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Trade Liberalization and Regional Dynamics†

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We study the evolution of trade liberalization's effects on Brazilian local labor markets. Regions facing larger tariff cuts experienced prolonged declines in formal sector employment and earnings relative to other regions. The impact of tariff changes on regional earnings 20 years after liberalization was three times the effect after 10 years. These increasing effects on regional earnings are inconsistent with conventional spatial equilibrium models, which predict declining effects due to spatial arbitrage. We investigate potential mechanisms, finding empirical support for a mechanism involving imperfect interregional labor mobility and dynamics in labor demand, driven by slow capital adjustment and agglomeration economies. This mechanism gradually amplifies the effects of liberalization, explaining the slow adjustment path of regional earnings and quantitatively accounting for the magnitude of the long-run effects. (*JEL* F16, J23, J31, J61, O15, O19, R23)

Prominent theories of international trade typically focus on long-run equilibria in which the reallocation of resources across economic activities is achieved without frictions. These models have traditionally given little attention to the adjustment process in transitioning from one equilibrium to another, creating a tension between academic economists advocating trade liberalization and policymakers concerned with the labor market outcomes of workers employed in contracting sectors or firms (Salem and Benedetto 2013; Hollweg et al. 2014). While theory tends to focus on long-run outcomes, empirical studies of the labor market effects of trade liberalization typically emphasize short- or medium-run effects. Frequently changing designs of cross-sectional household surveys forced researchers to focus on relatively short intervals to guarantee consistency over the periods analyzed (Goldberg and Pavcnik 2007). Thus, although many countries underwent major

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trade liberalization episodes throughout the 1980s and 1990s (e.g., Brazil, Mexico, and India, among others), we still know very little about the evolution of the effects of these policy reforms on labor markets.

We fill this gap in the literature by using 25 years of administrative employment data from Brazil to study the dynamics of local labor market adjustment following the country's trade liberalization in the early 1990s. We exploit variation in the tariff declines across industries and variation in the industry mix of local employment across Brazilian regions to measure changes in local labor demand induced by liberalization. We then compare formal employment and earnings growth between regions facing larger and smaller tariff declines, while controlling for preexisting trends in these outcomes.[1] This approach allows us to observe the ensuing regional labor market dynamics for 20 years following the beginning of liberalization.

The results are striking. We find large and steadily increasing effects on regional earnings and employment. Regions facing larger tariff declines experience deteriorating formal labor market outcomes compared to other regions. These effects grow for more than a decade before beginning to level off in the late 2000s. This pattern is robust to a wide variety of alternative measurement strategies, weighting schemes, and controls for preexisting trends across multiple decades. The growing effects are not driven by post-liberalization shocks such as later tariff changes, exchange rate movements, privatization, or the commodity price boom of the 2000s. We conclude that liberalization's effects on regional earnings and employment grew substantially over time.

This pattern challenges the conventional wisdom that labor mobility gradually arbitrages away spatial differences in local labor market outcomes (Blanchard and Katz 1992; Bound and Holzer 2000). If that were the case, one would observe *declining* regional effects of liberalization on earnings, such that the short- and medium-run estimates of trade exposure in prior work would be an upper bound on the long-run effects.[2] Instead, we document *increasing* effects of liberalization; the effect on regional earnings 20 years after the start of liberalization is more than 3 times larger than the effect after 10 years. Liberalization's long-run effects on regional labor market outcomes are therefore much larger than initially supposed.

This surprising finding leads us to evaluate a variety of alternative mechanisms that might account for the growth in liberalization's effects on regional earnings. The evidence rules out mechanisms based on slow urban decline (as in Glaeser and Gyourko 2005), changing worker composition (based on observable or unobservable characteristics), and slow responses of trade quantities to tariff changes. Instead, we find strong evidence for a mechanism involving imperfect interregional labor mobility and dynamics in labor demand, driven by a combination of slow regional capital adjustment and agglomeration economies. Intuitively, as capital slowly reallocates away from harder-hit regions, workers' marginal products steadily fall. Similarly,

[1] In this paper, we focus on formal labor market outcomes, covering workers with a signed work card providing access to the benefits and labor protections afforded by the legal employment system. See Dix-Carneiro and Kovak (2017) for an analysis covering the informal labor market, which includes the self-employed and employees without signed work cards.

[2] Papers documenting short- and medium-run regional effects of trade exposure include Autor, Dorn, and Hanson (2013); Costa, Garred, and Pessoa (2016); Edmonds, Pavcnik, and Topalova (2010); Hakobyan and McLaren (2016); Hasan, Mitra, and Ural (2006); Hasan et al. (2012); Kondo (2017); Kovak (2013); McCaig (2011); Topalova (2010); and many others.

with agglomeration economies, a negative local labor demand shock decreases local economic activity, reducing regional productivity, and further decreasing the marginal product of labor. We find minimal responses of regional working-age population to regional tariff declines, suggesting imperfect worker mobility across regions. In this setting, dynamic labor demand, driven by slow capital adjustment or agglomeration economies, can rationalize the steady relative decline in wages in regions facing larger tariff declines.

We present a wide array of evidence in support of this mechanism. Regions facing larger tariff reductions experience steady declines in the number of formal establishments and declining average establishment size, suggesting that capital stocks slowly reallocate away from negatively affected regions. Capital investment shifts away from these regions on impact, with immediate declines in establishment entry and job creation. In contrast, establishment exit and job destruction increase slowly over time, consistent with firm owners waiting for installed capital to depreciate before contracting or closing down regional establishments. Supporting the presence of agglomeration economies, we show that employment in a given industry $\times$ region pair falls more when other industries in the region face larger tariff cuts. Regional labor market equilibrium would suggest the opposite in the absence of agglomeration economies (Helm 2017). Finally, we extend the specific-factors model of regional economies in Kovak (2013) to incorporate slow factor adjustment and agglomeration economies. Within this framework, we show that a proxy for regional capital adjustment quantitatively accounts for a substantial portion of the long-run earnings effects that we observe. Standard magnitude agglomeration economies and perfect long-run capital mobility quantitatively account for all of the long-run earnings effects. In contrast to the other alternative mechanisms that we considered, this dynamic labor demand mechanism is both qualitatively and quantitatively consistent with the observed earnings responses.

Only recently have researchers begun measuring reallocation costs and the dynamics of labor market adjustment following trade policy reforms. The papers in this literature calibrate or estimate small open economy models in order to study their quantitative implications for welfare and the implied transitional dynamics when facing *hypothetical* changes in trade policy.[3] We contribute to this literature by describing empirical transitional dynamics in response to a *real-world* trade liberalization. We document the importance of dynamic labor demand in the evolution of liberalization's effects on labor markets and suggest that incorporating this mechanism into quantitative models is an important task for future work.

A growing empirical literature finds substantial differences in the effects of trade exposure across local labor markets with different industry structures.[4] Each of these papers measures the effects of trade shocks over a fixed time window of seven to ten years. We contribute to this literature by placing the single-year estimates from prior work into a dynamic context, documenting the evolution of trade liberalization's regional effects over time. This exercise is possible because our data provide complete yearly coverage of the formal labor market, even at fine geographic levels,

[3] Examples include Artuç, Chaudhuri, and McLaren (2010); Caliendo, Dvorkin, and Parro (2015); Coşar (2013); Dix-Carneiro (2014); Kambourov (2009); Traiberman (2016); and many others.

[4] See footnote 2 for citations.

and because Brazilian liberalization represents a discrete shock occurring during a well-defined time period. A similar analysis would be much more challenging when studying shocks that continually evolve over time, such as Chinese export growth, because it is difficult to separate the influence of dynamics from the effects of newly arriving shocks.[5]

Our paper proceeds as follows. Section I describes the history and institutional context of Brazil's early 1990s trade liberalization. Section II describes the data sources, local labor market definition, and empirical approach. Section III presents (i) our main results for liberalization's effects on regional earnings and employment, (ii) a wide array of robustness tests, and (iii) analyses ruling out the influence of post-liberalization shocks. Section IV evaluates potential mechanisms that could account for the growing earnings effects of liberalization. Section V concludes.

I. Trade Liberalization in Brazil

Brazil's trade liberalization in the early 1990s provides an excellent setting in which to study the labor market effects of changes in trade policy. The unilateral trade liberalization involved large declines in average trade barriers and featured substantial variation in tariff cuts across industries. Many papers have examined the labor market effects of trade liberalization in the Brazilian context to take advantage of this variation.[6]

In the late 1980s and early 1990s, Brazil ended nearly 100 years of extremely high trade barriers imposed as part of an import substituting industrialization policy.[7] In 1987, nominal tariffs were high, but the degree of protection actually experienced by a given industry often deviated substantially from the nominal tariff rate due to (i) a variety of non-tariff barriers such as suspended import licenses for many goods and (ii) a system of "special customs regimes" that lowered or removed tariffs for many transactions (Kume, Piani, and de Souza 2003).[8] In 1988 and 1989, in an effort to increase transparency in trade policy, the government reduced tariff redundancy by cutting nominal tariffs and eliminating certain special regimes and trade-related taxes, but there was no effect on the level of protection faced by Brazilian producers (Kume 1990).

Liberalization effectively began in March 1990, when the newly elected administration of President Collor suddenly and unexpectedly abolished the list of suspended import licenses and removed nearly all of the remaining special customs regimes (Kume, Piani, and de Souza 2003). These policies were replaced by a set of import tariffs providing the same protective structure, as measured by the gap

[5] Autor et al. (2014) discuss this point in their study of the effects of Chinese export growth across US industries.

[6] Examples include Arbache, Dickerson, and Green (2004); Goldberg and Pavcnik (2003); Gonzaga, Filho, and Terra (2006); Kovak (2013); Krishna, Poole, and Senses (2014); Menezes-Filho and Muendler (2011); Pavcnik et al. (2004); Paz (2014); Schor (2004); and Hirata and Soares (2016), among many others.

[7] Although Brazil was a founding signatory of the General Agreement on Tariffs and Trade (GATT) in 1947, it maintained high trade barriers through an exemption in Article XVIII Section B, granted to developing countries facing balance of payments problems (Abreu 2004). Hence, trade policy changes during the period under study were unilateral.

[8] These policies were imposed quite extensively. In January 1987, 38 percent of individual tariff lines were subject to suspended import licenses, which effectively banned imports of the goods in question (Authors' calculations from *Bulletin International des Douanes* 6 (11), Supplement 2). In 1987, 74 percent of imports were subject to a special customs regime (de Carvalho 1992).

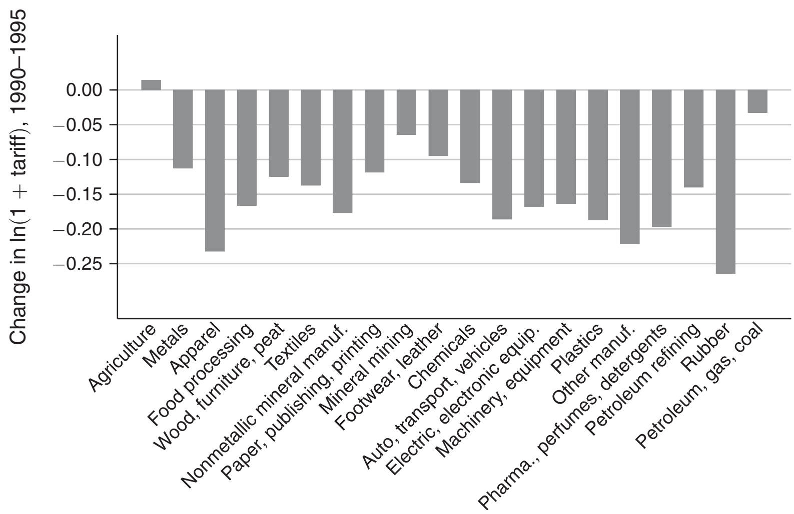


Figure 1. Tariff Changes

Notes: Tariff data from Kume, Piani, and de Souza (2003) are aggregated to allow consistent industry definitions across data sources. See online Appendix Table A1 for details of the industry classification. Industries are sorted based on 1991 national employment (largest on the left and smallest on the right).

between prices internal and external to Brazil, in a process known as tariffication (*tarificação*) (de Carvalho 1992). In some industries, this process required modest tariff increases to account for the lost protection from abolishing import bans.[9] Although these changes did not substantially affect the protective structure, they left tariffs as the main instrument of trade policy, such that tariff levels in 1990 and later provide an accurate measure of protection.

The main phase of trade liberalization occurred between 1990 and 1995, with a gradual reduction in import tariffs culminating with the introduction of Mercosur. Tariffs fell from an average of 30.5 percent to 12.8 percent, and remained relatively stable thereafter.[10] Along with this large average decline came substantial heterogeneity in tariff cuts across industries, with some industries such as agriculture and mining facing small tariff changes, and others such as apparel and rubber facing declines of more than 30 percentage points. We measure liberalization using long-differences in the log of 1 plus the tariff rate from 1990 to 1995, shown in Figure 1. During this time period, tariffs accurately measure the degree of protection faced by Brazilian producers, and tariff changes from 1990 to 1995 reflect the full extent of liberalization faced by each industry. We do not rely on the timing of tariff cuts between 1990 and 1995, because this timing was chosen to maintain support for the liberalization plan, cutting tariffs on intermediate inputs earlier and consumer goods later (Kume, Piani, and de Souza 2003).

As discussed below, along with regional differences in industry mix, the cross-industry variation in tariff cuts provides the identifying variation in our

[9] Online Appendix Figure A1 shows the time series of tariffs. Note the tariff increases in 1990 for the auto and electronic equipment industries.

[10] Simple averages of tariff rates across *Nível 50* industries, as reported in Kume, Piani, and de Souza (2003). See online Appendix A.1 for details on tariff data.

analysis. Following the argument in Goldberg and Pavcnik (2005), we note that the tariff cuts were nearly perfectly correlated with the pre-liberalization tariff levels (correlation coefficient = -0.90). These initial tariff levels reflected a protective structure initially imposed in 1957 (Kume, Piani, and de Souza 2003), decades before liberalization. This feature left little scope for political economy concerns that might otherwise have driven systematic endogeneity of tariff cuts to counterfactual industry performance.

To check for any remaining spurious correlation between tariff cuts and other steadily evolving industry factors, we regress pre-liberalization (1980–1991) changes in industry employment and average monthly earnings on the 1990–1995 tariff reductions, with detailed results reported in online Appendix B.1. We attempted a variety of alternative specifications and emphasize that the results should be interpreted with care, as they include only 20 tradable-industry observations. Most specifications exhibit no statistically significant relationship, but heteroskedasticity-weighted specifications place heavy weight on agriculture and find a positive relationship. Agriculture was initially the least protected industry, and it experienced approximately no tariff reduction. It also had declining wages and employment before liberalization, driving the positive relationship with tariff reductions. Consistent with earlier work, when omitting agriculture, tariff cuts are unrelated to pre-liberalization earnings trends (Krishna, Poole, and Senses 2011). Given these varying results, we include controls for pre-liberalization outcome trends in all of the analyses presented below, to account for any potential spurious correlation. Consistent with the notion that the tariff changes were exogenous in practice, these pretrend controls have little influence on the vast majority of our results.

II. Data and Empirical Approach

A. *Data*

Our main data source for regional labor market outcomes is the Relação Anual de Informações Sociais (RAIS), spanning the period from 1986 to 2010 (Ministério do Trabalho 2017). This is an administrative dataset assembled yearly by the Brazilian Ministry of Labor, providing a high quality census of the Brazilian formal labor market (De Negri et al. 2001; Saboia and Tolipan 1985). Accurate information in RAIS is required for workers to receive payments from several government benefits programs, and firms face fines for failure to report, so both agents have an incentive to provide accurate information. RAIS includes nearly all formally employed workers, meaning those with a signed work card providing them access to the benefits and labor protections afforded by the legal employment system. It omits interns, domestic workers, and other minor employment categories, along with those without signed work cards, including the self-employed.[11] These data have recently been used by Dix-Carneiro (2014); Helpman et al. (2017); Krishna, Poole, and Senses (2014); Lopes de Melo (forthcoming); and Menezes-Filho and Muendler (2011), though these papers utilize shorter panels. The data consist of job records including

[11] See online Appendix B.2 for summary statistics on the informal sector, and Dix-Carneiro and Kovak (2017) for analyses covering the informal labor market.

worker and establishment identifiers, allowing us to track workers and establishments over time. We utilize the establishment's geographic location (municipality) and industry, and worker-level information including gender, age, education (nine categories), and December earnings.[12]

These data have various advantages relative to previous work on the effects of trade on local labor markets. First, relative to Kovak (2013) and Autor, Dorn, and Hanson (2013), we can analyze the dynamics of adjustment to the trade liberalization shock, as RAIS data are available every year. Second, RAIS is a census rather than a sample, so it is representative at fine geographic levels.[13] Third, a rich set of labor market outcomes can be analyzed with such data, including how liberalization affected job creation and job destruction rates, the number of active establishments, and the establishment size distribution. Fourth, the ability to follow workers over time allows us to control for both observable and unobservable worker characteristics.

As is typically the case in administrative employment datasets, the limitation of RAIS is a lack of information on workers who are not formally employed, making it impossible to tell whether a worker is out of the labor force, unemployed, informally employed, or self-employed. This is important in the Brazilian context, with informality rates often exceeding 50 percent of all employed workers during our sample period.[14] When we need information on individuals who are not formally employed, or information before 1986, we supplement the analysis using the decennial Brazilian Demographic Census, covering the period 1970–2010 (IBGE 1970–2010a). While these data provide much smaller samples and do not permit following individuals over time, they cover the entire population, including the informally employed, unemployed, and those outside the labor force.[15] When possible, we corroborate results from RAIS using the Demographic Census, finding very similar results across datasets.

Throughout the analysis, we limit our sample to include working-age individuals, aged 18–64. When studying employed individuals, we omit those working in public administration and those without valid information on industry of employment.[16]

To analyze outcomes by local labor market, we must define the boundaries of each market. We use the "microregion" definition of the Brazilian Statistical Agency (IBGE), which groups together economically integrated contiguous municipalities (counties) with similar geographic and productive characteristics (IBGE 2002), closely paralleling an intuitive notion of a local labor market. When necessary, we combine microregions whose boundaries changed during our sample period, to ensure that we consistently define local labor markets over time. This process leads to a set of 475 consistently identifiable local labor markets for

[12] RAIS reports earnings for December and average monthly earnings during employed months in the reference year. We use December earnings to ensure that our results are not influenced by seasonal variation or month-to-month inflation. See online Appendix A.2 for more detail on the RAIS database.

[13] The National Household Survey (Pesquisa Nacional por Amostra de Domicílios (PNAD)) would be a natural alternative data source for a yearly analysis, but it only provides geographic information at the state level, does not allow one to follow individual workers over time, and provides a much smaller sample.

[14] Authors' calculations using Brazilian Demographic Census.

[15] See online Appendix A.3 for more detail on the Demographic Census data and Dix-Carneiro and Kovak (2017) for analyses covering the informal labor market.

[16] We exclude public administration because the labor market in this field operates quite differently from the rest of the market. This choice has no substantive effect on any of our results.

analyses falling within the period 1986–2010 and 405 markets for analyses using data from 1980 and earlier.[17]

B. *Empirical Approach*

Our empirical analysis follows the literature on the regional effects of trade by comparing the evolution of labor market outcomes in regions facing large tariff declines to those in regions facing smaller tariff declines. Intuitively, regions experience larger declines in labor demand when their most important industries face larger liberalization-induced price declines (Topalova 2007). Kovak (2013) presents a specific-factors model of regional economies that captures this intuition (a generalization of this setup appears in Section IVD). In this model, the regional labor demand shock resulting from liberalization is

$$(1) \qquad \sum_i \beta_{ri} \hat{P}_i, \quad \text{where} \quad \beta_{ri} \equiv \frac{\lambda_{ri} \frac{1}{\varphi_i}}{\sum_j \lambda_{rj} \frac{1}{\varphi_j}},$$

hats represent proportional changes, r indexes regions, i indexes industries, φ_i is the cost share of nonlabor factors, and λ_{ri} is the share of regional labor initially allocated to tradable industry i. The variable $\hat{P}_i$ is the liberalization-induced price change facing industry i, and (1) is a weighted average of these price changes across tradable industries, with more weight on industries capturing larger shares of initial regional employment.[18] Thus, although all regions face the same vector of liberalization-induced price changes, differences in the regional industry mix generate regional variation in labor demand shocks.

We operationalize this shock measure by defining the "regional tariff reduction" (*RTR*), which utilizes only liberalization-induced variation in prices, replacing $\hat{P}_i$ with the change in log of 1 plus the tariff rate:

$$(2) \qquad RTR_r = -\sum_i \beta_{ri}\, d \ln(1 + \tau_i).$$

Here, τ_i is the tariff rate in industry i, and d represents the long difference from 1990–1995, the period of Brazilian trade liberalization. We calculate tariff changes using data from Kume, Piani, and de Souza (2003), λ_{ri} using the 1991 Census,

[17] This geographic classification is a slightly aggregated version of the one in Kovak (2013), accounting for additional boundary changes during the longer sample period. Related papers define local markets based on commuting patterns (e.g., Autor, Dorn, and Hanson 2013). Our local market definition performs well based on this standard as well—only 3.4 and 4.6 percent of individuals lived and worked in different markets in 2000 and 2010, respectively. The main regional definition is shown in Figure 2. The analysis omits 11 microregions, shown with a cross-hatched pattern in the figure. These include (i) Manaus, which was part of a Free Trade Area and hence not subject to tariff cuts during liberalization; (ii) the microregions that constitute the state of Tocantins, which was created in 1988 and hence not consistently identifiable throughout our sample period; and (iii) a few other municipalities that are omitted from RAIS in the 1980s. The inclusion or exclusion of these regions when possible has no substantive effect on the results. We also implemented the main analyses using a more aggregate local labor market definition, "mesoregions" defined by IBGE, and results are nearly identical.

[18] Following Kovak (2013), we drop the nontradable sector, based on the assumption that nontradable prices move with tradable prices. We confirm this assumption by calculating a measure of local nontradables prices in Section IIIA.

and φ_i using 1990 National Accounts data from IBGE.[19] Together, these allow us to calculate the weights, β_{ri}. Note that RTR_r is more positive in regions facing larger tariff *reductions*, which simplifies the interpretation of our results, since nearly all regions faced tariff declines during liberalization.

Figure 2 maps the spatial variation in RTR_r. Regions facing larger tariff reductions are presented as lighter, while regions facing smaller cuts are shown as darker. The region at the tenth percentile faced a tariff reduction of 0.2 percentage points, while the region at the ninetieth percentile faced a 10.7 percentage point decline. Hence, in interpreting the regression estimates below, we compare regions whose values of RTR_r differ by 10 percentage points, closely approximating the 90–10 gap of 10.5 percentage points. Note that there is substantial variation in the tariff shocks even among local labor markets within the same state. As we include state fixed effects in our analyses, these within-state differences provide the identifying variation in our study.[20]

We use the following specification to compare the evolution of labor market outcomes in regions facing large tariff reductions to those in regions facing smaller tariff declines:

$$(3) \qquad y_{rt} - y_{r,1991} = \theta_t RTR_r + \alpha_{st} + \gamma_t(y_{r,1990} - y_{r,1986}) + \epsilon_{rt}.$$

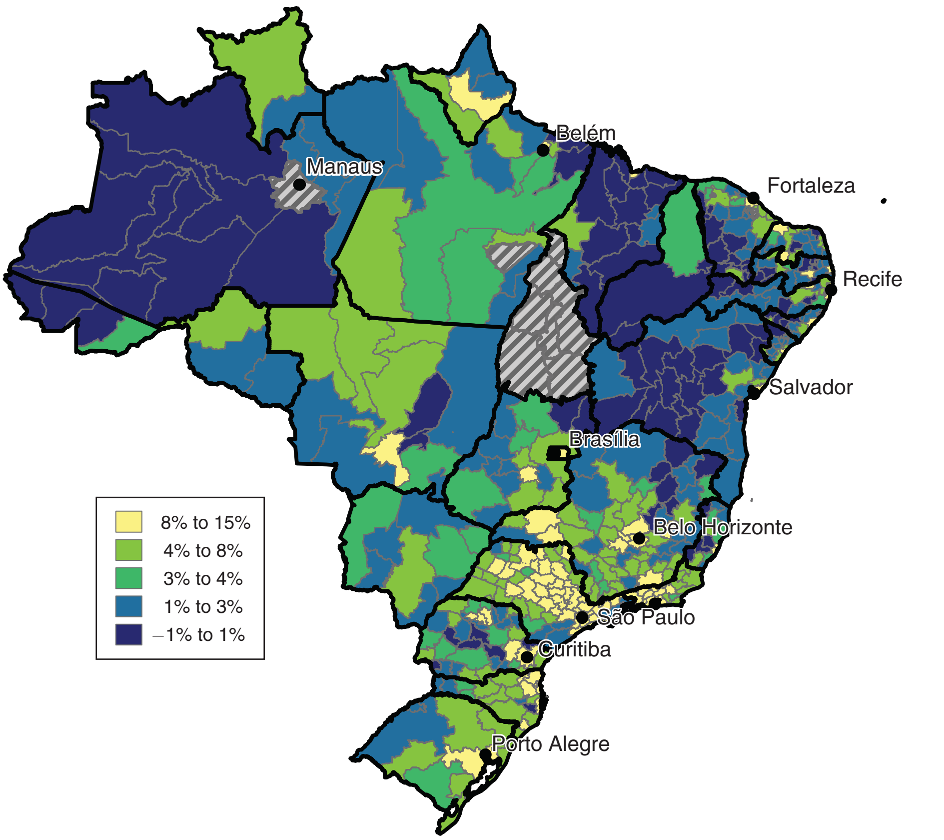
We estimate this equation separately for each year $t \in [1992, 2010]$, as reflected by the t subscripts. The variable y_{rt} is the value of a regional outcome such as earnings or employment, θ_t is the cumulative effect of liberalization on outcomes by year t, α_{st} are state fixed effects (allowed to differ across years), and $(y_{r,1990} - y_{r,1986})$ is a pre-liberalization trend in the outcome variable. While the change in outcome varies with the year t under consideration, the liberalization shock, RTR_r, does not. Instead, it always reflects the regional measure of tariff reductions *during liberalization*, from 1990 to 1995. Using this strategy, each year's θ_t represents one point on the empirical impulse response function describing the cumulative local effects of liberalization as of each post-liberalization year. This methodology captures only relative effects across regions, as does the rest of the literature examining the regional or sectoral effects of trade.

We use 1991 as the base year for outcome changes, and include state fixed effects to account for any state-specific policies that might commonly affect outcomes for all regions in the same state, such as state-specific minimum wages, introduced in 2002 (Neri and Moura 2006).[21] We control for pre-liberalization changes in outcomes $(y_{r,1990} - y_{r,1986})$ to address the possibility of confounding preexisting trends, and consider longer pre-liberalization trends as a robustness test. For our

[19] See online Appendix A.4 for more detail on the construction of (2). We use the Census to calculate λ_{ri} because the Census allows for a more detailed industry definition than what is available in RAIS (see online Appendix A.1) and because the Census allows us to calculate weights that are representative of overall employment, rather than just formal employment. That said, shocks using formal employment weights yield very similar results (online Appendix Table B6, panel D).

[20] A regression of RTR_r on state fixed effects yields an R^2 of 0.36: i.e., 64 percent of the variation in RTR_r is not explained by state effects. Our main conclusions are unaffected by the inclusion or exclusion of state fixed effects.

[21] Using 1991 as the base year allows us to take advantage of more detailed industry information in the 1991 Census when calculating the industry distribution of regional employment (λ_{ri}), and makes our results comparable with Kovak (2013).



	Percentile				
Mean	10	25	50	75	90
0.044	0.002	0.012	0.031	0.066	0.107

FIGURE 2. REGIONAL TARIFF REDUCTIONS

Notes: Local labor markets reflect microregions defined by IBGE, aggregated slightly to account for border changes between 1986 and 2010. Regions are colored based on the regional tariff reduction measure, RTR_r, defined in (2). Regions facing larger tariff reductions are presented as lighter, while regions facing smaller cuts are shown as darker. Dark lines represent state borders, gray lines represent consistent microregion borders, and cross-hatched microregions are omitted from the analysis. These microregions were either (i) part of a Free Trade Area; (ii) part of the state of Tocantins and not consistently identifiable over time; or (iii) not included in the RAIS sample before 1990.

main outcomes, we present results with and without state fixed effects and pretrends, with little effect on the coefficients of interest. Since many of our dependent variables are themselves estimates, we weight regressions based on the inverse of their standard error to account for heteroskedasticity. We also cluster standard errors at the mesoregion level to account for potential spatial correlation in outcomes across neighboring regions.

To consistently estimate θ_t, ϵ_{rt} must be uncorrelated with RTR_r, conditional on the state fixed effects and outcome pretrend. For this identification assumption to be violated, there would need to be an omitted variable that (i) drives wage or employment growth across regions within a state and (ii) is correlated with RTR_r but (iii) is *not* captured by pre-liberalization outcome trends. While such a feature is unlikely to exist, in Section IIIB we confirm that our results are robust to a wide variety of potential confounders and alternative specifications.

Our empirical approach is similar to prior studies examining the local effects of trade liberalization, but we make two important contributions to that literature. First, the RAIS data allow us to calculate changes in regional outcomes in each year following liberalization. We trace out the dynamic regional response to liberalization as it evolves over time, rather than observing liberalization's local effect in only one post-shock period, as in the prior literature (e.g., Topalova 2007; Autor, Dorn, and Hanson 2013; or Kovak 2013). The RAIS data also allow us to control for pre-liberalization trends that might otherwise confound the analysis. Second, we study a discrete, well-defined trade policy shock that was complete by 1995. This contrasts with Autor et al. (2014), who use US panel data to study the effects of growing trade with China. They emphasize that the continuously evolving nature of Chinese trade confounds their ability to study the dynamic response to a trade shock at any given point in time.

III. Results

A. *Main Findings*

We begin by examining the effects of liberalization on formal sector earnings and employment in local labor markets. First, we calculate "regional earnings premia," which reflect average log monthly earnings for workers in a given region, controlling for the composition of the regional workforce.[22] For each year t, we regress log December earnings for worker j on flexible controls for age, sex, and education (X_{jt}); industry fixed effects (ϕ_{it}); and region fixed effects (μ_{rt}):[23]

$$\ln(earn_{jrit}) = X_{jt}\Gamma_t + \phi_{it} + \mu_{rt} + e_{jrit}. \tag{4}$$

We estimate this equation separately for each year $t \in [1991, 2010]$, allowing the regression coefficients (Γ_t) and fixed effects (ϕ_{it} and μ_{rt}) to differ across years. The region fixed effect estimates from these regressions, $\hat{\mu}_{rt}$, represent the regional log earnings premia for the relevant year. By estimating these regressions separately in each year, we allow for changes in the regional composition of workers (X) and changes in the returns to worker characteristics (Γ) over time.[24] This approach ensures that our earnings estimates are not driven by changes in observable worker composition, changing discrimination, changes in the returns to schooling, or any other changes in the returns to observable characteristics that operate at the national level. Our dependent variable when studying earnings is then the change in regional log earnings premium from 1991 to each subsequent year, 1992 to 2010. Table 1

[22] Estimating the regional earnings premia for each year separately from the effects of liberalization on regional earnings reduces the computational demands relative to pooling across years and estimating both steps jointly.

[23] We use monthly earnings rather than hourly wages because RAIS only provides hours from 1994 onward. Census results using hourly wages are similar.

[24] Online Appendix B.3 presents the coefficient estimates from (4) for 1991, 2000, and 2010. In Section IVB, we control for observable *and unobservable* worker heterogeneity by pooling across years and including individual fixed effects. The results are very similar.

Table 1—Descriptive Statistics

	1991–1995	1991–2000	1991–2005	1991–2010	
Panel A. Liberalization shock					
Regional tariff reductions (RTR_r)	0.044				
	(0.039)				
Panel B. Main outcome variables					
Change in log formal earnings premium	0.258	0.305	0.401	0.712	
	(0.161)	(0.174)	(0.189)	(0.201)	
Change in log informal earnings premium[a]		−0.050		0.161	
		(0.135)		(0.197)	
Change in log formal employment	0.268	0.599	0.976	1.308	
	(0.377)	(0.549)	(0.576)	(0.614)	
Change in log informal employment[a]		0.269		0.291	
		(0.162)		(0.228)	
Change in log num. formal establishments	0.358	0.728	1.055	1.271	
	(0.230)	(0.318)	(0.389)	(0.444)	
Change in log avg. formal establishment size	−0.180	−0.260	−0.220	−0.128	
	(0.279)	(0.387)	(0.375)	(0.384)	
Change in log formal job destruction	−1.014	−0.824	−0.966	−1.135	
	(0.398)	(0.421)	(0.466)	(0.516)	
Change in log formal job creation	−0.608	−0.099	0.143	0.299	
	(0.387)	(0.270)	(0.221)	(0.176)	
Change in log formal establishment exit	−1.206	−1.092	−1.183	−1.305	
	(0.226)	(0.285)	(0.343)	(0.405)	
Change in log formal establishment entry	−0.397	0.052	0.251	0.366	
	(0.287)	(0.175)	(0.139)	(0.114)	
Change in log working-age population[a]		0.198		0.388	
		(0.103)		(0.178)	
Panel C. Region characteristics	1991	1995	2000	2005	2010
Average formal earnings (2010 R$)	755.98	1,105.83	944.18	939.93	1,152.40
	(273.08)	(394.71)	(323.97)	(480.00)	(469.95)
Formal employment	30,466	34,929	40,100	51,631	70,170
	(152,267)	(161,657)	(163,917)	(197,206)	(269,602)
Share agriculture/mining[a]	0.399				
	(0.194)				
Share manufacturing[a]	0.113				
	(0.077)				

Notes: 475 microregion observations. See the text for descriptions of all measures.

[a]Calculated using the Census, so estimates are available only for 1991, 2000, and 2010.

presents summary statistics for this and other main dependent variables throughout the paper.

Table 2 shows the results of estimating (3) for regional formal sector log earnings premia and formal log employment. All estimates for the coefficient on RTR_r are negative, indicating that regions facing larger tariff reductions experience relative declines in earnings or employment. Consider panel A, which presents liberalization's effect on regional earnings. Columns 1–3 examine changes in earnings from 1991 to 2000, while columns 4–6 examine changes from 1991 to 2010, such that the effects cumulate over time. Columns 2 and 4 add state fixed effects, and columns 3 and 6 add pretrend controls for the change in the regional outcome from 1986 to 1990. The coefficient estimate of −0.529 in column 3 indicates that a region

Table 2—Regional log Formal Earnings Premia and Employment, 2000 and 2010

Change in outcome:	1991–2000			1991–2010		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. log formal earnings premia						
Regional tariff reduction (RTR_r)	−0.451	−0.638	−0.529	−1.885	−1.736	−1.594
	(0.152)	(0.154)	(0.141)	(0.316)	(0.184)	(0.169)
Formal earnings pretrend, 1986–1990			−0.312			−0.418
			(0.149)			(0.144)
State fixed effects (26)		✓	✓		✓	✓
R^2	0.040	0.225	0.268	0.320	0.501	0.537
Panel B. log formal real earnings premia: regional deflators following Moretti (2013)						
Regional tariff reduction (RTR_r)				−1.594	−1.382	−1.260
				(0.306)	(0.180)	(0.168)
Formal earnings pretrend, 1986–1990						−0.359
						(0.133)
State fixed effects (26)					✓	✓
R^2				0.238	0.449	0.477
Panel C. log formal employment						
Regional tariff reduction (RTR_r)	−3.748	−3.545	−3.533	−6.059	−4.675	−4.663
	(0.516)	(0.563)	(0.582)	(0.560)	(0.660)	(0.679)
Formal employment pretrend, 1986–1990			−0.033			−0.032
			(0.147)			(0.156)
State fixed effects (26)		✓	✓		✓	✓
R^2	0.072	0.291	0.291	0.149	0.409	0.410

Notes: Negative coefficient estimates for the regional tariff reduction imply larger declines in formal earnings or employment in regions facing larger tariff reductions. Microregion observations: panels A and C, 475; panel B, 456 (omits a few sparsely populated locations with insufficient data to calculate regional price deflators). Regional earnings premia calculated controlling for age, sex, education, and industry of employment. Panels A and B: efficiency weighted by the inverse of the squared standard error of the estimated change in log formal earnings premium. Pretrends computed for 1986–1990. Standard errors (in parentheses) adjusted for 112 mesoregion clusters.

facing a 10 percentage point larger tariff reduction (approximately the 90–10 gap in RTR_r) experienced a 5.29 percentage point larger proportional decline (or smaller increase) in formal earnings from 1991 to 2000. This magnitude is similar in size to the corresponding estimate in Kovak (2013) (-0.439), which used a different data source (Census of Population) and covers all workers rather than restricting attention to the formally employed. The estimate of -1.594 in column 6 indicates that the gap in earnings growth expanded to 15.94 percentage points by 2010.[25]

This increase in liberalization's effect on earnings from 2000 to 2010 is a striking feature of Table 2. It indicates that the divergent earnings growth in regions facing different tariff reductions continued well beyond the liberalization period. Figure 3 confirms this pattern by plotting the coefficients on RTR_r (θ_t) for each year. The points for 2000 and 2010 correspond to the RTR_r coefficients in columns 3 and 6 of Table 2. The vertical lines indicate that liberalization began in 1991 and was complete by 1995. We present coefficient estimates for the period 1992–1994, but

[25] Online Appendix B.4 presents an alternative research design at the industry × region level finding similar growth in the regional earnings effects of liberalization and confirming the importance of cross-industry regional equilibrium in driving the main earnings effects discussed here.

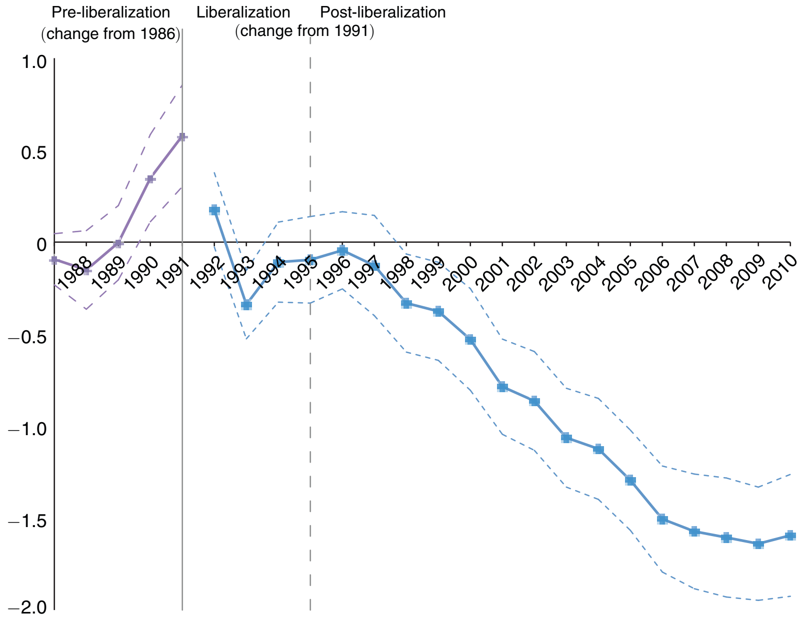


FIGURE 3. REGIONAL LOG FORMAL EARNINGS PREMIA, 1987–2010

Notes: Each point reflects an individual regression coefficient, $\hat{\theta}_t$, following (3), where the dependent variable is the change in regional log formal earnings premium and the independent variable is the regional tariff reduction (RTR_r), defined in (2). Note that RTR_r always reflects tariff reductions from 1990 to 1995. For circles, the earnings changes are from 1991 to the year listed on the x-axis. For diamonds, the changes are from 1986 to the year listed. All regressions include state fixed effects, and post-liberalization regressions control for the 1986–1990 outcome pretrend. Negative estimates imply larger earnings declines in regions facing larger tariff reductions. Vertical bars indicate that liberalization began in 1991 and was complete by 1995. Dashed lines show 95 percent confidence intervals. Standard errors adjusted for 112 mesoregion clusters.

these should be interpreted with care, as liberalization was still ongoing.[26] The local earnings effects of liberalization appear just after liberalization and steadily grow for more than a decade, before leveling off in the late 2000s, a pattern that is very robust to details of the specification.[27] Figure 3 also shows pre-liberalization coefficients, in which the dependent variable is the change in regional earnings premium from 1986 to the year listed on the x-axis, and the independent variable is RTR_r. If anything, the relative earnings declines in regions facing larger tariff reductions represent a reversal of the pre-liberalization trend. Recall that all post-liberalization results control for pre-liberalization trends, as shown in (3).

It is likely that the prices of local nontradable goods change in response to the regional shocks to the prices of traded goods (Kovak 2013; Monte 2016). If this is the case, the relative decline in nominal earnings in regions facing larger tariff reductions may be partly offset by declines in the local price index. To empirically

[26] However, the tariff cuts were almost fully implemented by 1993, so these early coefficients are still informative regarding liberalization's short-run effects. When regressing RTR_r on an alternate version measuring tariff changes from 1990 to 1993, the R^2 is 0.93.

[27] See Section IIIB for a variety of robustness tests. Online Appendix B.5 shows the underlying scatter-plots, confirming our choice of linear estimating equation and showing that the results are not driven by outliers. Online Appendix B.6 shows that the same pattern appears when estimating formal earnings or formal hourly wages using Census data.

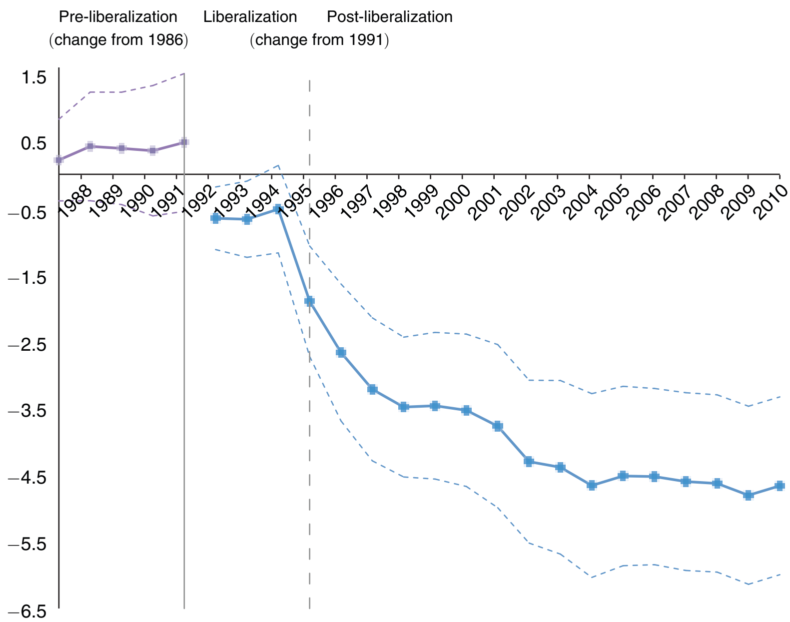


Figure 4. Regional log Formal Employment, 1987–2010

Notes: Each point reflects an individual regression coefficient, $\hat{\theta}_t$, following (3), where the dependent variable is the change in regional log formal earnings premium and the independent variable is the regional tariff reduction (RTR_r), defined in (2). Note that RTR_r always reflects tariff reductions from 1990 to 1995. For circles, the earnings changes are from 1991 to the year listed on the x-axis. For diamonds, the changes are from 1986 to the year listed. All regressions include state fixed effects, and post-liberalization regressions control for the 1986–1990 outcome pretrend. Negative estimates imply larger earnings declines in regions facing larger tariff reductions. Vertical bars indicate that liberalization began in 1991 and was complete by 1995. Dashed lines show 95 percent confidence intervals. Standard errors adjusted for 112 mesoregion clusters.

evaluate this possibility, we construct local price indexes using housing rents information in the Census, following the approach of Moretti (2013).[28] Only the 1991 and 2010 Censuses included rent questions, so we can only calculate the change in rental prices for the period 1991–2010. Our local price index uses consumption weights from the Brazilian Consumer Price Index system (IPC) and accounts for the fact that the prices of non-housing nontradables tend to move with housing prices. See online Appendix A.5 for details on constructing the index. We then calculate the change in log real earnings as the change in log nominal earnings minus the change in log local price level. Panel B of Table 2 shows the effect of regional tariff reductions on the change in *real* regional earnings for 1991–2010. The effect on real earnings in column 6 is smaller than the effect on nominal earnings by about 21 percent. This difference confirms that regional nontradable prices move with tradable prices, falling more in places facing larger tariff reductions. However, the long-run effects of liberalization on real regional earnings are still large and statistically significant.

Panel C of Table 2 and Figure 4 both examine liberalization's effects on regional log formal employment. The year 2000 estimate of −3.533 shows that a region

[28] As in the United States, the Brazilian government does not produce local price indexes outside of a few large cities.

Table 3—Regional log Working-Age Population, 2000 and 2010

Change in log working-age population:	1991–2000			1991–2010		
	(1)	(2)	(3)	(4)	(5)	(6)
Regional tariff reduction (RTR_r)	0.333	−0.061	0.018	0.392	−0.175	−0.059
	(0.243)	(0.330)	(0.204)	(0.319)	(0.473)	(0.294)
Population pretrend, 1980–1991	0.406		0.328	0.632		0.531
	(0.164)		(0.171)	(0.225)		(0.235)
Population pretrend, 1970–1980		0.297	0.137		0.445	0.190
		(0.072)	(0.047)		(0.087)	(0.073)
State fixed effects (26)	✓	✓	✓	✓	✓	✓
R^2	0.654	0.557	0.678	0.666	0.554	0.685

Notes: Positive (negative) coefficient estimates for the regional tariff reduction imply larger increases (decreases) in population in regions facing larger tariff reductions. Outcomes calculated using Census data. There are 405 microregion observations. Efficiency weighted by the inverse of the squared standard error of the dependent variable estimate. Pretrends computed for 1980–1991 and 1970–1980. Standard errors (in parentheses) adjusted for 90 mesoregion clusters.

facing a 10 percentage point larger tariff reduction experienced a 35.3 percentage point larger proportional decline (smaller increase) in formal employment from 1991 to 2000. As with earnings, the employment effect grew substantially from 2000 to 2010, indicating that employment growth continued to diverge for regions facing different regional tariff changes. Most of this divergence was complete by 2004, after which the estimates level off.[29]

Note that since Table 2 and Figure 4 examine *formal* employment, there are two channels through which formal employment might decline in regions facing more negative shocks. Formally employed workers may migrate away from negatively affected places to more favorably affected places, or existing residents of the region may shift out of formal employment and into nonemployment or informal employment. Table 3 rules out the interregional migration mechanism, showing that a region's working-age population did not respond to RTR_r.[30] We measure working-age population using Census data, so we can observe individuals outside formal employment, and control for 1980–1991 and 1970–1980 population pretrends. None of the population estimates are significantly different from zero, and the point estimates with extensive pretrend controls (columns 3 and 6) are quantitatively small. These results suggest that workers losing formal employment in harder hit regions did not leave the region, but transitioned out of formal employment.

Using Census data on informal workers, panel A of Table 4 confirms that in regions facing larger tariff reductions informal employment increased relative to

[29] To assess the scale of our long-run estimates, consider Dix-Carneiro (2014), which studies a very similar setting with slow adjustment of labor across Brazilian industries rather than regions. After estimating the model's parameters using RAIS data, he simulates the economy's response to a price shock when capital is mobile across industries (see Dix-Carneiro 2014, Figures 4 and 6). The long-run wage elasticity in the adversely affected sector (High-Tech Manufacturing) is −1.56. This is exceedingly close to our 2010 earnings estimate of −1.594. The long-run employment elasticity in Dix-Carneiro (2014) is −3.2. Although this is somewhat smaller than our 2010 employment estimate of −4.663, the two effects are similar in magnitude, suggesting that our findings are reasonable in the context of this type of model.

[30] Similarly, Autor, Dorn, and Hanson (2013) find little evidence for population responses to trade shocks in the United States.

Table 4—Regional log Informal Employment and Earnings Premia, 2000 and 2010

Change in outcome:	1991–2000			1991–2010		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. log informal employment						
Regional tariff reduction (RTR_r)	2.017	1.706	1.593	2.122	1.448	1.196
	(0.431)	(0.344)	(0.532)	(0.468)	(0.491)	(0.705)
Informal employment pretrend (1980–1991)	0.069		0.050	0.149		0.109
	(0.115)		(0.114)	(0.132)		(0.126)
All employment pretrend, 1970–1980		0.121	0.110		0.263	0.239
		(0.056)	(0.044)		(0.080)	(0.063)
State fixed effects (26)	✓	✓	✓	✓	✓	✓
R^2	0.579	0.589	0.592	0.524	0.552	0.562
Panel B. log informal earnings premia						
Regional tariff reduction (RTR_r)	−0.027	−0.217	−0.034	0.352	0.054	0.338
	(0.161)	(0.160)	(0.163)	(0.256)	(0.298)	(0.251)
Informal earnings pretrend, 1980–1991	−0.191		−0.193	−0.288		−0.291
	(0.049)		(0.048)	(0.086)		(0.084)
All workers' earnings pretrend, 1970–1980		0.008	−0.016		0.001	−0.035
		(0.064)	(0.060)		(0.109)	(0.102)
State fixed effects (26)	✓	✓	✓	✓	✓	✓
R^2	0.676	0.654	0.676	0.690	0.667	0.690

Notes: Positive (negative) coefficient estimates for the regional tariff reduction imply larger increases (declines) in informal earnings or employment in regions facing larger tariff reductions. Outcomes calculated using Census data. There are 405 microregion observations. Regional earnings premia calculated controlling for age, sex, education, and industry of employment. Efficiency weighted by the inverse of the squared standard error of the dependent variable estimate. Pretrends computed for 1980–1991 and 1970–1980. Standard errors (in parentheses) adjusted for 112 mesoregion clusters.

the national average. For example, the estimate 1.196 in column 6 implies that on average a region facing a 10 percentage point larger tariff reduction experienced an 11.96 percentage point larger increase in informal employment by 2010. Rather than migrating away, many workers who lose formal employment in negatively affected regions appear to transition into the informal sector in the same region.[31] Panel B of Table 4 implements a similar exercise for regional earnings premia in the informal sector. Somewhat unexpectedly, there is no significant relationship between regional tariff reductions and earnings in the informal sector. A potential explanation for the lack of effect on informal wages is that consumers in harder-hit regions experience declining incomes and shift toward lower-priced, lower-quality goods produced in the informal sector, offsetting wage declines for informally employed workers.[32]

[31] See Dix-Carneiro and Kovak (2017) for a more extensive discussion of the various margins of labor market adjustment following Brazilian liberalization.

[32] Burstein, Eichenbaum, and Rebelo (2005) show that lower quality goods gain market share in recessions, while McKenzie and Schargrodsky (2011) make a similar argument in the context of the 2002 economic crisis in Argentina. While there is little direct evidence on the relative quality of goods produced by formal and informal firms, it is well known that informal firms are significantly smaller than formal firms (LaPorta and Schleifer 2014; Meghir, Narita, and Robin 2015; Ulyssea 2017), and Kugler and Verhoogen (2012) show that larger firms produce higher quality goods than small firms, on average. Moreover, LaPorta and Schleifer (2008) show that informal firms use lower quality inputs and speculate that they produce lower quality outputs as a result.

Together, these results are quite surprising, particularly compared to the conventional wisdom from the literature studying local labor demand shocks. The standard framework predicts initially large wage effects of local labor demand shocks, as labor supply is approximately fixed in the very short run, after which employment adjustment arbitrages away spatial wage differences, and observed wage effects fall in magnitude (Blanchard and Katz 1992; Bound and Holzer 2000). This mechanism is consistent with the steadily growing employment effects in Figure 4, but is at odds with the growing earnings effects in Figure 3. It predicts large negative coefficients shortly after liberalization, but then declining magnitude effects as arbitrage partly equalizes earnings growth across regions. Even in the absence of equalizing migration, as shown in Table 3, one would expect constant effects over time. Instead, we find continuing divergence in earnings growth for 14 years following the end of liberalization, with earnings growth in regions facing larger tariff reductions lagging further and further behind other regions. This pattern means that the local labor market effects of trade estimated in prior work for a single post-liberalization year actually *understate* the longer run effects. The remainder of the paper focuses on examining and explaining this surprising result.

B. *Robustness*

We first establish that the steadily growing earnings effects are robust to alternative measurement and specification choices and that they were not driven by confounding effects from other shocks to Brazilian local labor markets. Detailed analyses appear in online Appendix Sections B.7–B.9, and we summarize the results here.

Online Appendix B.7 shows that the growing earnings estimates are robust to alternative pretrend controls, RTR_r shock measures, earnings premium measures, and weighting. We use Census data to construct longer pre-liberalization earnings trends, from 1970–1980 and from 1980–1991, and control for these alongside the 1986–1990 RAIS pre-liberalization trends present in our main specification.[33] We construct alternative RTR_r measures (i) using industry weights, λ_{ri}, reflecting only formal employment; (ii) using effective rates of protection, which account for the effects of tariffs on inputs and outputs for each industry; and (iii) including a zero price change for the nontradable sector. We also construct alternative earnings premium measures. The first is calculated without controlling for industry fixed effects, maintaining national industry-level earnings variation in the regional earnings premia.[34] The second measure simply uses mean log earnings, without controlling for any worker characteristics. Finally, we present results weighting regions equally or weighting by the region's 1991 formal employment. In all cases, our main results

[33] Because 1991 is the base year for our post-liberalization earnings growth outcome, 1980–1991 pre-liberalization trends are subject to mechanical endogeneity. We resolve this problem by calculating an alternative earnings growth measure with 1992 as the base year. See panel C of Table B6.

[34] By omitting the industry fixed effects, these regional earnings measures include both direct industry effects and local general equilibrium effects. As shown in online Appendix B.7, the associated estimates are only a bit larger than the main results, indicating that local equilibrium effects account for the majority of the overall effects of liberalization on regional earnings.

are confirmed, finding steady growth in liberalization's effects on regional earnings. The employment effects are similarly robust to these alternatives.

Although our findings are robust to these specification and measurement changes, the effects of liberalization could appear to grow over time because of correlated shocks occurring after trade liberalization. To explain the smooth growth of the effects in Figures 3 and 4, such confounders would need to affect industries or regions similarly to liberalization and would need to grow steadily over time or occur quite regularly. Although these circumstances are unlikely, in online Appendix B.8 we construct controls for a wide variety of salient economic shocks in the post-liberalization period, demonstrating that they cannot account for the growing earnings effects.

If tariff changes after 1995 were correlated with those occurring during liberalization (1990–1995), they might drive the apparently increasing effects of liberalization, although this is unlikely since post-1995 tariff changes were very modest. We calculate post-liberalization regional tariff reductions as in (2), but use tariff reductions between 1995 and each year $t > 1995$, and include these post-liberalization tariff reductions as additional controls alongside RTR_r. Other potential confounders are the Brazilian Real devaluations that occurred in 1999 and 2002. If these exchange rate movements affected industries differently, they might have been correlated with tariff changes during liberalization. We construct industry-specific real exchange rates as import- or export-weighted averages of real exchange rates between Brazil and its trading partners. We then take the change in log real exchange rate from 1990 to year $t > 1995$, and calculate regional shocks using weighted averages as in (2). There was also a substantial wave of privatization during our sample period. We address privatization by controlling flexibly for the 1995 share of regional employment at state-owned firms or the change in this share from 1995 to t. Controlling for each of these post-liberalization shocks has little effect on the earnings results, which continue to exhibit substantial post-liberalization growth in all cases.

The global commodity price boom of the late 2000s is another potential post-liberalization confounder that might explain our growing earnings results, particularly since agricultural products faced the most positive tariff change during liberalization. In online Appendix B.8, we provide extensive evidence ruling out this possibility. First, the timing of the commodity price boom does not correspond to the timing of our effects. Commodity prices were flat or declining between 1991 and 2003, during which our earnings and employment results grew substantially. Commodity prices then grew sharply after 2004, when our results began to level off.[35] We show that the substantial growth in earnings effects remains when (i) dropping regions most exposed to commodity price growth, by restricting the region sample to include only those with below-median or bottom-quartile employment shares in agriculture and mining or (ii) when restricting our regional earnings measure only to workers in manufacturing. Finally, we use three approaches to directly control for the regional effects of the commodity price boom. We control for the share of workers in agriculture and mining and for changes in regional

[35] A similar argument applies to Bustos, Caprettini, and Ponticelli (2016), who study the effects of genetically modified crops in Brazil. Genetically modified crops were outlawed before 2003 and only permanently authorized in 2005, so this channel cannot explain the substantial growth in earnings effects before 2005.

commodity prices using the measure introduced by Adão (2016). We also use more detailed commodity price data from the IMF Primary Commodity Price Series to construct similar regional controls for commodity price changes. Finally, we control for China's effects on commodity markets using the import and export quantity measures and instruments from Costa, Garred, and Pessoa (2016).[36] All of these controls have little influence on the observed earnings effects of RTR_r.

As a final set of robustness tests, online Appendix B.9 presents results when splitting the sample by tradable and nontradable sector and by skill. We find growing earnings effects in all of these subsamples. This pattern is particularly noteworthy for the nontradable sector, as it confirms that regional labor market equilibrium transmits the effects of liberalization from the tradable sector to the nontradable sector, as predicted in the model of Kovak (2013), which is the basis for the RTR_r shock. The earnings effects for more skilled workers are a bit larger than those for less skilled workers, while the employment effects are larger for less skilled workers. However, these results should be interpreted with care, as the RTR_r shocks are derived from a model with a single type of labor.[37]

Together, the results in this section demonstrate the robustness of our main findings to alternative measures and estimation approaches and rule out a wide variety of salient post-liberalization shocks as potential confounders. We conclude that the earnings and employment profiles shown in Figures 3 and 4 reflect growing causal effects of liberalization over time. In the next section, we consider a variety of potential mechanisms that could drive this growth in liberalization's effects on local labor market outcomes.

IV. Mechanisms

As mentioned above, the conventional model of local labor markets predicts large effects of liberalization just after the tariff change and smaller effects as labor reallocation arbitrages away spatial differences in earnings growth. Our findings contradict this prediction, instead exhibiting increasing differences in earnings growth for 15 years after liberalization between regions facing larger and smaller tariff reductions. In this section, we consider a variety of potential mechanisms that might explain these growing earnings effects, finding strong empirical support for mechanisms involving imperfect interregional labor mobility and dynamic labor demand, particularly slow capital adjustment and agglomeration economies.

A. *Urban Decline*

Glaeser and Gyourko (2005) and Notowidigdo (2013) present models of urban decline in which the slow depreciation of housing stocks drives slow adjustment in local labor markets facing permanent negative labor demand shocks. In their models, the price of housing falls sharply in depressed markets, incentivizing individuals to remain in the city in spite of nominal earnings losses following the demand

[36] Special thanks to Rodrigo Adão for providing commodity price data and code, and to Francisco Costa for providing the shock and instrument measures from Costa, Garred, and Pessoa (2016).

[37] For a more general model with two skill types, see Dix-Carneiro and Kovak (2015).

decline. As housing slowly depreciates, this incentive dissipates, so population and therefore employment steadily decline. This mechanism could therefore rationalize the slowly growing employment effects we document in Figure 4.

However, as in the conventional model of local labor markets, this mechanism predicts the opposite of what we find for earnings in Figure 3. Although wages fall on impact in regions facing negative shocks, they recover slowly over time as workers leave the market due to housing depreciation.[38] Moreover, the mechanism depends on declining population in cities facing negative shocks. In Brazil, overall population growth was large enough during our sample period that out of 475 local labor markets, only 11 experienced population decline between 1991 and 2000, and only 6 did so between 1991 and 2010.[39] Table 3 also finds no response of local working-age population to RTR_r. Thus, while the slow housing depreciation mechanism is quite relevant for rust-belt cities in the United States, it does not appear to apply in the Brazilian context.

B. *Changing Composition of Worker Unobservables*

Liberalization might cause average earnings to slowly decline in regions facing larger tariff reductions relative to other regions because of worker selection. Higher-earning workers may be more likely to leave the formal labor market in harder-hit regions, and this selective worker reallocation may increase over time. Although we flexibly control for detailed worker characteristics including age, sex, and education when calculating regional earnings premia in our main specifications, worker composition may also adjust along unobservable dimensions.

To examine this possibility, we calculate alternative earnings premia, pooling the RAIS data across years and controlling for worker-level fixed effects, which capture time-invariant worker characteristics, including unobservables.[40] We implement this procedure in two ways. First, we use a straightforward worker fixed effects regression,

$$\ln(earn_{jairt}) = \alpha_j + \psi_a + \phi_{it} + \mu_{rt} + \epsilon_{jairt}, \tag{5}$$

where α_j are worker fixed effects, ψ_a are age effects (indicators for falling within each age bin shown in Table B3), and ϕ_{it} are time-varying industry effects. We then calculate the change in log regional earnings premium using the regional earnings estimates, $\hat{\mu}_{rt}$, and examine their response to RTR_r. As shown in panel B of Table 5, when controlling for worker unobservables in this fashion, the earnings effects continue to grow over time.

[38] Note that Glaeser and Gyourko (2005) do not model a production side and instead directly shock wages or amenities. However, a simple extension of their model to include labor market equilibrium would have the features cited here, as in Notowidigdo (2013). Glaeser and Gyourko (2005) also argue that local average wages will decline over time in negatively shocked markets because the most productive workers have the strongest incentive to leave. As shown in Section IVB, since we control for worker characteristics when calculating regional earnings premia, selection effects of this kind are not driving our results.

[39] Authors' calculations using Census data.

[40] For computationally tractability, we draw a 3 percent random sample of all valid individual IDs that appear in RAIS with a positive earnings observation between 1986 and 2010. This procedure yields 450 microregions with formally employed workers earning labor income in December for all years in our sample.

Table 5—Mechanisms: Changing Worker Composition, 1995, 2000, 2005, and 2010

Change in log formal earnings premia:	1991–1995 (1)	1991–2000 (2)	1991–2005 (3)	1991–2010 (4)
Panel A. Main specification				
Regional tariff reduction (RTR_r)	−0.096	−0.529	−1.294	−1.594
	(0.120)	(0.141)	(0.139)	(0.169)
Panel B. Earnings premia controlling for individual fixed effects (fixed returns)				
Regional tariff reduction (RTR_r)	−0.193	−0.514	−1.119	−1.271
	(0.115)	(0.144)	(0.147)	(0.172)
Panel C. Earnings premia controlling for individual fixed effects (time-varying returns)				
Regional tariff reduction (RTR_r)	−0.230	−0.551	−1.322	−1.454
	(0.093)	(0.098)	(0.094)	(0.119)
Formal earnings pretrend, 1986–1990	✓	✓	✓	✓
State fixed effects (26)	✓	✓	✓	✓

Notes: Negative coefficient estimates for the regional tariff reduction (RTR_r) imply larger declines in formal earnings in regions facing larger tariff reductions. Microregion observations: panel A, 475; panels B and C, 450 (omits regions with insufficient observations to identify region-year fixed effects in any particular year). Regional earnings premia: panel A: calculated controlling for age, sex, education, and industry of employment; panels B and C: controlling for individual fixed effects. Standard errors (in parentheses) adjusted for 112 mesoregion clusters. Efficiency weighted by the inverse of the squared standard error of the estimated change in log formal earnings premium. See text for detailed description of each panel.

A limitation of (5) is that it restricts the returns to worker characteristics to be constant over time. Since the returns to observable characteristics change substantially over time (see online Appendix B.3), we allow for time-varying returns (δ_t) to worker characteristics (α_j) in the following specification:

$$\ln(earn_{jairt}) = \delta_t \alpha_j + \psi_a + \phi_{it} + \mu_{rt} + \epsilon_{jairt}. \tag{6}$$

The value of δ_t can vary arbitrarily over time, but does so identically for all workers. This restriction distinguishes $\delta_t \alpha_j$ from worker $\times$ time fixed effects, which would absorb all variation in the data. We estimate (6) using the iterative algorithm described by Arcidiacono et al. (2012) and calculate standard errors using the wild bootstrap method suggested by Davidson and MacKinnon (2006), with 500 iterations. Panel C of Table 5 presents earnings estimates using the resulting regional earnings premia. The growth in earnings effects remains, and the results from the more flexible earnings premium specification in panel C are quantitatively very close to the baseline specification in panel A. These findings rule out worker selection as a mechanism driving the observed growth in the earnings effects of liberalization.

C. *Slow Response of Imports or Exports*

Although trade liberalization was complete by 1995, it is possible that trade *quantities* were slow to respond to the sharp change in trade policy, perhaps because of difficulty in forming new trade links with firms abroad. Prices faced by Brazilian producers may evolve slowly in response to tariff cuts if import quantities respond slowly to liberalization. If so, the slow evolution of imports in response to the tariff cuts could potentially explain the slow growth in the effects of liberalization over time. To examine this possibility, we (i) study the relationship between regional tariff reductions and trade quantity measures to determine whether such a slow trade response occurred in practice and (ii) control for changes in trade quantities to see whether they mediate the relationship between tariff changes and earnings. We follow Autor, Dorn, and Hanson (2013) by constructing changes in imports and exports per worker for each industry from 1991 to each subsequent year, using Comtrade data.[41] We then form regional weighted averages of these changes in trade flows, weighting by the industry's initial share of regional employment. See online Appendix A.6 for details on the construction of these measures.

We first examine the effect of regional tariff reductions on these regional measures of import, export, and net export growth, looking for evidence of slow growth in trade quantities that might drive the slow growth in earnings effects. We do so using the trade growth measures as dependent variables in (3). Figure 5 plots the effects of RTR_r on each trade flow measure.[42] First, consider the effects on regional imports (circles). As expected, regions facing larger tariff reductions experienced larger increases in the regional import measure. These import increases occurred immediately after liberalization, with large positive coefficients already present in 1995. Because we measure trade flows in \$100,000 units, the 1995 coefficient of 0.144 implies that a region facing a 10 percentage point larger tariff reduction experienced a \$1,440 larger increase in imports per worker. These import effects actually *decrease* on average until 2003 (coefficient estimate = 0.070), in sharp contrast to the earnings effects, which *grew* to more than two-thirds of their long-run level during the same time period. After 2003, the import effects increase, but this coincides with a leveling-off in the earnings and employment effects. This timing is inconsistent with slow import growth driving our results.

The sign of the export effects (triangles) is positive, indicating that industries experiencing larger export increases were on average located in the same regions as industries facing larger tariff reductions.[43] This effect works against the hypothesis that slow trade quantity growth drove relative earnings declines in these regions. After 2003, both the import and export effects grow quite substantially, following the overall trends in Brazilian imports and exports. Note, however, that the relationship between RTR_r and net exports (diamonds) falls from 2005 to 2010, again a time period with substantial growth in the earnings effects. Overall, the evolution of import and export quantities is not consistent with the hypothesis that slow trade quantity growth explains our results.

To confirm this point, we include controls for regional import and export growth when examining the effect of RTR_r on regional earnings premia. If the growing earnings effects remain when including these controls, we can be confident that a different mechanism is at play. We examine the relationship between earnings growth and

[41] Online Appendix B.10 shows results for an ad hoc alternative functional form using the change in log trade, yielding the same conclusions.

[42] In Figure 5 we do not have pre-liberalization trends for trade flows because Comtrade data for Brazil begin in 1989.

[43] The positive sign for the export effect is not driven by any particular industry or industries and is robust to dropping agriculture and/or natural-resource industries.

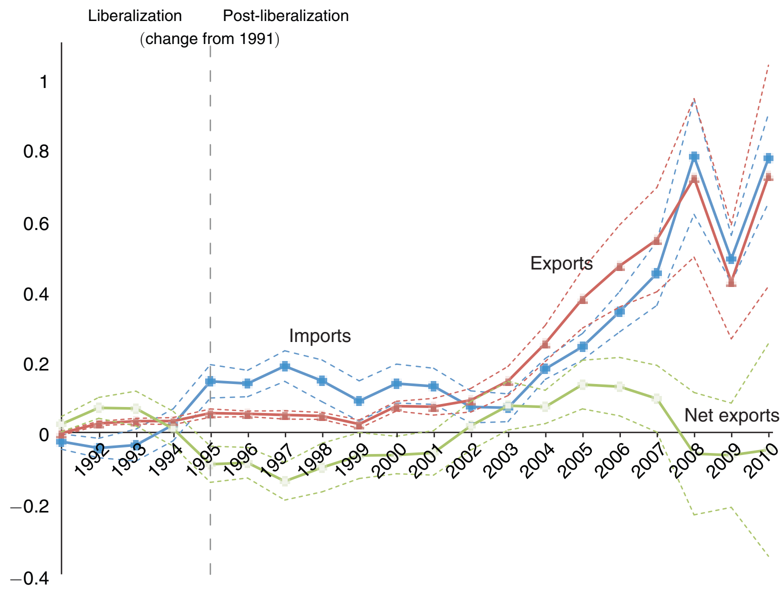


Figure 5. Regional Imports, Exports, and Net Exports per Worker, 1991–2010

Notes: Each point reflects an individual regression coefficient, $\hat{\theta}_t$, following (3), where the dependent variable is the change in regional imports per workers (circles), exports per worker (triangles), or net exports per worker (diamonds), measured in \$100,000 units. The independent variable is the regional tariff reduction (RTR_r), defined in (2). Note that RTR_r always reflects tariff reductions from 1990 to 1995. All regressions include state fixed effects, but do *not* include pre-liberalization trends due to a lack of Comtrade trade data before 1989. Positive estimates imply larger increases in trade flow per worker in regions facing larger tariff reductions. Vertical bar indicate that liberalization was complete by 1995. Dashed lines show 95 percent confidence intervals. Standard errors adjusted for 112 mesoregion clusters.

RTR_r, as in (3), including controls for the regional growth in imports ($RegImp_{rt}$) and exports ($RegExp_{rt}$) from 1990 to year t:

$$(7) \qquad y_{rt} - y_{r,1991} = \theta_t RTR_r + \beta_1 RegImp_{rt} + \beta_2 RegExp_{rt} + \alpha_{st} + \gamma_t(y_{r,1990} - y_{r,1986}) + \epsilon_{rt}.$$

The import and export coefficients, β_1 and β_2, are constant over time, allowing us to test whether the slow evolution of trade flows explains the evolution of earnings growth (since $RegImp_{rt}$ and $RegExp_{rt}$ change over time, unlike RTR_r). Panel B of Table 6 shows that the effect of RTR_r on regional earnings still grows steadily over time when controlling for changes in regional imports and exports, implying that slow trade quantity growth is not driving the relationship between the tariff reductions and earnings.

A remaining concern is that if regional imports and exports are endogenous to regional earnings growth, then the coefficients on RTR_r will be biased along with the trade flow coefficients, invalidating the analysis just described. Panels C and D address this issue following the strategy of Autor, Dorn, and Hanson (2013),

Table 6—Mechanisms: Slow Response of Imports or Exports, 1995, 2000, 2005, and 2010

Change in log formal earnings premia:	1991–1995 (1)	1991–2000 (2)	1991–2005 (3)	1991–2010 (4)
Panel A. Main specification				
Regional tariff reduction (*RTR*)	−0.096	−0.529	−1.294	−1.594
	(0.120)	(0.141)	(0.139)	(0.169)
Panel B. Controls for trade quantities (OLS)				
Regional tariff reduction (*RTR*)	−0.089	−0.521	−1.287	−1.562
	(0.112)	(0.138)	(0.181)	(0.221)
Import quantity control		−0.382		
		(2.242)		
Export quantity control		0.142		
		(3.355)		
Panel C. Latin America IV				
Regional tariff reduction (*RTR*)	−0.129	−0.569	−1.342	−1.757
	(0.106)	(0.129)	(0.173)	(0.212)
Import quantity control		1.668		
		(2.631)		
Export quantity control		−0.149		
		(3.861)		
First-stage F (Kleibergen-Paap)		93.04		
Panel D. Colombia IV				
Regional tariff reduction (*RTR*)	−0.049	−0.488	−1.372	−1.502
	(0.108)	(0.132)	(0.161)	(0.213)
Import quantity control		−3.489		
		(2.427)		
Export quantity control		5.379		
		(3.268)		
First-stage F (Kleibergen-Paap)		876.2		
Formal earnings pretrend, 1986–1990	✓	✓	✓	✓
State fixed effects (26)	✓	✓	✓	✓

Notes: Negative coefficient estimates for the regional tariff reduction (RTR_r) imply larger declines in formal earnings in regions facing larger tariff reductions. Panel A replicates the earnings results in columns 3 and 6 of Table 2. Panels B–D include regional import and export quantity controls as in (7). We instrument for the potentially endogenous import and export controls using regional measures of commodity price growth from Adão (2016) and with regional trade flows for other countries. "Latin America" consists of Argentina, Chile, Colombia, Paraguay, Peru, and Uruguay. We measure imports and exports between Latin America or Colombia and the rest of the world excluding Brazil. Due to Comtrade data availability, changes in Colombian trade flows are measured from 1991 to each subsequent year and Latin American trade flows from 1994. We allow for time-varying first-stage coefficients, so we have 2 endogenous variables ($RegImp_{rt}$ and $RegExp_{rt}$) and 57 instruments for Colombia (3 instruments × 19 years) and 48 instruments for Latin America (3 instruments × 16 years). First-stage Kleibergen-Paap F-statistics are compared to the Stock and Yogo (2005) critical value of 21 to reject 5 percent bias relative to OLS. Standard errors (in parentheses) adjusted for 112 mesoregion clusters. Efficiency weighted by the inverse of the squared standard error of the estimated change in log formal earnings premium.

instrumenting for Brazilian trade flows using trade flows for other countries.[44] We consider instruments based on the combination of Argentina, Chile, Colombia, Paraguay, Peru, and Uruguay ("Latin America") and on Colombia alone, which liberalized during the same time period as Brazil and imposed similar tariff cuts across industries (Paz 2014). In each case, we measure imports and exports between these

[44] We also include regional measures of commodity price growth from Adão (2016) in the set of instruments.

countries and the rest of the world, excluding Brazil.[45] Panels C and D show the results. In both cases, the effects of RTR_r continue to grow over time, with a similar magnitude to the main results, shown in panel A. These and the preceding results in this section rule out slow import or export responses as the mechanism driving the slowly growing earnings effects.

D. *Dynamic Labor Demand*

A remaining potential mechanism driving the growing effects of liberalization on earnings and employment involves dynamics in labor demand. If labor is imperfectly mobile across regions and an initial labor demand shock is followed by a dynamic process that amplifies the shock's effects over time, one will observe the growing regional earnings and employment effects we document. We consider two potential sources of these dynamics: agglomeration economies (e.g., Kline and Moretti 2014) and slow adjustment of capital stocks (e.g., Dix-Carneiro 2014). As we will show, both appear to play important roles in explaining our findings.

Evidence on the Importance of Dynamic Labor Demand.—To study these mechanisms and formalize our argument, we generalize the specific-factors model in Kovak (2013) to include agglomeration economies and slow adjustment of labor and capital. We focus on the formal economy, consisting of many regions, indexed by r, which may produce goods in many industries, indexed by i. Production in each industry uses Cobb-Douglas technology with constant returns to scale and three inputs: labor, a fixed factor, and capital. Formal labor, L_r, is assumed to be perfectly mobile between industries within a region. The fixed factor, T_{ri}, is usable only in its respective region and industry and is fixed over time. This factor represents inputs such as natural resources, land, or very slowly depreciating infrastructure and capital that are effectively fixed over the time horizons we consider. Capital, K_{ri}, is also usable only in its respective region and industry but may change slowly over time through depreciation and investment decisions.[46] Output of industry i in region r is

$$Y_{ri} = A_{ri} L_{ri}^{1-\varphi_i} \left(T_{ri}^{\zeta_i} K_{ri}^{1-\zeta_i} \right)^{\varphi_i}, \tag{8}$$

where $\varphi_i, \zeta_i \in (0,1)$. Goods and factor markets are perfectly competitive, and producers face exogenous prices P_i, which are common across regions and fixed by world prices and tariffs. To allow for the possibility of agglomeration economies, we allow productivity, A_{ri}, to vary with the amount of local economic activity. We also allow for factor adjustment by letting L_r and K_{ri} change over time. Recall that changes in L_r primarily reflect workers entering or leaving the formal workforce rather than other channels such as interregional migration, as shown in Table 3.

[45] Due to Comtrade data availability, the changes in trade flows for Latin America are calculated from 1994 to each subsequent year and those for Colombia alone are calculated from 1991 to each subsequent year.

[46] We separate fixed factors and variable capital for two reasons. First, our research design is based on regional differences in industry mix, which are driven by fixed factors. Second, including fixed factors in each region ensures that all regions maintain some economic activity even when faced with very negative shocks. Hence, this formulation is common in the literature on agglomeration economies (e.g., Helm 2017 and Kline and Moretti 2014).

We assume that changes in K_{ri} reflect depreciation and firms' investment decisions rather than physical mobility via secondary markets for installed capital.

As shown in online Appendix C, factor market clearing, zero profits, and cost minimization imply the following equilibrium relationship, in which hats represent proportional changes:

$$\hat{w}_r = \sum_i \beta_{ri} \hat{P}_i + \sum_i \beta_{ri} \hat{A}_{ri} - \delta_r \Big(\hat{L}_r - \sum_i \lambda_{ri} (1 - \zeta_i) \hat{K}_{ri} \Big), \tag{9}$$

$$\text{where } \beta_{ri} \equiv \frac{\lambda_{ri} \frac{1}{\varphi_i}}{\sum_j \lambda_{rj} \frac{1}{\varphi_j}} > 0 \quad \text{and} \quad \delta_r \equiv \frac{1}{\sum_j \lambda_{rj} \frac{1}{\varphi_j}} > 0.$$

Here, $\hat{w}_r$ is the proportional change in the regional wage, and λ_{ri} is the initial share of regional employment in industry i. This is an equilibrium relationship because the factor supplies and productivity levels may respond endogenously to the liberalization shock reflected by $\hat{P}_i$.

As a thought exercise, suppose we were to hold productivity and factor supplies constant ($\hat{A}_{ri} = \hat{L}_r = \hat{K}_{ri} = 0$). In that case, the wage change equals the simple weighted average price shock in (1). In this restricted model, there is no scope for dynamic effects of liberalization, and one would observe a substantial wage effect of liberalization on impact, with no changes thereafter. More realistically, if productivity or factor supplies evolve over time in response to the liberalization-induced price shocks, then the effects of liberalization on regional wages can change over time as well.

First, we consider factor supply responses. Imagine that only regional labor supply responds to liberalization, while maintaining $\hat{A}_{ri} = \hat{K}_{ri} = 0$. Immediately following liberalization, wages decline more in regions facing larger tariff reductions, and formal employment falls more in these regions, as in Figure 4. Equation (9) shows that this change in employment partly offsets the wage losses experienced on impact, since $\delta_r > 0$. If employment adjusts slowly, then the observed wage effects of liberalization get smaller over time. In other words, with labor adjustment only, the model reflects the conventional prediction that liberalization's effects on local wages decline over time. If we allow both regional employment and regional capital stocks to vary in response to liberalization, complex patterns can emerge, depending on the relative speed of labor and capital adjustment. For example, if regional labor is held fixed and capital stocks contract more in regions facing larger tariff declines (as we show below), the marginal product of labor will fall, and relative wages will decline even further in harder hit regions, as seen in Figure 3. More generally, the model can qualitatively rationalize growing earnings effects of liberalization if the labor supply elasticity is finite and capital adjusts more quickly than labor.

Now consider changes in productivity, $\hat{A}_{ri}$. We assume that these result from agglomeration economies in which changes in the amount of local economic activity drive changes in the productivity of local firms. There is little agreement on the specific source of agglomeration economies, with various papers arguing that they result from changes in population, overall employment, or employment in particular industries (Melo, Graham, and Noland 2009).[47] For agglomeration economies to be relevant in our context, we must observe effects of regional tariff reductions on at least one of these agglomeration sources. In Table 3 and online Appendix B.11, we show that neither working-age population nor overall employment (sum of formal and informal) respond substantially to RTR_r, while Figure 4 shows that liberalization substantially affected *formal* employment. For agglomeration economies to be relevant in our context, agglomeration must apply to regional formal employment, since other potential sources of agglomeration do not significantly respond to liberalization. This is plausible, as labor market pooling and knowledge spillovers are more likely to apply in formal employment than in informal employment, which disproportionately includes agricultural production. In this case, a negative labor demand shock decreases wages on impact, which endogenously decreases formal employment and therefore decreases regional productivity through agglomeration economies. As shown in (9), this productivity decline amplifies the wage decline from the initial shock, leading to further reductions in local formal employment and productivity, etc. If this amplification occurs slowly over time, perhaps due to slow labor supply responses or slow responses of productivity to formal employment (Kline and Moretti 2014), then the observed effects of liberalization may also grow over time.

Therefore, given imperfect labor mobility across regions, both capital adjustment and agglomeration economies could qualitatively explain the earnings and employment patterns in Figures 3 and 4. To provide evidence for the relevance of dynamic labor demand, we rearrange (9) to infer the labor demand shifts needed to rationalize the changes in earnings with the observed regional tariff reductions and changes in formal employment. For consistency with the agglomeration literature, we assume identical factor cost shares across industries ($\varphi_i = \varphi \; \forall i$ and $\zeta_i = \zeta \; \forall i$, which implies $\delta_r = \varphi$).[48] The economy-wide value of φ is 0.544 (see online Appendix A.4), and we discuss the value of ζ in Section IVD. Under these assumptions,

$$\sum_i \beta_{ri} \hat{A}_{ri} + \varphi(1-\zeta) \sum_i \lambda_{ri} \hat{K}_{ri} = \underbrace{\hat{w}_r - \sum_i \beta_{ri} \hat{P}_i + \varphi \hat{L}_r}_{\text{observed}}. \tag{10}$$

The left-hand side of (10) captures the overall shifts in labor demand resulting from agglomeration economies and capital adjustment, which we can measure as a residual using the observable quantities on the right-hand side. We measure $\hat{w}_r$ as the change in regional earnings premium, $-\sum_i \beta_{ri} \hat{P}_i$ as RTR_r, and $\hat{L}_r$ as the change in regional formal employment. Figure 6 (solid circles) shows the relationship between this inferred labor demand measure and regional tariff reductions in each year following the start of liberalization. We can infer that labor demand steadily declined in regions facing larger tariff reductions and that these dynamics were complete by the late 2000s. Given this evidence for dynamic labor demand in general, we examine

[47] Many papers argue that population or employment *density* is the relevant quantity, but since we utilize regions with fixed boundaries, the change in log population or employment *density* is identical to the change in log population or employment *level*.

[48] When assuming identical factor cost shares across industries, our production function is identical to those in Kline and Moretti (2014) and Helm (2017). Hanlon and Miscio (2017) use a slightly different Cobb-Douglas production function, but also assume constant cost shares across industries.

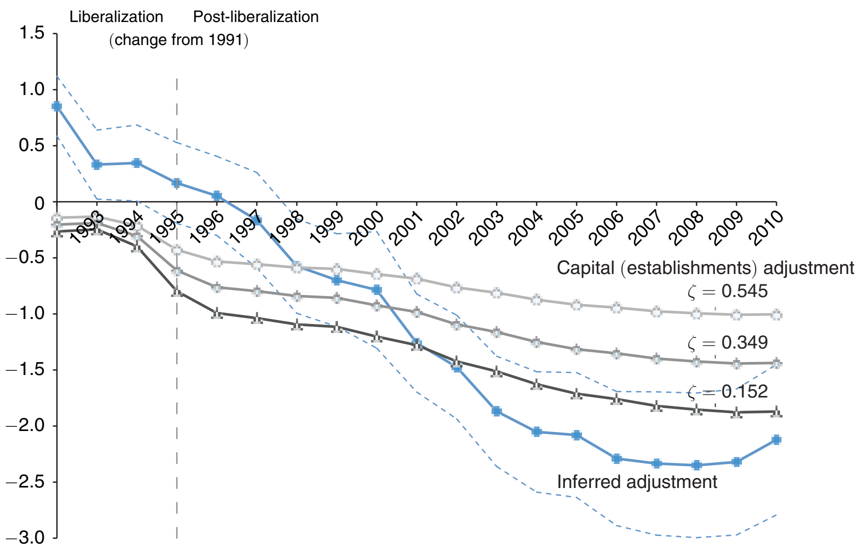


Figure 6. Inferred Adjustment and Capital Adjustment Quantification, 1992–2010

Notes: Each point reflects an individual regression coefficient, $\hat{\theta}_t$, following (3). For the profile with solid circles, the dependent variable is the inferred labor demand shifts from agglomeration and capital adjustment, defined in (10). For the gray profiles with hollow markers, the dependent variable is capital's contribution to overall adjustment, using the change in the number of regional formal establishments as a proxy for the change in regional capital, $\sum_i \lambda_{ri} \hat{K}_{ri}$. We present profiles for three values of ζ, specific factors' share of nonlabor inputs, based on Valentinyi and Herrendorf (2008). The independent variable is the regional tariff reduction (RTR_r), defined in (2). Note that RTR_r always reflects tariff reductions from 1990 to 1995. All regressions include state fixed effects, and post-liberalization regressions control for the 1986–1990 outcome pretrend. Negative estimates imply larger declines in residual labor demand or the number of establishments in regions facing larger tariff reductions. Vertical bar indicates that liberalization was complete by 1995. Dashed lines show 95 percent confidence intervals. Confidence intervals for capital adjustment profiles shown in online Appendix B.12. Standard errors adjusted for 112 mesoregion clusters.

evidence for the two specific sources of dynamics: agglomeration economies and slow capital adjustment.

Evidence for Agglomeration Economies and Capital Adjustment.—To examine these mechanisms in more detail, we follow the literature by imposing additional long-run assumptions that allow us to compare our results to prior work and to quantify the roles of agglomeration and slow capital adjustment. We assume a constant elasticity long-run agglomeration function:[49]

$$\hat{A}_{ri} = \kappa \hat{L}_r, \quad \kappa \geq 0. \tag{11}$$

Table 3 shows that working-age population does not substantially respond to liberalization, indicating that the main margin of labor supply adjustment is workers' choice of whether to pursue formal employment within a given region. Table 4 shows informal sector earnings do not substantially respond to liberalization. Therefore, we assume that changes in formal labor $(\hat{L}_r)$ depend upon changes in the regional

[49] Kline and Moretti (2014) provide empirical support for a constant agglomeration elasticity.

formal wage ($\hat{w}_r$), and assume a constant elasticity long-run local formal labor supply function:

$$\hat{L}_r = \frac{1}{\eta}\hat{w}_r, \quad \eta \geq 0. \tag{12}$$

Finally, we assume perfectly mobile capital in the long run ($\hat{R}_r = \hat{R}\ \forall r$, where R is the price of capital).[50] We take 2010 to be the long run (20 years following the start of liberalization), consistent with the flat earnings and employment responses by the late 2000s. Imposing these assumptions on the model yields the following expressions for the long-run regional wage change and the change in employment in a given region $\times$ industry combination (derived in online Appendix C):

$$\hat{w}_r = \frac{\eta}{\eta[1-\varphi(1-\zeta)] - \kappa + \varphi\zeta}\sum_i \beta_{ri}\hat{P}_i - \frac{\varphi(1-\zeta)\eta}{\eta[1-\varphi(1-\zeta)] - \kappa + \phi\zeta}\hat{R}; \tag{13}$$

$$\begin{aligned}\hat{L}_{ri} = {} & \frac{1}{\varphi\zeta}\hat{P}_i - \frac{1}{\varphi\zeta}\cdot\frac{\eta[1-\varphi(1-\zeta)] - \kappa}{\eta[1-\varphi(1-\zeta)] - \kappa + \varphi\zeta}\sum_i \beta_{ri}\hat{P}_i \\ & - \frac{\varphi(1-\zeta)}{\eta[1-\varphi(1-\zeta)] - \kappa + \varphi\zeta}\hat{R}.\end{aligned} \tag{14}$$

We test for the presence of agglomeration economies using the change in employment in each region $\times$ industry combination, following an approach similar to that of Helm (2017). As shown in (14), in the absence of agglomeration ($\kappa = 0$), holding fixed an industry's own price decline, larger regional tariff reductions increase local industry employment. Intuitively, if other industries in the same region face larger tariff cuts, more laborers will locally transition into the reference industry in equilibrium. However, in a setting with agglomeration economies ($\kappa > 0$), price reductions in other industries in the same region reduce the local productivity of the reference industry. If agglomeration forces are strong enough, larger regional tariff reductions can *reduce* local industry employment conditional on the industry's own price change. We therefore estimate the following specification:

$$\hat{L}_{ri} = \gamma_0 + \gamma_1\hat{P}_i + \gamma_2 RTR_r + \epsilon_{ri}. \tag{15}$$

This expression is the reduced form of (14). Here, γ_0 captures the term for $\hat{R}$, which does not vary across industries or regions, and $\gamma_2 < 0$ implies the presence of agglomeration economies.[51] We measure $\hat{L}_{ri}$ using changes from 1991 to 2010 to capture long-run adjustment. We control for industry price changes either directly using tariff reductions ($-d\ln(1+\tau_i)$), or with industry fixed effects. Since the nontradable sector does not directly experience a tariff change, we use RTR_r to measure

[50] Perfect long-run capital mobility is a standard assumption in this literature (Hanlon and Miscio 2017; Helm 2017; Kline and Moretti 2014).

[51] Recall that $RTR_r \equiv -\sum_i \beta_{ri}\hat{P}_i$, so $\gamma_2 < 0$ implies $\frac{\eta[1-\varphi(1-\zeta)] - \kappa}{\eta[1-\varphi(1-\zeta)] - \kappa + \varphi\zeta} < 0$ in (14), which in turn implies $\kappa > 0$.

Table 7—Test for Agglomeration Economies

	Change in log region × industry employment					
	All industries				Tradable industries	
	(1)	(2)	(3)	(4)	(5)	(6)
Regional tariff reduction (RTR_r)	−7.751	−6.084	−6.183	−6.333	−6.708	−6.704
	(0.625)	(0.623)	(0.631)	(0.646)	(0.675)	(0.694)
Industry tariff reduction	−1.790	−1.666	−1.669		−2.017	
	(0.294)	(0.290)	(0.291)		(0.332)	
Formal employment pretrend, 1986–1990			−0.106	−0.147	−0.110	−0.150
			(0.036)	(0.032)	(0.037)	(0.032)
Industry fixed effects (20)				✓		✓
State fixed effects (26)		✓	✓	✓	✓	✓

Notes: Negative coefficient estimates for the regional tariff reduction imply the presence of agglomeration economies, following (15). Observations represent region × industry pairs. The dependent variable is the change in log formal employment in a given region × industry pair from 1991 to 2010. Columns 1–4 cover all industries, including the nontradable sector, while columns 5 and 6 restrict attention to tradable industries. For tradable industries, industry tariff reductions are given by the decline in the log of 1 plus the tariff rate. For the nontradable sector, the industry tariff reduction is measured using RTR_r. Standard errors (in parentheses) adjusted for 112 mesoregion clusters.

its price change, following the arguments in (Kovak 2013). The results of estimating (15) appear in Table 7. In all cases, the coefficient on RTR_r is negative and highly significant; an industry's local employment actually falls when other industries in the same region face larger tariff reductions, implying the presence of agglomeration economies. This finding is robust to including state fixed effects and outcome pretrends, to using direct industry price change controls or industry fixed effects, and to restricting attention to tradable industries.[52]

We also find evidence for slow capital adjustment. Although regional capital stock measures are unavailable, we can observe changes in the number of formal establishments in a given region, which are likely to approximate changes in regional capital stocks.[53,54] Figure 7 shows that regions facing larger tariff reductions experienced steady relative declines in the number of formal establishments, with the effect increasing most quickly in the early 2000s and leveling out later in the sample period. It is possible that capital simply reallocated from smaller exiting establishments to larger continuing establishments in harder-hit locations. If this were the case, the change in the number of establishments would not be particularly informative about the change in regional capital stock. However, the decline in the number of establishments was not offset by increases in the average size of remaining establishments; if anything these establishments shrank on average. Moreover, online Appendix B.13 shows that larger tariff declines drove increases in exit rates throughout the establishment size distribution. These results strongly support the

[52] Because we use RTR_r to measure the industry-specific price change for nontradable industries, it is not possible to separately identify the effects of industry-specific and regional tariff reductions for nontradable industries alone.

[53] It is not possible to construct regional capital stocks in Brazil during our sample period. Capital investment in manufacturing firms could in principle be constructed from the Annual Manufacturing Survey (PIA) beginning in 1996, but the Brazilian Statistical Agency (IBGE) has a strict policy against constructing PIA variables at the regional level. Moreover, with investment data beginning in 1996, we would not have credible capital stock measures until well after liberalization. Data sources covering nonmanufacturing sectors also begin well after liberalization.

[54] Regional capital could slowly reallocate from firms in the formal sector to firms in the informal sector, but this is unlikely, as firms in the informal sector are much less capital intensive than those in the formal sector (LaPorta and Schleifer 2014; Fajnzylber, Maloney, and Montes-Rojas 2011).

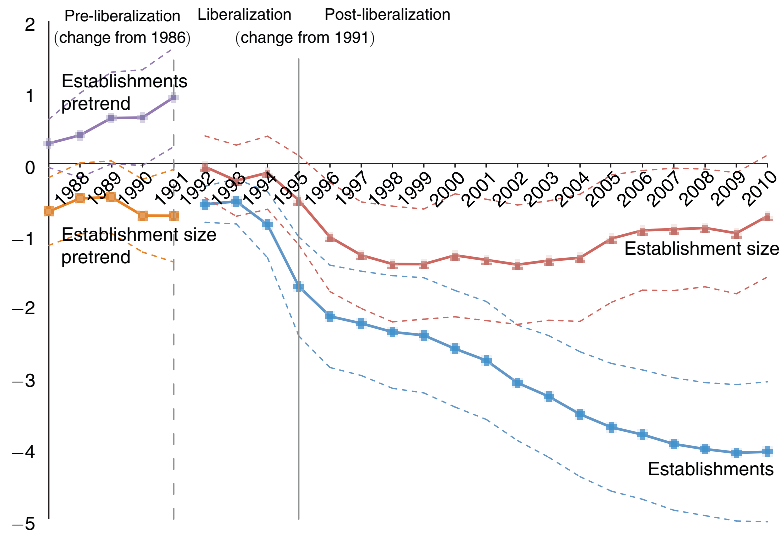


FIGURE 7. REGIONAL LOG NUMBER OF FORMAL ESTABLISHMENTS AND LOG AVERAGE FORMAL ESTABLISHMENT SIZE (*Number of Workers*), 1987–2010

Notes: Each point reflects an individual regression coefficient, $\hat{\theta}_t$, following (3), where the dependent variable is the change in regional log number of formal establishments or the change in regional log average formal establishment size. The independent variable is the regional tariff reduction (RTR_r), defined in (2). Note that RTR_r always reflects tariff reductions from 1990 to 1995. For circles and triangles, the changes are from 1991 to the year listed on the x-axis. For diamonds and squares, the changes are from 1986 to the year listed. All regressions include state fixed effects, and post-liberalization regressions control for the 1986–1990 outcome pretrend. Negative estimates imply larger declines in the number of establishments or average establishment size in regions facing larger tariff reductions. Vertical bars indicate that liberalization began in 1991 and was complete by 1995. Dashed lines show 95 percent confidence intervals. Standard errors adjusted for 112 mesoregion clusters.

interpretation that trade shocks induced a gradual reallocation of capital away from harder hit locations.

To reinforce this conclusion, we present evidence on the margins of capital adjustment. We expect investment to respond immediately following liberalization, with new investment directed toward more favorable markets and away from markets facing larger tariff reductions. In contrast, depreciation takes time to erode the capital stock in a negatively affected region. We confirm these patterns using measures of regional establishment entry and exit and job creation and destruction. We measure *cumulative* entry, exit, job creation, and job destruction by observing changes from 1991 to each subsequent year, and calculate each measure following Davis and Haltiwanger (1990).[55] We then examine the relationship between the log of each measure and RTR_r. Figure 8 reports the results for entry and exit, and Figure 9 shows the results for job creation and destruction. New investment, as observed in establishment entry and job creation, falls immediately in negatively affected regions and stays low throughout the sample period. In contrast, the exit and job destruction effects grow slowly over time as existing establishments in regions facing larger

[55] For establishment entry and exit, the Davis and Haltiwanger (1990) measure reduces to the number of establishments that entered or exited between 1991 and year t as a share of active establishments in year t. See online Appendix A.7 for details.

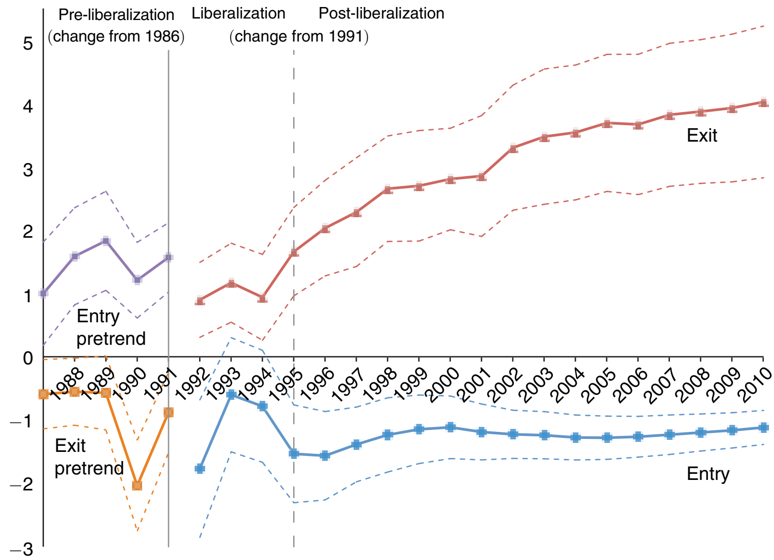


Figure 8. Regional log Cumulative Formal Establishment Entry and Exit, 1987–2010

Notes: Each point reflects an individual regression coefficient, $\hat{\theta}_t$, following (3). The dependent variable is the log cumulative formal establishment entry or exit from 1991 to the year listed on the x-axis (circles and triangles) or from 1986 to the year listed (diamonds and squares), calculated as in Davis and Haltiwanger (1990). The independent variable is the regional tariff reduction (RTR_r), defined in (2). Note that RTR_r always reflects tariff reductions from 1990 to 1995. All regressions include state fixed effects, and post-liberalization regressions control for log cumulative establishment entry or exit during the period 1986–1990. Positive exit estimates and negative entry estimates imply larger rates of exit and smaller rates of entry in regions facing larger tariff reductions. Vertical bars indicate that liberalization began in 1991 and was complete by 1995. Dashed lines show 95 percent confidence intervals. Standard errors adjusted for 112 mesoregion clusters.

tariff cuts allow their installed capital stocks to erode through depreciation, directing investment elsewhere. Together, these results support the conclusion that capital slowly reallocated away from regions facing larger tariff declines, steadily amplifying the earnings effects of liberalization.

Quantification.—The preceding results provide evidence that both slow capital adjustment and agglomeration economies play qualitatively important roles in driving the evolution of liberalization's effects on earnings and employment. We now investigate the extent to which these mechanisms can quantitatively explain the long-run labor market effects we observe.

We begin by examining the share of the long-run change in labor demand that can be explained by regional capital adjustment. In (10), capital's contribution to overall adjustment is given by $\varphi(1-\zeta)\sum_i \lambda_{ri}\hat{K}_{ri}$. We proxy for $\sum_i \lambda_{ri}\hat{K}_{ri}$ using the change in log number of regional formal establishments (as discussed above) and measure ζ (fixed-factors' share of nonlabor input costs), using estimates of equipment, structures, and land cost shares from Valentinyi and Herrendorf (2008).[56] We consider three alternative values for ζ, defining fixed factors as (i) land only

[56] Agglomeration estimation exercises regularly require cost share calibrations along these lines, e.g., Kline and Moretti (2014).

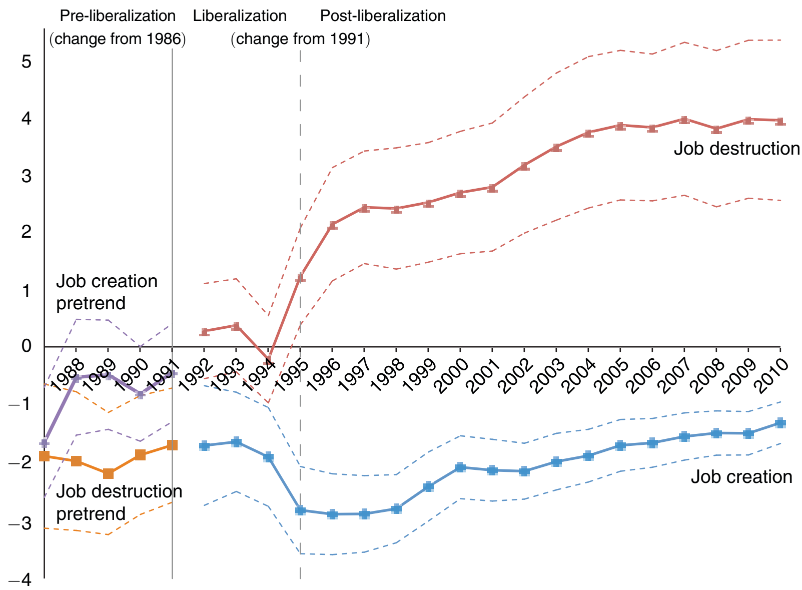


Figure 9. Regional log Cumulative Job Creation and Destruction, 1987–2010

Notes: Each point reflects an individual regression coefficient, $\hat{\theta}_t$, following (3). The dependent variable is the log cumulative job creation or destruction rate from 1991 to the year listed on the x-axis (circles and triangles) or from 1986 to the year listed (diamonds and squares), calculated as in Davis and Haltiwanger (1990). The independent variable is the regional tariff reduction (RTR_r), defined in (2). Note that RTR_r always reflects tariff reductions from 1990 to 1995. All regressions include state fixed effects, and post-liberalization regressions control for log cumulative job creation or destruction during the period 1986–1990. Positive job destruction estimates and negative job creation estimates imply larger rates of job destruction and smaller rates of job creation in regions facing larger tariff reductions. Vertical bars indicate that liberalization began in 1991 and was complete by 1995. Dashed lines show 95 percent confidence intervals. Standard errors adjusted for 112 mesoregion clusters.

($\zeta = 0.152$); (ii) land and structures ($\zeta = 0.545$); and (iii) land and half of structures ($\zeta = 0.349$).[57] Figure 6 shows the evolution of liberalization's effect on these capital adjustment measures compared to the overall labor demand adjustment inferred from (10). Although the shapes of the capital adjustment and overall adjustment profiles are not identical, they both grow over time and have similar scales. Depending on the value of ζ, capital adjustment can account for between 47 and 88 percent of the inferred labor demand adjustment in 2010. While this is a somewhat wide range, it is clear that capital adjustment accounts for an important share of overall long-run labor demand adjustment, but that it is unlikely to account for all of the adjustment in the absence of agglomeration.

To quantify the strength of agglomeration economies needed to rationalize the data, we first need to estimate the inverse labor supply elasticity, η. We do so following (12) by regressing the 1991–2010 change in log formal employment on the change in log regional earnings premium with RTR_r serving as an instrument for $\hat{w}_r$. The resulting estimate of 0.363 is shown in panel A of Table 8. Given this value for η, we estimate κ using nonlinear least squares based on long-run changes in regional

[57] While (i) is likely an underestimate because there are fixed inputs other than land (e.g., heavy infrastructure), (ii) is likely an overestimate, because some structures depreciate substantially at a 15-year time horizon. Thus, the intermediate value, (iii), is our preferred estimate.

Table 8—Agglomeration Elasticity Estimates

Panel A. Inverse labor supply elasticity (η)	0.363 (0.060)		
	low ζ (0.152)	mid ζ (0.349)	high ζ (0.545)
Panel B. Agglomeration elasticity (κ)	(1)	(2)	(3)
Wage-based agglomeration elasticity	0.042 (0.023)	0.188 (0.023)	0.333 (0.025)
Employment-based agglomeration elasticity	0.215 (0.032)	0.330 (0.038)	0.461 (0.043)

Notes: Labor supply elasticity, η, estimated from (12) using RTR_r an instrument for the change in regional log earnings premium. The first-stage partial *F*-statistic (Kleibergen-Paap) for this regression is 59.14. Given the estimate of η, the agglomeration elasticity, κ, is estimated using two alternative methods. The earnings-based approach estimates (13), and the employment-based approach estimates (14), both using nonlinear least squares, and both including 1986–1990 pre-liberalization outcome trends and state fixed effects. The employment-based estimates control for industry price changes as in column 3 of Table 7, and results using other approaches are very similar. We present estimates for three different values of ζ, specific factors' share of nonlabor inputs, based on Valentinyi and Herrendorf (2008). See text for details. Standard errors (in parentheses) bootstrapped by regional resampling.

earnings in (13) or long-run changes in employment in (14). In both cases, the $\hat{R}$ term is captured by the intercept, and the regional weighted average price shocks are measured by RTR_r. When estimating equation (14), we include all industries and control for industry price changes using tariff changes, as in column 3 of Table 7, though the results are nearly identical when using the alternative approaches in columns 4–6 of Table 7. We show estimates for each value of ζ and bootstrap the entire estimation procedure when calculating standard errors to account for potential correlation between the η and κ estimates.

The resulting estimates of κ appear in panel B of Table 8. All of the estimates are positive and fall within the range of the prior literature (Melo, Graham, and Noland 2009). For example, Kline and Moretti (2014) find an estimate of 0.2, which is quite close to our wage-based estimate of 0.188 for the intermediate value of ζ. The value of ζ is important in determining the magnitude of the agglomeration elasticity, which is unsurprising since Figure 6 showed that capital adjustment explains a smaller share of overall adjustment for higher values of ζ, leaving a larger role for agglomeration economies.

The estimates in Table 8 and the patterns in Figure 6 show that capital adjustment and standard agglomeration economies can quantitatively account for the long-run behavior of regional earnings in response to liberalization. Along with this long-run evidence, Figures 4, 7, and 8 show that regional labor and capital evolved slowly over time following liberalization and did so in a way consistent with growing earnings effects of liberalization. In contrast to the other mechanisms that we considered, dynamic labor demand, driven by slow capital adjustment and agglomeration economies, is both qualitatively and quantitatively consistent with the earnings responses in Figure 3.

V. Conclusion

This paper documents regional labor market dynamics following the Brazilian trade liberalization of the early 1990s. Using 25 years of administrative employment data, we find large and growing effects of trade liberalization on regional formal earnings and employment. Contrary to conventional wisdom, which assumes wage-equalizing labor adjustment, the regional effects of liberalization grow for more than a decade before leveling off. This pattern is not driven by post-liberalization economic shocks and is robust to a wide variety of alternative specifications. After ruling out a number of potential mechanisms that could generate these growing effects over time, we find strong evidence in support of a combination of imperfect interregional labor mobility and dynamic labor demand, driven by slow capital adjustment and agglomeration economies.

Our results have important implications for our thinking about the labor market effects of trade liberalization. A growing literature has shown in a variety of contexts that trade and trade policy have heterogeneous effects across regions in the short run. However, most researchers, ourselves included, generally assumed that these effects would be upper bounds on the long-run effects, as labor reallocation would arbitrage away regional differences. This paper finds precisely the opposite. Short-run effects vastly underestimate the long-run effects, indicating that the costs and benefits of liberalization remain sharply unevenly distributed across geography, even 20 years after the policy began.

Our empirical results also inform a large and growing literature using structural models of the labor market to study trade-induced transitional dynamics. We document the importance of regional adjustment to trade liberalization, even in the long run, and highlight margins of adjustment that have received little attention by this line of work.[58] We find evidence for slow capital adjustment in response to trade liberalization, reinforcing the message of Dix-Carneiro (2014) that jointly quantifying mobility frictions for labor and other factors such as capital is key to understanding trade adjustment.[59] We also find that agglomeration economies are quantitatively important in accounting for the magnitudes of trade's effects on regional earnings, suggesting another feature for inclusion in models examining the effects of trade shocks on labor markets.

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[58] With the exception of Caliendo, Dvorkin, and Parro (2015), this literature has abstracted from geography.
[59] Artuç et al. (2014) take an initial step in this direction.

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