based on worker-level differences such as the worker's age group. Given the lack of statistical evidence in support of adjustments along the intensive margin, we restrict the analysis forward to the impact on job creation and job destruction. The results for hours and contract type are available upon request.

**Technological Intensity** Our main argument rests on the fact that a trade shock will reallocate factors of production towards more efficient use. We consider labor as the relevant factor of production in this paper. However, the same shock may also influence adjustments in the short-term in terms of capital and other physical materials factors of production. To ensure that the effects of labor market regulatory enforcement we find in Tables 5.1 and 5.2 reflect plants' constraints in adjusting labor in the short-run, we split our main sample into sectors depending on the technological intensity. Our assumption here, consistent with much of the literature, is that sectors relying on technology are relatively capital-intensive. Therefore, low technology sectors are assumed to be more labor-intensive. For this reason, we anticipate that our main findings are driven by plants in low-tech (labor-intensive) industries. We rely on data from the World Bank to define sectors' technological intensity. Examples of high-tech industries are: petroleum refining, chemical manufacturing, and automobile manufacturing. Examples of low-tech industries are: food and beverage, textile, and wood manufacturing.

We report coefficients from the estimation of equation (3) by the sector's technological intensity in Table 5.5. In the top panel, the dependent variable represents an indicator for job creation, while in the bottom panel, we use a job destruction indicator as the dependent variable. The results mostly confirm our hypotheses. The result that domestic plants in strictly-enforced municipalities limit hiring by more in response to a trade shock is driven by domestic plants in low-tech (labor-

intensive) industries. Table 5.2's finding that domestic plants located in strictly-enforced areas increase job destruction by more is also largely driven by domestic plants in low-tech industries. The main interaction parameter of interest is negative and statistically significant at the 10% level of significance.

**Plant Size** As is emphasized in Cardoso and Lage (2007), there is a strong correlation between the size of the firm, as a proxy for the visibility of the firm, and the number of inspections. The results in Kugler (2004) reinforce this finding. The author reports Colombian labor market reforms had a greater impact on workers in larger firms. Moreover, it is now well-established in the international economics literature that exporters and non-exporters differ substantially in terms of productivity and size, among other attributes (Bernard and Jensen 1995). For these reasons, we next explore the heterogeneity of our main findings by the size of the plant. We define a time-invariant large plant indicator equal to one for those plants with average employment between 1996 and 2001 greater than the median value.[36] We argue that comparing similarly-sized plants helps to minimize any possible selection bias associated with the plant's globalization status.

Table 5.6 displays results from the estimation of equation (3), by the size of the plant, for all plants and by the plant's mode of globalization, where the dependent variable in the top panel is a worker-plant job creation. First, we note that the positive, but insignificant, interaction coefficient in column (1) of Table 5.1 is driven by the impact on below-median sized plants. Furthermore, it is evident that this effect is wholly concentrated among small, non-exporting plants. This makes sense as these are the plants for which we argue labor market regulations are most restrictive. Due to their visibility, as is suggested by Cardoso and Lage (2007), large plants are more likely already inspected and are more likely to follow existing regulations. As such, small non-exporters who experienced an increase in enforcement over our sample period demonstrate significant differences in job creation and job destruction in response to trade reform as compared to similar plants facing less labor market regulatory enforcement.

In addition, we note that when comparing large exporters to large non-exporters, the main coefficients on the trade-weighted real exchange rate remain statistically significant and of the same sign in comparison to Table 5.1. Considering similarly-sized plants helps to minimize potential bias

[36] Median employment across all plants is 9 employees. We maintain this cutoff threshold across all plant-types.

associated with selection into exporting post-trade reform. The results for worker-plant job destruction are reported in the bottom panel. Our conclusions are in line with those from the job creation results in the top panel.

**Worker Age** Our main findings show that, following a trade shock, labor adjustment (and particularly at non-exporting plants) varies depending on the degree of enforcement of labor market regulations. However, in addition to this main effect, the composition of employment is also likely to be affected by the stringency of enforcement of labor regulations. We hypothesize that in environments facing strict enforcement, those already employed are more likely to remain employed, while new entrants or re-entrants into the labor force—as is likely the case with younger workers—are less likely to be hired. An implication of this is that within plants after a currency depreciation, we expect to observe more older workers in the workforce. Table 5.7 reports estimates for equation (3), dividing the sample by the age of the worker and the plant's globalization status. We define workers as "older" when they are older than 32 (the mean worker age in the sample) and "younger" when they are 32 or less.

As predicted, we see that increases in *de facto* labor regulations decrease the hiring of young workers at non-exporting plants. The sign on the main interaction term therefore shows that, in response to a real exchange rate depreciation, non-exporting plants in strictly-enforced municipalities differentially decrease hiring of youth workers in particular. Similarly, the result that non-exporters in strictly-enforced municipalities differentially increase job destruction relative to non-exporters in weakly-enforced areas is driven by the impact on young workers.

### 5.3. Aggregate Implications

A key argument in favor of trade liberalizing reforms is that factors can reallocate to more efficient uses, allowing for enhanced productivity and growth. However, in this paper we demonstrate that the efficient reallocation of labor in response to trade shocks is inhibited by strict *de facto* labor market regulations. In this section, we investigate the extent to which dampened labor reallocation also restricts the within plant productivity gains associated with trade openness.

There are a few potential channels linking increased enforcement to lower plant-level productivity. For instance, the inability of plants to adjust to changing conditions and to reallocate from declining

to dynamic sectors may reduce plant-level productivity. In addition, more regulations may prevent plants from introducing new goods or investing in more complex production technologies which may have higher value-added, but also face more volatile demand and thus require greater adjustments. Finally, given the high costs of dismissals in areas with strict employment protection, employers may now be forced to retain unproductive workers they would have otherwise fired. Also, given the expectation of a job-for-life, employees may now have less incentive to exert effort, thus lowering their plant and worker productivity.

On the other hand, we can also imagine effects in the other direction; that is, stricter regulations increasing plant-level productivity. As labor market regulations increase the costs associated with formal employment, plants may also raise the bar for the quality of workers they are willing to hire, given the increased costs, and consequently increase plant-level productivity. Moreover, the expectation of a long-term relationship may increase investments in plant-specific training, which neither the employer nor the worker would be willing to incur if the relationship was short-term. Finally, businesses may switch away from hiring workers and use mechanized technologies to replace workers, which may raise productivity for the remaining workers.

In the absence of direct data on plant-level productivity and profitability, we rely on information on plant-level average wages under the assumption that increases in productivity and profitability will be positively associated with increases in plant-average wages when plants share rents with workers.[37] In Table 5.8, we present coefficients from the estimation of equation (2), where the dependent variable is the plant-level average wage as a proxy for plant-level productivity. Across all plant-types, a depreciation significantly increases within-plant productivity (consistent with studies like Pavcnik (2002)). Moreover, globalization increases productivity and profitability by more at exporting plants relative to plants serving only the domestic market, also consistent with the literature (e.g., Trefler (2004)). We also find a statistically significant and negative impact of increased enforcement on plant-level productivity, as proxied by plant-average wages, suggesting that the first effect pertaining to restricted labor reallocation dominates any potential positive impact of enforcement on firm productivity via increases in investments in training or physical capital.

[37] For instance, in a model like the one in Amiti and Davis (2012), where workers' wages are directly linked to firm profits through a "fair wage" mechanism.

Our focus, however, is on the interaction term, where our predictions are confirmed. Across all plant-types, strict enforcement of labor regulations limits potential within-plant productivity gains (due to the efficient reallocation of workers) associated with trade openness, as proxied by plant-average wages. Regulatory enforcement inhibits productivity gains for both exporting and non-exporting plants.

## 6. Conclusions and Policy Implications

Economists have long debated the effects of trade liberalization on labor market outcomes in developing countries. Early studies have found little impact on plant-level employment changes. We argue a potential explanation relates to how restrictive labor market regulations are in inhibiting the reallocation of workers. In this paper, we revisit the question of the impact of trade liberalization on labor reallocation using data for Brazil. Brazil is an especially interesting case study given the stringency of the *de jure* labor market regulations in the country (see Botero, *et al* (2004)). Furthermore, the topic is also at the forefront of economic policy discussions as the country considers new ways of fostering industrial productivity and of creating a more competitive workforce.[38] Finally, the size and geographic heterogeneity of the country also creates significant variation within the country on the enforcement of the labor law.

We explore the fact that within countries, plants vary in the degree of exposure to global markets and in the incidence of *de facto* labor regulations they face. We use a difference-in-difference methodology to identify the effects of openness on labor turnover, for firms with different degrees of exposure to trade and *de facto* labor regulations. In particular, we analyze the impact of increased exposure to trade, following Brazil's currency crisis in 1999, and discern the impact depending on the plant's exposure to global markets and to the enforcement of labor market regulations based on the plant's municipal location.

We show that, in Brazil, the extent to which trade affects labor market outcomes depends on the *de facto* degree of stringency of the labor regulations faced by plants. Conditional on the (unobservable) time-invariant plant-worker match, and several time-varying worker, plant, and

[38] For example, the Brazilian government has recently launched a program of incentives to promote industrial growth and competitiveness. The program proposes an exemption from a 20% social security levy on worker payrolls for certain sectors. Eligible sectors include automotives, textiles, footwear and plastics (see Financial Times 2012).

sector characteristics, we note that, as is predicted by new heterogeneous firm trade models, trade openness is associated with an increase in the probability of hiring at exporting plants and a decrease in the probability of hiring at domestically-oriented plants. Furthermore, we find that labor inspections largely influence labor adjustment along the extensive margin at small, labor-intensive, non-exporting firms for which labor regulations are likely most binding. This is an especially interesting finding for policymakers, given the current challenge of revamping industrial growth, through a more competitive labor force, in the face of a globalizing world.

In summary, our results strongly suggest that more stringent *de facto* regulations limit job creation with enhanced trade openness. Overall, stricter labor enforcement increases job destruction and decreases job creation at firms most restricted by labor regulations. We also show this increased enforcement is associated with lower productivity gains post-trade reform. Our work therefore may offer an explanation for the mixed results in the literature on the causal impact of exporting on plant-level productivity. From a policy standpoint, our work also suggests that increasing the flexibility of *de jure* labor regulations will allow for increased job creation and thus offer broader access to the gains from trade.

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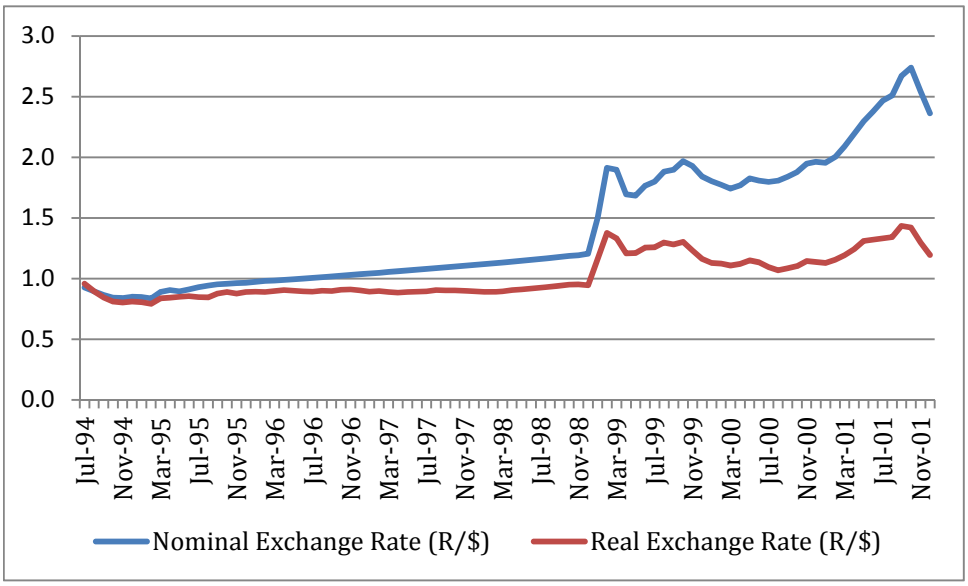
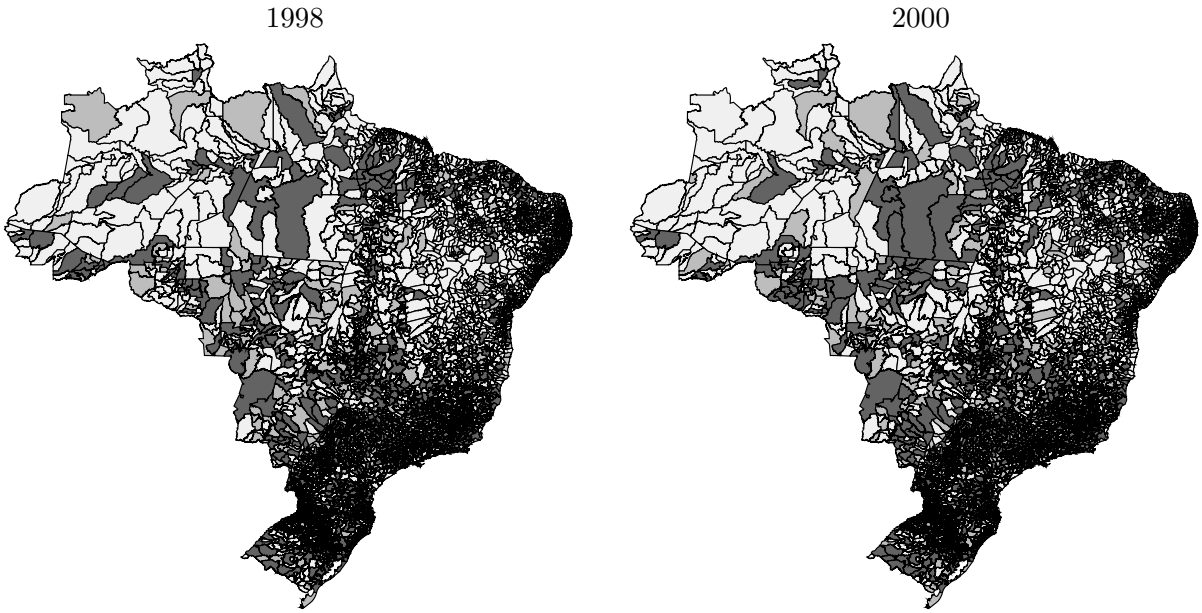
**Figure 3.1: Nominal and Real Exchange Rate Series for Brazil, 1994 – 2001**



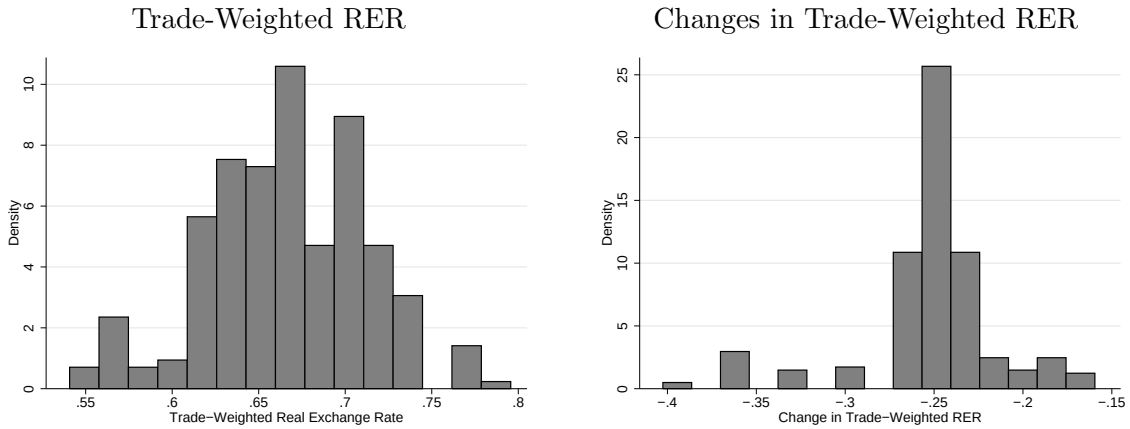

Source: Muendler (2003).

Figure 3.2: Enforcement Intensity by Municipality, 1998 and 2000




Source: Authors' calculations based on administrative data from the Brazilian Ministry of Labor (1996-2001).
Note: This figure reports the number of inspections per Brazilian municipality, with darker shades representing higher numbers of inspections. The map on the left is for the year 1998, while the map on the right is for the year 2000.

Figure 3.3: Industry Variation in Trade-Weighted RER (Levels and Changes), 1999




Source: Authors' calculations based on bilateral real exchange rate data from the IMF and trade flows data from the NBER (1998-1999).
Note: This figure illustrates industry-level heterogeneity in the level of the trade-weighted real exchange rate (left panel) and in the annual change of the trade-weighted real exchange rate (right panel).

**Table 3.1: Enforcement Data, 1996-2001**

| | Share of Cities Inspected | Average Number of Inspections in each City | Average Number of Inspections Per 100 Registered Plants | Average Number of Inspections Per 10,000 Registered Workers |
|---|---|---|---|---|
| 1996 | 0.33 | 13.2 | 2.16 | 14.83 |
| 1997 | 0.44 | 13.0 | 2.19 | 16.90 |
| 1998 | 0.38 | 12.8 | 2.39 | 20.90 |
| 1999 | 0.50 | 13.8 | 2.62 | 21.55 |
| 2000 | 0.43 | 14.8 | 2.68 | 22.91 |
| 2001 | 0.52 | 14.3 | 2.28 | 16.42 |

Source: Authors' calculations based on administrative data from the Brazilian Ministry of Labor (1996-2001).
Note: This table reports different statistics at the city level between 1996 and 2001. Column (1) reports the share of cities that have at least one manufacturing labor inspection. Column (2) reports the average number of labor inspections in each city. Column (3) reports the average number of inspections per 100 registered plants in the city and column (4) reports the average number of labor inspections per 1,000 registered workers ("*com carteira* ").

**Table 3.2: Descriptive Statistics, 1996-2001**

| | All Plants | Exporters | Non-Exporters |
|---|---|---|---|
| | (1) | (2) | (3) |
| *Dependent Variable* | | | |
| *Share of workers:* | | | |
| Hired | 0.29 | 0.24 | 0.34 |
| Fired | 0.25 | 0.22 | 0.28 |
| Temporary Contract | 0.02 | 0.02 | 0.02 |
| *Average:* | | | |
| Hours Per Week | 43.5 | 43.4 | 43.6 |
| *Worker-level Covariates* | | | |
| Age | 32 | 32 | 31 |
| *Share of workers:* | | | |
| Less than High School | 0.68 | 0.62 | 0.74 |
| High School | 0.25 | 0.28 | 0.22 |
| More than High School | 0.07 | 0.10 | 0.03 |
| Unskilled Blue Collar | 0.10 | 0.10 | 0.10 |
| Skilled Blue Collar | 0.66 | 0.64 | 0.68 |
| Other White Collar | 0.07 | 0.07 | 0.07 |
| Professional or Technical | 0.17 | 0.19 | 0.15 |
| *Plant-level Covariates* | | | |
| Employment | 59 | 161 | 27 |
| Average Wage (in logs) | 8.00 | 8.50 | 7.84 |
| Average Match Duration (in years) | 2.57 | 2.73 | 2.40 |
| *Municipality-level Covariates* | | | |
| Inspections | 35.1 | 60.8 | 38.5 |
| *Industry-level Covariates* | | | |
| Trade-weighted RER | 0.62 | 0.62 | 0.62 |
| Employment | 18,328 | 18,755 | 18,912 |
| Unionization Rate | 0.21 | 0.21 | 0.21 |
| Number of Observations | 313,940 | 171,012 | 142,928 |
| Number of Workers | 81,254 | 46,181 | 45,543 |
| Number of Plants | 53,011 | 13,891 | 39,120 |
| Number of Matches | 122,017 | 62,547 | 59,470 |
| Number of Municipalities | 2,769 | 1,466 | 2,578 |
| Number of Industries | 240 | 237 | 236 |

Source: Authors' calculations based on RAIS, Ministry of Labor administrative data on inspections, IMF bilateral exchange rates, NBER trade flows, PNAD, and SECEX (1996-2001).

Note: This table reports descriptive statistics for the main variables used in our empirical work. We report on worker-level variables (averages across workers), plant-level variables (averages across plants), municipality-level variables (averages across municipalities), and industry-level variables (averages across industries). Column (1) reports statistics for all plants in our manufacturing sample. Column (2) reports statistics for the set of exporting plants (defined to be those firms exporting any positive dollar amount at any point during the 1996 to 2001 sample period) and column (3) reports statistics for the group of non-exporters.

**Table 5.1: Trade, Enforcement, and Job Creation**

| Dep. Variable: Match Creation | All | All | Exporters | Non-Exporters |
|---|---|---|---|---|
| TRER*Enforcement | | -0.011 | -0.011 | **0.042*** |
| | | (0.015) | (0.021) | (0.018) |
| Trade-weighted RER | -0.020 | -0.103 | -0.356** | 0.522** |
| | (0.042) | (0.080) | (0.103) | (0.112) |
| Number of Obs. | 300,857 | 300,857 | 165,176 | 135,681 |
| Plant-Year Controls | YES | YES | YES | YES |
| City-Year Controls | YES | YES | YES | YES |
| Sector-Year Controls | YES | YES | YES | YES |
| Worker-Year Controls | YES | YES | YES | YES |
| State-Year Dummies | YES | YES | YES | YES |
| Match Fixed Effects | YES | YES | YES | YES |

Source: Authors' calculations based on RAIS, Ministry of Labor administrative data on inspections, IMF bilateral real exchange rates, NBER trade flows, and SECEX (1996-2001).

Note: This table reports coefficients from the ordinary least squares estimation of equation (3) in the paper, where the dependent variable is an indicator variable which takes the value one if a match between worker $i$ and plant $j$ is created in time $t$, for all plants and by the plant's export status. ** denotes significance at the 1% level; * denotes significance at the 5% level. Robust standard errors, clustered at the city-industry level, are reported in parentheses. All regressions also include the following city controls: log (inspections), log (number of plants), and an interaction between the trade-weighted real exchange rate and log (number of plants). Unreported covariates at the worker level include the worker's age (and age squared), tenure at the plant in months, education (as two dummy variables—at least high school and more than high school where less than high school is the omitted category) and occupation (as three dummy variables—skilled blue collar worker, unskilled white collar worker, and professional/managerial worker where unskilled blue collar worker is the omitted category). At the plant level, we include average plant wages, plant employment, average worker tenure at the plant, and the age, gender, educational, and occupational composition of the plant. We also include the following industry characteristics: the industry unionization rate, industry employment, average worker tenure in the industry, and the age, gender, educational, and occupational composition of the industry.

**Table 5.2: Trade, Enforcement, and Job Destruction**

| Dep. Variable: Match Destruction | All | All | Exporters | Non-Exporters |
|---|---|---|---|---|
| TRER*Enforcement | | -0.005 | 0.003 | **-0.041*** |
| | | (0.012) | (0.016) | (0.017) |
| Trade-weighted RER | 0.027 | 0.114 | 0.194* | -0.124 |
| | (0.036) | (0.065) | (0.083) | (0.094) |
| Number of Obs. | 300,857 | 300,857 | 165,176 | 135,681 |
| Plant-Year Controls | YES | YES | YES | YES |
| City-Year Controls | YES | YES | YES | YES |
| Sector-Year Controls | YES | YES | YES | YES |
| Worker-Year Controls | YES | YES | YES | YES |
| State-Year Dummies | YES | YES | YES | YES |
| Match Fixed Effects | YES | YES | YES | YES |

Source: Authors' calculations based on RAIS, Ministry of Labor administrative data on inspections, IMF bilateral real exchange rates, NBER trade flows, and SECEX (1996-2001).

Note: This table reports coefficients from the ordinary least squares estimation of equation (3) in the paper, where the dependent variable is an indicator variable which takes the value one if a match between worker $i$ and plant $j$ is destroyed in time $t$, for all plants and by the plant's export status. ** denotes significance at the 1% level; * denotes significance at the 5% level. Robust standard errors, clustered at the city-industry level, are reported in parentheses. All regressions also include the following city controls: log (inspections), log (number of plants), and an interaction between the trade-weighted real exchange rate and log (number of plants). Unreported covariates at the worker level include the worker's age (and age squared), tenure at the plant in months, education (as two dummy variables—at least high school and more than high school where less than high school is the omitted category) and occupation (as three dummy variables—skilled blue collar worker, unskilled white collar worker, and professional/managerial worker where unskilled blue collar worker is the omitted category). At the plant level, we include average plant wages, plant employment, average worker tenure at the plant, and the age, gender, educational, and occupational composition of the plant. We also include the following industry characteristics: the industry unionization rate, industry employment, average worker tenure in the industry, and the age, gender, educational, and occupational composition of the industry.

**Table 5.3: Trade, Enforcement, and Hours**

| Dep. Variable: Log (Hours) | All | All | Exporters | Non-Exporters |
|---|---|---|---|---|
| TRER*Enforcement | | 0.004 | 0.000 | 0.007 |
| | | (0.003) | (0.003) | (0.005) |
| Trade-weighted RER | -0.003 | 0.013 | -0.011 | 0.036 |
| | (0.011) | (0.014) | (0.015) | (0.025) |
| Number of Obs. | 300,857 | 300,857 | 165,176 | 135,681 |
| Plant-Year Controls | YES | YES | YES | YES |
| City-Year Controls | YES | YES | YES | YES |
| Sector-Year Controls | YES | YES | YES | YES |
| Worker-Year Controls | YES | YES | YES | YES |
| State-Year Dummies | YES | YES | YES | YES |
| Match Fixed Effects | YES | YES | YES | YES |

Source: Authors' calculations based on RAIS, Ministry of Labor administrative data on inspections, IMF bilateral real exchange rates and NBER trade flows, and SECEX (1996-2001).

Note: This table reports coefficients from the ordinary least squares estimation of equation (3) in the paper, where the dependent variable is the logarithm of hours worked per week for worker i in plant j at time t, for all plants and by the plant's export status. ** denotes significance at the 1% level; * denotes significance at the 5% level. Robust standard errors, clustered at the city-industry level, are reported in parentheses. All regressions also include the following city controls: log (inspections), log (number of plants), and an interaction between the trade-weighted real exchange rate and log (number of plants). Unreported covariates at the worker level include the worker's age (and age squared), tenure at the plant in months, education (as two dummy variables—at least high school and more than high school where less than high school is the omitted category) and occupation (as three dummy variables—skilled blue collar worker, unskilled white collar worker, and professional/managerial worker where unskilled blue collar worker is the omitted category). At the plant level, we include average plant wages, plant employment, average worker tenure at the plant, and the age, gender, educational, and occupational composition of the plant. We also include the following industry characteristics: the industry unionization rate, industry employment, average worker tenure in the industry, and the age, gender, educational, and occupational composition of the industry.

**Table 5.4: Trade, Enforcement, and Contract Type**

| Dep. Variable: Full-Time Contract | All | All | Exporters | Non-Exporters |
|---|---|---|---|---|
| TRER*Enforcement | | 0.003 | -0.002 | 0.009 |
| | | (0.005) | (0.007) | (0.006) |
| Trade-weighted RER | 0.022 | 0.025 | -0.007 | 0.060* |
| | (0.012) | (0.022) | (0.030) | (0.028) |
| Number of Obs. | 300,857 | 300,857 | 165,176 | 135,681 |
| Plant-Year Controls | YES | YES | YES | YES |
| City-Year Controls | YES | YES | YES | YES |
| Sector-Year Controls | YES | YES | YES | YES |
| Worker-Year Controls | YES | YES | YES | YES |
| State-Year Dummies | YES | YES | YES | YES |
| Match Fixed Effects | YES | YES | YES | YES |

Source: Authors' calculations based on RAIS, Ministry of Labor administrative data on inspections, IMF bilateral real exchange rates and NBER trade flows, and SECEX (1996-2001).

Note: This table reports coefficients from the ordinary least squares estimation of equation (3) in the paper, where the dependent variable is an indicator variable if worker i is employed with a full-time contract in plant j at time t, for all plants and by the plant's export status. ** denotes significance at the 1% level; * denotes significance at the 5% level. Robust standard errors, clustered at the city-industry level, are reported in parentheses. All regressions also include the following city controls: log (inspections), log (number of plants), and an interaction between the trade-weighted real exchange rate and log (number of plants). Unreported covariates at the worker level include the worker's age (and age squared), tenure at the plant in months, education (as two dummy variables—at least high school and more than high school where less than high school is the omitted category) and occupation (as three dummy variables—skilled blue collar worker, unskilled white collar worker, and professional/managerial worker where unskilled blue collar worker is the omitted category). At the plant level, we include average plant wages, plant employment, average worker tenure at the plant, and the age, gender, educational, and occupational composition of the plant. We also include the following industry characteristics: the industry unionization rate, industry employment, average worker tenure in the industry, and the age, gender, educational, and occupational composition of the industry.

**Table 5.5: Trade, Enforcement, and Turnover, By Sector Tech-Intensity**

| | Low-Tech Sectors | | | High-Tech Sectors | | |
|---|---|---|---|---|---|---|
| Dep. Variable: Match Creation | All | Exporters | Non-Exporters | All | Exporters | Non-Exporters |
| TRER*Enforcement | 0.015 | 0.020 | **0.042*** | -0.038 | -0.028 | 0.032 |
| | (0.016) | (0.023) | (0.021) | (0.029) | (0.035) | (0.036) |
| Trade-weighted RER | 0.086 | -0.276 | 0.547** | 0.009 | -0.117 | 0.599** |
| | (0.101) | (0.142) | (0.135) | (0.125) | (0.146) | (0.203) |
| Number of Obs. | 208,270 | 102,615 | 105,655 | 92,593 | 62,567 | 30,026 |
| Dep. Variable: Match Destruction | All | Exporters | Non-Exporters | All | Exporters | Non-Exporters |
| TRER*Enforcement | -0.017 | -0.013 | -0.036 | 0.018 | 0.020 | -0.046 |
| | (0.014) | (0.018) | (0.020) | (0.023) | (0.027) | (0.032) |
| Trade-weighted RER | 0.099 | 0.202 | -0.051 | -0.088 | -0.016 | -0.389* |
| | (0.081) | (0.110) | (0.113) | (0.106) | (0.130) | (0.170) |
| Number of Obs. | 208,270 | 102,615 | 105,655 | 92,593 | 62,567 | 30,026 |
| Plant-Year Controls | YES | YES | YES | YES | YES | YES |
| City-Year Controls | YES | YES | YES | YES | YES | YES |
| Sector-Year Controls | YES | YES | YES | YES | YES | YES |
| Worker-Year Controls | YES | YES | YES | YES | YES | YES |
| State-Year Dummies | YES | YES | YES | YES | YES | YES |
| Match Fixed Effects | YES | YES | YES | YES | YES | YES |

Source: Authors' calculations based on RAIS, Ministry of Labor administrative data on inspections, IMF bilateral real exchange rates, NBER trade flows, and SECEX (1996-2001).
Note: This table reports coefficients from the ordinary least squares estimation of equation (3) in the paper, where the dependent variable in the top panel is an indicator variable which takes the value one if a match between worker i and plant j is created in time t and the dependent variable in the bottom panel is an indicator variable equal to one if a match between worker $i$ and plant $j$ is destroyed in time $t$, for all plants, by the plant's export status, and by the sector's tech-intensity. ** denotes significance at the 1% level; * denotes significance at the 5% level. Robust standard errors, clustered at the city-industry level, are reported in parentheses. All regressions also include the following city controls: log (inspections), log (number of plants), and an interaction between the trade-weighted real exchange rate and log (number of plants). Unreported covariates at the worker level include the worker's age (and age squared), tenure at the plant in months, education (as two dummy variables—at least high school and more than high school where less than high school is the omitted category) and occupation (as three dummy variables—skilled blue collar worker, unskilled white collar worker, and professional/managerial worker where unskilled blue collar worker is the omitted category). At the plant level, we include average plant wages, plant employment, average worker tenure at the plant, and the age, gender, educational, and occupational composition of the plant. We also include the following industry characteristics: the industry unionization rate, industry employment, average worker tenure in the industry, and the age, gender, educational, and occupational composition of the industry.

**Table 5.6: Trade, Enforcement, and Turnover, By Plant Size**

| | Small Plants | | | Large Plants | | |
|---|---|---|---|---|---|---|
| Dep. Variable: Match Creation | All | Exporters | Non-Exporters | All | Exporters | Non-Exporters |
| TRER*Enforcement | 0.101** | 0.042 | **0.112**** | -0.007 | -0.014 | 0.033 |
| | (0.032) | (0.139) | (0.033) | (0.016) | (0.021) | (0.021) |
| Trade-weighted RER | 0.695** | 0.368 | 0.775** | -0.107 | -0.366** | 0.532** |
| | (0.193) | (0.786) | (0.201) | (0.084) | (0.103) | (0.129) |
| Number of Obs. | 39,097 | 2,010 | 37,087 | 261,778 | 163,176 | 98,602 |
| Dep. Variable: Match Destruction | All | Exporters | Non-Exporters | All | Exporters | Non-Exporters |
| TRER*Enforcement | -0.066* | -0.038 | **-0.069*** | -0.002 | 0.006 | -0.030 |
| | (0.029) | (0.114) | (0.030) | (0.013) | (0.016) | (0.020) |
| Trade-weighted RER | -0.059 | -0.055 | -0.079 | 0.101 | 0.200* | -0.150 |
| | (0.171) | (0.577) | (0.179) | (0.069) | (0.084) | (0.111) |
| Number of Obs. | 39,097 | 2,010 | 37,087 | 261,778 | 163,176 | 98,602 |
| Plant-Year Controls | YES | YES | YES | YES | YES | YES |
| City-Year Controls | YES | YES | YES | YES | YES | YES |
| Sector-Year Controls | YES | YES | YES | YES | YES | YES |
| Worker-Year Controls | YES | YES | YES | YES | YES | YES |
| State-Year Dummies | YES | YES | YES | YES | YES | YES |
| Match Fixed Effects | YES | YES | YES | YES | YES | YES |

Source: Authors' calculations based on RAIS, Ministry of Labor administrative data on inspections, IMF bilateral real exchange rates, NBER trade flows, and SECEX (1996-2001).
Note: This table reports coefficients from the ordinary least squares estimation of equation (3) in the paper, where the dependent variable in the top panel is an indicator variable which takes the value one if a match between worker i and plant j is created in time t and the dependent variable in the bottom panel is an indicator variable equal to one if a match between worker i and plant j is destroyed in time t, for all plants, by the plant's export status, and by the plant's size. ** denotes significance at the 1% level; * denotes significance at the 5% level. Robust standard errors, clustered at the city-industry level, are reported in parentheses. All regressions also include the following city controls: log (inspections), log (number of plants), and an interaction between the trade-weighted real exchange rate and log (number of plants). Unreported covariates at the worker level include the worker's age (and age squared), tenure at the plant in months, education (as two dummy variables—at least high school and more than high school where less than high school is the omitted category) and occupation (as three dummy variables—skilled blue collar worker, unskilled white collar worker, and professional/managerial worker where unskilled blue collar worker is the omitted category). At the plant level, we include average plant wages, plant employment, average worker tenure at the plant, and the age, gender, educational, and occupational composition of the plant. We also include the following industry characteristics: the industry unionization rate, industry employment, average worker tenure in the industry, and the age, gender, educational, and occupational composition of the industry.

**Table 5.7: Trade, Enforcement, and Turnover, By Worker Age**

| | Young Workers | | | Older Workers | | |
|---|---|---|---|---|---|---|
| Dep. Variable: Match Creation | All | Exporters | Non-Exporters | All | Exporters | Non-Exporters |
| TRER*Enforcement | 0.005 | -0.009 | **0.056*** | -0.005 | 0.003 | 0.048 |
| | (0.020) | (0.028) | (0.025) | (0.017) | (0.021) | (0.025) |
| Trade-weighted RER | -0.058 | -0.422** | 0.477** | -0.009 | -0.230* | 0.753** |
| | (0.110) | (0.147) | (0.155) | (0.089) | (0.103) | (0.144) |
| Number of Obs. | 173,642 | 92,230 | 81,412 | 127,220 | 72,951 | 54,269 |
| Dep. Variable: Match Destruction | All | Exporters | Non-Exporters | All | Exporters | Non-Exporters |
| TRER*Enforcement | -0.018 | -0.007 | **-0.055*** | -0.003 | 0.013 | -0.044 |
| | (0.017) | (0.022) | (0.024) | (0.014) | (0.018) | (0.023) |
| Trade-weighted RER | 0.113 | 0.229 | -0.118 | 0.030 | 0.115 | -0.233 |
| | (0.092) | (0.119) | (0.133) | (0.079) | (0.098) | (0.128) |
| Number of Obs. | 173,642 | 92,230 | 81,412 | 127,220 | 72,951 | 54,269 |
| Plant-Year Controls | YES | YES | YES | YES | YES | YES |
| City-Year Controls | YES | YES | YES | YES | YES | YES |
| Sector-Year Controls | YES | YES | YES | YES | YES | YES |
| Worker-Year Controls | YES | YES | YES | YES | YES | YES |
| State-Year Dummies | YES | YES | YES | YES | YES | YES |
| Match Fixed Effects | YES | YES | YES | YES | YES | YES |

Source: Authors' calculations based on RAIS, Ministry of Labor administrative data on inspections, IMF bilateral real exchange rates, NBER trade flows, and SECEX (1996-2001).
Note: This table reports coefficients from the ordinary least squares estimation of equation (3) in the paper, where the dependent variable in the top panel is an indicator variable which takes the value one if a match between worker i and plant j is created in time t and the dependent variable in the bottom panel is an indicator variable equal to one if a match between worker i and plant j is destroyed in time t, for all plants, by the plant's export status, and by the worker's age. ** denotes significance at the 1% level; * denotes significance at the 5% level. Robust standard errors, clustered at the city-industry level, are reported in parentheses. All regressions also include the following city controls: log (inspections), log (number of plants), and an interaction between the trade-weighted real exchange rate and log (number of plants). Unreported covariates at the worker level include the worker's age (and age squared), tenure at the plant in months, education (as two dummy variables—at least high school and more than high school where less than high school is the omitted category) and occupation (as three dummy variables—skilled blue collar worker, unskilled white collar worker, and professional/managerial worker where unskilled blue collar worker is the omitted category). At the plant level, we include average plant wages, plant employment, average worker tenure at the plant, and the age, gender, educational, and occupational composition of the plant. We also include the following industry characteristics: the industry unionization rate, industry employment, average worker tenure in the industry, and the age, gender, educational, and occupational composition of the industry.

**Table 5.8: Trade, Enforcement, and Plant-Level Wages**

| Dep. Variable: Log (Average Wage) | All | Exporters | Non-Exporters |
|---|---|---|---|
| TRER*Enforcement | 0.032** | 0.020* | 0.032** |
| | (0.006) | (0.010) | (0.007) |
| Trade-weighted RER | -0.123** | -0.170** | -0.153** |
| | (0.036) | (0.060) | (0.043) |
| Number of Obs. | 364,555 | 86,249 | 278,306 |
| Plant-Year Controls | YES | YES | YES |
| City-Year Controls | YES | YES | YES |
| Sector-Year Controls | YES | YES | YES |
| State-Year Dummies | YES | YES | YES |
| Plant Fixed Effects | YES | YES | YES |

real exchange rates and NBER trade flows, and SECEX (1996-2001).

Note: This table reports coefficients from the ordinary least squares estimation of equation (2) in the paper, where the dependent variable is the logarithm of plant-level average wages, for all plants and by the plant's export status. ** denotes significance at the 1% level; * denotes significance at the 5% level. Robust standard errors, clustered at the city-industry level, are reported in parentheses. Unreported covariates at the plant-level include average worker tenure at the plant, the age, gender, educational, and occupational composition of the plant, as well as city-level enforcement, and industry-level unionization rate, industry employment, average worker tenure in the industry, and the age, gender, educational, and occupational composition of the industry.

**Table A.1: Trade, Enforcement, and Plant-Level Employment**

| Dep. Variable: Log (Employment) | | Log (Inspections) | Log (Inspections Per 100 Plants) |
|---|---|---|---|
| TRER*Enforcement | | 0.055** | 0.034* |
| | | (0.005) | (0.014) |
| Trade-weighted RER | -0.119** | -0.430** | -0.171** |
| | (0.044) | (0.049) | (0.048) |
| Number of Obs. | 367,978 | 367,978 | 367,978 |
| Plant-Year Controls | YES | YES | YES |
| City-Year Controls | YES | YES | YES |
| Sector-Year Controls | YES | YES | YES |
| State-Year Dummies | YES | YES | YES |
| Plant Fixed Effects | YES | YES | YES |

Source: Authors' calculations based on RAIS, Ministry of Labor administrative data on inspections, IMF bilateral real exchange rates, and NBER trade flows (1996-2001).

Note: This table reports coefficients from the ordinary least squares estimation of equations (1) and (2) in the paper, where the dependent variable is the logarithm of plant-level employment. In columns (1) and (2), enforcement is measured as the logarithm of the number of inspections in the city (plus one). In column (3), enforcement is measured as the logarithm of the number of inspections in the city (plus one) per 100 plants in the city. ** denotes significance at the 1% level; * denotes significance at the 5% level. Robust standard errors, clustered at the city-industry level, are reported in parentheses. Unreported covariates at the plant-level include average worker tenure at the plant, the age, gender, educational, and occupational composition of the plant, as well as city-level enforcement, and industry-level unionization rate, industry employment, average worker tenure in the industry, and the age, gender, educational, and occupational composition of the industry.