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Abstract

We look at the natural experiment represented by the Brazilian trade liberalization from the early 1990s to study the effect of increased competition in the market for final goods on the racial wage gap. Changes in tariffs and initial employment structures are used to show that, in locations where there were relatively larger increases in exposure to foreign competition between 1990 and 1995, there were also relatively larger declines in the conditional racial wage gap between 1991 and 2000. The initial wage gap and its decline were more pronounced in regions with more employment in concentrated sectors. We find evidence consistent with a negative and permanent effect of increased competition in the market for final goods on discrimination in the labor market.

Keywords: discrimination, racial wage gap, competition, labor market, trade reform, Brazil
JEL Codes: J31, J71, J78, F66

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1 Introduction

This paper uses the Brazilian trade liberalization episode from the early 1990s to test whether increased competition in the market for final goods is associated with reduced wage differentials between blacks and whites in the labor market. Brazil implemented a major unilateral reduction in import tariffs between 1990 and 1995. We analyze whether local labor markets that experienced relatively larger increases in exposure to international trade also experienced relatively larger reductions in the conditional wage gap between black and white workers. As Topalova (2010) and Kovak (2013), we focus on local labor markets as the unit of analysis and use the initial structure of employment to calculate the relevant tariff reduction from the perspective of each local labor market. We then look at the impact of this exogenous change in exposure to foreign competition on the conditional racial wage gap, using an approach inspired by Charles and Guryan (2008). By combining these two strategies, we show that the increased exposure to foreign competition across Brazilian local labor markets was associated with a permanent decline in the wage differential across races.

Theories of taste-based employer discrimination predict that higher competition in the market for final goods should lead to lower discrimination against minorities in the labor market (see Becker, 1957, or the more recent treatment by Ederington and Sandford, 2016). This implication comes from the fact that discrimination in the labor market requires the existence of pure economic rents.[1] Our results are consistent with the predictions of these theories, but, in principle, could also reflect wage compression driven by trade liberalization, as long as compression operated across races conditionally on observable productive attributes.[2] We present various pieces of evidence supporting the interpretation that reduced discrimination due to reduced rents is an important part of the story.

We concentrate on the reductions in tariffs that took place between 1990 and 1995, the period of effective trade liberalization in Brazil, and look mainly at data from the 1991 and 2000 censuses (though we also analyze data from 1980 and 2010 in some exercises). The literature on local labor markets has documented that the Brazilian trade liberalization represented a shock to competition in

[1] Ederington and Sandford (2016) extend the traditional Beckerian setting and look at a dynamic model of monopolistic competition with sunk costs and sequential entry. They show that firms that discriminate less are more likely to survive in the long-run, that increased competition reduces the equilibrium level of labor market discrimination, and that the effect of increased competition is particularly strong when firms operate in more concentrated markets for final goods. Charles and Guryan (2008) test some key predictions of Becker's (1957) employer discrimination model, but do not address the effect of increased competition on the market for final goods on labor market discrimination. We review the literature on competition and labor market discrimination later on in the introduction. For the interested reader, the appendix presents a simple version of the taste-based employer discrimination model.

[2] For the case of the US, it has been documented that slightly less than half of the decline in the racial wage gap in the 1940s was due to wage compression along observable margins, while the remainder was due to reductions in wage differentials along unobservable margins correlated with race (Maloney, 1994).

the market for final goods, which was reflected on reduced local rents in the long run and on a shift of employment away from tradeable sectors, accompanied by a temporary reduction in the employment rate in the medium run (Dix-Carneiro and Kovak, 2017 and 2018). We are not interested in the effect of increased competition on discrimination through the reduced price of final goods to consumers. For tradable final goods, this effect should be homogeneous across the entire economy. Rather, we look at the effect of reduced rents for firms in the market for final goods on the equilibrium racial wage gap in local labor markets.

Our main empirical strategy is implemented in two stages. First, by running Mincer regressions, we estimate the conditional wage gap between white and black workers for each local labor market (micro-region) in 1991 and 2000. Then, in the second stage, we estimate the impact of increased openness on the racial wage gap by running, at the level of local labor markets, a regression of the estimated change in the wage gap on the change in tariffs.

The results show that the conditional wage gap between whites and blacks fell more in regions associated with larger reductions in tariffs, or, in other words, in regions that experienced larger increases in exposure to international competition. According to our preferred specification, a reduction in tariffs equivalent to the average observed in the sample (9.7 percentage points) would lead to a reduction in the racial wage gap of 18%. Our main result is robust to the composition of the sample and is not correlated with changes in returns to productive attributes, in the structure of employment, or in other observable labor market outcomes. We also show that previous changes in the racial wage gap (between 1980 and 1991) were uncorrelated with future changes in tariffs (between 1990 and 1995), so that there is no evidence of differential pre-existing trends being correlated with the degree of increase in exposure to foreign competition. We use data from the 2010 census to document that the effect of increased competition on the racial wage gap seen between 1991 and 2000 is permanent, remaining virtually unchanged up to 2010. This gives further support to our empirical strategy, since the increase in competition brought about by the trade liberalization was also a one-shot permanent shock, while other dimensions of the response of local labor markets were heterogeneous over time. Finally, in order to provide further support to this interpretation, we show that the impact of trade liberalization on the racial wage gap was stronger in locations with a higher initial share of employment in concentrated sectors.

Though we cannot rule out wage compression as a contributing factor, various dimensions of our results suggest an interpretation based on the theories of employer discrimination, such as Becker (1957) and Ederington and Sandford (2016). First, our analysis is conducted conditionally on observable characteristics, so it would have to be the case that wage compression is also operating across races conditionally on observable productive attributes. Second, the correlation between changes in tariffs and changes in the racial wage gap that we document is roughly orthogonal to changes in average returns to schooling, so wage compression would have to be operating in a way that is roughly orthogonal to changes in the price of skills, which would seem odd. Third, the change in the racial wage gap happens at once and remains roughly constant afterwards, while the labor market responses to the trade liberalization, in terms of both employment and wages, play out slowly over time (Dix-Carneiro and Kovak, 2018). So the effect of wage compression due to overall changes in labor market conditions would have to be, by chance, concentrated exclusively on the period immediately after the implementation of the reform. For all these reasons, we believe that reduced labor market discrimination is a main contributing factor to the results reported here, even though we cannot rule out some contribution also coming from wage compression. This reading of the results suggests pervasive labor market discrimination due to prejudice in the case of Brazil, a country with a highly mixed population and typically seen as racially integrated (see, for example, Theodoro, 2008).

There is a substantial literature exploring the impact of international trade on wage inequality across genders, focusing on industries instead of local labor markets as the unit of analysis. Black and Brainerd (2004) analyze the impact of increased imports on the gender wage gap in the US, while various other papers apply similar methodologies to analyze the response of the gender wage gap to trade in various countries – sometimes involving an explicit process of trade liberalization and others not – including India, Mexico, South Korea, Taiwan, and groups of developed and developing countries (Artecona and Cunningham, 2002, Berik et al., 2004, Anderson, 2005, Jacob, 2006, Oostendorp, 2009, Juhn et al., 2013, Wolszczak-Derlacz, 2013).[3] This body of research finds conflicting evidence on the impact of trade liberalization on the gender wage gap.

By looking at markets that are relatively self-contained and exploring an exogenous shock, we are arguably able to identify the change in equilibrium outcomes of specific labor markets. We concentrate on racial discrimination among prime-aged men, given the relevance of race in Brazilian society (Lovell and Wood, 1998). By focusing on prime-aged men, in addition, we make participation

[3] Jacob (2006) also analyzes the impact of trade on discrimination against lower castes, but finds no robust effect. There is also a small literature on the effect of deregulation of the banking and transportation sectors in the US on discrimination against minorities, focused mostly on gender (Ashenfelter and Hannan, 1986, Black and Strahan, 2001, Peoples and Talley, 2001, and Levine et al., 2008). Hellerstein et al. (2002) and Kawaguchi (2007) look at the relationship between variation in market power across sectors and discrimination. Zweimüller et al. (2008) analyze cross-country data and report a negative correlation between market friendly institutions – such as openness to trade and protection of property rights – and the gender wage gap.

decisions a second order issue, minimizing selection problems. So, besides relying on a clear and widely studied natural experiment – the Brazilian trade liberalization episode –, our approach differs from the literature in two other dimensions: it uses local labor markets as the unit of analysis and focuses on racial discrimination.

Because of our empirical setting and methodological choices, we can provide direct evidence in support of our key identifying assumption. We find robust evidence that increased competition in the market for final goods following the trade liberalization had a significant negative impact on racial wage gaps and show that the timing of this impact matched precisely the moment of the trade liberalization. We also show that this impact was stronger in regions with a higher share of employment in concentrated sectors, which should have suffered more from increased international competition.

The paper also speaks to a broader literature on the impacts of globalization on inequality in developing countries. Goldberg and Pavnik (2007) review this literature and do not find robust evidence supporting the predictions of the classical trade theorems. Most of the literature documents increased inequality as a result of increased openness to international trade. In the case of Brazil, controversy still persists, with some studies finding a reduction in inequality due to the 1990s trade reform, and others pointing to null or even opposite results (see, for example, Arbache and Menezes-Filho, 2000, Arbache and Corseuil, 2004, Gonzaga et al., 2006, Ferreira et al., 2010, Kovak, 2013, Dix-Carneiro and Kovak, 2015). We present evidence that the Brazilian trade liberalization has unequivocally led to reductions in labor market inequality along the racial dimension.

The remainder of the paper is organized as follows. Section 2 describes the process of trade reforms implemented in Brazil between 1988 and 1995 and discusses its impacts on local labor markets. Section 3 describes the data. Section 4 describes our empirical strategy. Section 5 presents the results of the paper. Finally, Section 6 concludes the paper.

2 The 1990s Trade Reform in Brazil

From 1957 to 1988, there was little change in trade legislation in Brazil.[4] During this period, there was widespread use of non-tariff barriers, including quotas and lists restricting the variety and quantity of goods that could be imported. The redundancy of tariffs and the existence of various additional taxes – such as, for example, the additional freight fee for renewal of the Merchant Navy –,

[4] Our description of the trade reform is based to a great extent on Kume et al. (2003). For further details on the process of trade liberalization in Brazil, refer to these authors.

besides 42 special regimes allowing for tariff exemptions or reductions, generated a heavily bureaucratic structure, distorting relative prices. As a result, Brazil had very little exposure to competition from foreign goods.

Planning of the trade reform started in 1987, during the Sarney government. But implementation was halted due to pressure from interests groups who wanted to maintain the trade barriers in certain sectors. Between 1988 and 1989, the government managed to eliminate only the redundant part of the tariff structure and reduce part of the non-tariff barriers. The process of liberalization was reinitiated under the Collor and Franco governments. Starting in 1990, non-tariff barriers and special regimes were eliminated and typically immediately replaced by equivalent import tariffs, in a process known as "tariffication." This left the actual protection structure unaltered, but effectively turned tariffs into the main instrument for trade policy. Additionally, a timeline for the gradual reduction in tariffs was approved and implemented. Initially planned to be executed until 1994, the timing was anticipated and by the end of 1993 the major part of tariff reductions had already taken place. In a further movement towards openness, the Cardoso government reduced some additional tariffs in 1994, as part of a broader effort focused on economic stabilization (Real Plan). Overall, one can see the tariffs in 1990 as accurately reflecting the historical levels of trade protection in Brazil, and the reductions in tariffs between 1990 and 1995 as capturing the main implications of the reform in terms of exposure of the domestic industry to foreign competition.

Figure 1 portrays the evolution of nominal tariffs in Brazil between 1987 and 1998 for the 10 sectors with the highest shares of employment (data from Kume et al., 2003). There is a clear pattern of generalized reduction and homogenization of tariffs up until 1994, when the minimum levels are attained in most sectors. During this period, the simple average of tariff reductions across manufacturing sectors was 43 percentage points (75%). As a result, the share of trade in the Brazilian GDP increased from around 15% in the second half of the 1980s, to 22% in 2000 (data from the World Development Indicators).

We focus on nominal tariffs throughout the paper. At the level of aggregation we use, which is the one that makes the data from Kume et al. (2003) compatible with the census, nominal tariffs are almost perfectly correlated with effective tariffs, so this decision is of no consequence to the results. Dix-Carneiro et al. (2018) report that tariff changes at the level of local labor markets calculated using output tariffs and effective rates of protection display a coefficient of correlation of 0.99.

It is worth pointing out that, as the figure makes clear, there was a very mild reversion in the trend towards increased openness after 1995. This was mostly a response of the Brazilian government to domestic pressures derived from the international financial crises of the late 1990s. In any case, this mild reversion pales in comparison to the magnitude of the reductions in tariffs from the first half of the 1990s. In effect, import tariffs remained virtually unchanged in Brazil after the reform at least until 2010, reflecting the levels reached in 1995 (Dix-Carneiro et al., 2018).

Figure 2 presents the evolution of Brazilian imports by sector from 1985 to 1999. For each sector, the real value of imports (in 1999 Brazilian Reais) is normalized to 1 in 1985 (data from Gonzaga et al., 2006). Apart from apparel and textiles, imports are stable until 1991, without any clear trend. But, starting in the first half of the 1990s, there is a sharp change in trend towards increased imports in most sectors. Even for apparel and textiles, for which imports started increasing already before 1990, there is a strong acceleration in the growth rate after 1992. Increases in imports in the short period between the early 1990s and 2000 are way above 200% for the majority of sectors portrayed in the figure. The data show that the reductions in tariffs from Figure 1 had major effects for local producers, representing real increases in exposure to foreign competition and, most likely, reductions in market power and profits.

Three characteristics of the trade reform in Brazil are particularly important for our empirical strategy. First, it was very significant and concentrated in time: in a period of roughly 5 years, trade barriers were aggressively reduced and large increases in imports were observed. Second, liberalization was driven by a centralized decision at the federal level, unrelated to economic conditions in local labor markets. And third, sectors differed greatly both in terms of the distribution of employment across regions and of tariff cuts, generating large geographic heterogeneity in the impact of the reform.

A potential concern in using the trade liberalization as a natural experiment is that reductions in tariffs might have been determined by the political influence of interest groups, which in turn might have been affected by local labor market conditions. In this hypothetical setting, tariff reductions would be endogenous to labor market conditions and the identification strategy would be compromised. Figure 3 shows that this does not seem to be the case. The figure plots, by sector of activity, the 1990-1995 tariff reduction in the vertical axis (percentage points) against the initial tariff level in the horizontal axis. The pattern shows that the reform led to a homogenization of tariffs: sectors with initially higher tariffs experienced larger subsequent reductions in tariffs. The average tariff reduction of 60% during this period was accompanied by a reduction of 53% in the sectorial dispersion of tariffs (standard deviation).

The unit of analysis in our empirical exercise is a local labor market, defined as a micro-region, not a sector of economic activity as in Figure 3. So Figure 4 reproduces the same diagram from Figure 3 but for average tariffs at the level of local labor markets (we discuss how these average tariffs are calculated in the next section). The pattern is even more extreme than that observed in Figure 3: micro-regions with initially higher tariffs experienced larger subsequent reductions in tariffs. In Figure 4, this relationship is linear and close to deterministic. Again, average reductions in tariffs in micro-regions did not seem to be determined by political influence of particular sectors or regions. Notwithstanding the evidence from Figure 4, we do consider explicitly the possibility of pre-existing trends when implementing our empirical strategy.

It is worth mentioning that the average tariff reduction by micro-regions is lower than that observed across sectors. This comes from the fact that employment shares are used to construct average tariffs by micro-region, and some sectors with large employment shares had very small reductions in tariffs after 1990 (this is the case, for example, for the agricultural sector, which accounted for half of the employment outside of the services sector; see Appendix Table A.1).

A large literature has analyzed various dimensions of labor market effects of the 1990s trade reform in Brazil. More recently, papers within the local labor markets literature – such as Kovak (2013) and Dix-Carneiro and Kovak (2015 and 2017) – analyze the medium and long-run consequences of the reform in terms of wages, employment, informality, and skill-premium. The local labor markets literature assumes that, by increasing competition in the market for final goods, the trade liberalization reduced the demand for labor in regions that had a higher concentration of employment in sectors that suffered larger reductions in tariffs (when compared to other regions). Consistent with this view, this literature documents that the labor demand shocks induced by the liberalization had large effects on wages and employment in the medium run (2000), which were followed by a recovery – virtually complete in the case of employment, but only minor for wages – in the long run (2010).

Though there is no direct evidence available on the effects of the trade liberalization on firms' profit margins or mark ups for Brazil (due to lack of data), this type of evidence has been recently documented for various other trade reforms around the world (see, for example, Edmond et al., 2015, Lu and Yu, 2015, De Locker et al., 2016). In the case of Brazil, the literature has documented a permanent reduction in the number of formal firms in markets that suffered relatively higher increases in exposure to foreign competition, consistent with an increase in competition and lower survival rates for the less efficient firms (Dix-Carneiro and Kovak, 2017 and Dix-Carneiro et al., 2018).

The labor market responses to the trade reform are also consistent with these procompetitive effects and can be seen as driven by changes in labor demand from firms facing reduced profit margins and increased competition in the market for final goods.

In order to inform our later exercise, Appendix B reproduces the main results from this literature and extends them in a few directions. For the interested reader, we advise reading Appendix B after reading Section 3, where we describe the data and the construction of the key variable used in the paper. In short, the evidence presented in the appendix is consistent with a reduction in labor demand in regions that previously enjoyed higher rents due to protection from foreign competition. As labor shifted from protected sectors and searched for employment elsewhere, the employment rate was reduced in the medium run, until the reallocation was complete. But, as local rents were permanently reduced due to increased competition in the market for final goods, the reduction in wages was permanent. Similarly, there was a permanent reduction in the number of firms operating in the formal sector and in the total mass of wages paid by these firms. Part of the reallocation of labor took place through the movement of employment from the formal to the informal sector, which also experienced a sustained increase in regions more exposed to foreign competition. Overall, the dynamic response of the local labor markets facing higher tariff cuts is consistent with the effects of a one-off reduction in rents due to an increase in competition in the market for final goods. This is the experiment we have in mind when discussing the impact of trade liberalization on the racial wage gap.

3 Data

3.1 Data Sources

The main source of data used in the paper is the Brazilian census. We use data from four rounds of the census: 1980, 1991, 2000, and 2010. The census provides typical demographic and labor market data on wages, employment, schooling, occupation, formality status, etc. The unit of observation in our analysis is a micro-region, taken to represent a local labor market. We use micro-regions as representing local labor markets, instead of municipalities, following Kovak (2013), Costa et al. (2016), Dix-Carneiro and Kovak (2015 and 2017), and Dix-Carneiro et al. (2018). Micro-regions are sets of economically integrated contiguous municipalities, sharing similar geographic and productive characteristics, defined by the Brazilian Census Bureau (*Instituto Brasileiro de Geografia e Estatística*). They approximate the notion of local economies and have been the dominant choice as unit of analysis in the literature on local labor markets in Brazil. This choice is supported by the fact that only 3.2

percent of workers lived and worked in different micro-regions in 2000 (Dix-Carneiro et al., 2018). In most specifications, we use definitions of micro-regions that are compatible across the 1991, 2000, and 2010 censuses, resulting in a total of 485 observations. When we include data from the 1980 census in some robustness and placebo exercises, we end up with 411 micro-regions consistently defined over time.[5]

The tariff data we use to characterize the trade liberalization episode is from Kume et al. (2003). Kume et al. (2003) compute average tariffs for 32 sectors directly from international trade legislation. These 32 sectors are not entirely consistent with the sectorial classification used by the Brazilian census, so we merge some of them in order to make the two datasets compatible (Appendix Table A.2 describes how the two sectoral classifications were merged). This gives us 20 sectors plus services. The tariffs of the “new” merged sectors are calculated as weighted averages of their subsectors, where the weights are given by the relative value added of each subsector.

In order to compute the micro-region-specific tariffs, we need value added and total labor earnings by sector. These are provided by the National Accounts from the Brazilian Census Bureau (IBGE). The National Accounts also provide value of production by sector, needed to compute the alternative measures of exposure to trade that we use in some robustness exercises (import-production ratio and import-penetration coefficient). Import and export data, used in these same robustness exercises, are from Gonzaga et al. (2006), while data on market concentration in Brazil, used in the heterogeneity analysis, are from Ferreira and Fachini (2005).

Finally, when characterizing the competitive impacts of the trade reform in Appendix B, we also make use of the Annual Registry of Social Information, from the Brazilian Ministry of Labor (RAIS, after *Relação Anual de Informações Sociais*). In Brazil, every registered firm is legally required to report on its employees during each calendar year. This dataset therefore provides information on the number of operating formal firms and employees in each micro-region.

3.2 Measuring Increased Exposure to Competition at the Level of Local Labor Markets

Trade policy in Brazil is determined at the federal level, so tariffs are the same for each sector irrespectively of location. But the structure of employment varies across locations, so the impact of a given reduction in tariff is not homogeneous across the territory. To take advantage of this fact and explore the differential impact of the trade reform across local labor markets with different initial structures, we follow Kovak (2013). Kovak (2013) proposes a methodology for calculating average

[5] Since municipalities’ boundaries change over time, micro-regions tend to be larger as one moves back into older censuses.

tariffs for local labor markets based on a model specifically developed to analyze the regional impacts of trade liberalization. His model treats each region (local labor market) as a specific-factors economy with two inputs: labor and an immobile factor. Labor is supplied inelastically in each region and can move across sectors, but cannot migrate across regions. The immobile factor, which we call capital here, cannot move across sectors or across regions, and represents location specific factors that augment the productivity of labor in a given industry.[6] Technology is assumed to have constant returns to scale and to vary across sectors, but not within sectors across regions. Finally, there is a single national market for the goods produced in the different regions.

This model justifies the use of a measure of tariffs at the subnational level that is similar to a formulation that was already present in the empirical literature (Topalova, 2010), but had no theoretical foundation. Consider an economy with sectors *r = 1,…, R*, where *R* represents the non-tradable sector. From the perspective of local labor market *j*, the relevant variation in tariffs between period *t-1* and *t* is

$$\Delta\left(tariff_j\right) = \sum_{r\neq R} \psi_{jr}\{ln(1 + tariff_{rt}) - ln(1 + tariff_{rt-1})\}, \quad (1)$$

where $\psi_{jr} = \frac{L_{jr}\epsilon_{jr}}{\sum_{r\neq R} L_{jr}\epsilon_{jr}}$, L_{jr} indicates employment in sector r in local labor market (micro-region) j, $\epsilon_{jr} = \frac{\sigma_{jr}}{\theta_{jr}}$ is the elasticity of the demand for labor, σ_{jr} is the elasticity of substitution between inputs, and θ_{jr} is the share of capital in total cost.

In the model, the equilibrium change in local wages is proportional to this change in average tariffs. The change in average tariffs, in turn, captures the shock to local labor demand determined by the change in equilibrium prices due to trade liberalization. We use this same variable to analyze the impact of increased competition on labor market discrimination, with the understanding that the reduction in equilibrium prices following trade liberalization comes precisely from increased competition in the market for final goods and, therefore, is intrinsically associated with reduced average profits in the tradable sector.

The relevant change in tariffs faced by a local labor market is a weighted average of the changes in tariffs experienced by the different sectors, where the weights are functions of the elasticities of labor demand and employment levels observed in each sector. Notice that the non-tradable sector is not explicitly considered in the weighted average, a result that comes directly from the theoretical

[6] These could include natural resources, land, agglomeration effects, and specific capital, as suggested by Kovak (2013).

model (in fact, according to the model, the relevant variation for the non-tradable sector is equivalent to the average variation across sectors). Since changes in employment and elasticities may be endogenous, only values from the initial period (1991) are considered in the calculation.

In practical terms, given the limited information available, some simplifying assumptions are needed. Following Kovak (2013), we assume that the technology is Cobb-Douglas, which implies a constant elasticity of substitution for every j and r: $\sigma_{jr} = 1$. Second, we assume that the share of capital in total cost (θ_{jr}) varies across sectors, but not across regions, so that $\theta_{jr} = \theta_j$. The value of each θ_j is calculated from the National Accounts as the fraction of value added not associated with labor earnings: $\theta_r = \frac{VA_r - LE_r}{VA_r}$, where VA_r is the value added in sector r and LE_r indicates labor earnings in sector r. In fact, under these additional assumptions, the incorporation of the elasticity of demand in the calculation of average tariffs is of little consequence.[7]

As discussed before, our benchmark specification uses the change in tariffs between 1990 and 1995, as Kovak (2013), because this period concentrates the part of the trade reform that effectively represented increased openness. Since we look at changes in wages between 1991 and 2000, and then between 1991 and 2010, we implicitly assume that: (i) the change in policy was perceived as permanent; (ii) the main labor market adjustments due to the trade reform were already completed by 2010; and (iii) the minor additional changes to trade legislation introduced after 1995 were not critical for labor market outcomes up to 2010. Assumptions (i) and (iii) are supported by the fact that there was little variation in tariffs after 1995, with long-term changes between 1990 and 2010 remaining highly correlated with those observed up to the moment immediately after liberalization in 1995 (correlation coefficient above 0.96 up to 2010 when looking at changes in average tariffs for local labor markets; see Dix-Carneiro et al., 2018). Still, in some robustness exercises, we also consider changes in tariffs from 1990 to 1998, from 1987 to 1995, and from 1987 to 1998.

Finally, we also test the robustness of our results to other commonly used measures of trade openness: the ratio of imports to production (M/P) and the import penetration coefficient (MPC, defined as MPC ≡ Imports/(Production + Imports – Exports)). These data are only available by sectors at the national level. We use equation 1 and apply the same strategy used for tariffs to calculate M/P and MPC at the micro-region level.

[7] Kovak (2013) reports a correlation of 0.996 between the results of calculations with and without the inclusion of ϵ_{jr}.

4 Empirical Strategy

We estimate the impact of the reduction in tariffs on the racial wage gap in two stages. First, we run individual level Mincer regressions to estimate the conditional wage gap between white and black workers in each local labor market in 1991 and 2000. Following, the estimated conditional wage gaps are used to construct the dependent variable for the second stage: the change in the wage gap between 1991 and 2000. The change in the racial wage gap is then regressed on the change in tariffs between 1990 and 1995. In the second stage, the unit of analysis is a local labor market.

4.1 First Stage

In the first stage, we estimate the conditional racial wage gap for 1991 and 2000, controlling for correlates of individual productivity. For each year t, we estimate individual level Mincer regressions by OLS. Our basic specification is the following:

$$\ln wages_{ijt} = \alpha_t + \sum_j \delta_{jt} white_{ijt} \times micro\_region_{jt} + \psi_{jt} micro\_region_{jt} + \gamma_t{}' \boldsymbol{X}_{ijt} + \varepsilon_{ijt}, \qquad (2)$$

where i indicates individual, j indicates micro-region, *wages* denote hourly earnings, *white* is a dummy for race (equal to 1 for whites and Asians, and 0 for blacks and mixed), *micro_region* is a micro-region specific dummy, $\boldsymbol{X}$ is a vector of demographic controls, and ε is a random term. In the benchmark specification, the vector $\boldsymbol{X}$ includes age, age squared, an entirely flexible function of years of schooling (one dummy for each completed year of schooling), and a dummy indicating urban residence. This same specification is estimated separately for 1991 and 2000. In robustness exercises, we estimate similar regressions for 1980 and 2010.

Our focus in the first stage is the coefficient δ_{jt}, which we call the conditional racial wage gap for local labor market j in year t. Specifically, δ_{jt} indicates the wage advantage of a white worker in comparison to a black worker with similar observable characteristics. The fact that we estimate the equation separately for each year means that parameters can change from one year to the other, reflecting potential changes in returns to productive attributes due to labor market conditions. In some robustness exercises, we run equation 2 separately for each micro-region, allowing also for the parameters in γ to vary with j. Though this specification is more flexible, allowing the model to better capture the conditions of each local labor market, it also demands much more from the data, leading to estimates of the conditional racial wage gap that are less precise in smaller micro-regions. Therefore, we use this specification only to assess the robustness of our benchmark results.

4.2 Second Stage

The estimated racial wage gap, $\hat{\delta}_{jt}$, is used to construct the change in the racial wage gap over time for each local labor market *j*: $\Delta(\hat{\delta}_j) = \hat{\delta}_{jt} - \hat{\delta}_{jt-1}$. This variable becomes the dependent variable in our second stage regression, estimated by WLS:

$$\Delta(\hat{\delta}_j) = \mu + \beta\Delta(tariff_j) + \pi_s + \lambda'\boldsymbol{W}_j + \omega_j, \quad (3)$$

where $\Delta(tariff)$ represents the change in average tariffs between 1990 and 1995, π_s represents a set of state dummies, $\boldsymbol{W}$ is a vector of controls, and ω is a random term. The controls included in the vector $\boldsymbol{W}$ capture changes in aggregate market conditions in the micro-regions, which may affect the determination of wages and, indirectly, the racial wage gap. Following Charles and Guryan (2008), the second stage regression is weighted by the precision of the first stage estimates (inverse of the standard-error of $\Delta(\hat{\delta}_j)$). Notice that, since we estimate the regression in differences, our state dummies π_s play the role of state-specific time trends, therefore controlling for any change in state-level policies or socioeconomic conditions (such as minimum wage or public education) that may affect blacks and whites differently.

Our parameter of interest in the second stage is β, which captures the impact of the change in average tariffs on the conditional racial wage gap. If $\beta > 0$, reductions in tariffs would be associated with reductions in the wage advantage that whites have in relation to blacks.

Our discussion on identification makes it clear that changes in tariffs were not driven by sectoral or local political influence and, therefore, were not endogenous to local labor market conditions. Still, there remains the possibility that changes in tariffs might have been correlated with previous or concurrent changes in labor market conditions, which in turn might affect the racial wage gap. This is the main concern in the estimation of our second stage and guides our choice of the control variables to be included in $\boldsymbol{W}$ as well as some of our robustness exercises.

First, it is important to notice that our first stage already controls for schooling, therefore netting out changes in returns to schooling. So Stolper-Samuelson-type effects, which predict an increase in the return to the relatively abundant factor (in the case of Brazil, lower levels of schooling), in principle cannot account for any remaining positive correlation between changes in tariffs and changes in the racial wage gap. Together with our state fixed effects, this means that our results are netted out of changes in returns to schooling and in state-level policies and socioeconomic conditions.

Still, unobserved skills might interfere in the relationship between changes in tariffs and changes in racial wage gaps. This would be the case, for example, if blacks had access to education of lower (unobserved) quality and there was a reduction in wage differentials across (unobserved) qualities of education. To address this potential problem, the main controls included in our vector $\boldsymbol{W}$ are changes in average wages between 1991 and 2000 by level of schooling: up to 7 years (less than elementary), from 8 to 10 years (complete elementary and high-school drop-outs), from 11 to 14 years (complete high-school and college drop-outs), and 15 years or more (college graduates).

By controlling for wages by levels of schooling, we are accounting for changes in returns to productive attributes in local labor markets. Even if we cannot measure returns to unobserved productive attributes, this strategy should go a long way towards shedding light on whether they are a threat to identification. If changes in returns to unobserved attributes tend to follow changes in returns to observed attributes – in the sense that reductions in returns to unobserved ability follow reductions in returns to schooling –, by controlling for the latter we are capturing labor market equilibrium conditions associated with overall changes in returns to skill, and indeed partially controlling for the former. Still, to address potential concerns that changes in wages by levels of schooling may have been affected by the trade liberalization itself, we present all of our main results controlling and not controlling for changes in wages by skill level.

In some robustness exercises, we also include in $\boldsymbol{W}$ other labor market changes that could be important in the presence of market imperfections and that, in principle, might have differential effects across races. First, there was continuous improvement in schooling levels in Brazil during this period. So we also control directly for the change in the supply of workers by skill level and race (share of workers by years of schooling and race, using the same educational classification discussed before). In addition, to account for investments in technology that may have affected the return to manual work, we control for the share of blue collar workers. If there is some technological shock leading to changes in the demand for different types of labor, one should expect it to be partly reflected on changes in the relative share of blue collar workers. We also control for unemployment and informality rates (among salaried workers), to account for other margins of labor market adjustment, and for migration, which might affect the response of local labor markets to exogenous shocks. These latter labor market variables are partially functions of the trade liberalization itself, so they do not strictly belong to the right-hand side of our estimating equation. Still, we believe that their inclusion in a few specifications helps shed light on the type of variation that is driving our results. Overall, the different sets of controls included in $\boldsymbol{W}$ try to account for broader patterns in the Brazilian labor market that could be correlated with changes in wage differentials across races.[8] Finally, in some exercises discussed in further detail in the results section, the vector $\boldsymbol{W}$ includes pre-trends of the dependent variable and participation rates by race.

Our main specification uses a sample of male employees (excluding public servants, self-employed, employers, and domestic workers), with positive earnings, aged between 20 and 60. We choose to focus on prime aged male employees to minimize issues of heterogeneity and selection into the labor market. Under these restrictions, there are 1.8 million observations in the 1991 census sample and 2.3 million observations in the 2000 census sample. To assess the robustness of the results to potential market imperfections associated with labor market attachment, insertion, and mobility, some alternative samples are also considered in the appendix. For example, we present results including self-employed men, women, and restricting the sample to full-time workers.

Table 1 presents descriptive statistics for the 1991 and 2000 censuses, based on our main sample (male employees between ages 20 and 60, with positive earnings). In addition to providing a broad overview of the labor market conditions in Brazil during the period of our analysis, the table also helps guide our later discussion of the results.

The typical individual in the sample in both years has around 34 years of age, works full time (more than 90%), did not complete elementary school, works in the services sector, and in a blue collar occupation. It is worth noticing the reduction of 7 percentage points in the fraction of workers with less than complete elementary education between 1991 and 2000, the increase in the fraction of workers attending school, and the reduction in the share of workers in manufacturing. Average real wages are approximately stable during the period, due mostly to the change in the composition of the labor force, since wages fell for most educational levels (with the exception of college, which comprises a small fraction of the population; real wages in 2000 values, deflated by the National Consumer Price Index, following Corseuil and Foguel, 2002).

The last rows in the table present numbers on the trade variables used and on the conditional racial wage gap.[9] The average tariff reduction across micro-regions was 9.7 percentage points

[8] The local labor market variables introduced as controls in the second stage are constructed directly from census files. We define blue collar occupations as those that typically do not require formal (technical or college) training or education, as opposed to professional occupations. In the 1991 census, we define blue collar occupations as those associated with codes 301-928. These include, among various others, fishermen, miners, mechanics, shoemakers, bricklayers, merchandise packers, sellers, cashiers, drivers, cleaners, and dustmen. Informal employees are defined as those who do not have a registered labor contract (or, in terms of the Brazilian legislation, do not have their "labor card" signed by the employer). Regarding migration, census data allow us to calculate the percentage of the population that immigrated to a given micro-region within the previous five years.

[9] The conditional racial wage gap in the table is the average of the gaps estimated for each micro-region in our first stage.

(corresponding to 48% of the initial level), and was accompanied by increases of 85% in the ratio of imports to production and 81% in the import penetration coefficient. The seemingly small initial level of tariffs when looking at micro-region averages, when compared to Figure 1 for example, comes from the role of the agricultural sector. Tariffs were already low in agriculture by 1990 and, excluding the services sector, agriculture employed a substantial fraction of the labor force (see Appendix Table A.1). Irrespectively, the reduction in tariffs represented a substantial change in exposure to foreign competition, which indeed ended up reflected on the measures of import penetration.

It is also important to notice that the conditional racial wage gap remained roughly stable in Brazil during this period (in fact, it was reduced by 0.4 percentage point). This highlights the fact that we are exploring the role of increased competition in reducing the racial wage gap in a context where there is no widespread trend in this direction.

5 Main Results

We concentrate on the results from the second stage, since our first stage reproduces commonly used estimation procedures for Mincer regressions. Still, when useful, we briefly mention the specification used to estimate the racial wage gap in the first stage.

Table 2 presents the main results from our empirical exercise. Column 1 shows the coefficient of a univariate regression of the change in the conditional racial wage gap on the change in tariffs, without additional controls. Column 2 introduces state dummies (26 states plus the Federal District), and column 3 adds controls for changes in earnings by level of schooling (primary, elementary, high school, and college). Panel A corresponds to our benchmark specification, where a single Mincer regression is used to estimate the racial wage gap for all micro-regions in a given year, while Panel B corresponds to an alternative specification where a different Mincer regression is estimated separately for each micro-region in each year.

Column 1 in Panel A shows that there is a positive correlation between changes in tariffs and changes in the conditional racial wage gap. This means that local labor markets that experienced larger reductions in average tariffs also experienced larger reductions in the conditional wage differential across races (remember that our race dummy indicates white workers). The introduction of state dummies in column 2 increases the magnitude of the estimated coefficient. Several policies in Brazil are either formulated or ran by states, so controlling for state fixed effects is particularly important in this specification. Since we estimate our regressions in differences, the state dummies play the role of state-specific time trends, therefore capturing any state-level change – driven by policy or otherwise – that might affect blacks and whites differently.

In column 3, we control for changes in wages by levels of schooling, which should account for changes in returns to skills. If the positive and significant coefficient on *Δ(tariff)* in columns 1 and 2 were only capturing a relative change in the return for unskilled labor – with blacks being less skilled than whites – one should expect to see a reduction in the estimated coefficient as we move from column 2 to column 3. Notice that this should be the case even if the coefficients estimated in columns 1 and 2 were related to unobserved skills, as long as the change in returns to unobserved skills followed the same pattern of the change in returns to observed skills (meaning that, on average, locations with reduced returns to schooling also displayed reduced returns to unobserved ability). But once we control for changes in earnings by level of schooling, the coefficient barely changes and is estimated even more precisely than before, remaining strongly significant. The correlation between changes in tariffs and changes in racial wage gaps captured by our empirical strategy is not correlated with changes in returns to productive attributes. This should be expected, since our first stage already controls for schooling and allows for changes in returns to schooling between 1991 and 2000.[10]

The regression from column 3, including state dummies and changes in wages by level of schooling, is the benchmark specification that we focus on throughout the paper. But we also present all subsequent results with the specification from column 2. In virtually all the regressions estimated, results are very similar across these two specifications, so the decision related to the inclusion of these controls is of no consequence to the main point of the paper.[11]

[10] Notice that the coefficient on the change in primary schooling wages in column 3 is negative and statistically significant. This is consistent with the argument made in the text that increases in the relative gains of low skill workers in terms of observed attributes (in the case of Brazil, primary educated workers, in which blacks are overrepresented) should be correlated with relative gains to low skill workers also in terms of unobserved attributes, which should in turn be correlated with reductions in the conditional racial wage gap. Still, this is orthogonal to the relationship between reductions in tariffs and reductions in the racial wage gap and, therefore, does not interfere with our results.

[11] The results from Table 2 remain similar if we run the same specifications without weights, as long as we trim outliers (very small micro-regions for which the change in the racial wage gap is estimated with a lot of noise; results available from the authors upon request). For the interested reader, Appendix Table A.3 presents yet another alternative specification, where we estimate the impact of the reduction in tariffs directly, in one single step, together with the Mincer equation. This strategy estimates a single Mincer regression including both years and adding year and micro-region dummies. In this case, akin to a difference-in-differences, the effect of the tariff reduction on the racial wage gap is identified from the interaction of the micro-region-specific tariff (which changes between years) with the race dummy. To come as close as possible to our two-stage strategy, we include the same individual and aggregate (micro-region) level controls used in our previous first and second stages and let the coefficients on the individual level variables vary across years. Table A.3 reproduces specifications analogous to those from Tables 2 and 3 using this strategy. Estimated coefficients are positive, statistically significant, and very similar in magnitude to the respective coefficients in Tables 2 and 3 (if anything, slightly larger).

Panel B in Table 2 reproduces the same sequence of results from Panel A, but using a separate Mincer regression in the first stage to estimate the conditional racial wage gap in each micro-region and year.[12] This strategy is more flexible in that it allows for returns to productive attributes – such as education or experience – to vary across micro-regions in the same year, therefore better capturing the specific characteristics of the equilibrium in each local labor market. On the other hand, it demands much more from the data, since estimating a wage equation in micro-regions with few observations can lead to a lot of noise. Results in Panel B are similar to those from panel A, though point estimates are typically smaller by 0.07 and less precisely estimated. Most importantly, the qualitative pattern of change in coefficients as we move from columns 1 to 3 remains the same, so the discussion related to Panel A applies here as well.

We check the robustness of our results to other labor market changes that could be important in the presence of market imperfections and that, in principle, might have differential effects across races. Some of these may be functions of the phenomenon we are analyzing and may not strictly belong to the right-hand side of our estimating equation. So we incorporate these additional variables in Table 3 but do not carry them over to other specifications in the paper.[13]

Column 1 in Table 3 includes controls for the shares of employment by level of schooling and race (where the excluded category is primary schooling). Column 2, instead, controls for labor market changes associated with occupational structure (share of blue-collar occupations), informality, and unemployment. Finally, column 3 adds the control for migration and column 4 includes all previous controls simultaneously.

In columns 1, 2, and 3 of Table 3, results are close to those from Table 2, though coefficients vary a bit in magnitude across specifications. In column 1, the coefficient is slightly smaller in magnitude, but remains strongly significant, while it is slightly larger, and still significant, in columns 2 and 3. Most importantly, when all controls are included simultaneously in column 4, the coefficient is again very similar to that estimated in column 3 of Table 2 and remains statistically significant.

Concerns related to changes in schooling or access to education, to other labor market patterns, to migration, or to broader macroeconomic trends – which in principle might have heterogeneous impacts across races – do not seem to be first order issues. Some of these dimensions did affect the

[12] The equation estimated for each micro-region j and year t is: $\ln(wage)_{ijt} = \alpha_{jt} + \delta_{jt} white_{ijt} + \gamma_{jt}' \boldsymbol{Z}_{ijt} + \varepsilon_{ijt}$, where the vector $\boldsymbol{Z}$ includes all variables included in $\boldsymbol{X}$.

[13] In any case, the vast majority of qualitative results reported in other tables in the paper remain unchanged under any of the other specifications from Table 3. These results are available from the authors upon request.

racial wage gap during the 1991-2000 period, but in ways that are on average orthogonal to the relationship between competition in the market for final goods and the racial wage gap.

The discussion on the labor market impacts of the trade reform in Appendix B shows that employment responses to the trade liberalization were similar across races, so that compositional changes across black and white workers are unlikely to be a relevant factor in our analysis. Nevertheless, we address this issue explicitly in Appendix Table A.4, where we replicate the main results from Table 2 controlling for differential changes in employment across races in our second stage. Results remain very similar to those discussed before.

Finally, we also re-estimate our entire procedure controlling for informality and sector of employment in the first stage. The results from these exercises are presented in Appendix Table A.5. Racial discrimination may partly work through restricted access to better paying jobs in protected and more formalized sectors, so this specification overcontrols for factors that could be partly manifestations of discrimination itself. Still, it is informative as to what extent the reduction in discrimination took place through reductions in differential access to better paying sectors, or through reductions in wage differentials within sectors. Panel A in Appendix Table A.5 shows that results remain virtually identical when we control for informality status alone in the first stage. So the composition of employment across formal and informal sectors by itself does not help explain the reduction in the racial wage gap. Panels B and C, on the other hand, show that the sectoral composition of employment does seem to have played a role in this process. When we control for sectoral dummies, either alone or together with informality, our results remain positive and statistically significant, but point estimates are reduced. Taking the coefficient in column 3 of Panel B at face value would imply that roughly 40% of the reductions in the racial wage gap were due to changes in the structure of employment across sectors, while 60% were due to reductions in the wage gap within sectors.[14]

[14] With imperfect labor mobility and differential entry and exit, the impact of increased competition on wages may be heterogeneous across groups of workers. To assess this possibility, Appendix Table A.6 also estimates our first stage with different samples (always restricting to individuals between 20 and 60 years of age): all male workers, all male and female workers, all male and female employees and self-employed, all male and female employees, and all male employees not attending school and working full-time (at least 35 hours per week). When we look at all men, results are larger in magnitude than those from Table 2 and still significant. This is consistent with part of the labor market adjustments after the reform taking place through differential transition of workers across occupational categories (most importantly, between employees and self-employed), but it may also be simply a result of more precisely estimated racial wage gaps in the first stage due to larger sample sizes. As we move to other samples, considering all men and women and then employees and self-employed, and only employees, the coefficient remains strongly significant. Finally, when we consider only men who do not attend school and are employed full-time, coming even closer to inelastic labor supply, results rise again in magnitude and remain statistically significant. Differences in sample across genders, labor market insertion, and labor market attachment do not seem to affect the qualitative results. This pattern suggests a reasonable degree of flexibility in local labor markets in Brazil, which is supported by evidence of a high degree of mobility across the formal and informal sectors (see, for example, review in Ulyssea, 2006).

Our benchmark specification (column 3 in Table 2) implies that a reduction in average local tariffs higher by 9.7 percentage points (equivalent to the average observed in the sample) leads to a relative reduction of 2.3 percentage points in the conditional racial wage gap (or 18% of its 1991 value, which was 12.3). Alternatively, a reduction in tariffs higher by one standard deviation in the initial period (7 percentage points in 1991) would lead to a relative reduction of 13% in the racial wage gap. Though this effect may not seem particularly large, one should bear in mind that over the 1991-2000 period there was hardly any reduction at all in the conditional racial wage gap in Brazil (0.4 percentage point). In other words, according to our estimates, had the liberalization process not taken place, the conditional racial wage gap would have increased by 1.9 percentage points (abstracting from aggregate general equilibrium effects of the trade liberalization on the racial wage gap, which are not captured by the difference-in-differences logic of our empirical strategy). Increased competition may have helped reverse a widening of the racial wage gap that could otherwise have occurred.

It is also worth noting that the impact of increased competition on the racial wage gap documented in Table 2 summarizes almost in its entirety the effects observed in the long-run. Table 4 presents the results from a regression that reproduces our same benchmark exercise from Panel A in Table 2 looking at the 1991 and 2010 censuses (instead of 1991 and 2000). The coefficients estimated for this extended 20-year period are very similar in magnitude to – and statistically undistinguishable from – those in Table 2. In particular, when we look at the specification from column 3, we see a long-run effect (0.256) that is only slightly larger than the medium-run effect detected before (0.233).

In line with the discussion on the labor market and competitive impacts of the trade reform, this pattern reinforces the idea that we are documenting the response of labor market discrimination to a once-and-for-all increase in competition in the market for final goods. The evidence indicates that the trade liberalization led to a reduction in the racial wage gap that persisted over time, consistent with the idea that rents were permanently reduced in these local labor markets. As documented in Appendix B, the labor market responses in terms of employment, wages, informality, and other aspects of these local economies were not constant over time, due to labor market frictions in the reallocation of labor across sectors. These different dynamic patterns are particularly relevant because they make alternative explanations based on wage compression due to changes in overall labor market conditions less appealing, since one would normally expect those to operate for as long as the labor market dynamics were still playing out. In addition, in principle, the effects of wage compression would be expected to be correlated with changes in returns to skills and in the composition of the labor force, but the results discussed in this section show that this was not the case in our setting. Overall, the evidence presented up to now suggests that reduced discrimination due to reduced rents seems to have been a key driving force. Still, we cannot rule out entirely that wage compression may have played a role in the results.

5.1 Alternative Timing and Measures of Trade Liberalization

Our first robustness exercises consider alternative timings and measures of trade liberalization. The benchmark specification uses changes in tariffs between 1990 and 1995, corresponding to the period of effective liberalization (see discussion in section 2). One might think that this would exaggerate the extent of liberalization, possibly biasing our estimates. To address this concern, in Table 5 we consider alternative specifications that use the change in tariffs between 1990 and 1998, between 1987 and 1995, and between 1987 and 1998 (Panel A controls for changes in wages by level of schooling and Panel B does not).[15] The results are presented in columns 1-3 of Table 5. In column 1, the estimated coefficient remains almost identical to that in Table 2 (column 3). In columns 2 and 3, it increases in magnitude and remains strongly significant. So the specific timing of the measurement of the change in tariffs does not seem to interfere with the main results.

Other concern related to the measurement of the extent of the reform refers to the use of tariffs as sufficient statistics for trade liberalization. Various other dimensions of economic policy and regulation affect the effective degree of protection in a given economy, including non-trade barriers, exchange rate regimes, and red tape. For these reasons, some consider that variables related to the flow of international trade are more adequate measures of the actual degree of openness in an economy. In fact, Brazil experienced changes in exchange rate regimes during the 1990s as well as successive elimination and reintroduction of non-tariff barriers. This concern may therefore be indeed legitimate. In addition, as argued by Gonzaga et al. (2006), the pass through of tariff changes to prices may vary across sectors. Still, trade flows are endogenous to economic and labor market conditions and this limits their use in this type of exercise.

In any case, we also consider alternative measures of exposure to foreign competition based on trade flows: the ratio of imports to production (M/P) and the import penetration coefficient (MPC). The results obtained when changes in these variables are used as independent variables are presented in columns 4 and 5 in Table 5. It is worth remembering that, contrary to tariffs, increases in these variables indicate increased exposure to international trade. So the prediction of the theory is that

[15] In terms of coverage, 1987 is the first and 1998 the last year for which consolidated data on tariffs by sector are computed by Kume et al. (2003). To calculate average tariffs for the merged sectors (see section 5.1) in 1998, we use value added from the 1995 National Accounts, and, for 1987, we use value added from the 1985 National Accounts.

these two variables should be negatively associated with changes in the racial wage gap (increased exposure to international trade associated with reduced advantages of whites in the labor market).

Consistent with the results for tariffs, both measures of trade flows indicate that increased imports during the period of trade reforms were associated with reductions in the conditional racial wage gap. The coefficients in columns 4 and 5 are negative and statistically significant. Though the scales of the three independent variables are different and, therefore, estimated coefficients cannot be directly compared to one another, their quantitative implications in normalized units are very similar. An increase in M/P corresponding to one standard deviation in the initial period (0.011 in 1991) would be associated with a reduction of 13% in the conditional racial wage gap, a result identical to those obtained with the MPC variable and with tariffs. In other words, the specific variable used to represent the process of trade liberalization does not affect the results either qualitative or quantitatively.

5.2 Pre-trends and Falsification Exercises

The timing of the reform and the measures of exposure to trade based on flows provide us with an opportunity to falsify our identification strategy. We do that in Table 6 (presenting results controlling and not controlling for changes in wages by schooling levels, in Panels A and B respectively).

First, we account for pre-trends by controlling for the change in the racial wage gap before the trade reform (between 1980 and 1991). Since there was a different political organization of Brazil in 1980, we are able to reconstruct 411 micro-regions in this analysis (micro-regions were aggregated to be made compatible across 1980 and 2000, and there were fewer municipalities and micro-regions in 1980 than in 1991). For purposes of comparison, we first estimate again the specification from column 3 in Table 2 using data from 1991 and 2000 with this new geographic division including 411 micro-regions (instead of 485). Results, shown in column 1 from Table 6, remain positive, statistically significant and of similar magnitude. So the different definition of micro-regions does not substantially affect the results obtained before. In columns 2, 3, and 4, we estimate our benchmark specification with the three alternative measures of trade liberalization controlling for pre-existing trends (the change in the racial wage gap between 1980 and 1991). Results remain very similar, both in terms of significance and magnitude.[16]

[16] Notice that there is a mechanical correlation between the dependent variable (change in the racial wage gap between 1991 and 2000) and the pre-trend variable (change in the wage gap between 1980 and 1991) in columns 2 to 4, since the wage gap in 1991 is used in the construction of both. For example, any measurement error in the wage gap in 1991 would

Another falsification exercise that allows us to assess whether pre-existing trends seem to be a problem is to test if the change in the racial wage gap between 1980 and 1991 was correlated with (future) changes in exposure to foreign competition between 1990 and 1995. If the trade reform was truly exogenous to local changes in the racial wage gap, one should expect such regression to yield small and non-significant coefficients. Otherwise, if the change in exposure to trade in the 1990s was associated with labor market trends before that date – which might have continued into the future – this regression might yield significant results.

Columns 5, 6, and 7 in Table 6 show the results from these regressions, in which the change in the racial wage gap between 1980 and 1991 is regressed on our three measures of change in exposure to foreign competition between 1990 and 1995. All estimated coefficients are very small in magnitude and far from statistically significant. Pre-existing trends indeed do not seem to be a concern in our empirical exercise. Together with our previous results on the medium and long-run effects of the trade liberalization, this result provides further support to our identification strategy and interpretation. The estimated impact of the reduction in tariffs between 1990 and 1995 on the racial wage gap is zero before the trade liberalization actually took place (1980-1991) and then basically constant afterwards (1991-2000 and 1991-2010). This is once more in line with the idea that we are capturing the response of labor market discrimination to a once-and-for-all increase in competition in the market for final goods.

Finally, we can perform an additional placebo exercise to reinforce the role that the natural experiment represented by the 1990s trade reform plays in our identification strategy. If our identification strategy is indeed capturing the effect of the trade reform from the 1990s, we should find no significant result once we repeat an analogous exercise using data from 1980 and 1991, since there was no major change in trade protection during this period. Otherwise, if we are just capturing some spurious correlation between changes in imports and changes in labor market outcomes, we should also find a significant effect when looking at data between 1980 and 1991. Though we do not have changes in tariffs by sector for the 1980s (there were no substantial changes between 1980 and 1990), we do have data on imports, exports, and production.

Columns 8 and 9 in Table 6 reproduce the same exercise from Table 2 but comparing data from the 1980 and 1991 censuses and using the two measures of exposure to trade that we have for 1980 and 1990 (M/P and MPC). Both estimated coefficients are small in magnitude and far from statistically

lead to a negative correlation between these two variables. So we do not attach much weight to the negative and significant coefficients estimated for the change in the wage gap between 1980 and 1991 presented in the table. The most important point is that the estimated impact of the change in tariffs is not affected by the inclusion of this variable.

significant. There is no indication of a spurious correlation between changes in international trade and changes in the racial wage gap before the reforms were implemented. This evidence suggests that the reduction in the racial wage gap in response to tariff reductions (or increases in imports) documented in Table 2 is indeed associated with the shock represented by the process of trade liberalization from the 1990s.

In this section, we provided evidence supporting the key identifying assumption underlying the use of our natural experiment. No other paper that we are aware of in the literature on competition and wage differentials was able to validate its empirical strategy in a similar fashion.

5.3 Industry Concentration and the Response to Increased Competition

We also explore the heterogeneity in the response to increased exposure to foreign competition to shed further light on the mechanism behind the effects estimated with our benchmark specification. If this effect is indeed operating through the reduction of rents in the market for final goods, as predicted by the theories of employer discrimination (Becker, 1957, Ederington and Sandford, 2016), local labor markets dominated by firms that initially faced lower competition in the domestic market should respond more to liberalization than markets with firms that faced more competition. This prediction comes directly from the fact that discrimination requires the existence of pure economic rents.[17]

We explore this dimension of heterogeneity and assess whether the initial level of the racial wage gap and the impact of increased liberalization on the wage gap were indeed stronger in labor markets dominated by firms with more monopoly power in the market for final goods. In order to measure the degree of monopoly power in the market for final goods, we follow Ferreira and Fachini (2005) and use the four-firm concentration ratio (CR4), which considers a sector to be concentrated if the share of the four largest firms in total revenue is above 40%.[18] We use the classification of concentrated sectors from Ferreira and Fachini (2005) and, to translate it to the level of local labor markets, calculate the share of workers occupied in concentrated sectors in each micro-region.

In order to make coefficients more easily interpretable, we standardize the measure of market concentration (subtract the mean and divide by the standard deviation). Table 7 starts in column 1 by

[17] Ederington and Sandford (2016), for example, develop a dynamic monopolistic competition model with firm entry and exit and show that the response of labor market discrimination to increased competition in the market for final goods should be unequivocally stronger in more concentrated markets.

[18] Ferreira and Fachini (2005) classify as concentrated the following sectors in 1985: transportation, rubber, chemicals, perfumery, and tobacco. The authors do not analyze extractive sectors. Since petroleum, natural gas, and charcoal are monopolies or concessions, they are also considered concentrated.

showing the result of a simple micro-region OLS regression for 1991, where the conditional racial wage gap is regressed on market concentration and state dummies. This is simply a descriptive exercise and should not be interpreted causally. Nevertheless, as predicted by the theories of employer discrimination, market concentration is positively and significantly correlated with the initial racial wage gap.

In columns 2 and 3, we incorporate heterogeneity along this dimension into our main empirical exercise by including an interaction of the change in tariffs with the index of market concentration in our benchmark specification. The estimated coefficient on the interaction term is positive and statistically significant. Local labor markets dominated by firms operating in more concentrated markets for final goods experienced larger declines in the racial wage gap following the trade reform from the 1990s. According to the point estimates, a level of concentration one-standard deviation above the mean in 1991 was associated with a reduction in the racial wage gap more than two times larger than that observed at average levels of market concentration.[19]

Both the cross-sectional distribution of the conditional racial wage gap and its response to the reduction in tariffs are consistent with the idea that higher market concentration should be associated with more discrimination and with a stronger impact of increased exposure to competition. These results agree with our interpretation of the mechanism behind the reduction in racial wage gaps in a way that, given the body of evidence presented in the paper, is otherwise difficult to rationalize.

6 Concluding Remarks

We use the episode of trade liberalization in Brazil during the 1990s to present evidence on the effect of increased competition in the market for final goods on the racial wage gap. We show that local labor markets that experienced relatively higher exposure to international competition due to trade liberalization also observed relatively larger reductions in the conditional wage differential between white and black workers. As predicted by the theory of taste-based discrimination, the initial racial wage gap and the impact of increased competition were larger in local labor markets dominated by firms in more concentrated sectors.

[19] Unfortunately, one cannot analyze directly the impact of trade liberalization on market concentration in Brazil because the Brazilian industrial surveys changed exactly in 1995. So the data available before 1995 are not comparable to the data available after 1995. This is the reason why none of the large number of papers on the effects of the Brazilian trade liberalization episode analyzed directly the impact of the reform on, for example, industrial concentration or mark-ups. Pre-1995, the data come from the Brazilian Industrial Censuses, and, post-1995, from the Annual Industrial Surveys. These two datasets are different along various dimensions, including coverage and variables.

Our empirical setting delivers a clean identification of the effect of increased exposure to foreign competition on racial wage gaps and provides direct evidence validating the identification hypothesis. Our results also incidentally suggest that labor market discrimination due to racial prejudice is a prevalent phenomenon in the case of Brazil, a highly mixed country often regarded as racially integrated. By analyzing wage differentials across races, our paper also speaks to the broader literature on trade liberalization and inequality, and identifies a specific dimension over which increased openness contributed to reduce earnings inequality in Brazil.

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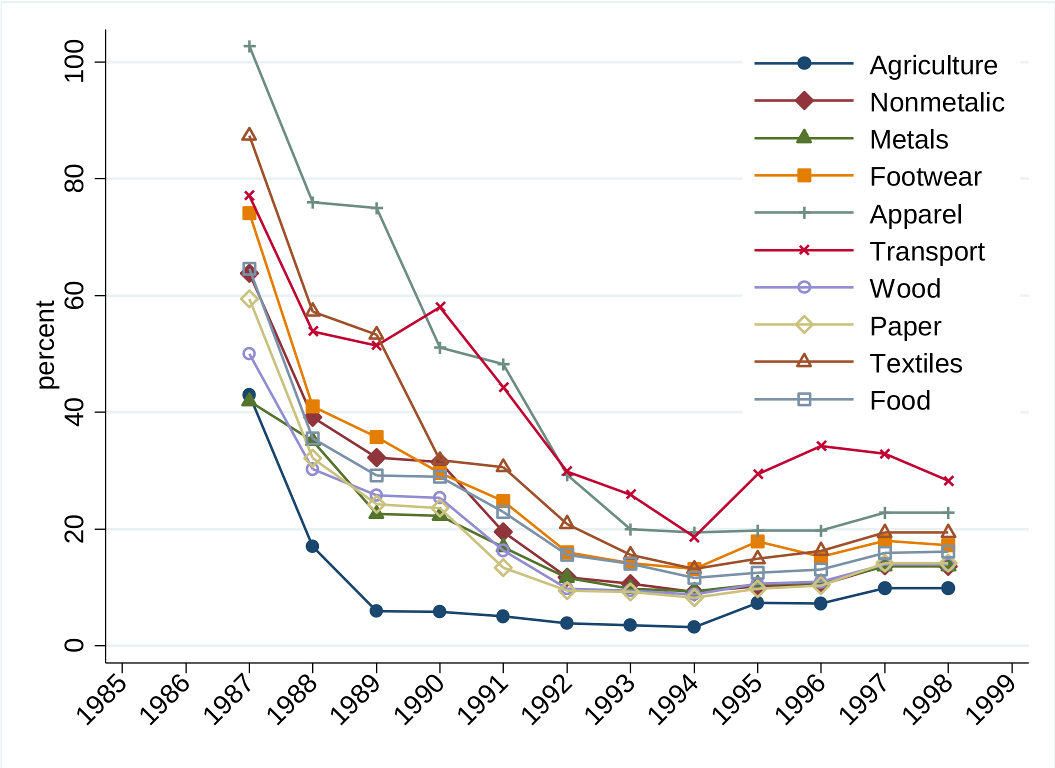
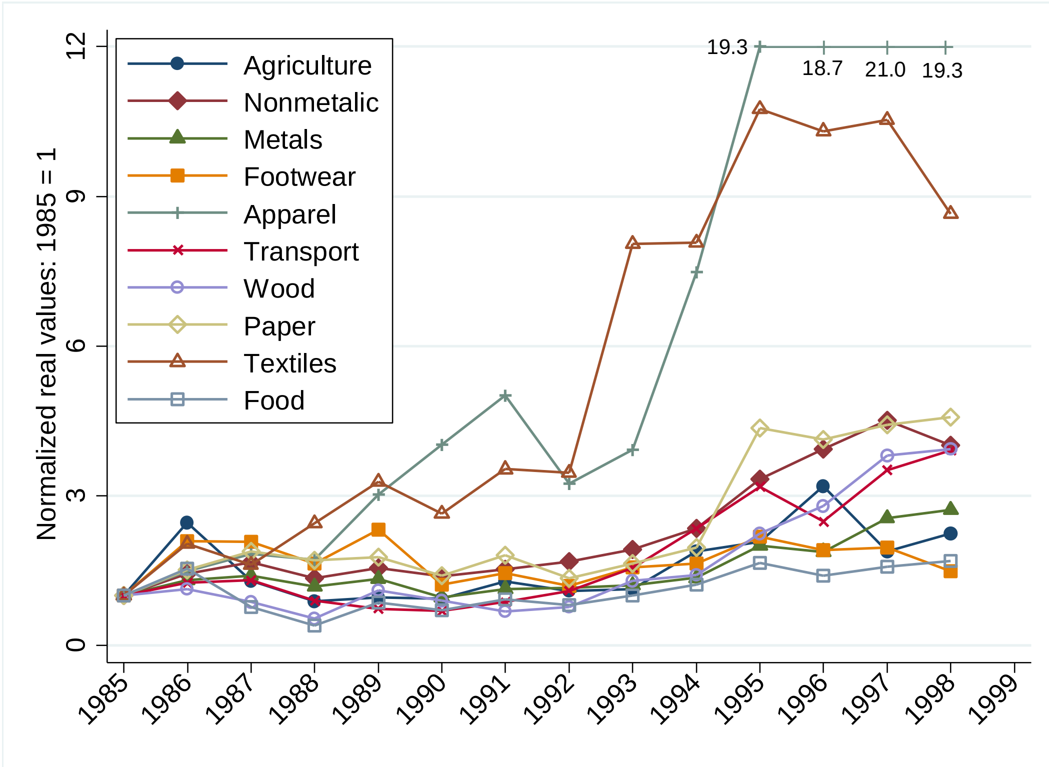
Figure 1: Nominal Tariffs during the Late 1980s and 1990s, Brazil



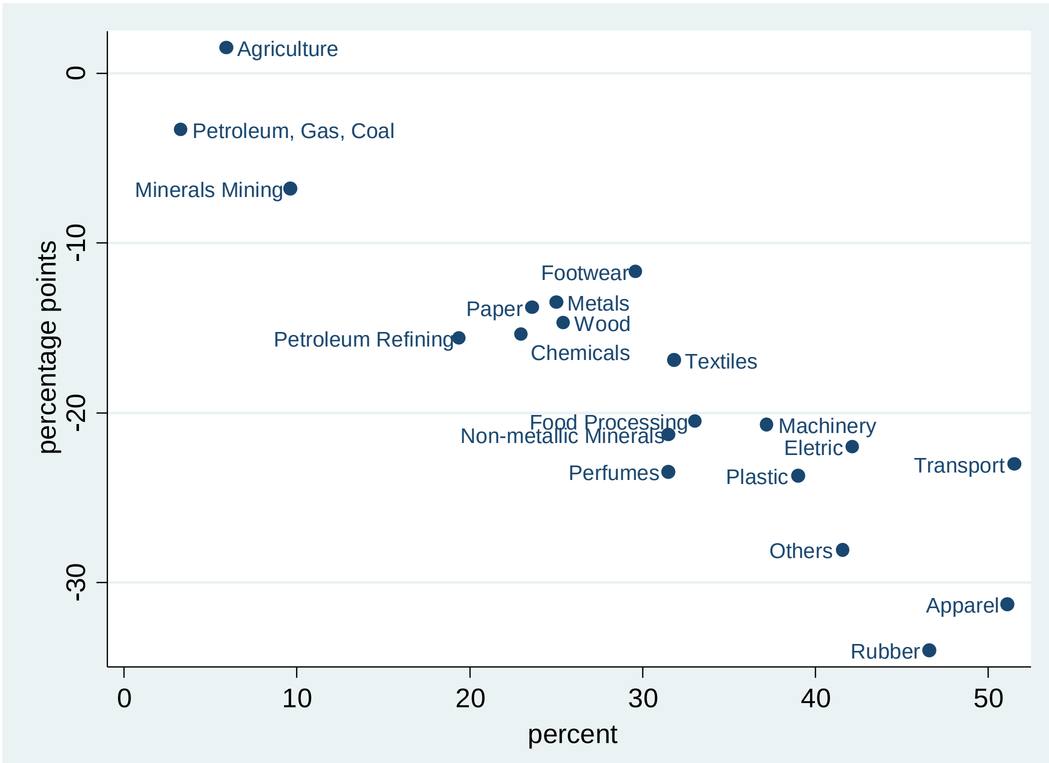
Source: Data from Kume et al. (2003).

Figure 2: Imports during the Late 1980s and 1990s, Brazil (1985 value normalized to 1)



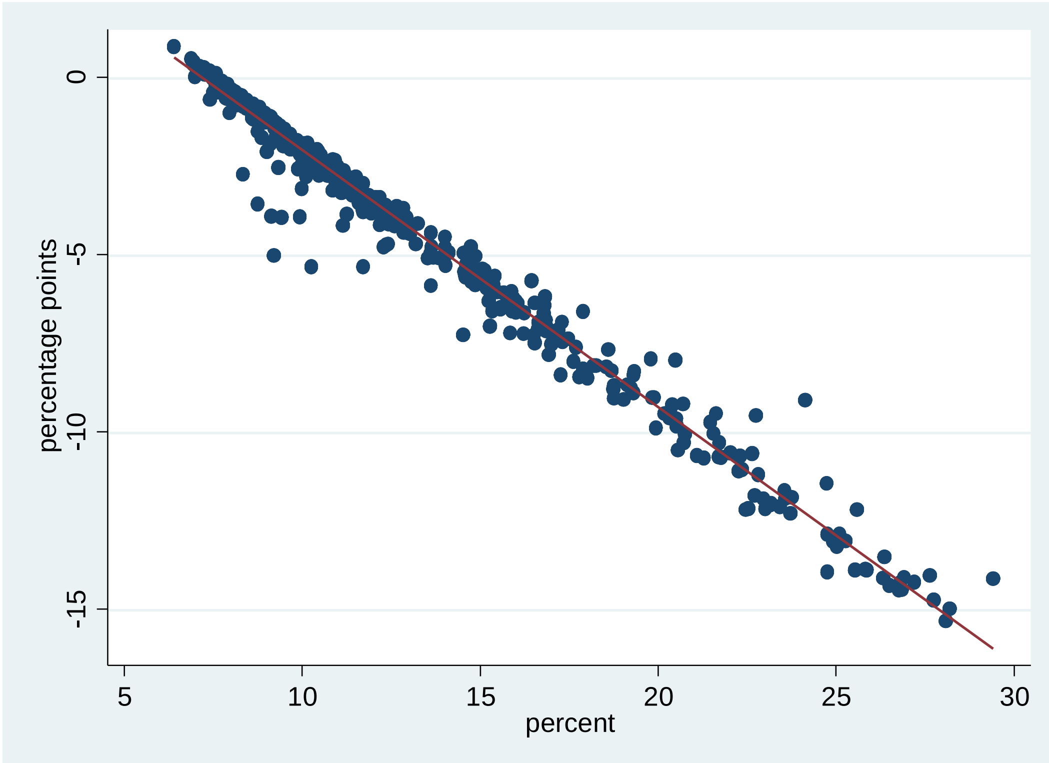
Source: Data from Gonzaga et al. (2006).

Figure 3: Tariff Reductions between 1990 and 1995 and Initial Tariff Levels in 1990 by Sector of Economic Activity, Brazil



Source: Data from Kume et al. (2003).

Figure 4: Tariff Reductions between 1990 and 1995 and Initial Tariff Levels in 1990 by Micro-Region, Brazil



Source: Authors' calculations based on data from the 1991 census and from Kume et al. (2003).

Table 1: Descriptive Statistics, Brazil, 1991 and 2000, Male Employees, ages 20-60

	1991		2000	
	Mean	s.d.	Mean	s.d.
Black	0.445	0.497	0.451	0.498
Age	33.7	10.0	34.1	10.2
Full-time Work (35 hours/week or more)	0.945	0.227	0.928	0.258
Migrant	0.105	0.306	0.087	0.282
Attend school	0.045	0.207	0.091	0.288
Primary School (up to 7 years of schooling)	0.621	0.485	0.551	0.497
Elementary School (8 to 10 years of schooling)	0.154	0.361	0.175	0.38
High School (11 to 14 years of schooling)	0.163	0.369	0.216	0.412
College/University (above 15 years of schooling)	0.062	0.242	0.058	0.234
Blue collar	0.786	0.41	0.816	0.388
Informal	0.196	0.397	0.332	0.471
Agriculture	0.116	0.32	0.148	0.355
Mineral Mining	0.016	0.127	0.008	0.089
Manufacture	0.278	0.448	0.208	0.406
Services	0.59	0.492	0.636	0.481
Wage per hour (R$)	3.37	6.41	3.29	7.29
Wage per hour - Primary School	1.92	3.18	1.80	3.14
Wage per hour - Elementary School	3.09	4.39	2.73	4.15
Wage per hour - High School	5.29	6.81	4.62	7.56
Wage per hour – College/University	13.52	16.04	14.34	20.23
Observations (millions)	1.8		2.3	
Local Market Characteristics*				
Mean tariff **	0.202	0.07	0.105	0.021
Imports/Product (M/P)**	0.026	0.011	0.048	0.023
Import Penetration Coefficient (MPC)**	0.027	0.011	0.049	0.022
White-Black conditional wage gap***	0.123	0.072	0.119	0.052

Notes: * Average across micro-regions (488), weighted by sample size in each micro-region. ** Calculated for 1990 and 1995. *** Average racial wage gap across micro-regions estimated following our first stage strategy. Numbers based on census data from 1991 and 2000. Real wages in 2000 values (deflated by the National Consumer Price Index, following Corseuil and Foguel, 2002).

Table 2: Impact of Tariff Change on Racial Wage Gap, Brazilian Micro-regions, 1991-2000 – Dependent Variable: Change in Conditional Racial Wage Gap

	(1)	(2)	(3)
Panel A: One Regression in the First Stage			
Δ(tariff)	0.152**	0.216***	0.233***
	(0.073)	(0.073)	(0.068)
Δ(primary wage)			-0.185***
			(0.040)
Δ(elementary wage)			0.070*
			(0.041)
Δ(high school wage)			0.055*
			(0.029)
Δ(college wage)			0.010
			(0.013)
State Dummies		X	X
Observations	485	485	485
R-squared	0.009	0.117	0.168
Panel B: Regressions by Micro-region in the First Stage			
Δ(tariff)	0.071	0.142**	0.155**
	(0.057)	(0.063)	(0.064)
Δ Avg W by Schooling			X
State Dummies		X	X
Observations	485	485	485
R-squared	0.002	0.091	0.097

Notes: Robust standard errors in parentheses. ***p < 0.01; **p < 0.05, *p < 0.10. Panel A first stage is a regression for 20-60 year-old male employees. First stage independent variables in Panel A: age, age squared, dummies for years of schooling, urban area, and micro-region, and interactions between a dummy for white and micro-region dummies. Panel B first stage is a set of independent regressions, estimated separately for each micro-region. First stage independent variables in Panel B: age, age squared, dummies for years of schooling and urban area, and dummy for white. Second stage independent variables: state dummies (not shown), and changes in micro-region average wages by level of schooling (primary, elementary, high school, and college). Unit of observation is a micro-region. Census data from 1991 and 2000. Regressions weighted by the precision of first-stage estimates of the dependent variable (inverse of the standard errors).

Table 3: Additional Controls, Impact of Tariff Change on Racial Wage Gap, Brazilian Micro-regions, 1991-2000 – Dependent Variable: Change in Conditional Racial Wage Gap

	(1)	(2)	(3)	(4)
Δ(tariff)	0.186**	0.290***	0.250***	0.231**
	(0.094)	(0.088)	(0.070)	(0.103)
Δ(% elementary)$_{blacks}$	0.371**			0.325*
	(0.179)			(0.175)
Δ(% high school)$_{blacks}$	-0.038			-0.046
	(0.178)			(0.178)
Δ(% college)$_{blacks}$	1.396*			1.376*
	(0.758)			(0.737)
Δ(% elementary)$_{whites}$	-0.410			-0.420
	(0.316)			(0.301)
Δ(% high school)$_{whites}$	-0.291*			-0.341**
	(0.154)			(0.158)
Δ(% college)$_{whites}$	-0.111			-0.193
	(0.295)			(0.298)
Δ(% blue collar)		0.251		0.201
		(0.245)		(0.251)
Δ(% informal)		-0.163**		-0.190***
		(0.068)		(0.070)
Δ(% unemployed)		0.047		0.081
		(0.186)		(0.178)
Δ(% migrant)			-0.114	-0.017
			(0.173)	(0.155)
ΔAvg W by Schooling	X	X	X	X
State Dummies	X	X	X	X
Observations	485	485	485	485
R-Squared	0.211	0.183	0.169	0.229

Notes: Robust standard errors in parentheses. ***p < 0.01; **p < 0.05, *p < 0.10. Independent variables: state dummies and changes in average wages by level of schooling (not shown), and changes in the composition of the labor force by level of schooling and race, in the share of informal employees, in the % of unemployed, and in the % of migrants. Unit of observation is a micro-region. Census data from 1991 and 2000. Regressions weighted by the precision of first-stage estimates of the dependent variable (inverse of the standard errors). First stage is a regression for 20-60 year-old male employees. First stage independent variables: age, age squared, dummies for years of schooling, urban area, and micro-region, and interactions between a dummy for white and micro-region dummies.

Table 4: Long-term Impact of Tariff Change on Racial Wage Gap, Brazilian Micro-regions, 1991-2010 – Dependent Variable: Change in Conditional Racial Wage Gap between 1991 and 2010

	(1)	(2)	(3)
Δ(tariff)	0.144*	0.181**	0.256***
	(0.087)	(0.080)	(0.075)
Δ(primary wage)			-0.156***
			(0.034)
Δ(elementary wage)			0.031
			(0.035)
Δ(high school wage)			0.065**
			(0.032)
Δ(college wage)			0.040***
			(0.014)
State Dummies		X	X
Observations	485	485	485
R-squared	0.008	0.137	0.203

Notes: Robust standard errors in parentheses. ***p < 0.01; **p < 0.05, *p < 0.10. First stage is a regression for 20-60 year-old male employees. First stage independent: age, age squared, dummies for years of schooling, urban area, and micro-region, and interactions between a dummy for white and micro-region dummies. Second stage independent variables: state dummies (not shown), and changes in micro-region average wages by level of schooling (primary, elementary, high school, and college). Unit of observation is a micro-region. Census data from 1991 and 2010. Regressions weighted by the precision of first-stage estimates of the dependent variable (inverse of the standard errors).

Table 5: Impact of Alternative Measures of Trade Liberalization on Racial Wage Gap, Brazilian Micro-regions, 1991-2000 – Dependent Variable: Change in Conditional Racial Wage Gap

Indep. var.:	$\Delta(\text{tariff})_{1990-1998}$	$\Delta(\text{tariff})_{1987-1995}$	$\Delta(\text{tariff})_{1987-1998}$	$\Delta(M/P)_{1990-1995}$	$\Delta(MPC)_{1990-1995}$
	(1)	(2)	(3)	(4)	(5)
Panel A – Controlling for changes in micro-region average wages by level of schooling					
Coefficient	0.244***	0.328***	0.343***	-1.492***	-1.546***
	(0.068)	(0.125)	(0.122)	(0.275)	(0.288)
Observations	485	485	485	485	485
R-squared	0.169	0.164	0.165	0.183	0.183
Panel B – Not controlling for changes in micro-region average wages by level of schooling					
Coefficient	0.226***	0.366***	0.374***	-1.320***	-1.379***
	(0.073)	(0.128)	(0.125)	(0.293)	(0.302)
Observations	485	485	485	485	485
R-squared	0.117	0.117	0.118	0.128	0.128

Notes: Robust standard errors in parentheses. ***p < 0.01; **p < 0.05, *p < 0.10. Independent variables: state dummies in both panels and changes in average wages by level of schooling (not shown) in Panel A. Columns 4 and 5 use, respectively, the share of imports and the import penetration coefficient as measures of the trade reform, instead of tariffs. Unit of observation is a micro-region. Census data from 1991 and 2000. Regressions weighted by the precision of first-stage estimates of the dependent variable (inverse of the standard errors). First stage is a regression for 20-60 year-old male employees. First stage independent variables: age, age squared, dummies for years of schooling, urban area, and micro-region, and interactions between a dummy for white and micro-region dummies.

Table 6: Pre-trends and Falsification Exercises, Impact of Tariff Change on Racial Wage Gap, Brazilian 1980 Micro-regions, 1980-1991 – Dependent Variable: Change in Conditional Racial Wage Gap

	Controlling for Pre-Trends Dep. Var.: Δ(w gap)$_{1991-2000}$				Placebo with Pre-Reform Period Dep. Var.: Δ(w gap)$_{1980-1991}$				
	Indep. Var.: 1990-95				Indep. Var.: 1990-95			Indep. Var.: 1980-90	
	Δ(tariff)	Δ(tariff)	Δ(M/P)	Δ(MPC)	Δ(tariff)	Δ(M/P)	Δ(MPC)	Δ(M/P)	Δ(MPC)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A – Controlling for changes in wages by level of schooling									
Coefficient	0.265***	0.223***	-1.201***	-1.256***	-0.001	0.192	0.210	-0.041	-0.140
	(0.070)	(0.055)	(0.245)	(0.253)	(0.073)	(0.259)	(0.291)	(0.062)	(0.385)
Δwgap 1980-91		-0.649***	-0.643***	-0.644***					
		(0.067)	(0.067)	(0.067)					
Observations	411	411	411	411	411	411	411	411	411
R-squared	0.186	0.520	0.528	0.528	0.231	0.233	0.233	0.232	0.232
Panel B – Not controlling for changes in wages by level of schooling									
Coefficient	0.247***	0.197***	-1.035***	-1.088***	-0.076	0.352	0.399	-0.093	-0.463
	(0.076)	(0.056)	(0.242)	(0.250)	(0.071)	(0.262)	(0.292)	(0.065)	(0.393)
Δwgap 1980-91		-0.664***	-0.660***	-0.660***					
		(0.066)	(0.067)	(0.066)					
Observations	411	411	411	411	411	411	411	411	411
R-squared	0.136	0.497	0.502	0.502	0.153	0.155	0.155	0.153	0.153

Notes: Robust standard errors in parentheses. ***p < 0.01; **p < 0.05, *p < 0.10. Independent variables: state dummies in all panels; changes in average wages by level of schooling (not shown) in Panel 2. Unit of observation is a micro-region, according to the 1980 definition. Census data from 1980, 1991, and 2000. Regressions weighted by the precision of first-stage estimates of the dependent variable (inverse of the standard errors). First stage is a regression for 20-60 year-old male employees. First stage independent variables: age, age squared, dummies for years of schooling, urban area, and micro-region, and interactions between a dummy for white and micro-region dummies. In Panel A, dependent variable is the change in the wage gap between 1991 and 2000, and the change in the wage gap between 1980 and 1991 is included as an additional control for pre-trends. In Panel B, dependent variable is the change in the wage gap between 1980 and 1991.

Table 7: Heterogeneity of the Impact of Tariff Changes, Brazilian Micro-regions, 1991-2000 – Dependent Variable: Change in Conditional Racial Wage Gap

	(1)	(2)	(3)
	Level of in Initial (1991) Wage Gap	Impact of Tariff Changes	
Δ(tariff)		0.149*	0.168**
		(0.079)	(0.076)
Δ(tariff) × Market Concentration		0.145**	0.182***
		(0.063)	(0.059)
Market Concentration	0.017***		
	(0.005)		
Δ Avg W by Schooling			X
State Dummies	X	X	X
Observations	485	485	485
R-squared	0.216	0.124	0.179

Notes: Robust standard errors in parentheses. ***p < 0.01; **p < 0.05, *p < 0.10. Independent variables: state dummies (not shown), 1991 levels of industrial concentration, and changes in average wages by level of schooling (not shown). Industrial concentration is standardized. Unit of observation is a micro-region. Census data from 1991 and 2000. Regressions weighted by the precision of first-stage estimates of the dependent variable (inverse of the standard errors). First stage is a regression for 20-60 year-old male employees. First stage independent variables: age, age squared, dummies for years of schooling, urban area, and micro-region, and interactions between a dummy for white and micro-region dummies.

Appendix A: Additional Tables

Table A.1: Employment Share by Sector – Brazil, 1991 Census

	1991	
	Including services	Excluding services
Agriculture	21.63	55.45
Mineral mining	0.80	2.05
Petroleum, gas extraction	0.11	0.28
Nonmetallic metallic	0.94	2.41
Metals	2.75	7.05
Machinery, equipment	0.50	1.28
Electric, electronic equipment	0.54	1.38
Transport	0.59	1.51
Wood, furniture	1.67	4.28
Paper, publishing, printing	0.87	2.23
Rubber	0.16	0.41
Chemicals	0.73	1.87
Petroleum refining	0.17	0.44
Pharma, perfumes	0.22	0.56
Plastic	0.34	0.87
Textiles	1.06	2.72
Apparel	2.52	6.46
Footwear	0.74	1.90
Food processing	2.33	5.97
Other manufacturing	0.34	0.87
Services	61.00	-
Total	100	100

Table A.2: Matching of Sectors between Kume et al. (2003) and the 1991 Census

	Kume et al (200)	1991 Census	Aggregated	
1	Agriculture	11-37, 41, 42, 581	Agriculture	1
2	Mineral mining	50, 53-59	Mineral mining	2
3	Petroleum, gas extraction	51, 52	Petroleum, gas extraction	3
4	Nonmetallic mineral	100	Nonmetalic mineral	4
5	Metals	110	Metals	5
6	Nonmetallic manufacturing	110	Metals	5
7	Other nonmetalic manufacturing	110	Metals	5
8	Machinery, equipment	120	Machinery, equipment	6
10	Electric materials	130	Electric, electonic equipment	7
11	Electonic equipment	130	Electric, electonic equipment	7
12	Automobile, transportation	140	Transport	8
13	Vehicle parts, other vehicles	140	Transport	8
14	Wood, furniture	150, 151, 160	Wood, furniture	9
15	Paper, publishing, printing	170, 290	Paper, publishing, printing	10
16	Rubber	180	Rubber	11
17	Chemicals	200	Chemicals	12
18	Petroleum refining	201, 202, 352, 477	Petroleum refining	13
19	Other chemicals	200	Chemicals	12
20	Pharma, perfume	210. 220	Pharma, perfume	14
21	Plastic	230	Plastic	15
22	Textile	240. 241	Textile	16
23	Apparel	250, 352	Apparel	17
24	Footwear	190, 251	Footwear	18
25	Coffee	260, 261, 270, 280	Food processing	19
26	Vegetables	260, 261, 270, 280	Food processing	19
27	Animal Slaughter	260, 261, 270, 280	Food processing	19
28	Dairy	260, 261, 270, 280	Food processing	19
29	Sugar	260, 261, 270, 280	Food processing	19
30	Vegetable oils	260, 261, 270, 280	Food processing	19
31	Other food processing	260, 261, 270, 280	Food processing	19
32	Other manufaturing	300	Other manufaturing	20

Table A.3: Individual Level Estimates of the Impact of Tariff Change on Racial Wage Gap, Brazilian Micro-regions, 1991-2000 – Dependent Variable: Natural Logarithm of Hourly Wage

	(1)	(2)	(3)	(4)	(5)	(6)
White × Tariff	0.280***	0.265***	0.278***	0.281***	0.280***	0.270***
Controls:						
Individual Characts.	X	X	X	X	X	X
Individual Characts. x Year Dummy	X	X	X	X	X	X
Agg. Micro-region Vars.:						
Avg. Wage by School.		X				X
School. Comp. by Race			X			X
Labor Market Characts.				X		X
Migrantion					X	X
Year dummy	X	X	X	X	X	X
Micro-region dummies	X	X	X	X	X	X
Year x State Dummies	X	X	X	X	X	X
Observations (millions)	4.01	4.01	4.01	4.01	4.01	4.01
R-squared	0.514	0.516	0.514	0.515	0.514	0.516

Notes: Robust standard errors in parentheses (clustered at micro-region level). ***p < 0.01; **p < 0.05, *p < 0.10. Individual controls: age, age squared, dummies for years of schooling, dummy for urban area, and interactions with the year dummy. Aggregate (micro-region) controls: average wages by level of schooling, composition of the labor force by level of schooling and race, share of informal employees, % of unemployed, and % of migrants. Unit of observation is an individual. Census data from 1991 and 2000.

Table A.4: Controlling for Labor Market Participation, Impact of Tariff Change on Racial Wage Gap, Brazilian Micro-regions, 1991-2000 – Dependent Variable: Change in Conditional Racial Wage Gap

	(1)	(2)
Δ(tariff)	0.254***	0.279***
	(0.076)	(0.073)
Δ(Black participation)	-0.216	-0.244
	(0.187)	(0.188)
Δ(White participation)	0.299	0.354*
	(0.184)	(0.183)
State Dummies	X	X
Δ(Avg W by Schooling)		X
Observations	485	485
R-squared	0.122	0.176

Notes: Robust standard errors in parentheses. ***p < 0.01; **p < 0.05, *p < 0.10. Independent variables: state dummies and changes in average Black and White labor market participation and changes in average wages by level of schooling. Unit of observation is a micro-region. Census data from 1991 and 2000. Regressions weighted by the precision of first-stage estimates of the dependent variable (inverse of the standard errors). First stage is a regression for 20-60 year-old male employees. First stage independent variables: age, age squared, dummies for years of schooling, urban area, and micro-region, and interactions between a dummy for white and micro-region dummies.

Table A.5: Controlling for Informality and Sector in the First Stage, Impact of Tariff Change on Racial Wage Gap, Brazilian Micro-regions, 1991-2000 – Dependent Variable: Change in Conditional Racial Wage Gap

	(1)	(2)	(3)
	Panel A – Controlling for Informality		
Δ(tariff)	0.164**	0.221***	0.238***
	(0.072)	(0.071)	(0.066)
State Dummies		X	X
Δ(wage) by schooling			X
Observations	485	485	485
R-squared	0.010	0.117	0.169
	Panel B – Controlling for Sector		
Δ(tariff)	0.095	0.126*	0.143**
	(0.072)	(0.074)	(0.069)
State Dummies		X	X
Δ(wage) by schooling			X
Observations	485	485	485
R-squared	0.004	0.112	0.162
	Panel C – Controlling for Informality and Sector		
Δ(tariff)	0.096	0.126*	0.143**
	(0.072)	(0.073)	(0.067)
State Dummies		X	X
Δ(wage) by schooling			X
Observations	485	485	485
R-squared	0.004	0.111	0.163

Notes: Robust standard errors in parentheses. ***p < 0.01; **p < 0.05, *p < 0.10. First stage is a regression for 20-60 year-old male employees. First stage independent variables in Panel A: age, age squared, dummies for years of schooling, urban area, and micro-region, interactions between a dummy for white and micro-region dummies, plus an indicator for informal employment (without a signed labor card). First stage independent variables in Panel B: age, age squared, dummies for years of schooling, urban area, and micro-region, interactions between a dummy for white and micro-region dummies, plus sectoral dummies. First stage independent variables in Panel C: age, age squared, dummies for years of schooling, urban area, and micro-region, interactions between a dummy for white and micro-region dummies, and informality dummy and sectoral dummies. Second stage independent variables: state dummies (not shown), and changes in micro-region average wages by level of schooling (primary, elementary, high school, and college). Unit of observation is a micro-region. Census data from 1991 and 2000. Regressions weighted by the precision of first-stage estimates of the dependent variable (inverse of the standard errors).

Table A.6: Alternative Samples in the 1st Stage, Impact of Tariff Change on Racial Wage Gap, Brazilian Micro-regions, 1991-2000 – Dependent Variable: Change in Conditional Racial Wage Gap

	Men	Men & Women	Men & Women, Employee & Self-empl.	Men & Women Employees	Men, Not in School, Full-time
	(1)	(2)	(3)	(4)	(5)
		Panel A – Controlling for changes in wages by level of schooling			
Δ(tariff)	0.298***	0.248***	0.254***	0.274***	0.289***
	(0.062)	(0.062)	(0.060)	(0.076)	(0.064)
Observations	485	485	485	485	485
R-squared	0.221	0.257	0.207	0.158	0.188
		Panel B – Not controlling for changes in wages by level of schooling			
Δ(tariff)	0.367***	0.297***	0.327***	0.191***	0.349***
	(0.064)	(0.058)	(0.059)	(0.069)	(0.063)
Observations	485	485	485	485	485
R-squared	0.248	0.317	0.251	0.259	0.222

Notes: Robust standard errors in parentheses. ***p < 0.01; **p < 0.05, *p < 0.10. Independent variables: state dummies in both panels and changes in average wages by level of schooling (not shown) in Panel A. Unit of observation is a micro-region. Census data from 1991 and 2000. Regressions weighted by the precision of first-stage estimates of the dependent variable (inverse of the standard errors). First stage is a regression for 20-60 year-old male employees. First stage independent variables: age, age squared, dummies for years of schooling, urban area, and micro-region, and interactions between a dummy for white and micro-region dummies.

Appendix B: The Impact of the Trade Liberalization on Local Labor Markets

This appendix presents results related to the impact of the trade liberalization episode in Brazil on local labor markets. As mentioned in the text, the literature has documented a permanent reduction in the number of formal firms in markets that suffered relatively higher increases in exposure to foreign competition, consistent with an increase in competition and lower survival rates for the less efficient firms (Dix-Carneiro and Kovak, 2017 and Dix-Carneiro et al., 2018). The labor market responses to the trade reform are also consistent with these procompetitive effects and can be seen as driven by changes in labor demand from firms facing reduced profit margins and increased competition in the market for final goods.

In this section, we reproduce some results from this literature and extend them in a few directions. Specifically, in addition to what has been documented in the papers cited above, we look at the change in the sectoral composition of employment and discuss the race heterogeneity of the effects on employment and earnings. Anticipating the main points, we document that increased exposure to foreign competition due to the trade reform was accompanied by a process of reallocation of employment away from previously tradable sectors. During this process, as employment shifted across sectors, there was a reduction in employment rates in the medium run (2000), followed by a close to complete recovery in the long-run (2010). Consistent with this characterization, there was substantial reductions in wages in the medium run, with only a partial recovery in the long run as workers were reallocated to sectors with lower rents, including the informal sector. These basic patterns were very similar across black and white workers. In addition, throughout this process, the number of operating formal firms was permanently reduced.

B.1 Estimation and Data

Following Dix-Carneiro and Kovak (2017) and Dix-Carneiro et al. (2018), we estimate regressions relating changes in economic outcomes in local labor markets to average reductions in tariffs, considering medium (1991-2000) and long-run (1991-2010) outcomes:

$$\Delta(Outcome_j) = \rho + \theta\Delta(tariff_j) + \varphi_s + v_j, \tag{B.1}$$

where j indicates micro-region, $\Delta(Outcome)$ is the change in some local labor market outcome, $\Delta(tariff)$ represents the change in average tariffs between 1990 and 1995 discussed in equation 6, φ_s is a set of state fixed effect, and v is a random term. The data used in these exercises are from the

Brazilian censuses and from the Brazilian Ministry of Labor's Annual Registry of Social Information (RAIS).

In order to account for potential differential demographic changes across local labor markets, we use dependent variables netted out of compositional effects, as typically done in this literature (Dix-Carneiro and Kovak, 2017 and Dix-Carneiro et al., 2018). In order to net out the composition effects, we first run an OLS regression using individual level census data. The sample includes all individuals between ages 20 and 60, with additional restrictions according to each outcome (see below). For each Census year, we run a linear regression of each outcome variable on race, gender and years of schooling dummies, age and age squared, and micro-region dummies. The micro-region coefficients capture the year-specific share of employment in manufacturing, employment rate, informal rate and (log-)earnings. Then, we use these coefficients to compute the second stage outcome as the difference between coefficients for each micro-region and relevant period. The second stage regression (equation B.1) is weighted by the precision of the first stage estimates (the inverse of the standard error).

The other two outcomes, the number of operating plants and wage bill, come from RAIS. For each micro-region and year, we compute the total number of plants and the sum of wages paid to all formal workers in December (in 2010 values). Because they are aggregate variables, there is no first stage and, consequently, no weights when running the equation B.1. The outcomes are computed as the log-differences between 1991 and 2000 or 2010.

Regarding the census variables, the definition of employment status differs between 1991 and more recent waves. While in 1991 the reference period is the 12 months prior to assessment, in 2000 and 2010, it is the census reference week (in July). We follow Dix-Carneiro and Kovak (2017) to define employment status: in 1991 it refers to the person who worked on a regular basis during the previous 12 months; and in 2000 and 2010, it refers to the person who, in the reference week, either worked (for pay or not) or had a job but did not work for any reason. The dummy variable assumes value 1 if the individual fits the definition, and zero otherwise.

Employment in manufacturing is a dummy variable that assumes value 1 if the worker's sector is manufacturing, and zero otherwise (unemployed and individuals out of the labor force are not included in the sample). The definition of manufacturing sector comes from IBGE's sector codes, which have changed over time. For 1991 and 2000, manufacturing workers are associated with codes 100-300. For 2010, codes 15000-37000.

Informal worker status refers to the worker who is self-employed or employed without a signed work card (dummy value equals 1). Other workers are considered formal workers (dummy value equals zero). Unpaid workers are not included in the sample.

Earnings are the hourly earnings received by the worker in the reference month (August in 1991 and July in 2000 and 2010) in 2010 values. The questionnaire asks how much the worker earned in the reference month and the number of hours worked during a week. The hourly earnings are computed as the reported earnings divided by 4.33 × the number of hours worked. The sample includes only workers with positive earnings.

B.2 Results

The first results are presented in Table B.1. The table presents the estimated effects of tariff reductions in the medium (1991-2000) and long-run (1991-2010) on employment composition (share in manufacturing), employment, wages, number of operating formal plants, total formal wage bill, and informality.

Table B.1: Impact of Tariff Change on Labor Market Outcomes, Brazilian Micro-regions, 1991-2000 and 1991-2010 – Dependent Variables: Change in Share of Employment in Manufacturing, Employment, Hourly Wage, Number of Plants, Wage Bill and Informality

	Share of Employment in Manufacturing (Census)		Employment (Census)		Wage (Census)	
	2000	2010	2000	2010	2000	2010
	(1)	(2)	(3)	(4)	(5)	(6)
Δ(tariff)	0.445***	0.555***	0.319***	-0.053	0.688***	0.406**
	(0.039)	(0.066)	(0.036)	(0.044)	(0.101)	(0.167)
Observations	485	485	485	485	485	485
R-squared	0.471	0.315	0.380	0.546	0.589	0.556
	# of Operating Plants (RAIS)		Formal Wage Bill (RAIS)		Informality (Census)	
	2000	2010	2000	2010	2000	2010
	(7)	(8)	(9)	(10)	(11)	(12)
Δ(tariff)	2.456***	3.843***	4.827***	8.101***	-0.660***	-0.435***
	(0.357)	(0.444)	(0.583)	(0.733)	(0.043)	(0.068)
Observations	484	484	484	484	485	485
R-squared	0.500	0.586	0.397	0.565	0.550	0.504

Notes: Robust standard errors in parentheses. ***p < 0.01; **p < 0.05, *p < 0.10. Independent variables: state dummies. Unit of observation is a micro-region. Census and RAIS data from 1991, 2000, and 2010. Regressions using Census data weighted by the precision of first-stage estimates of the dependent variable (inverse of the standard errors). First stage is a regression for 20-60 year-old individuals, estimated separately for each year. First stage independent variables: age, age squared, and dummies for race, gender, years of schooling, and micro-region.

Remember that reductions in tariffs have a negative sign, so a positive coefficient in the table means a negative impact of trade liberalization. The results show that regions experiencing relatively larger reductions in tariffs experienced relatively larger reductions in employment in tradeable sectors (manufacturing). These reductions in the share of employment in manufacturing tended to increase slightly over time. At the same time, there was a substantial reduction in overall employment in the medium run, followed by complete recovery in the long run.

Table B.2: Impact of Tariff Change on Employment and Informality by Race, Brazilian Micro-regions, 1991-2000 and 1991-2010 – Dependent Variables: Change in Employment and Informality

	2000		2010	
	White	Black	White	Black
	(1)	(2)	(3)	(4)
		Panel A – Employment		
Δ(tariff)	0.268***	0.373***	-0.042	-0.048
	(0.035)	(0.042)	(0.043)	(0.050)
State Dummies	X	X	X	X
Observations	485	485	485	485
R-squared	0.352	0.384	0.557	0.503
		Panel B – Informality		
Δ(tariff)	-0.565***	-0.530***	-0.455***	-0.291***
	(0.049)	(0.055)	(0.079)	(0.080)
State Dummies	X	X	X	X
Observations	485	485	485	485
R-squared	0.506	0.442	0.524	0.521

Notes: Robust standard errors in parentheses. ***p < 0.01; **p < 0.05, *p < 0.10. Independent variables: state dummies. Unit of observation is a micro-region. Census data from 1991, 2000, and 2010. Regressions weighted by the precision of first-stage estimates of the dependent variable (inverse of the standard errors). First stage is a regression for 20-60 year-old individuals, estimated separately for white and black. First stage independent variables: age, age squared, dummies for gender, years of schooling, and micro-region.

Local labor markets facing reductions in tariffs larger by 10 percentage points typically experienced a large medium run reduction in the employment rate, of the order of 3 percentage points, but employment fully recovered over the long run. Together with the medium run reduction in employment, these local economies also experienced substantial reductions in average wages – of the order of 7 percentage points for 10 percentage-point larger reductions in tariffs – but this reduction in wages persisted in the long run, with only a partial recovery. This process was accompanied by a permanent reduction in the number of formal firms operating in these area, which, if anything, intensified over time. Accordingly, the total formal wage bill paid by formal firms in these locations also shrank. Finally, the informal sector expanded throughout the period, absorbing part of the labor that was released from the shrinking manufacturing sectors following the trade liberalization episode.

One concern that might arise, given that we want to analyze the impact of increased competition on the racial wage gap, is that the changes in employment portrayed in Table B.1 were heterogeneous across races. If that were the case, changes in the ability composition of black and white workers might end up reflected on the estimated racial wage gap, invalidating our empirical strategy. To address this concern, Table B.2 reproduces the results related to employment and informality by race. The patterns of medium and long-term responses of employment to the trade liberalization are very similar across races and the coefficients are not statistically different. Employment is reduced for both blacks and whites in the medium run, but fully recovers in the long run. For informality, the medium-run response is very similar across races, and the long-run response somewhat larger for whites (though the difference is only marginally significant). The relatively larger response of informality among whites in the long run may be precisely a result of increased competition and the movement of white workers away from tradable sectors (Dix-Carneiro and Kovak, 2018). We revisit this point when analyzing the results of our main empirical exercise in the main text of the paper.