The Impact of Privatization of State-Owned Enterprises on Workers*

David Arnold†

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Abstract

While privatization of state-owned enterprises (SOEs) remains a popular policy tool in many countries, the impacts on workers are unclear. This paper studies the case of Brazil, which implemented a large privatization programs in the 1990s. Following privatization, incumbent workers in privatized SOEs suffer a wage decline of roughly 26 percent relative to a matched control group. Additionally, private-sector firms that are connected to privatized SOEs by labor mobility also reduce wages. A summary calculation suggests that privatization decreased the formal sector wage by 3.1 percent, with about two-thirds of this effect due to the indirect impact on private-sector workers.

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†University of California, San Diego. Email: daarnold@ucsd.edu

I. Introduction

State-owned enterprises (SOEs) employ about 2 percent of all workers in OECD countries, with a substantially higher proportion in many developing countries (Subramanian and Megginson, 2018). Given the value of SOEs in many countries, privatization remains a popular policy tool for debt-burdened governments to quickly raise large amounts of revenue.[1] Supporters often argue that privatization will both raise revenue for the government as well as improve the efficiency of firms. However, workers and unions often oppose privatization, fearing it will reduce employment and wages. While there is a large literature studying the effects of privatization on firm productivity, there is little evidence on how privatization affects workers.

This paper studies both the direct impact of privatization on incumbent workers in privatized SOEs as well as the broader labor market impacts on private-sector workers. Given the scope of privatization in many countries, large reductions in employment and wages at privatized SOEs may also impact private-sector firms, resulting in market-level wage declines. To estimate both the direct impacts and market-level effects, I utilize a matched employer-employee dataset which contains information on all formal-sector workers in Brazil. The identification strategy makes use of Brazil's 1990s privatization program which privatized about 3 percent of all formal-sector employment in Brazil.[2] To identify market-level effects, I construct a measure of exposure to privatization at the occupation-by-region level which traces how the impacts of privatization propagate through firms in labor markets connected to privatized SOEs by labor mobility.

To understand the privatization process in Brazil and how it might affect workers it is helpful to discuss the case of Telebrás, a large telecommunications company that was privatized in 1998. Before privatization, employees of Telebrás organized multiple protests and filed lawsuits seeking to block the privatization sale, fearing it would result in lower wages and employment. Later, they sought to include stipulations regarding employment and compensation into the purchase (Guimarães, 2007). In the end, these stipulations were not included when Telebrás was sold in 1998. After privatization, unions in the telecommunications industry report losing bargaining power with the new owners, leading to layoffs and worse compensation packages. The goal of this paper is twofold. First, to understand whether the case of Telebrás is a general story of privatization in Brazil, or merely a single well-publicized event. Second, given the size and influence of SOEs in many countries, large changes at privatized firms may impact private-sector labor markets. These market-level effects are not only important to identify in order to determine the distributional impacts of privatization, but also provide a unique setting in which to study how firm-level shocks propagate across labor markets.

In the first part of the paper, I estimate the direct impact of privatization on incumbent workers and establishments. To identify privatization events, I use changes in the legal nature of establishments using administrative data from Brazil. Changes in legal nature reflect changes in the ultimate control of the firm, not necessarily that the firm transitions from 100 percent state-owned to 100 percent private-sector owned. For example, in the case of Telebrás, the government initially owned about a 21 percent stake in the firm,

[1] In 2015, privatization revenues peaked at over 300 billion USD (Megginson, 2017).

[2] Author's calculation. As will be discussed in Section II, this paper restricts analysis to privatization events that occur between 1996-2000. A number of large SOEs were privatized in the early 1990s in Brazil. Therefore, the number cited understates the total number of workers directly impacted by privatization in Brazil in the 1990s.

but owned over 50 percent of the voting shares, and so maintained control over company decisions. The privatization sought to sell the 21 percent stake and relinquish all of the voting shares. Therefore, in this case, control of the firm went from majority state-owned to fully privately owned. It is also possible for the state to maintain minority ownership after the privatization event. Cases in which the government does not sell majority control or sells only minority shares are not classified as privatization events in this paper, as these do not reflect changes in ultimate control of the firm.

To estimate the impact of privatization on incumbent workers in privatized SOEs, I construct a control group using a coarsened-exact-matching algorithm (CEM) to match privatized workers to similar workers in private-sector establishments or never-privatized SOEs. I find that while wages experienced similar trends in the years prior to privatization, wages fall by roughly 26 percent in the long run among privatized workers following privatization.[3] The large decline in earnings is due to two separate effects. First, workers displaced by privatization move to lower-paying establishments. Second, workers who remain employed at the privatized SOEs experience wage reductions relative to control workers. In order to identify the within-establishment changes in wages, I estimate the impact of privatization for workers who stay within the same establishment in the years following privatization. I find that wages for establishment stayers fall by about 16 percent in the long run. Therefore, while a portion of the total effect is due to displacement, there is still a large decline in wages within the privatized establishment.

I next consider the impact of privatization on establishment-level employment. While privatized workers are more likely to be displaced than control workers, displacement of individual workers might be counterbalanced by increased overall hiring in privatized firms. To identify the impact on establishment-level employment, I implement a matching strategy analogous to the worker-level results and find employment in privatized establishments falls by about 22 percent following privatization. Therefore, together with the worker-level analysis, this implies privatized SOEs shed incumbent workers and do not replace these workers through increased hiring.

In the second part of the paper, I explore whether these large negative impacts on privatized SOEs impact other firms in the same labor market. Theoretically, these market-level effects may arise for a number of reasons. First, changes in product-market conditions could impact workers. For example, privatization may impact the efficiency of production and the competitiveness of the product market. Second, declines in employment and wages at privatized SOEs will decrease the expected value of the outside option for private-sector workers. In many models of wage determination (Abowd and Lemieux, 1993; Pissarides, 2000; Postel-Vinay and Robin, 2002; Hornstein et al., 2011; Card et al., 2018; Kline et al., 2019), the wage paid to a worker depends on the outside option of the worker, and therefore shocks to the value of outside options will decrease overall wages. Lastly, even in perfectly competitive labor markets, large declines in labor demand at privatized SOEs could lead to drops in aggregate demand for labor in the market, again causing market-level declines in wages.

[3]The matching procedure does not explicitly match on earnings, allowing for a transparent analysis of pretrends prior to privatization. Additionally, this analysis excludes observations with zero earnings as Brazil has a large informal sector (40 percent) and therefore years of zero earnings in the administrative data are not indicative of unemployment for many workers. However, I do find workers in SOEs are 10 percentage points less likely to be working in the formal sector following privatization, suggesting the results using only positive earnings is likely a lower bound.

To identify market-level effects, I construct a measure of exposure to privatization at the labor market level. I follow Azar, Marinescu and Steinbaum (2020) and define a labor market as an occupation-by-region cell. While defined at the occupation-by-region level, I allow the exposure measure to depend on both the fraction of jobs privatized within the occupation-by-region cell, as well as the fraction of jobs privatized in closely related occupation-by-region cells, where two occupation-by-region cells are closely related if workers commonly transition between them. More formally, I define the distance between labor market i and labor market j as the probability a worker from labor market i transitions to labor market j conditional on a job transition. The exposure to privatization of labor market i is equal to the fraction of workers privatized in labor market i plus the fraction of workers privatized in all other labor markets, weighted by the distance between labor markets.[4]

I find substantial wage declines in markets that are more exposed to privatization relative to markets that are less exposed to privatization. While the direct impacts on privatized SOE workers predict an aggregate wage decline of about 1.0 percent in Brazil's formal sector, incorporating market-level effects magnifies this to 3.1 percent. Therefore, two-thirds of the aggregate impact of privatization on wages is due to indirect impacts on private-sector firms in this setting. Incorporating the indirect effects is therefore an essential component in estimating the total impact of privatization on workers.

In the final part of the paper, I exploit variation across industries to understand the drivers of these indirect market-level effects. First, to understand the role of product-market parameters, I compare market-level effects for tradables (manufacturing) vs. non-tradables. In tradable markets, the price of the output is determined on a global market, and therefore, may be less prone to product-market changes due to privatization. Additionally, all of the tradable industries are outside the primary industries privatized during this period, implying these estimates will not reflect changes in production technology due to privatization. I find quantitatively similar results across tradables and non-tradables, suggesting product-market parameters are not driving the results.

To disentangle market-level declines due to falling employment at privatized SOEs vs. falling wages, I exploit the fact that employment declines are much larger in the banking sector than in the telecommunications and electricity sectors, both of which experienced smaller and insignificant declines in employment. Conversely, wage declines are smallest in the banking sector, with both telecommunications and electricity sectors experiencing similarly sized wage declines. I find evidence most consistent with decreases in outside options due to falling wages at privatized SOEs as driving the market-level results. The elasticity of market-level earnings with respect to privatization exposure is lowest for labor markets exposed to privatization in the banking sector, the sector with the largest employment effects and smallest wage effects.

While this paper finds large negative impacts on workers due to privatization, two important points should be kept in mind while interpreting the results. First, there is a large literature that finds positive impacts of privatization on measures of firm efficiency (Boycko, Shleifer and Vishny, 1997; La Porta and Lopez-de Silanes, 1999; Megginson and Netter, 2001; Chong and Lopez de Silanes, 2005; Megginson,

[4]To ensure the maximum exposure measure sums to 1, the fraction of workers privatized in labor market i is weighted by the fraction of workers in labor market i who transition jobs, but stay within labor market i. For example, if 100 percent of electrical technician jobs in a region are privatized, the exposure measure will not necessarily be equal to 1, because electrical technicians may transition into other types of jobs that were not directly impacted by privatization.

2017), including studies in Brazil (Anuatti-Neto, Barossi-Filho, De Carvalho and Macedo, 2003). Therefore, while privatization does result in earnings losses for some workers, the total welfare implications are ambiguous. For example, increases in productivity due to privatization may increase aggregate wages in the economy, and this will not be captured by the research design in this paper. This suggests that the best policy response may be to provide support for workers negatively impacted by privatization, not to block privatization outright, similar to proposals often made to ameliorate the negative impacts of trade on workers. Second, how these results translate to other countries likely depends on a number of factors. For example, in Brazil, I find a substantial wage premium associated with working for an SOE, allowing for substantial scope to reduce wages post-privatization. While an SOE wage-premium is found in other countries, such as China (Xin and Frances, 1998; Zhao, 2002), if SOE workers and private-sector workers are paid similar amounts, then one might not expect to find such large wage impacts of privatization.

This paper contributes to a number of distinct literatures. First, it contributes to the long literature on the impacts of privatization. As discussed above, prior work generally finds that many measures of profitability increase following privatization. In the particular case of Brazil, Anuatti-Neto et al. (2003) finds that return on assets, return on equity, and sales all rise more in privatized firms relative to private-sector firms following privatization. While most papers studying privatization utilize firm-level financial data, two notable exceptions are Firpo and Gonzaga (2010) and Olsson and Tåg (2018), both of which use worker-level data and study the impacts of privatization on workers. Olsson and Tåg (2018) use Swedish data and find privatization increases the probability of unemployment but does not significantly impact worker earnings. These differing results emphasize how privatization effects likely depend on the institutions of the country being studied. Still, Brazil is a particularly interesting case study given the size of the privatization event, as well as the multitude of privatization events occurring in Latin America during this time. Across many different countries, including Brazil, public support fell for privatization after the wave of events in the 1980s and 1990s (Birdsall and Nellis, 2003).

The method used to identify market-level effects in this paper is similar in spirit to prior work that empirically estimates spillovers in response to firm-level shocks. For example, Bloom, Schankerman and Van Reenen (2013) estimate R&D knowledge spillovers by creating measures of a firm's position in both product space and technology space. Lane (2017) studies spillovers related to "big-push" industrial policies in South Korea by constructing measures of exposure to the policy using the sectoral input-output network. A number of studies estimate productivity spillovers due to agglomeration (Rosenthal and Strange, 2004; Moretti, 2011). Similarly, this paper uses a measure of distance in the labor market based on empirical worker flows in order to construct a measure of exposure to privatization, similar to Arnold (2019), which uses worker flows to measure concentration in a labor market.

Lastly, a recent literature documents significant wage dispersion across firms (Card et al., 2013; Barth et al., 2016; Song et al., 2019), even after controlling flexibly for worker quality. This significant dispersion is inconsistent with models of perfect competition and suggests labor market frictions may be important components in understanding the aggregate wage distribution. I find that SOEs pay significant wage premiums over private-sector firms. However, this wage premium declines significantly following privatization, suggesting that differential rent-sharing between SOEs and private-sector firms drives the dramatic decline

in earnings in privatized SOEs following privatization. I also provide suggestive evidence that a worker's outside option is an important component in explaining why private-sector firms reduce wages in response to privatization. While the outside option of a worker is a key ingredient in many wage-determination models (Abowd and Lemieux, 1993; Pissarides, 2000; Postel-Vinay and Robin, 2002; Hornstein et al., 2011; Card et al., 2018; Kline et al., 2019), only a few papers establish its importance empirically (Beaudry et al., 2012; Caldwell and Danieli, 2018; Caldwell and Nikolaj, 2019).

The structure of the paper is as follows. Section II discusses the institutional details and data. Section III presents a model of privatization that illustrates the channels through which privatization impacts workers. Section IV estimates the direct impact on privatized SOE workers and establishments. Section V estimates market-level effects of privatization and Section VI concludes.

II. Institutions and Data

A. Privatization in Brazil

In Brazil, SOEs played a fundamental role in the government's economic development goals. The government of Brazil invested heavily in a number of strategic industries, including mining, steel, chemicals, banking, telecommunications, and electricity. However, in the late 1980s, large fiscal debts led Brazil to sell a number of SOEs as a means to raise revenue. The privatization program began in 1991 with the federal government selling large federal SOEs. Similar programs were enacted at the state level beginning in 1996. Due to data limitations which will be discussed further below, this paper restricts the analysis to privatization events occurring after 1995 and therefore will exclude many privatization events of the federal program. Given these data restrictions, the primary industries studied in this paper are telecommunications, electricity, and banking.[5]

In order to determine which firms would be sold, a privatization committee was formed. Many factors determined which SOEs would be privatized. While selecting highly-profitable companies would raise the most revenue, some companies, such as the Bank of Brazil, were deemed politically infeasible to be privatized (Firpo and Gonzaga, 2010). Therefore, it is a priori unclear whether the SOEs selected for privatization were positively or negatively selected in terms of productivity.

After choosing the list of proposed companies, the Brazilian Development Bank (BNDES) hired two private consulting companies to value the SOEs that would be sold and to determine an acceptable minimum price. A public auction was then held, and the enterprise would be sold as long as the minimum acceptable price was reached. In the early 1990s, restrictions were placed on foreign investment, but these restrictions were later relaxed before the period of privatization (post-1994) studied in this paper.

The privatization program was part of broader market reforms aimed at stabilizing the macroeconomy. Perhaps the most important reform for this project involved a large trade liberalization program that started in 1988 and ended in 1994. Many studies (Menezes-Filho and Muendler, 2011; Kovak, 2013; Dix-Carneiro and Kovak, 2017) have found that trade liberalization in Brazil had important impacts on workers' outcomes.

[5] While telecommunication, electricity and banking are the primary sectors privatized during this time, there are also significant events in other industries. For example, the Brazilian government sold Vale mining in 1997 which is currently the largest producer of iron ore and nickel in the world.

While all privatization events studied in this paper occur after the end of the trade liberalization, lagged impacts of trade liberalization could bias the results. Therefore, I consider a number of robustness checks that are specifically aimed at showing the results are not be driven by trade liberalization, including specifications that control directly for exposure to trade liberalization using regional exposure measures constructed in Dix-Carneiro and Kovak (2017). Online Appendix C contains additional details about trade liberalization in Brazil as well as the construction of the trade liberalization measure in Dix-Carneiro and Kovak (2017).

B. Data

The analysis utilizes administrative data from the *Relação Anual de Informações Sociais* (RAIS) for the years 1992 to 2010. The RAIS data contain linked employer-employee records from a mandatory survey administered by the Brazilian Ministry of Labor and Employment (MTE). Fines are levied on firms that provide inaccurate or incomplete information on the survey. Brazil, however, does have a large informal sector that accounts for about 40 percent of all employment. Therefore, periods of zero earnings do not necessarily indicate unemployment. In Section IV, I discuss how the informal sector impacts the interpretation of the results.

Each entry in the RAIS dataset is an employee-employer match. Each individual and establishment are assigned unique administrative identifiers that do not change over time. Demographic data includes worker-level variables such as education, gender, ethnicity, and age. The data also includes information about the job, such as occupation, tenure, wage, hours, type of labor contract, whether the job has ended, and why the job has ended. Lastly, the data includes establishment-level information, including region and industry.

Most importantly for this study, the RAIS contains data on the legal nature of the establishment. SOEs and private-sector establishments are assigned different codes (Muendler et al., 2012). When an establishment is privatized, the legal nature of the establishment changes from state-owned to private, but the tax identifier of the establishment does not change (Firpo and Gonzaga, 2010). Therefore, privatization events can be identified at the establishment level by tracking when legal codes change from year to year.[6] However, the legal nature variable is only available from 1995 onwards. Therefore, only privatization events after 1995 can be identified in the RAIS data. In Figure 1, I plot the number of workers currently employed in privatized establishments during each year of privatization in the sample. As can be seen in Figure 1, from 1996 to 2000, around 900,000 workers are employed in privatized establishments. Overall, this corresponds to about 3 percent of all formal sector employment. For additional details on the data, see Online Appendix B.

C. Sample Selection

I restrict attention to workers age 18-65 working in full-time jobs (i.e. at least 35 hours per week). I exclude individuals with invalid identifiers (less than one percent of the data). In the RAIS, the wage is reported in multiples of the monthly minimum wage. The wage refers to total payments, including regular

[6]For a subset of large privatization events, the privatization event and timing is verified in the RAIS. For example, the privatization of the mining company, Vale, was a historic event and documented in the newspapers throughout 1997. I find that Vale's legal identifier changes the year of the privatization event in the RAIS data as well.

salary payments, holiday bonuses, performance-based and commission bonuses, tips, and profit-sharing agreements, divided by total months worked during the year for that employer. To convert this number to real earnings, I use data on the real value of the minimum wage from Brazil's Institute of Applied Economic Research (IPEA).

The RAIS dataset includes a variable that indicates the type of employment contract the worker is hired under (e.g. temporary vs. permanent). The most common contract in the data is *Consolidação das Leis do Trabalho* (CLT) which is the standard full-time employment contract in Brazil. CLT contracts come with government-mandated benefits such as severance pay if dismissed without cause. Importantly for this study, workers in SOEs and workers in the private sector are employed under the same legal contract. Therefore, changes in wages and employment due to privatization do not reflect changes in the contract held by workers. In contrast, public administration workers are employed under different contracts and entitled to further benefits, such as employment stabilization policies which limit the state's ability to fire civil servants (Firpo and Gonzaga, 2010). However, this project only compares workers in SOEs and workers in private-sector establishments and excludes public administration workers.

D. Do SOEs Pay Higher Wages?

Before considering the effect of privatization on wages, I first establish that there is a significant wage premium associated with working for an SOE before privatization occurs. To do so, I estimate a standard two-way fixed effects model with worker and establishment fixed effects following Abowd, Kramarz and Margolis (1999) (AKM) and Card, Heining and Kline (2013) utilizing data from the years prior to the privatization (1992-1995).[7] Formally, I estimate the following regression model:

$$ln(w_{it}) = \alpha_i + \psi_{J(i,t)} + \gamma_t + x'_{it}\beta + \varepsilon_{it} \tag{1}$$

where α_i is an individual fixed effect, ψ_j is an establishment fixed effect, γ_t is a year indicator, x'_{it} are time-varying covariates which include education and an age cubic interacted with year indicators, and $J(i,t)$ is a function which indicates the establishment individual i is employed at in time period t. Given the individual fixed effects, the establishment-specific wage premium ψ_j is interpreted as the premium paid by establishment j controlling for the quality of workers employed at establishment j. For details on the identifying assumptions and implementation details, see Online Appendix D.

In Table 1, I compare the average wage premium for SOEs that will be privatized to private-sector firms, controlling for two-digit industry. As can be seen in column 1, on average, the establishment effect for SOEs is 18.2 log points higher in SOEs relative to private-sector establishments, implying workers earn about 20.0 percent more in SOEs. Interestingly, the wage premium associated with working at an SOE varies drastically by education of the worker. As can be seen in columns 5-7, the wage premium is substantially larger for individuals with no high-school degree (48.9 percent) than individuals with a high-school degree (24.7 percent). College-educated workers, on the other hand, do not seem to benefit from working at an

[7] Instead of estimating an AKM model which relies on transitions between establishments, one could instead estimate a wage premium by regressing wages on a list of controls. Regressing wages on education, age, tenure, occupation fixed effects, industry fixed effects, and an indicator for working in an SOE yields a wage premium associated with working in an SOE of 25 percent.

SOE, with SOE wages and private-sector wages about equal.

Overall, this analysis provides the first evidence that SOEs pay higher wages. If the large wage premium is due to differential rent-sharing between workers and firms at SOEs relative to private-sector firms, then this could explain why unions and workers fear privatization will lead to lower wages for workers.

III. Model of Privatization

A. Overview

Before estimating the effect of privatization on workers, I present a simple model of state-owned enterprises to illustrate channels through which privatization may impact workers. This model builds from the union-wage determination models illustrated in Brown and Ashenfelter (1986) and Abowd and Lemieux (1993). The model also clarifies how privatization may indirectly impact non-privatized firms, generating market-level changes in labor-market outcomes.

B. Setup

To begin, I consider a group of $\bar{L}_j$ workers bargaining over both wages and employment level with a firm of type $j \in \{soe, p\}$, where soe denotes an SOE and p indicates a private-sector firm. The workers seek to maximize $L_j w_j + (\bar{L}_j - L_j)v$, where w_j is the bargained wage, L_j is the employment level, and v is the value of the outside option to the workers. In this case, I assume workers who do not obtain employment reenter the labor force and search for a new job. Therefore, the value of the outside option is equal to the expected wage of the new job minus any search costs c associated with finding a new job.

The workers bargain with a firm that has a concave production function $F(L_j)$, where the production function is constant across SOEs and private-sector establishments.[8] The profits of a firm are given by $\theta_j F(L_j) - w_j L_j$, where θ_j is a revenue shifter. For example, if the firm becomes more efficient, or the demand for the firm's good increases, then these changes will be captured by an increase in θ_j. I assume workers and firms Nash-bargain over wages, where the bargaining weight associated with labor is given by γ_j, which is allowed to differ between SOEs and private-sector firms. If $\gamma_{soe} > \gamma_p$ then SOEs allocate more rents to labor than private-sector firms.

The threat point for workers is the value of the workers' outside option, while the threat point for the firm is zero profits. SOEs, in addition to profits, may also receive additional revenue per worker, τ_j. For simplicity, I assume $\tau_{soe} > 0$ and $\tau_p = 0$, capturing the idea that politically connected firms may receive preferential treatment (Khwaja and Mian, 2005). Alternatively, one may conceptualize τ_{soe} as arising from political pressure and not directly from cash transfers. The parameter is included to capture any additional benefit of employment for an SOE above and beyond a worker's output. The bargaining solution chooses L_j and w_j to maximize:

$$\max_{L_j, w_j} [L_j w_j + (\bar{L} - L_j)v - \bar{L}v]^{\gamma_j} [\theta_j F(L_j) + \tau_j L_j - w_j L_j]^{1-\gamma_j} \tag{2}$$

[8] While the homogeneity of the production function is not strictly necessary, it simplifies the illustration.

Taking the first order conditions for the bargaining problem yields the following two optimality conditions:

$$w_j = \gamma_j \left(\frac{\theta_j F(L_j)}{L_j} + \tau_j - v \right) + v \tag{3}$$

$$\theta_j F'(L_j) = v - \tau_j \tag{4}$$

As can be seen in Equation (3), wages will be higher in firms with higher productivity θ_j, in firms with more incentives per worker from the state τ_j, and when the outside option v is higher. Equation (4) dictates the optimal size of the firm. If $F(L)$ is concave, then conditional on productivity and the outside option available to workers, SOEs will be larger as long as $\tau_{soe} > 0$. By allowing workers and firms to bargain over both wages and employment, the resulting bargaining outcome is strongly efficient (Farber, 1986). This implies that firm size does not depend on the negotiated wage. Restricting firms and workers to bargain over the wage only, however, yields similar predictions regarding the impact of privatization on workers. Therefore, I present only the strongly efficient bargaining solution as it simplifies the illustration.

C. Effect of Privatization on Workers in Privatized SOEs

In this formulation, there are three reasons why SOEs would offer higher wages than private-sector firms. First, they could be more productive due to differences in θ_j. This would cause SOEs to have both higher wages and higher employment levels. Second, as informally argued by unions opposing privatization, the bargaining parameter, γ_j, may differ between SOEs and private-sector firms. Lastly, I allow SOEs to retrieve transfers τ_j, which increases the surplus available, some of which gets transferred to workers in the form of higher wages.

Many policy-makers claim that privatization increases efficiency, which can be captured in this model as an increase in θ_j. In this model, as in search (Burdett and Mortensen, 1998) and wage-posting models (Kline et al., 2019), firm size and wages are positively correlated with productivity. Therefore, increases in productivity generate increases in both firm size and wages. In contrast, falling incentives per worker, τ_j, generate declines in wages and employment, while falling worker bargaining power, γ_j, generates declines in wages. If the strong efficiency assumption is violated, then falling worker bargaining power will generate declines in both wages and employment. Therefore, in practice, it is difficult to disentangle declining bargaining power γ_j from declining incentives τ_j, because under realistic deviations from a strongly efficient bargaining solution the two will have the same predicted impact on wages and employment.

D. Market-Level Effects of Privatization

In the model, there are two channels through which privatization will impact other firms in the labor market. The first is through the product-market parameters $F(L)$ and θ_j. For example, if privatization changes competition in the product market, this could shift θ_j for private-sector firms. If privatized firms adopt new technology, and this improved technology impacts private-sector firms through agglomeration spillovers, then this could impact $F(L)$. In order to study these channels, in Section V, I will restrict to tradable indus-

tries, the logic being that in tradable industries (which are different industries than the privatized industries) competition is expected to be more stable. Therefore, for these industries, it seems less likely that θ_j or $F(L)$ will be impacted by privatization.

The second channel arises in the model due to changes in the outside option v, which is defined as the expected wage of a new job minus any search costs associated with finding the job:

$$v = \mathbb{E}[w] - c = \lambda_u b + \lambda_p \mathbb{E}[w|j = p] + \lambda_{soe}\mathbb{E}[w|j = soe] - c \tag{5}$$

where λ_u is the probability a worker becomes unemployed, b is the value of unemployment, λ_p is the probability a worker gets a job at a private-sector firm, $\mathbb{E}[w|j = p]$ is the expected wage at a private-sector firm, λ_{soe} is the probability a worker gets a job at an SOE and $\mathbb{E}[w|j = soe]$ is the expected wage at an SOE.[9]

To understand how changes in wages and employment at privatized SOEs spill over to other firms, imagine that privatization decreased only employment at privatized SOEs, but kept wages exactly the same. This would cause λ_{soe} to decrease (i.e. it is now more difficult to get a job at an SOE given employment has fallen at SOEs). This decreases the value of the outside option because it is now less likely a private-sector worker can become employed at a high-wage establishment. A second effect arises if privatized firms decrease wages, holding employment fixed. In this case, the quality of the outside options has decreased, again leading to a decrease in the negotiated wage.

To study these two channels independently, I will take advantage of heterogeneity across privatized industries. In particular, while some industries experience large employment drops and small wage declines, others experience small (and insignificant) employment drops and large wage declines.

IV. Direct Impact of Privatization on Workers and Establishments

A. Overview

The goal of this section is to estimate the direct effect of privatization on wages and employment, at both the worker and establishment level.

B. Finding a Control Group

Workers in SOEs are likely different than the average worker. For one, SOEs are generally larger and produce in industries deemed strategic by the government. Therefore, there might be important differences between workers in SOEs compared to the general population of workers. In order to create a valid control group, I use a matching algorithm to find workers who are in the same occupation, industry, and of similar age. I do not match on wages in order for a transparent test of parallel trends before privatization. In the main specifications, I allow the control worker to be drawn from either never-privatized SOEs or private-sector establishments. A priori, it seems plausible that either group could be a potentially valid counterfactual

[9] As discussed previously, many workers are employed in the informal sector. While the informal sector is not directly accounted for in the model, unemployment can be reinterpreted as a combination of unemployment and informal sector employment.

and I therefore allow matches to be drawn from either group to increase the likelihood a match is found for a given treated worker. However, it is true that workers in never-privatized SOEs and private-sector establishments earn very different wages even conditional on occupation and sector. However, while the levels differ greatly, the trends are similar in practice, and therefore the qualitative results hold regardless of the control group.

Before finding the control group, I restrict the treated sample to individuals who are employed a privatized SOE in years $t-1$ and year $t-2$ prior to the privatization event in year t. This tenure restriction is chosen in order to limit the analysis to workers with attachment to the establishment. In order to find a control group for workers in a privatization event in year t, I first impose the same tenure restriction on private-sector workers and non-privatized SOE workers. Next, I implement a coarsened-exact-matching (CEM) procedure following Iacus et al. (2012). Specifically, the algorithm matches workers exactly on two-digit occupation codes, two-digit industry codes and five-year age bins. In additional robustness checks, I add more variables to the matching algorithm, such as exposure to trade liberalization, establishment size deciles, firm size deciles, and microregion, as well as matching on more specific occupation and industry codes. The matching algorithm utilized here is similar to a number of recent papers (Goldschmidt and Schmieder, 2017; Smith et al., 2019; Jaravel et al., 2018).

Table 2 displays the summary statistics for the privatized workers and the control group in the year prior to privatization. The privatized and control workers have similar educational attainment levels (48 percent of privatized workers are high-school graduates while 44 percent of control group workers are high-school graduates). The log monthly wage for privatized workers is slightly higher (8.64) than the control group (8.58). Privatized workers are employed in larger establishments than non-privatized workers (median establishment size in privatized establishments is 765 compared to 416 in the control group). Therefore, matching on occupation, industry, and age does not fully eliminate differences between privatized workers and non-privatized workers. However, the identification strategy relies on parallel trends between privatized workers and non-privatized workers, not complete balance on covariates.

One benefit of the matching approach is that comparing raw averages across groups is a meaningful comparison. Figure 2 plots the average log monthly wage for the group of privatized workers and the matched control group. Given workers can transition into unemployment, the informal sector, or out of the labor force, Figure 2 is not restricted to a balanced panel.[10] However, as can be seen in Figure 2, there is a stark divergence in wages following privatization, with the average log monthly wage in the privatized group falling roughly 26 percent relative to the control group.

[10] Appendix Figure A1 presents results that are in levels rather than log wages. This figure is balanced but assumes individuals that do not appear in the data have a wage of zero, which is likely not true in practice given the large informal sector.

C. Worker-Level Effects

To estimate the effect of privatization on workers' outcomes, I use a matched difference-in-differences framework by estimating regression models of the following form:

$$y_{it} = \sum_{k=-4}^{10} \delta_k (t = t^* + k) Privatized_i + \alpha_i + \gamma_t + x'_{it}\beta + \varepsilon_{it} \tag{6}$$

where y_{it} is an outcome variable, $Privatized_i = 1$ if individual i was employed in an establishment that is privatized, α_i are worker fixed effects, γ_t are time fixed effects, x_{it} are time-varying worker controls that include a cubic in age, and ε_{it} is an error term. Each δ_k measures the change in the outcome variable (y_{it}) for the privatized workers relative to the control group in year k relative to the privatization year t^*. Standard errors are two-way clustered at the individual and establishment level.

There are two key assumptions in estimating Equation (6). First, results in Table 2 show that the matched control group is similar but not identical to the privatized workers group. If workers in the privatized group have differential wage trends, then I will incorrectly attribute wage changes to privatization. These differential trends may arise for a variety of reasons. For example, it is likely more difficult to obtain a job in an SOE than in a private-sector establishment, and so when we compare a worker in an SOE to a worker in a private establishment, it may not be surprising to observe both different wage levels and different trends.[11] Similarly, comparing a privatized worker to a worker in a never-privatized SOE may be confounded if there is selection into which establishments get privatized. For example, if the most productive SOEs are privatized, then privatization may be correlated with unobserved productivity which could cause privatized workers to be on a different wage trend than workers in never-privatized SOEs.

The second assumption is that there are no shocks that coincide with privatization. In Section E, I will discuss potential confounding factors, such as trade liberalization. Another consequence of this assumption is that privatization has no treatment effect on non-privatized workers. However, as discussed in Section III, the impacts of privatization could impact other establishments through product-market changes and changes in the outside option of workers. These impacts will bias the direct impact of privatization toward zero. In Section V, I directly estimate these types of market-level effects and find they are quantitatively important. Therefore, the estimates in this section may be interpreted as lower bounds.

Panel A of Figure 3 plots $\hat{\delta}_k$ from estimating Equation (6). As can be seen in the figure, wages between the privatized workers and the control group experience similar trends in the years before privatization, lending credibility to the parallel trends assumption. In the year after privatization, there is a small but insignificant decline in wages. However, wages continue to fall in the following years, reaching a decline of roughly 26 percent that levels off and does not recover. Panel A of Appendix Table A1 summarizes the results. Given the dynamic nature of the treatment effects, I aggregate the impacts into a short-run effect (average δ^k within first four years of privatization) and a long-run effect (average δ^k years five through ten following privatization).[12] Column 1 shows that in the short run, wages fall by 5.6 percent in privatized

[11] Managers for public-sector establishments in Brazil must pass a difficult test in order to be hired.

[12] Following the advice of Borusyak and Jaravel (2016), the short-run and long-run effects are estimated by first estimating Equation (6) allowing for flexible dynamics in treatment effects and then manually averaging the coefficients, weighting by the

establishments relative to the matched control group, while in the long run, this loss grows to 20 percent, with both effects being significant at the one percent level.

These earnings losses could be due to privatized workers being laid off and moving to lower-paying firms or within-establishment wage declines. In Figure 4, I estimate Equation (6) with a job transition as the outcome variable. A job transition occurs if the worker changes establishments or transitions into the informal sector (which is defined as having zero earnings in the administrative data). As can be seen in Figure 4, the probability of a job transition spikes in the year after a privatization event, by about 9.5 percentage points, with the effect gradually tapering off over time. This increase in job transitions also implies that privatized workers are increasingly likely to work in the informal sector, as can be seen in Appendix Figure A2.

To isolate the effect of privatization on within-establishment wages, rather than compositional changes in the type of establishment workers are employed at, I estimate the effect for establishment stayers.[13] A worker is defined as a stayer in time period t if the individual is still employed at the same firm she was employed at in time period $t = t^* - 1$, where $t = t^*$ is the period the establishment is privatized.[14] For control workers, the year of privatization is defined as the year the corresponding treated worker's establishment is privatized. Panel B of Figure 3 plots δ^k from estimating Equation (6) restricting the estimation to individuals who remain employed at the establishment they were working for in the year prior to privatization.

As can be seen in Panel B of Figure 3, earnings for establishment stayers in privatized SOEs drop gradually, reaching a 16 percent decline ten years after privatization. The gradual decline in within-establishment wages may be due to worsening negotiation positions for unions after privatization. For example, Guimarães (2007) provides anecdotal evidence that before privatization of Telebrás, telecommunications unions sought to include conditions that ensured certain benefits for workers after the privatization. These conditions were not included in the sale, and afterward, unions report being unable to secure across-the-board wage increases for the workforce or wage increases pegged to inflation or seniority. These types of changes are consistent with the negative wage effects accumulating gradually over time. In summary, while displacement does explain a portion of the total effect, there is still a significant decline in wages even conditional on remaining in the same firm.

D. Heterogeneity in Effects

While the average impacts are large, it is unclear how they vary by characteristics of the workers, and therefore the distributional impacts of privatization are unclear. For example, it could be that politically connected managers are able to inflate their wages in SOEs relative to private-sector firms, and so these large losses mainly reflect reduced rents for managers. Additionally, exploring differences across industries will be helpful when discussing mechanisms further in Section V.

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sample size at each point.

[13] Online Appendix E provides a decomposition of the aggregate effect into a displacement and within-establishment effect.

[14] In administrative datasets firm identifiers can change for multiple reasons. For example, tax identifiers may change after mergers and acquisitions. I recode these transitions as remaining within the firm by identifying such administrative changes using worker flows following Benedetto et al. (2007). If at least 70 percent of workers in firm A move to firm B in period t, then all workers in firm A that move to firm B are classified as stayers.

To begin, in Figure 5, I re-estimate the direct effects for individuals with no-high school degree, high-school degree, and college degree separately. As can be seen in Panel A of Figure 5, wage losses are largest for individuals without a high-school degree, approximately a 39 percent decline ten years after privatization. For comparison, wages for individuals with a high-school degree fall by about 20 percent relative to the matched control group while workers with a college degree fall 16 percent relative to the control group.

Turning to Panel B of Figure 5, while the wage losses for stayers are still largest for workers without a high-school degree, the differences are much smaller in magnitude and not statistically different. Therefore, conditional on remaining in the firm, the effect of privatization does not vary by education. Panel B of Figure 4 presents results for the impact of privatization on the probability a worker experiences a job transition. Interestingly, the probability of a job transition is relatively similar between education groups.[15] Taken together, these results imply that that while low-educated and high-educated workers are laid off at similar rates following privatization, the low-educated workers suffer particularly large losses from displacement. This is consistent with the evidence in Table 1 that finds substantially large AKM establishment wage premiums for low-educated workers in SOEs vs. higher-educated workers in SOEs.

The displacement effects by education also highlight that high-skill workers, such as managers, were not able to isolate themselves from the disruptive impacts of privatization. I find that contrary to other settings which result in job displacement, such as recessions, workers with shorter within-establishment tenure are less likely to be laid off after privatization.[16] Appendix Figure A4 estimates the probability of a job transition by quartiles of within-establishment tenure, finding small impacts for the lowest quartile (newly hired workers) and large and significant impacts for workers in all other quartiles.

Turning to heterogeneity by race, Appendix Table A2 estimates the direct effects separately for white vs. nonwhite workers, where following Gerard et al. (2018), nonwhite workers include Black and mixed race individuals. While earnings losses are large for both groups, nonwhite workers suffer especially large losses, with a long-run decline in earnings equal to 25 percent (vs. 18 percent for white workers). However, again, conditional on remaining within the firm, white and nonwhite workers experience similar wage declines following privatization. Taken together, these heterogeneity results with respect to education and race imply that while privatization does lower wages across worker characteristics, the losses tend to be largest for the least economically advantaged groups.

Turning to heterogeneity across industry, I find in Appendix Table A3 that establishment stayers suffer the smallest losses in the banking sector. While wages fall by about 4.1 percent in the long-run in the banking sector, they fall by 16.7 percent in telecommunications and 13 percent in the electricity sector. On the other hand, in Appendix Table A4, I find that displacment effects are largest in the banking sector, with privatized workers facing a 17.0 percentage increase in transitioning into the informal sector. In contrast, in telecommunications and electricity, workers in privatized SOEs are 7.8 percentage points and 6.4 percentage point more likely to transition into the informal sector, respectively.

[15] Similarly, the probability of transitioning into the informal sector in Appendix Figure A3 is strikingly similar by education group.

[16] Farber (1993) for example finds that younger and less-educated workers are more likely to be displaced in a recession.

E. Robustness of Worker-Level Effects

I verify the robustness of these results under several deviations from the baseline specification, exploring different control groups, different matching algorithms aimed at controlling for potentially coinciding shocks, as well as different specifications of worker earnings.

Control Group: In the primary analysis, I allow privatized workers to be matched to workers in SOEs that are never privatized or to workers in private-sector firms. In Panel A of Appendix Figure A5, I match to private-sector workers only, while in Panel B of Appendix Figure A5, I match to SOE workers only, to see how this decision impacts the results. As can be seen in these figures, while wage levels are very different between these two control groups (with SOE wages being much higher), there is still a stark trend break after the year of privatization between the treated privatized workers and either control group. Columns 1-2 of Appendix Table A5 display the results of estimating Equation (6) for the private-sector control and SOE control respectively. In both cases, I find a significant decline in earnings for the privatized workers.

Coinciding Shocks: One key potential threat to identification is the presence of coinciding shocks. As discussed earlier, trade liberalization occurred before the privatization events studied in this paper, but lagged effects of trade reform could potentially bias the results. However, it is important to point out that the majority of the establishments that were privatized sold non-tradable goods, implying these establishments are not directly impacted by trade liberalization. Still, indirect impacts could affect the results if liberalization impacts outcomes for the entire region, not just for the impacted industries, as discussed in Kovak (2013). For example, if privatized SOEs happen to be in regions that are more exposed to trade liberalization, then this could bias the results and may not be apparent in pretrends if liberalization effects appear only gradually.

In order to control for exposure to trade liberalization, I use the regional measure of exposure to trade liberalization in Dix-Carneiro and Kovak (2017) that depends on the industrial composition of a given microregion.[17] In other words, a region with a large fraction of employment in industries that experienced large tariff reductions will be highly exposed to trade liberalization. I then add deciles of this exposure to trade liberalization measure to the matching algorithm and re-estimate the worker-level direct effects. As can be seen in column 6 of Appendix Table A5, these estimates are very similar to the main results, suggesting differences are not being driven by treated workers being more exposed to trade liberalization. Online Appendix C provides additional details concerning trade liberalization in general as well as the construction of the trade liberalization exposure measure.

In addition to matching on regional exposure, I also provide a more aggressive approach to control for trade liberalization by matching explicitly on microregion and establishment-size deciles (column 7 of Appendix Table A5) and microregion and firm-size deciles (column 8 of Appendix Table A5). The logic here is that matching on microregion ensures that the control and treated workers are perfectly balanced with respect to regional trade exposure. Additionally, the international trade literature (e.g. Eaton et al. (2011)) has emphasized firm heterogeneity playing an important role in computing the effects of trade liberalization. Comparing workers in similarly sized establishments and firms attempts to restrict to not

[17] A microregion is an administrative boundary in Brazil similar to a commuting zone in the United States.

only similar workers, but also similar workplaces. As can be seen in columns 7-8 of Appendix Table A5, while the sample size drops considerably given the additional matching variables, the estimated effects are quite similar to the main estimates, suggesting that differential exposure to trade liberalization is not driving the results.

Additional Robustness Checks: Columns 3-5 of Appendix Table A5 present additional robustness checks. In column 3 of Appendix Table A5, log earnings are winsorized at the 5^{th} percentile and 95^{th} percentile. In column 4, I match workers two years before privatization, rather than a year before privatization. In column 5, I match workers based on 5-digit industry and 5-digit occupation codes, rather than 2-digit industry and 2-digit occupation codes. In all cases I find very similar results.

A final concern with the worker-level results is that the results are conditional on positive earnings. While I find similar effect sizes when measuring earnings in levels (including zeros) in Appendix Figure A6, in Online Appendix F I discuss the interpretation of earnings losses in the presence of an informal sector and use data from a Brazilian Household survey to show that there is a large informal sector wage penalty in Brazil. In Appendix Table A6, I find privatization increases the probability of leaving the formal sector by about 9.5 percent, and therefore the effects here likely understate the total impact of privatization on earnings of privatized workers.

F. The Effect of Privatization on Establishments

While the worker-level analysis is informative about incumbent workers, it is important to understand how privatization impacts establishment-level outcomes. This will be crucial in predicting how privatization will impact other firms. For example, if privatized SOEs replace entrenched workers with new hires from the private-sector, then privatization may expand job opportunities to private-sector workers and improve outcomes. If privatization results in permanent drops in employment at privatized SOEs, then this could lead to wage declines at private-sector firms.

To identify the impact on establishment-level outcomes, I use a similar matching strategy as the worker-level analysis. In particular, I match privatized SOEs to never-privatized SOEs in the same industry and establishment-size decile, where size is measured as the number of employees in the year prior to the privatization event.[18] In particular, I use a matched difference-in-difference framework by estimating regression models of the following form:

$$y_{jt} = \sum_{k=-4}^{4} \delta_k (t = t^* + k) Privatized_j + \psi_j + \gamma_t + \varepsilon_{jt} \tag{7}$$

where y_{jt} is an outcome variable, $Privatized_j = 1$ if establishment j is a privatized SOE, ψ_j are establishment fixed effects, γ_t are time fixed effects and ε_{jt} is an error term. All standard errors are clustered at the establishment level. Each δ_k measures the change in the outcome variable y_{jt} for the privatized establishment relative to the control establishment.

[18] Due to the gradual rollout, the impact of privatization can be identified by comparing privatized establishments to SOEs that will be privatized at a later date. I pursue this alternative strategy in Appendix Figure A7 and continue to find similar results.

Figure 6 plots δ_k from Equation (7) with log employment as the outcome, where employment is measured as the number of workers employed in the establishment that work at least 35 hours a week. As can be seen in Figure 6, log employment falls dramatically following privatization. Converting the average of the four post-event indicators to a proportional effect yields an estimated 22 percent decline in employment. Together with the worker-level analysis, this shows that privatized establishments shed incumbent workers, do not replace these workers through increased hiring, and decrease wages for these incumbent workers.

In Appendix Figure A8 I estimate the effect of privatization on establishment employment separately for the banking, telecommunications, and electricity sectors, three sectors with a number of privatized establishments during this time. I find that the negative employment effects are driven mostly by declines in the banking sector (Panel A), with an enormous 60.5 percent decline in employment in these establishments in the four years after privatization. While the impacts are negative in telecommunications and electricity (11.5 percent and 6.0 percent, respectively), neither of these declines is significant.

In Online Appendix D, I discuss and estimate the impact of privatization on establishment wage premia. Unlike the worker-level stayer analysis, the establishment-level analysis will also incorporate wage effects on new hires. The analysis is quantitatively similar to the worker-level stayer results and confirms that that wages fall within privatized establishments.

V. Market-Level Effects

A. Overview

The results in Section IV establish that privatization decreases both wages and employment in privatized SOEs. In this section, I estimate the market-level effects due to privatization by exploiting variation across private-sector labor markets in the level of exposure to privatization. To proceed, I first provide a definition of a labor market and then define the exposure measure which will be used to identify the market-level effects of privatization. Intuitively, the exposure measure will depend on two factors: (1) the fraction of jobs privatized within a labor market and (2) the fraction of jobs privatized in closely related labor markets, where labor markets are closely related if workers commonly transition between them.

B. Labor Market Definition

I follow Azar et al. (2020) and define the labor market by a microregion-by-occupation cell. A microregion in Brazil is a concept similar to commuting zone in the United States and has been used in Kovak (2013) and Menezes-Filho and Muendler (2011) to define labor markets in Brazil. In the RAIS data, approximately 75 percent of job transitions are within microregions.

Occupational classifications in the RAIS follow the *CBO* (*Classificação Brasilieira de Ocupações*), a five-digit classification system with a total of 2,355 individual occupations. This system is more narrowly defined than the Standard Occupational Classification (SOC) system commonly used in the United States, and in particular, in Azar et al. (2020), which contains 840 different occupations. Using the narrowest definition of the labor market may overstate the level of exposure if workers easily transition into closely related occupations. I deal with this problem in two ways. First, the exposure measure will directly take into

account the possibility that workers can transition across labor markets. Therefore, while an observation is an occupation-by-microregion cell, exposure to privatization depends on the fractions of jobs privatized within the occupation-by-microregion cell, as well as the fraction of jobs in closely related occupation-by-microregion cells. Second, while the main specification defines occupations at the five-digit level, I present a series of robustness checks which vary the definition of an occupation.

C. Exposure Measure

To begin, let P_{it} be the proportion of jobs in labor market l_i in 1995 that are privatized at time t. One way to define the exposure measure would be to use P_{it} directly as the measure. However, this would ignore the fact that many transitions in the data are between labor markets, rather than within, and could understate (or overstate) a given labor market's exposure to privatization. For example, suppose all electrical technician jobs in a region are privatized, but electrical technicians can easily transition into technician jobs available in other engineering fields, such as civil or computer engineering. Then using $P_{it} = 100$ for electrical technicians is likely to overstate the exposure to privatization.[19]

Therefore, the exposure measure for a worker in labor market l_i is based on the level of privatization (P_{it}) in labor market l_i, as well as the level of privatization (P_{mt}) in other labor markets l_m, weighted by the distance between two labor markets. The distance metric $d_{i \to m}$, is defined by the fraction of individuals in labor market l_i who transition to a job in labor market l_m (conditional on a transition occurring) in the years prior to privatization. Formally, for labor market l_i, the exposure to privatization at time t is given by:

$$Exposure_{i,t} = \sum_{t' \leq t} \sum_{m=1}^{L} d_{i \to m} P_{mt'} \tag{8}$$

Where $P_{mt'}$ is the proportion of jobs in labor market m which are privatized in time period t'. Note that if all the jobs a worker may transition to are privatized, the exposure measure for this worker's labor market will be equal to 100. If none of the jobs a worker may transition to are privatized, the exposure measure will be equal to zero. Also, note that the exposure measure is cumulative. Since I restrict the analysis to events which occur between 1996-2000, the exposure measure for labor market l_i reaches its maximum value in 2000, at which point it stays constant. I denote the total exposure of labor market i as $Exposure_i^m = Exposure_{i,2000}$.

At the five-digit occupation level, there are 94,777 unique labor markets. Overall, about 3 percent of the workforce in 1995 is employed in an establishment that is eventually privatized.[20] The exposure level, however, is highly skewed. About 14 percent of labor markets have an exposure level equal to zero, whereas the maximum exposure level is equal to 100. Overall, the mean exposure level across labor markets is equal

[19] For example, 43 percent of electrical technicians who switch jobs transition into a different occupation. Of those who transition into other occupations, the most common transition is to mechanical engineering technician, which occurs in 10 percent of cases. Interestingly, in Brazil, transition rates across occupations and regions are relatively similar by education, as shown in Appendix Table A7.

[20] This statistic is restricted to privatization events occurring between 1996-2000 and therefore understates the total number of workers impacted by privatization in Brazil. As discussed in Section II, a number of large federal companies were privatized before 1995, but due to data limitations, these privatization events cannot be identified in the RAIS data.

to 3.0 with a standard deviation equal to 5.4. Appendix Figure A9 presents the distribution of $Exposure_i^m$ across labor markets in Panel A, as well as the distribution of $ln(Exposure_i^m + 1)$ in Panel B. In the next section, $ln(Exposure_i^m + 1)$ will be the primary measure of exposure, though I will show robustness to the exact transformation of the exposure variable.

D. The Impact of Exposure to Privatization on Market Wages

To begin, a simple way to estimate the effect of exposure to privatization on market wages is to compare earnings changes in highly exposed markets vs markets that are less exposed. Appendix Figure A10 presents a non-parametric binned scatterplot the shows the relationship between changes in log market wages against the primary exposure measure $ln(Exposure_i^m + 1)$. To construct the figure, I bin exposure into twenty equally sized groups and then plot the average change in the log market wage between a period after privatization (2001-2004) and a period before privatization (1992-1995). As can be seen in Appendix Figure A10, there is a clear negative relationship between wage growth and exposure, with a slope coefficient equal to -0.040. A one standard deviation in $ln(Exposure_i^m + 1)$ is equal to 0.76, implying a one standard deviation is associated with a roughly 3 percent decline in the market wage.

While this representation provides a clear way to visualize the variation driving the results, there are a number of potential concerns. First, it is possible that earnings were already falling in markets that were highly exposed to privatization. In order to understand the trends prior to privatization, I proceed to the main identification strategy that estimates a dynamic difference-in-differences specification of the following form:

$$ln(w_{it}) = \sum_{t=1992}^{2008} \delta_t \cdot (ln(Exposure_i^m + 1) \cdot \gamma_t) + \alpha_i + \gamma_t + \pi X_{it} + \varepsilon_{it} \tag{9}$$

where $ln(w_{it})$ is the average log wage in market i at time t, $ln(Exposure_i^m + 1)$ is the total cumulative exposure of a labor market to privatization, α_i are labor-market fixed effects, γ_t are year fixed effects, and X_{it} are market-level characteristics, which will be discussed further in the robustness section. The coefficients $\hat{\delta}_t$ capture the effect of exposure to privatization in year t. This specification leverages variation across microregion-by-occupation cells to identify the impact of exposure to privatization. For example, electrical technicians in a microregion with a lot of SOEs will probably be more exposed to privatization than electrical technicians in a microregion with few SOEs. Similarly, electrical technicians will likely be more exposed than civil engineers within a microregion, given the concentration of privatization in electrical companies. While the main estimating Equation (9) leverages all such variation (across microregion and across occupations), I provide additional specifications that restrict to identifying the effect of exposure using only within-occupation variation by including occupation-by-year fixed effects.

If exposure to privatization is random conditional on control variables, then δ_t should be zero in the years prior to privatization. If $ln(Exposure_i^m + 1)$ is correlated with other factors that impact wages even conditioning on labor market fixed effects and year effects, then δ_t will not be equal to zero in the years prior to privatization. For example, if electrical technicians (who are highly exposed to privatization) benefited from skill-biased technological change in the years before privatization, then δ_t would be trending upward in the years prior to privatization.

As can be seen in Panel A of Figure 7, there is a slight pretrend in average log wages at the labor market level in the years prior to privatization. The dotted line in the figure is a fitted line for the pre-privatization estimates (i.e. before 1996), extrapolated to the post-privatization period. The fact that this line lies well below zero suggests wages would have fallen in exposed labor markets even absent privatization. However, it is also clear that the actual estimates lie well below the fitted line. In order to allow for linear pretrends, I follow Dobkin et al. (2018) and fit a parametric event-study that allows for a linear pretrend with respect to exposure to privatization:

$$ln(w_{it}) = \sum_{t=1997}^{2008} \delta_t \cdot (ln(Exposure_i^m + 1) \cdot \gamma_t) + \alpha_i + \gamma_t + \delta \cdot Exposure_i^m \cdot t + \pi X_{it} + \varepsilon_{it} \quad (10)$$

The difference here is that the specification now allows for a linear pretrend (captured by $\delta \cdot Exposure_i^m \cdot t$) and therefore the estimates of interest, $\hat{\delta}_t$, now capture the effect of exposure relative to any preexisting pretrend. As can be seen in Panel B of Figure 7 (which plots $\hat{\delta}_t$) there is still a significant effect of exposure to privatization above-and-beyond the pretrend. To summarize the effect, Table 3 reports an average of the post-2000 coefficients ($\sum_{t=2001}^{2008} \frac{1}{8}\hat{\delta}_t$). The preferred estimate appears in column 3. The magnitude of this coefficient implies that a one standard deviation in log exposure (0.76) is associated with about a 1.4 percent decline in the average log wage in the market (0.014 = 0.76×-0.019).

To put this number in perspective, I perform a simple exercise to compute the total impact of privatization on the aggregate wage and decompose this into a direct effect on workers in privatized firms and an indirect effect due to the market-level changes estimated above. Conceptually, the exercise takes all workers in 1995 (the year prior to the privatization period discussed in this paper) and computes the change in the aggregate wage if privatization effects occurred immediately. To proceed, I first compute the impact of privatization using only the direct impacts on privatized workers by forming:

$$w_i^d = w_i + \hat{\beta} Privatized_i \quad (11)$$

where $\hat{\beta}$ is the direct impact on privatized workers and w_i is the actual wage. Note that for any worker not in a privatized establishment, the counterfactual wage will be equal to the actual wage. In Figure 3, I found the long-run impact of privatization on wages is equal to about -0.299. However, this estimate is downward biased if control workers are exposed to privatization, and therefore also experience wage declines following privatization. Therefore, I form $\hat{\beta}$ by taking into account the exposure of the control group to privatization:

$$\hat{\beta} = -0.299 + \hat{\delta} \cdot \mathbb{E}[Exposure|Control] \quad (12)$$

where $\hat{\delta}$ is the estimated impact of exposure to privatization on wages ($\hat{\delta} = -0.019$ in column 3 of Table 3). $\mathbb{E}[Exposure|Control] = 1.4$, which in percent terms is about 3.1 percent of the labor market being privatized in the control group, implying $\hat{\beta} = -0.326$. Using this as the direct impact, the aggregate impact of privatization using only direct impacts is equal to:

$$\Delta w^{direct} = \mathbb{E}[w_i^d] - \mathbb{E}[w_i] = -0.010, \quad (13)$$

with a corresponding standard error of 0.001.[21] Next, I form the counterfactual wages which incorporate market-level effects on private-sector workers:

$$w_i^{agg} = w_i + \hat{\beta} Privatized_i + (1 - Privatized_i) \cdot \hat{\delta} Exposure_i \tag{14}$$

The aggregate impact of privatization on wages with both direct and indirect effects is then formed by:

$$\Delta w^{total} = \mathbb{E}[w_i^{agg}] - \mathbb{E}[w_i] = -0.031 \tag{15}$$

with corresponding standard error of 0.005. Therefore, the aggregate impact incorporating market-level impacts is about a 3.1 percent decline in the wage, about three times as large as the direct impact alone. In this setting, incorporating market-level effects is crucial for estimating the total impact of privatization on the labor market. The magnitude of the impact is similar to Beaudry et al. (2012) who study a setting in which declines in employment in a high-wage industry (e.g. manufacturing) spill over to other industries through a reduction in the outside option available to workers in other industries. They find that spillovers on other industries magnify the direct impact of changing industrial composition by about 3-4 times. Online Appendix G provides additional discussion of the magnitude of these market-level effects relative to the prior literature, as well as a simple calibration of the model in Section III.

Converting these proportional effects to US dollars (USD) implies that yearly labor income would be about 6 billion USD lower due to privatization. In comparison, revenue accrued from privatization (67.9 billion USD in sales and 18.1 billion in transferred debt (Anuatti-Neto et al., 2003)). While these numbers are not directly comparable given the revenue partly accrued from sales that occurred prior to the privatization events studied in this paper, the lost revenue for the government from lower labor earnings is not insignificant, and could meaningfully impact the government's expected revenue gain from privatization. One caveat to this analysis, however, is that by utilizing cross-sectional variation in exposure to privatization, aggregate impacts that impact all labor markets are netted out. If these aggregate impacts increase wages, then these calculations will overestimate the fall in wages due to privatization. For example, privatization of electricity may lower earnings for electricity workers relative to other workers, but improved efficiency of the electricity sector could lead to higher overall growth in the economy.

E. Robustness of Market-level Effects

I verify the robustness of these market-level results under several deviations from the baseline specification, exploring the role of the definition of the labor market, the specification of the exposure measure, as well as potentially coinciding shocks.

Definition of a Labor Market: While the exposure measure does allow for links across labor markets, a priori, it is unclear what the "correct" definition of a labor market should be in practice. This issue

[21] A bootstrap procedure with 500 replications is used to compute the standard error for both the direct impact and indirect impact. The bootstrap procedure draws bootstrap samples by drawing from random normals with mean zero and standard deviation equal to the standard error of $\hat{\beta}$ and $\hat{\delta}$, to construct β^b and δ^b and then forming counterfactual wages using these alternative estimates.

can be interpreted as inducing measurement error into the exposure measure. If the measurement error is classical, then this will attenuate the impact of exposure to privatization toward zero. However, given the large mass of labor markets with zero exposure level, the measurement error is likely non-classical, making it impossible to determine the sign of the bias. Therefore, Appendix Table A8 estimates the impact of exposure to privatization using a number of alternative market definitions. In all specifications, I continue to find a negative impact of exposure on average market wages with a magnitude similar across the various aggregations. However, the coefficient becomes insignificant when occupations are defined at the 1-digit level, likely due to the drastic decrease in the number of observations in this specification.

Specification of Exposure Measure: Lastly, while the main exposure measure is reported in logs and adds one so that zero exposure markets are included in the estimation, there are other potential ways to define exposure to privatization. Appendix Table A9 provides alternative specifications showing the qualitative results are not sensitive to functional form chosen. In column 2, I exclude markets with zero exposure. In Column 3, instead of log exposure, the dependent variable is the inverse hyperbolic sine of exposure. In Column 4, the dependent variable is exposure in levels, and in Column 5, the exposure measure is an indicator for the labor market having a positive level of exposure. In all instances, I continue to find a negative and significant effect of the exposure to privatization measure on market-level wages. Therefore, while the main analysis measures exposure in logs to reduce the influence of outliers (exposure in levels has a long-right tail), the qualitative results remain similar under reasonable alternative specifications.

Coinciding Shocks: Similar to the direct worker-level effects, the market-level results could be confounded by differential exposure to trade liberalization. In order to control for this, I include the measure of regional trade exposure computed in Dix-Carneiro and Kovak (2017) interacted with year, in order to allow for different trends for labor markets that are differentially exposed to trade liberalization. Going from a specification that does not control for trade liberalization (column 1 of Table 3) to a specification that does control for trade liberalization (column 2 of Table 3) reduces the effect of exposure to privatization by around 22 percent (from -0.041 to -0.032). Labor markets that are more exposed to trade liberalization are also more exposed to privatization. My preferred estimate of the effect of exposure to privatization on log labor-market wages appears in column 3 of Table 3, which controls for both trade liberalization and allows for a linear pretrend in labor market wages with respect to exposure.[22]

One additional confounding variable comes from the fact that privatization events can only be identified in the data starting in 1996, even though there was a period of privatization in the early 1990s. Therefore, if the exposed labor markets were also more likely to be exposed in the earlier privatization period, then the effect size may be too large, as I am not taking into account the fact that the labor markets were already exposed to an earlier privatization event. One way to test this is to eliminate observations from industries that were heavily privatized in the early 1990s. Anuatti-Neto et al. (2003) report a list of the privatized firms in the earlier privatization period. The primary industries privatized include metal industries, rubber produc-

[22]Controlling for trade liberalization after allowing for a linear pretrend does not actually change the coefficient on exposure. That is, a specification similar to column 3 of Table 3, but that does not control for trade liberalization, also yields a coefficient of -0.019.

tion, chemicals and petrochemicals. Dropping workers in these industries, who are most heavily exposed to the earlier privatization period, results in a similar estimate of the effect of exposure to privatization on log market wages, as can be seen in column 3 of Appendix Table A10.

Additional Robustness Checks: Appendix Table A10 presents a few additional robustness checks. First, outcomes within a microregion or within an occupation may be correlated. In column 1 I two-way cluster at the microregion and occupation level. While the standard errors increase, the effect of exposure to privatization on wages is still significant at the 5 percent level (rather than 1 percent level as in Table 3). Second, while the primary specification utilizes variation across microregions and across occupations, occupations exposed to privatization are likely different than the average occupation, as the privatized industries are in banking, telecommunications, and electricity. Therefore, in column 2 I include a specification that includes occupation-by-year fixed effects, and therefore only leverages variation within a given occupation cell to identify the effect of exposure. Under this alternative identification strategy, I find an elasticity of log market wages with respect to exposure to privatization equal to -0.021, very similar to the main estimates.

F. Mechanisms

To summarize the results so far, privatization leads to large employment and wage declines at privatized SOEs, which in turn generate market-level declines in private-sector firms connected to SOEs by labor mobility. In this section, I discuss the various channels that could generate these market-level declines and use heterogeneity across industries to explore different mechanisms.

In terms of the model presented in Section III, if we assume SOEs are only a small portion of the market (both product and labor market) then privatization will have zero impact on other firms in the same labor market (i.e. no market-level effects). This follows because a firm's employment and wage decisions depend on revenue per worker, bargaining power, and the value of the workers' outside option. If SOEs are a negligible part of the market, then these parameters would not be shifted by changes at SOEs.

Once we assume that SOEs are not a small portion of the market, three channels can create market-level declines in wages. First, changes in the technology or prices of SOEs may spill over to other firms, generating market-level declines in wages. This channel can be interpreted through the model as changes to the revenue shifter of the firm θ_j or the technology of the firm $F(L_j)$, both of which will change the revenue per worker. To test this channel, I focus the analysis on manufacturing industries outside of the privatized industries, for which competition and technology are less likely to be impacted by privatization.

Second, when SOEs serve as a potential outside option for workers, then declines in employment decrease the availability of outside options for workers while declines in wages decrease the quality of the outside option. Therefore, both declines in employment and wages lower the expected value of the outside option. I use variation across industries in the size of the employment and wage responses to privatization in order to disentangle these two channels.

Lastly, I discuss potential alternative interpretations outside the wage-bargaining model in Section III. For example, the fall in employment at SOEs could generate market-level declines even in a perfectly competitive model of the labor market in which wages do not depend on the value of the outside option, but only

on the marginal product of workers. For example, a shift in aggregate demand for labor will lower wages and employment even in a perfectly competitive framework.

Product-Market Channel: As discussed previously, changes in product-market parameters due to privatization may impact labor demand or productivity. One way to test whether this channel can solely explain the market-level impacts is to restrict the analysis to industries for which we think competition and technology are more likely to be stable. In columns 4-6 of Table 3, I restrict the market-level analysis to manufacturing industries only, for which the price of the output is determined on a global market, and therefore, may be less prone to product-market changes due to privatization. Additionally, all of these industries are outside the primary industries privatized during this period. Therefore, these estimates will not reflect changes in wages at firms in the same industry as the privatized firms, which could arise due to increases in efficiency due to agglomeration spillovers.

Across all specifications in columns 4-6, I find that the elasticity of market wages with respect to exposure is similar in tradables industries, and significantly larger in the specification in column 6, which controls for trends prior to privatization. In column 6 the elasticity of labor-market earnings with respect to privatization exposure is -0.072, much larger than the corresponding estimate for all industries -0.019. As can be seen in the table, however, the magnitude of the elasticity increases quite dramatically when allowing for pretrends in manufacturing industries. This stems from the fact that more exposed manufacturing markets were experiencing large increases in wages relative to less exposed markets in the years prior to privatization. Therefore, one should interpret the exact magnitude with caution, as it will depend on how well the linear trend captures the counterfactual trends in earnings absent privatization. It is not clear that highly exposed manufacturing markets would have continued to experience substantial wage growth relative to less exposed manufacturing markets in the absence of privatization.

Still, the fact that the manufacturing coefficient is similar or larger might suggest that the product-market effects are actually positive for privatized industries, which could be rationalized by increases in productivity in privatized industries. However, it is important to note that these industries differ in many ways, and therefore differences in magnitudes could be rationalized through alternative channels. For example, in terms of the model, the bargaining weight (γ_j) could differ between manufacturing and non-manufacturing firms. Therefore, if a manufacturing firm and a non-manufacturing firm experience the same-size decrease in the value of outside options (v), there will be a different effect on wages, even if the product-market parameters are stable in both industries.[23] The ideal experiment to isolate product-market effects would be to find two labor markets with identical labor-market exposure to privatization, identical labor-market parameters (i.e. productivity and bargaining power) but different product-market exposures. Given both labor-market and product-market parameters likely vary across industries, it is unclear what specification would satisfy this requirement. The tradables analysis however does show that the market-level effects are not due *solely* to product-market changes.

[23]This can be seen in Equation (3). If $\frac{\theta_j F_{L_j}}{L_j} + \tau_j$ remains constant, v falls equally in manufacturing and non-manufacturing firms, the size of the wage response depends on the bargaining weight γ_j. Intuitively, if $\gamma_j = 1$, then the value of outside options is irrelevant, and so the fall in wages will be zero. As γ_j approaches zero the size of the wage change will increase.

Outside Options: Second, both declines in employment and wages at SOEs will reduce the expected value of the outside option available to private-sector workers. To study these two channels independently, I use variation across industries in the size of the employment and wage effects. Recall in Section IV that the negative employment effects were driven mostly by declines in the banking sector, with an enormous 60.5 percent decline in employment in these establishments in the four years after privatization. While the impacts are negative in telecommunications and electricity (11.5 percent and 6.0 percent, respectively), neither of these declines is significant. In terms of wage responses, wages fell by about 4.1 percent in the long-run in the banking sector, while they fell by 16.7 percent in telecommunications and 13 percent in the electricity sector. Therefore, overall, the banking sector experienced the largest negative employment effect, but the smallest wage effects. If the market-level declines are driven solely by employment losses at privatized firms, then we would expect negative declines in private-sector labor markets exposed to the banking sector, but not in private-sector firms exposed to telecommunications and electricity.

In Appendix Table A11, I estimate the effect of exposure to privatization separately by the source of exposure. In column 1, exposure is defined only by using jobs privatized in the banking industry. Therefore, a labor-market will be highly exposed if workers from that labor-market commonly transition to jobs in banking SOEs that have been privatized. In column 2, exposure is defined using only jobs privatized in the telecommunications sector, while in column 3, exposure is defined using only jobs privatized in the electricity sector.

The results are not consistent with the labor-supply channel being an important driver of the market-level effects. The elasticity of market wages with respect to privatization exposure is smallest for banking exposure (-0.030), the sector with the largest employment effects and the smallest wage effects. For comparison, the elasticity is slightly larger in the telecommunications sector (-0.044) and the largest in the electricity sector (-0.127). It is important to note that while these three sectors experienced a number of unique privatization events, allowing for sector-specific results on firm employment, privatization events occurred in other industries as well. This explains why the effects of exposure here are uniformly larger than the aggregate effect, suggesting that labor markets exposed to these specific industry privatizations experienced larger wage declines than labor markets exposed to privatization events outside these industries.

Given that both electricity and telecommunications experienced insignificant employment declines but large wage declines suggests that a decrease in the value of outside options due to decreased wages is likely a driving factor in explaining the market-level effects. However, the variation in elasticities across sources of exposure also highlights the potential importance of heterogeneity in this setting and likely others. In terms of the model, this heterogeneity can be seen clearly through two parameters. First, the fact that the expected wage is so high in SOEs $E[w|j = soe]$, implies that privatization will have a large impact on the outside option v. If SOEs paid similar amounts as private-sector firms, then we might not find such large wage effects, and therefore there would be no corresponding change in v. However, even conditional on a change in v, the outside option v is only important if changes to the outside option translate to changes in wages. If workers have all the bargaining power, $\gamma_j = 1$, wages will not depend on the outside option, but be completely determined by the productivity of labor, as in a perfectly competitive model of the labor market. Therefore, in order to predict the market-level impacts of privatization, it is important to understand

the size of the direct wage effect as well as the competitiveness of the labor market. In the case of Brazil, the direct effect is quite large, and the fact that the effect of exposure to privatization is the largest for electricity privatization suggests that the wages for workers exposed to electricity privatization are highly dependent on the value of outside options.

Alternative Interpretations: One potential channel which could drive the market-level effects and is not directly incorporated in the wage-bargaining model is through shifts in the aggregate demand of workers. This would rationalize decreased wages even absent any outside option channel. Again, the fact that private-sector markets exposed to telecommunications and electricity privatization suffered earnings losses suggests this is not the sole driver of the market-level declines, as these industries experienced smaller and insignificant drops in employment.

However, another way to probe this channel is to control directly for changes in the size of labor markets, as in Beaudry et al. (2012), who study whether changes in industrial composition generate spillovers through reductions in the outside option available to workers. If decreases in aggregate labor demand solely explain the results, then exposure should impact wages through the size of the labor market. Therefore, In column 4 of Table A10, I add the change in the logarithm of labor-market size to the regression specification and find the effect of exposure on wages to be unchanged. These results mirror Beaudry et al. (2012), who find a limited role for size effects explaining their results on how declines in high-wage sectors spill over to other sectors in the same region.

VI. Conclusion

Despite the importance of SOEs and the scope of privatization, few studies explore the impact on the labor market. While many studies find privatization increases the profits of firms Megginson and Netter (2001), theoretically, privatization could decrease worker welfare (Birdsall and Nellis, 2003). This paper studies the impact of privatization on workers in Brazil, which implemented a large privatization program in the 1990s. I find that wages fall by roughly 26 percent ten years after privatization for workers in privatized SOEs relative to control workers. On average, establishment employment fell by 22 percent.

These direct impacts had subsequent effects on private-sector labor markets hiring similar workers. In order to estimate the effect of privatization on private-sector labor markets, I construct a measure of exposure to privatization at the occupation-by-region level which traces how the impacts of privatization propagate through other firms in labor markets connected to privatized SOEs by labor mobility. I find that wages decline in labor markets that are more exposed to privatization relative to labor markets that are less exposed to privatization. A simple back-of-the-envelope calculation suggests that privatization decreased the aggregate wage in Brazil by about 3.1 percent, with about two-thirds of this effect attributable to indirect impacts on private-sector firms.

These results suggest that privatization can have large, negative impacts on workers in privatized firms, as well as private-sector workers in similar occupations. However, the total welfare consequences of privatization are still unclear. First, it could be wages and employment in SOEs were inefficiently high and subsidized by taxpayers. Therefore, privatization could redistribute income from high-paid SOE employees

to the general population by reducing the tax burden. Second, without firm-level data on output and prices, it is unclear how consumers are impacted by privatization. While a long literature finds firm profitability increases following privatization, the source of the gain remains unclear in many contexts. Increases in efficiency will contribute to economic growth and therefore privatization may be welfare-enhancing even if workers exposed to privatization are negatively impacted. On the other hand, if privatization increases profits only through reduced wages and employment, then the perceived benefits of privatization may work through decreased worker welfare and not increased output and lower prices.

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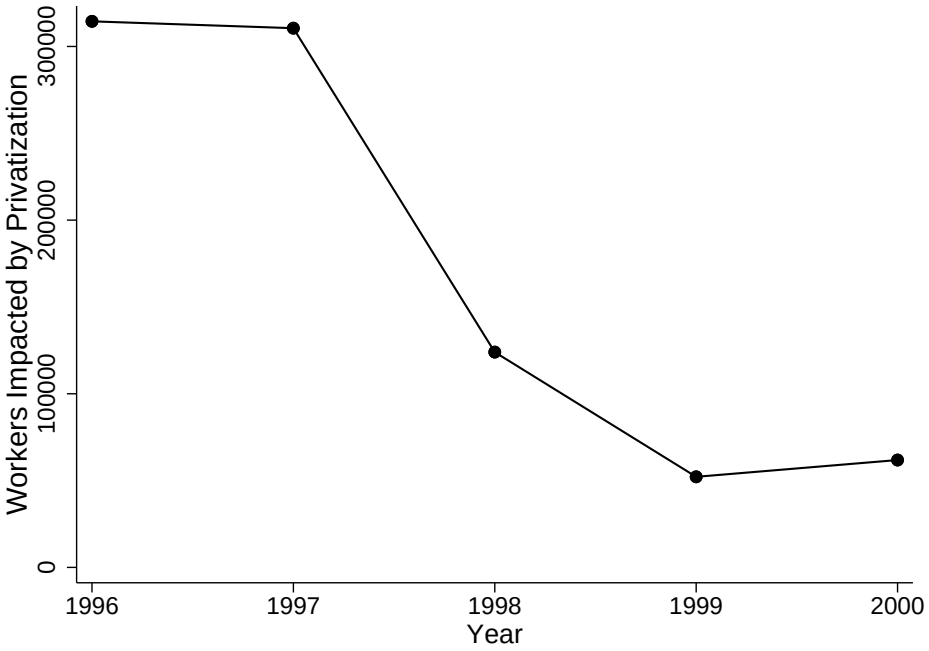
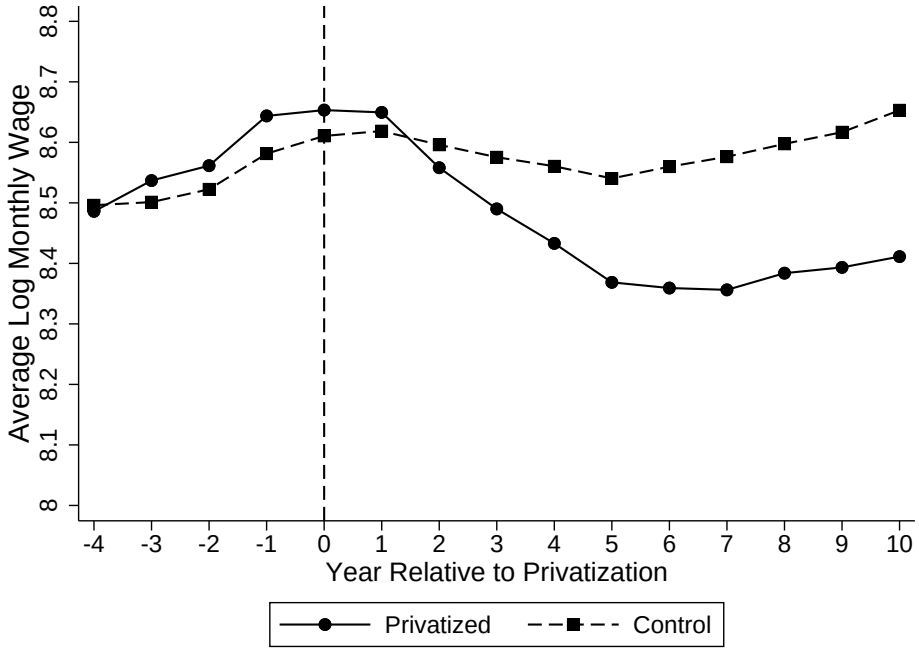
Figure 1: Number of Workers Employed in Privatized Establishments by Year



Note: This figure reports the number of workers employed in establishments that undergo privatization in a given year. Establishments are identified as privatized if the legal nature of the establishment changes from SOE to private and does not change back in a later year.

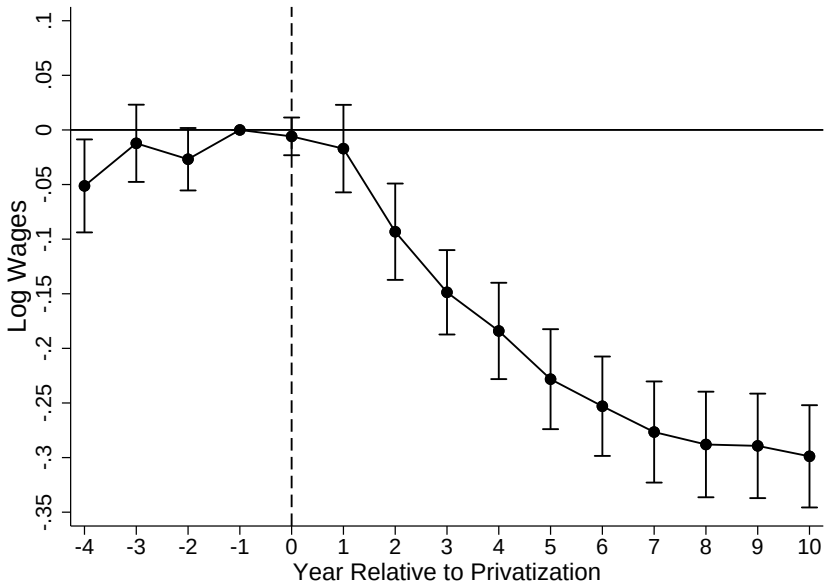
Figure 2: Average Log Monthly Wages of Privatized Workers and Matched Control Group



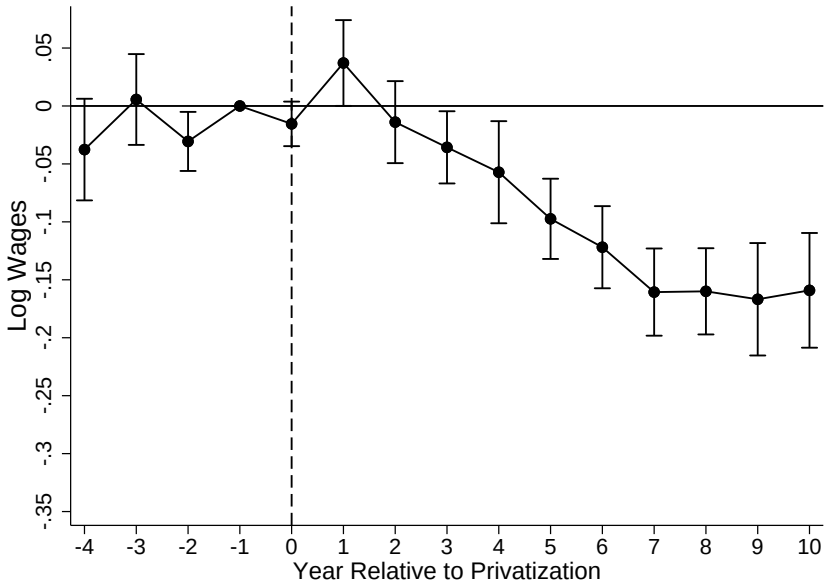
Note: This figure displays the average log monthly wage for workers in privatized establishments compared to a control group. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments). The panel is not balanced as individuals may transition into unemployment, retirement or the informal sector. The informal sector makes up roughly 40 percent of all employment in Brazil.

Figure 3: Matched Difference-in-Differences Estimates of the Effect of Privatization on Log Monthly Wages

Panel A: All Workers

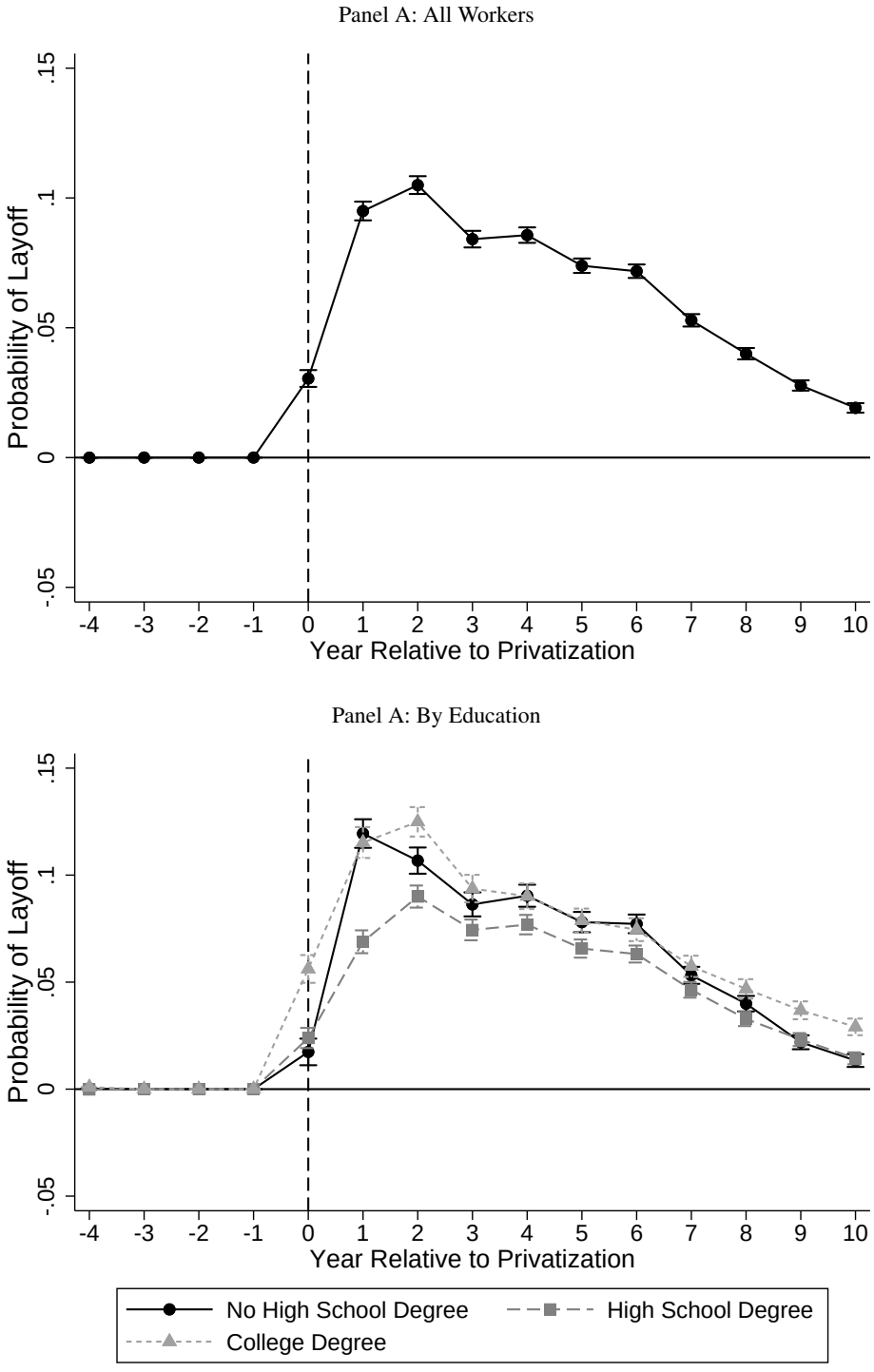


Panel B: Establishment Stayers



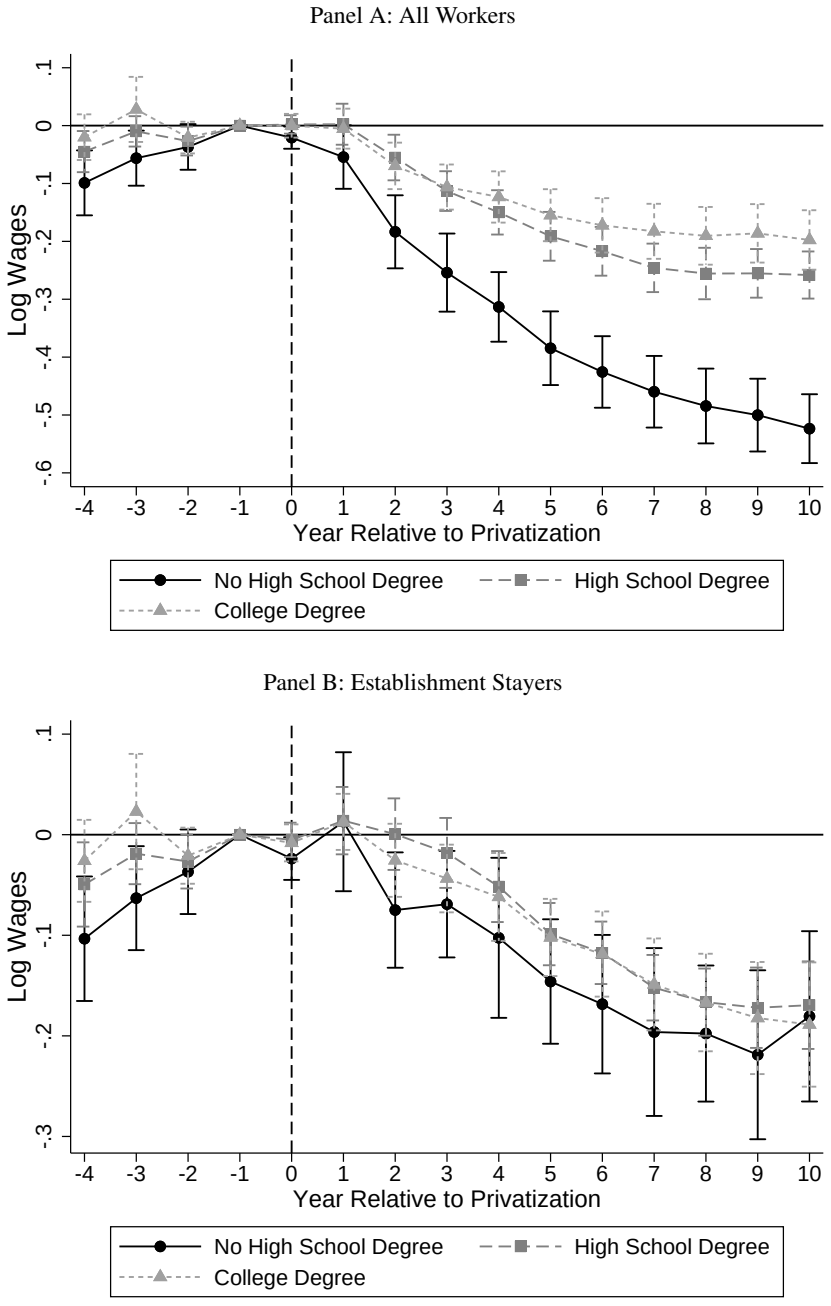
Note: This figure shows matched difference-in-differences estimates of the effect of privatization on log monthly wages. The omitted category is the year prior to the privatization event. The regressions control for individual fixed effects, time fixed effects, and a cubic in age. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments). Standard errors are two-way clustered at the individual and establishment level. Panel A includes all workers while Panel B restricts to establishment stayers. Stayers are defined as workers in time t who are employed in the same establishment as they were prior to privatization (i.e. year $t^* - 1$).

Figure 4: Matched Difference-in-Differences Estimates of Effect of Privatization on Probability of a Job Transition or Layoff



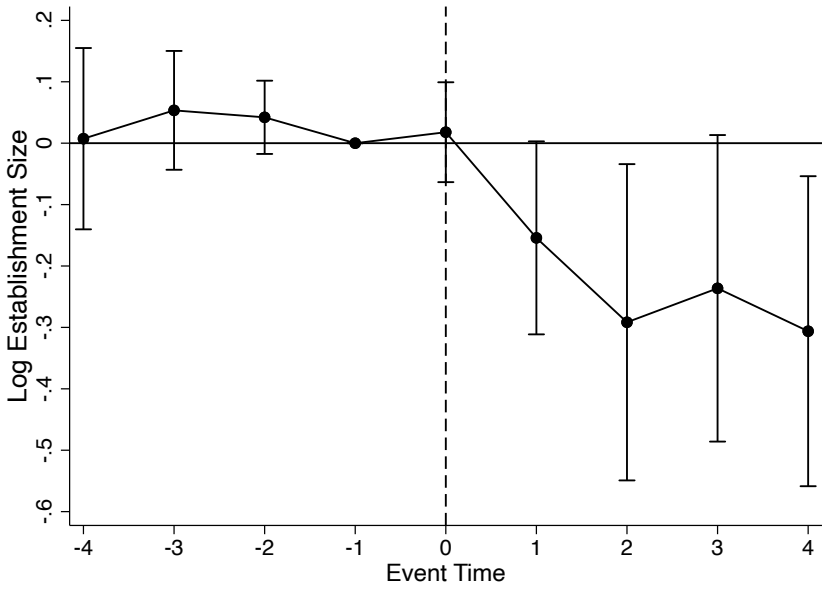
Note: This figure shows matched difference-in-differences estimates of the effect of privatization on probability of a worker transitions jobs or is laid off for all workers (Panel A) and by education (Panel B). A job transition is inferred from a change in establishment identifier while a layoff is inferred through a worker transitioning from the formal sector to the informal sector. The omitted category is three years prior to the privatization event, given workers must be in the same from from years $t^* - 2$ to $t^* - 1$ to be in the analysis sample. The regressions control for worker fixed effects, time fixed effects, and a cubic in age.

Figure 5: Matched Difference-in-Differences of the Effect of Privatization on Log Monthly Wages by Education



Note: This figure shows matched difference-in-differences estimates of the effect on privatization of log monthly wages separately by education. The omitted category is the year prior to the privatization event. The regressions control for individual fixed effects, time fixed effects, and a cubic in age. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments). Standard errors are two-way clustered at the individual and establishment level. Panel A includes all workers while Panel B restricts to establishment stayers. Stayers are defined as workers in time t who are employed in the same establishment as they were prior to privatization (i.e. year $t^* - 1$).

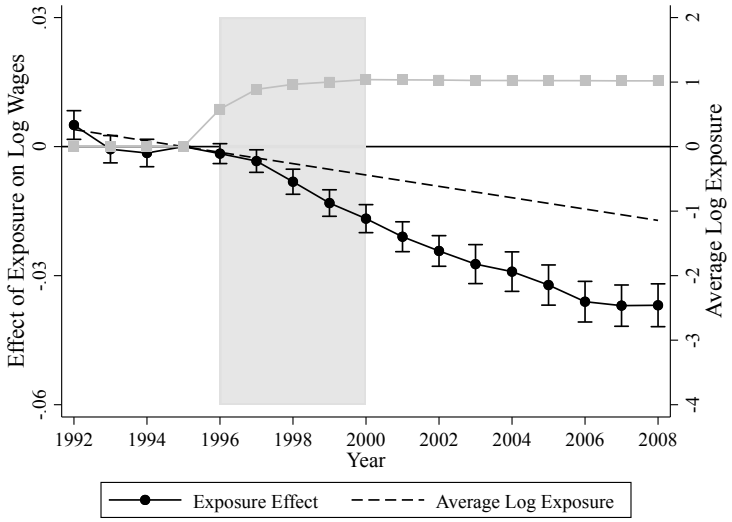
Figure 6: Effect of Privatization on Establishment Employment



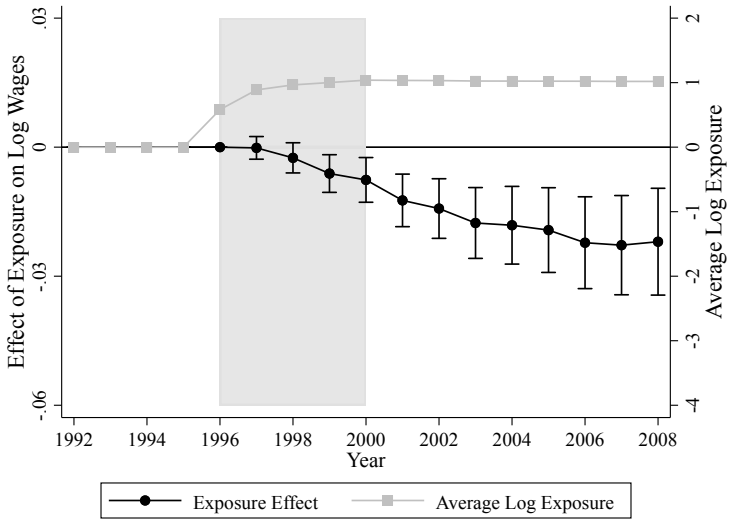
Note: This figure shows matched difference-in-differences estimates of the effect of privatization on log establishment employment. The control group is constructed by matching privatized establishments to never-privatized state-owned enterprises in the same industry and in the same size deciles (where size is equal to the number of employees in the year prior the privatization event). Standard errors are clustered at the establishment level.

Figure 7: Effect of Exposure to Privatization on Average Log Monthly Market Wages

Panel A: Fully Dynamic Event-Study Specification



Panel B: Parametric Event-Study Specification Controlling for Pretrends



Note: This figure shows event-study estimates of the effect of exposure to privatization on log monthly market wages. The labor market is defined as an occupation-by-microregion cell, where occupations are defined at the five-digit level. The regressions control for labor market fixed effects, year fixed effects, and an interaction between regional trade exposure and year. I exclude all privatized establishments when constructing the average market wage. The gray shaded area indicates the period of privatization. Panel A presents results from a fully dynamic specification. The dashed line in Panel A extrapolates the pretrend estimated in the years prior to privatization (1992-1996). Panel B presents a parametric event-study specification that allows for a pretrend in earnings with respect to exposure to privatization. The plotted coefficients in Panel B therefore capture the effect of privatization above-and-beyond the effect relative to the linear pretrend plotted in Panel A. Standard errors are clustered at the labor market level.

Table 1: Wage Premium in Privatized Establishments Before Privatization

	All Workers	Banking	Telecomm.	Electric.	No HS	HS	College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
AKM Premium	0.182	0.073	0.530	0.140	0.399	0.221	−0.007
	(0.002)	(0.005)	(0.007)	(0.002)	(0.006)	(0.003)	(0.005)
	[8.121]	[8.109]	[7.817]	[8.389]	[7.887]	[8.070]	[8.617]

Note: The wage premium is estimated in two steps using data from 1992-1995, the years prior to any privatization event studied in this paper. In the first step, I estimate an AKM model with worker and establishment fixed effects and collect the estimated establishment fixed effects. In the second step, I restrict to SOEs that will be privatized eventually and private-sector establishments in the banking, telecommunications, and electricity sectors. I then regress the establishment fixed effects on an indicator for privatized SOE and sector fixed effects. The coefficient on the indicator for privatized SOE captures the premium associated with working for a privatized SOE in the years prior to privatization. This regression is weighted by the number of workers in a given establishment. For columns 5-7 the AKM models are estimated separately by education allowing firms to have different effects for different levels of education. Heteroskedastic-robust standard errors are presented. Average log monthly wages are presented in brackets.

Table 2: Characteristics of Privatized and Non-Privatized Workers

	Privatized	Control
Panel A: Demographics	(1)	(2)
Age	39.17	39.11
High School Graduate	0.48	0.44
College Graduate	0.23	0.28
Male	0.71	0.73
Panel B: Job Characteristics		
Log Monthly Wage	8.64	8.58
Panel C: Establishment Characteristics		
Median Establishment Size	765	416
Banking	0.27	0.27
Telecommunications	0.25	0.25
Electricity	0.44	0.44
Mining Metals	0.03	0.03
Unique Establishments	3,196	14,661
Unique Workers	142,854	142,854

Note: This table displays the average characteristics for privatized workers and the matched control group. Privatized workers are in the sample if they worked at the privatized firm for at least two years prior to privatization and a matched worker is found among all potential control workers. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments).

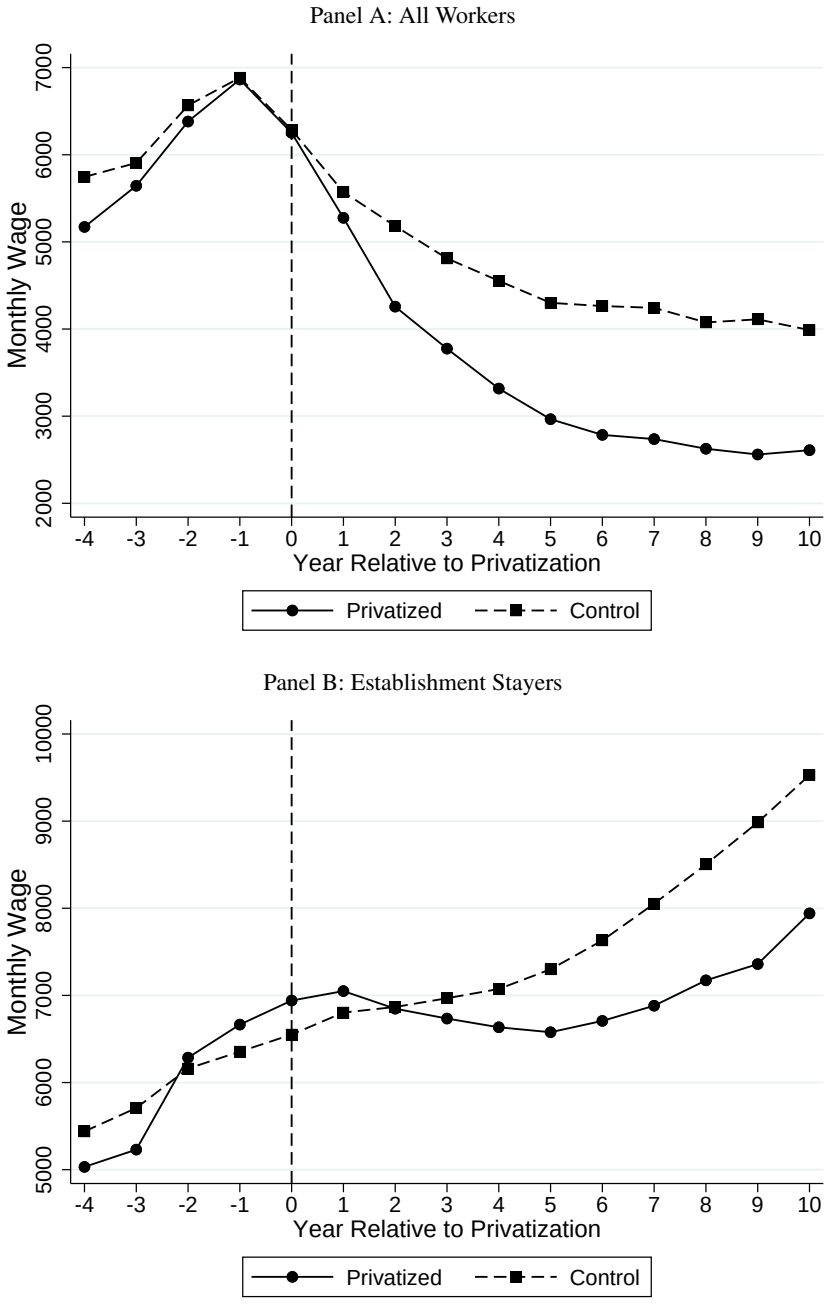
Table 3: The Effect of Exposure to Privatization on Average Log Monthly Market Wages

	All Industries			Tradables Only (Manufacturing)		
	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Exposure+1)	−0.041	−0.032	−0.019	−0.039	−0.017	−0.072
	(0.002)	(0.002)	(0.005)	(0.002)	(0.002)	(0.007)
Observations	1,408,374	1,408,374	1,408,374	714,940	714,940	714,940
Labor Market FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Trade Liberalization Control	No	Yes	Yes	No	Yes	Yes
Allow for Linear Trend	No	No	Yes	No	No	Yes

Note: This table presents OLS estimates of the effect of exposure to privatization on log average market wage in all sectors in Columns 1-3 and in manufacturing industries only in Columns 4-6. The labor market is defined as an occupation-by-microregion cell. To construct the average market wage, all observations from privatized establishments are excluded, and therefore, changes in the market wage do not reflect the direct impacts of privatization. Exposure to privatization depends on (1) fraction of jobs privatized within the labor market and (2) fraction of jobs privatized in closely related labor market. See Section V for more details on the construction of the exposure measure. The trade liberalization control is a measure of the average change in tariff levels in a region, as computed in Dix-Carneiro and Kovak (2017), interacted with a continuous year variable. Standard errors are clustered at the labor market level.

Online Appendix A: Additional Results

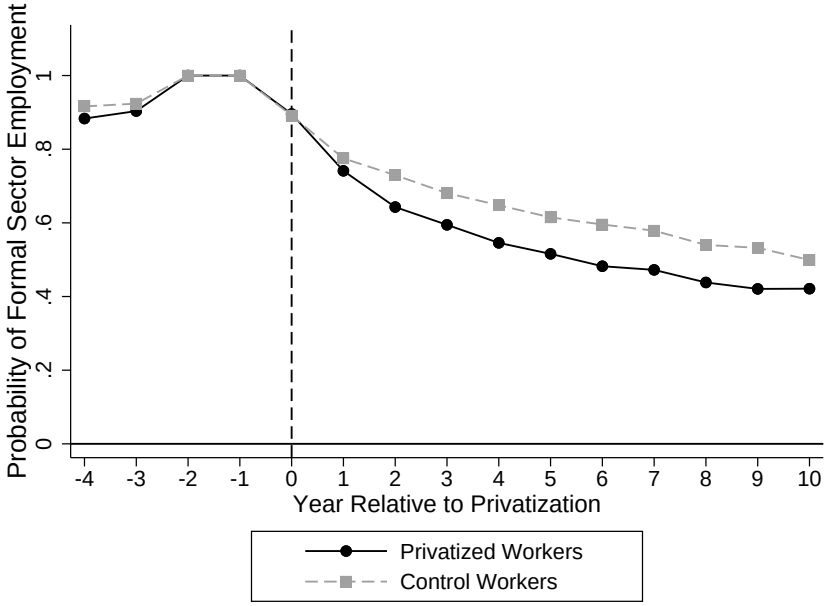
Appendix Figure A1: Monthly Wages in Levels of Privatized Workers and Matched Control Group



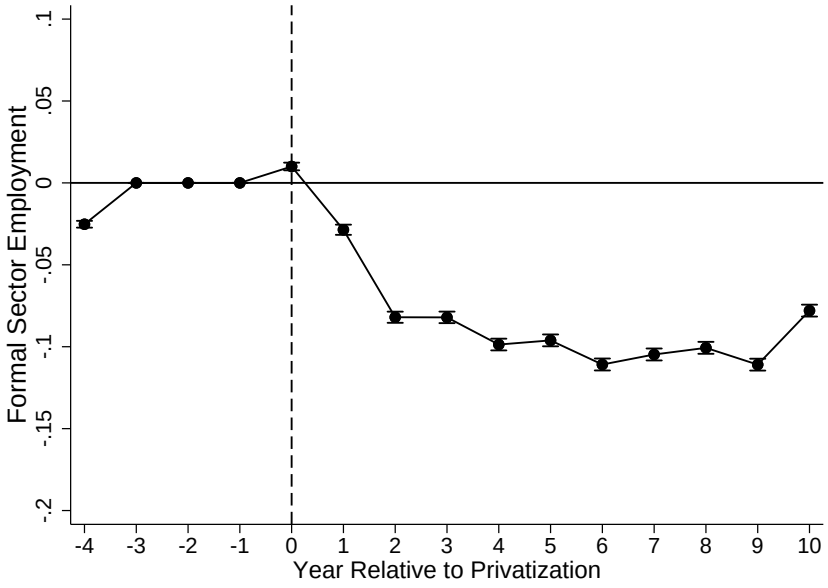
Note: This figure displays the average monthly wage for workers in privatized establishments compared to a matched control group. Panel A includes all workers while Panel B restricts to establishment stayers. Stayers are defined as workers in time t who are employed in the same establishment as they were prior to privatization (i.e. year $t^* - 1$). The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs (Panel B). The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments). The panel is balanced and individuals that transition into unemployment, retirement or the informal sector have imputed earnings equal to zero. The informal sector makes up roughly 40 percent of all employment in Brazil.

Appendix Figure A2: The Effect of Privatization on Formal Sector Employment

Panel A: Fraction of Workers Employed in Formal Sector

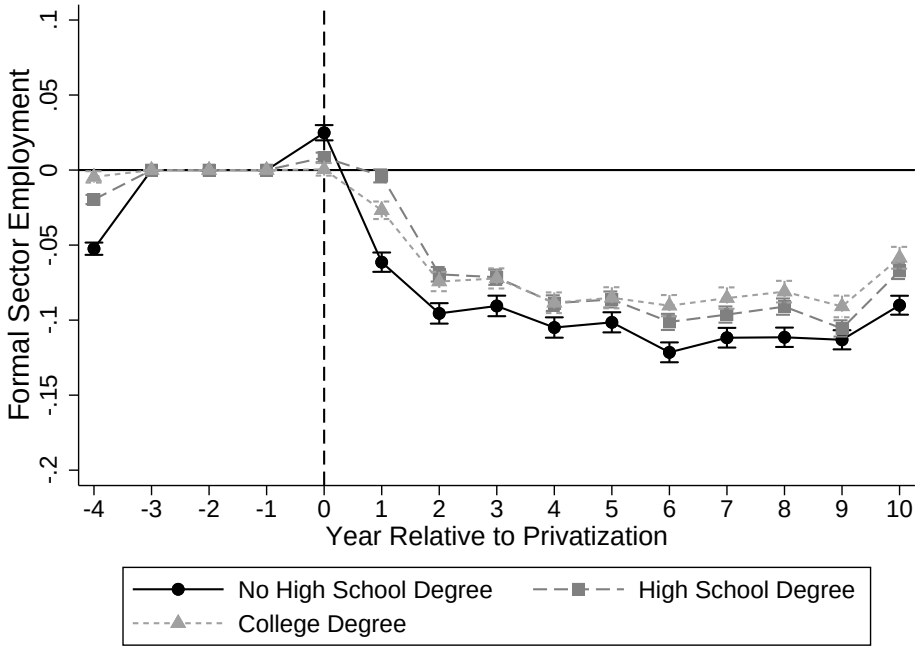


Panel B: Matched Difference-in-Differences Plot of the Impact of Privatization on Probability of Formal Sector Employment



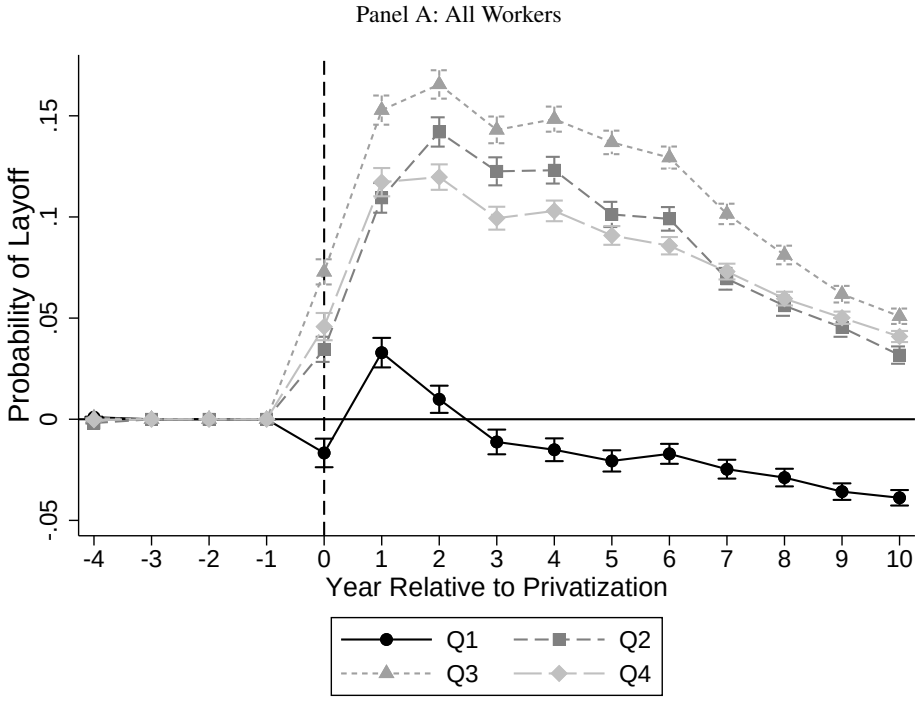
Note: This figure displays the fraction of the sample that is employed in the formal sector in each year relative to the privatization event in Panel A and matched difference-in-differences estimates of the effect of privatization on the probability of formal sector employment in Panel B. The omitted category is three years prior to the privatization event, given workers must be in the formal sector from years $t^* - 2$ to $t^* - 1$ to be in the analysis sample. The regressions control for worker fixed effects, year fixed effects and a cubic in age. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments). Standard errors in Panel B are clustered at the individual level.

Appendix Figure A3: Matched Difference-in-Differences Estimates of the Effect of Privatization on Formal Sector Employment by Education



Note: This figure shows matched difference-in-differences estimates of the effect of privatization on the probability of formal sector employment. The omitted category is three years prior to the privatization event, given workers must be in the formal sector from years $t^* - 2$ to $t^* - 1$ to be in the analysis sample. The regressions control for worker fixed effects, year fixed effects and a cubic in age. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments). Standard errors are clustered at the individual level.

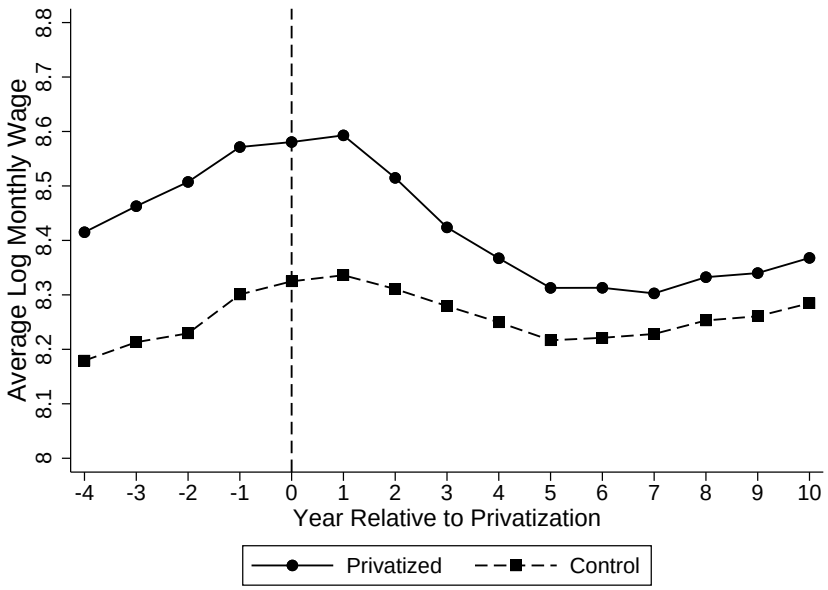
Appendix Figure A4: Matched Difference-in-Differences Estimates of Effect of Privatization on Probability of a Layoff



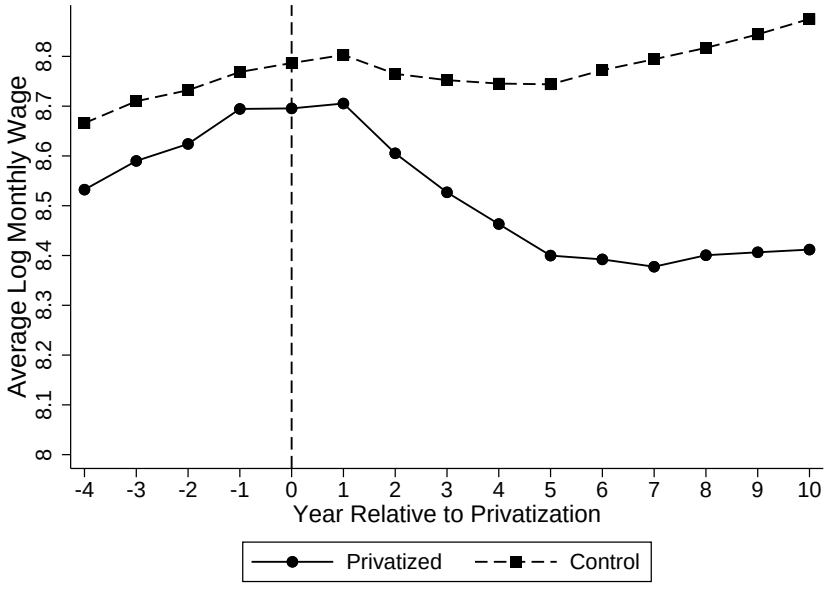
Note: This figure shows matched difference-in-differences estimates of the effect of privatization on probability of a worker is laid off by within-establishment tenure quartiles. A layoff is inferred from a change in establishment identifier or through a worker transitioning from the formal sector the informal sector. The omitted category is three years prior to the privatization event, given workers must be in the same firm from years $t^* - 2$ to $t^* - 1$ to be in the analysis sample. The regressions control for worker fixed effects, time fixed effects and a cubic in age

Appendix Figure A5: Log Monthly Wages of Privatized Workers and Matched Control Group

Panel A: Control Group: Private-Sector Workers

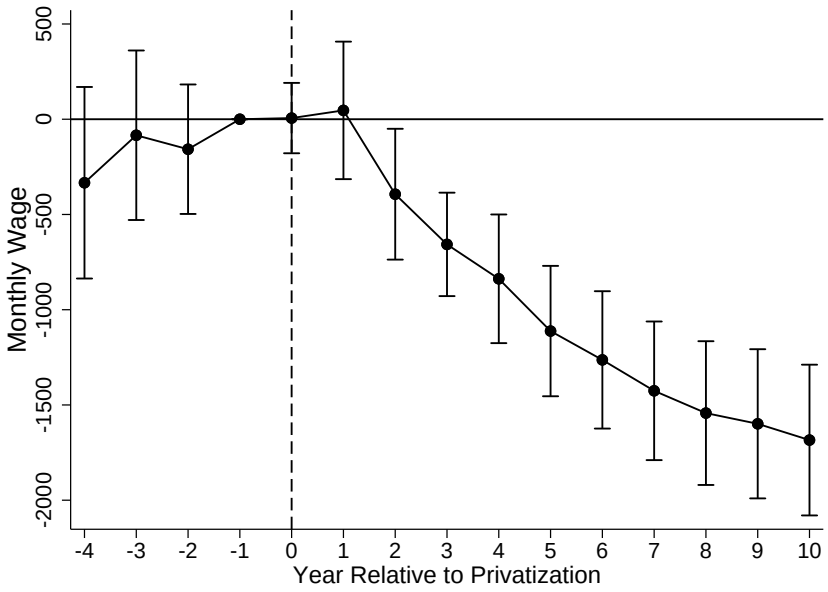


Panel B: Control Group: Never-privatized SOE Workers



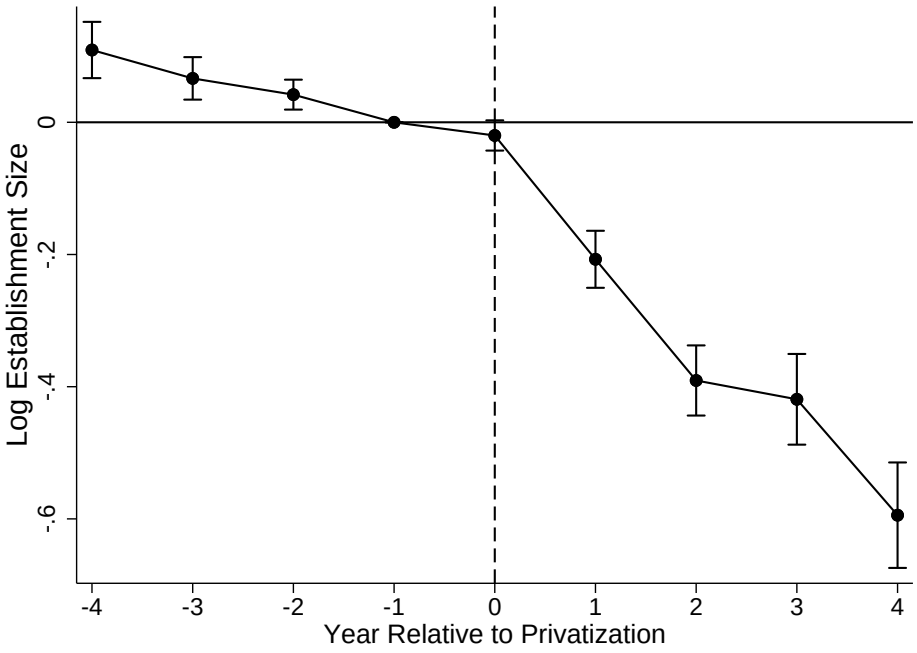
Note: This figure displays the average log monthly wage for workers in privatized establishments compared to a matched control group. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments (Panel A) or never-privatized SOEs (Panel B). The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments). The panel is not balanced as individuals may transition into unemployment, retirement or the informal sector. The informal sector makes up roughly 40 percent of all employment in Brazil.

Appendix Figure A6: Matched Difference-in-Differences Estimates of the Effect of Privatization on Monthly Earnings Including Zeros



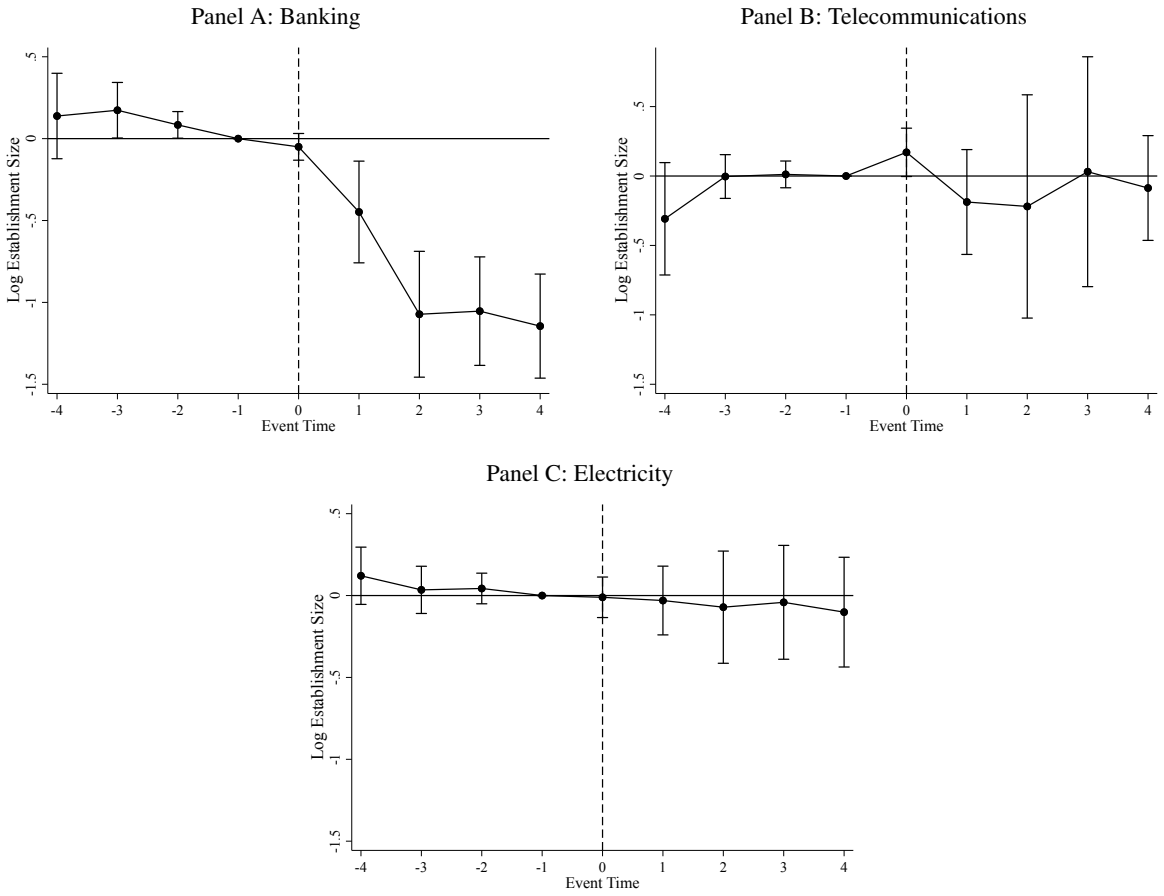
Note: This figure shows matched difference-in-differences estimates of the effect of privatization of monthly earnings in levels including zero earnings. The omitted category is the year prior to the privatization event. The regressions control for individual fixed effects, time effects, and a cubic in age. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments). Standard errors are two-way clustered at the individual and establishment level. The average monthly wage in the year prior to privatization is about 6,873 Real.

Appendix Figure A7: Event-Study Estimates of the Effect of Privatization on Establishment-Level Outcomes



Note: This figure shows event-study estimates of the effect of privatization of establishment-level log total employment. The omitted category is the year prior to the privatization event. The regressions control for establishment fixed effects and time fixed effects. The sample is restricted to all establishments that are privatized at some point during the sample period (1996-2000). Standard errors are clustered at the establishment level.

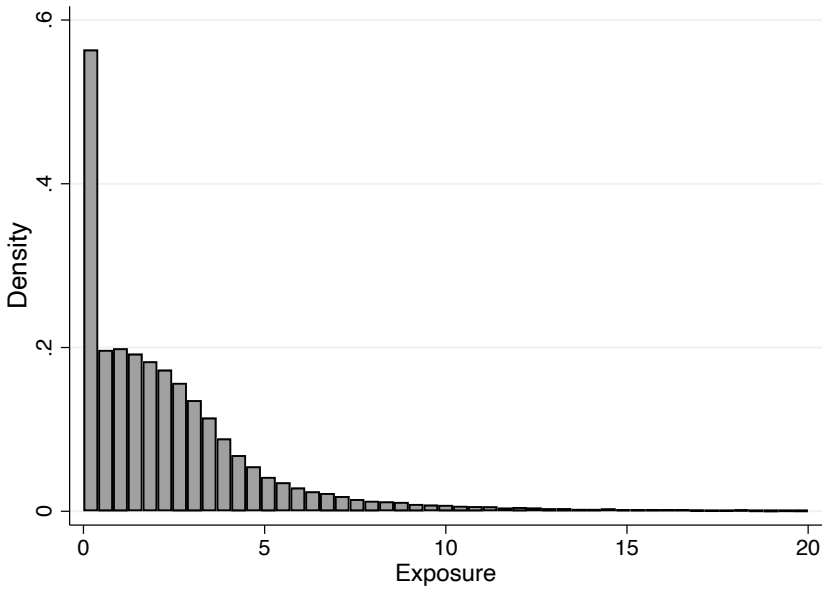
Appendix Figure A8: Matched Difference-in-Differences Estimates of the Effect of Privatization on Log Employment



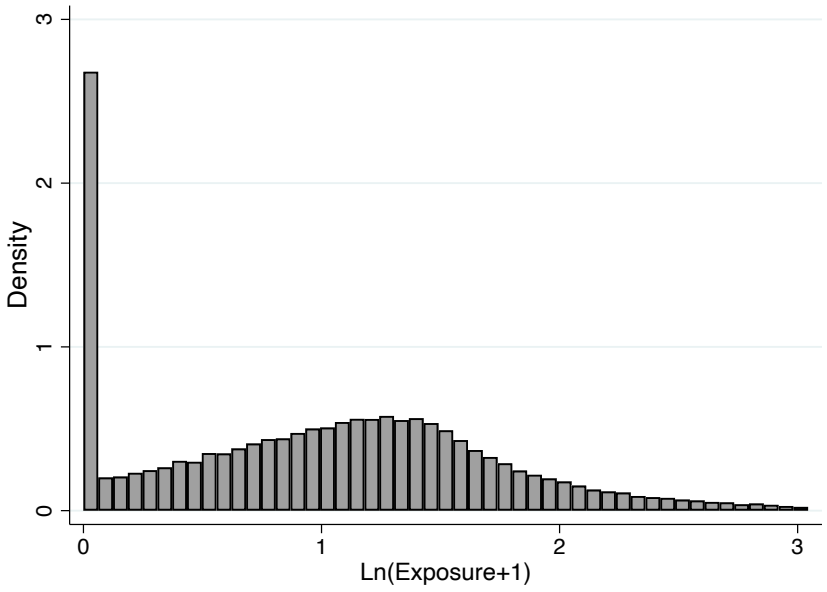
Note: This figure shows matched difference-in-differences estimates of the effect of privatization on log establishment employment. Panel A displays results for establishments in the banking sector, Panel B displays results for establishments in the telecommunications sector, and Panel C displays results for establishments in the electricity sector. The control gorup is constructed by matching privatized establishments to never-privatized state-owned enterprises in the same industry and in the same size deciles (where size is equal to the number of employees in the year prior the privatization event). Standard errors are clustered at the establishment level.

Appendix Figure A9: Histograms of Exposure Measures

Panel A: Histogram of Exposure

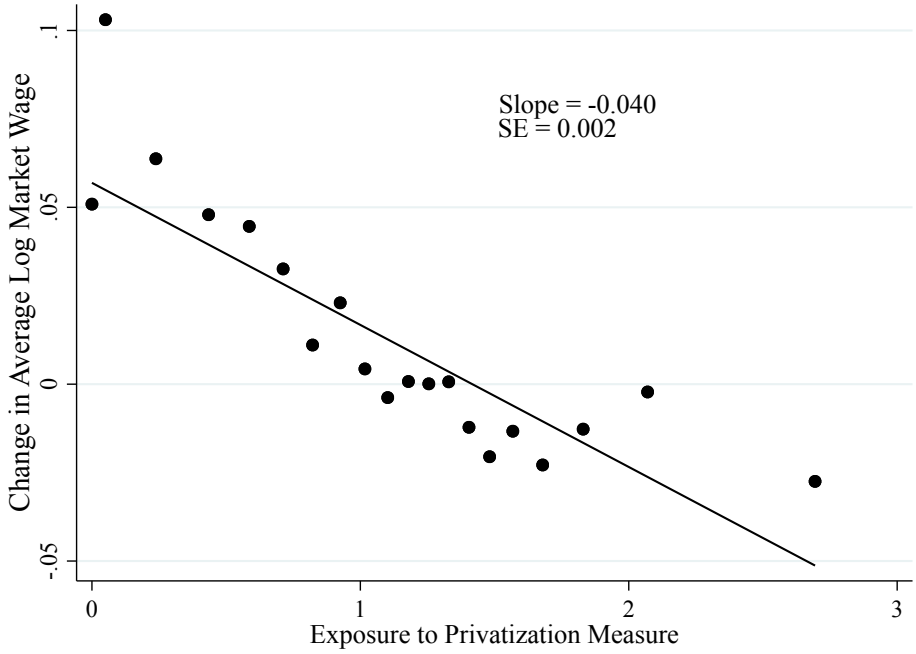


Panel B: Histogram of Ln(Exposure+1)



Note: This figure displays a histogram of exposure to privatization in Panel A and $ln(Exposure + 1)$ in Panel B. Each observation is a labor market, where a labor market is defined as an occupation-by-microregion cell. This plot defines occupations at the five-digit level.

Appendix Figure A10: Change in Market Wages vs. Exposure to Privatization Measure



Note: This figure presents a binned scatterplot of the relationship between changes in log market wages and log exposure to privatization. This figure compares wages in a post-privatization period (2001-2004) to wages in a pre-privatization period (1992-1995), where all privatization events studied in this paper occurred between 1996-2000. The distribution of the log exposure measure is given in Panel B of Appendix Figure A9.

Appendix Table A1: The Effect of Privatization on Log Monthly Wage

	All Workers	No HS Degree	HS Degree	College Degree
	(1)	(2)	(3)	(4)
Panel A: Effect of privatization on all workers				
Post Short-run	−0.058	−0.114	−0.035	−0.039
	(0.015)	(0.021)	(0.013)	(0.015)
Post Long-run	−0.225	−0.379	−0.191	−0.152
	(0.022)	(0.030)	(0.019)	(0.022)
Avg. Outcome at $t = -1$	8.612	8.386	8.542	8.999
Observations	3,746,461	864,978	1,822,663	1,058,820
Panel B: Effect of privatization on stayers				
Post Short-run	−0.007	−0.037	−0.003	−0.015
	(0.013)	(0.021)	(0.013)	(0.013)
Post Long-run	−0.103	−0.144	−0.098	−0.105
	(0.017)	(0.032)	(0.015)	(0.019)
Avg. Outcome at $t = -1$	8.612	8.386	8.542	8.999
Observations	2,416,378	607,995	1,160,806	647,577
Worker FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes

Note: This table presents matched difference-in-differences estimates of the effect of privatization on log monthly wages. Column 1 pools all workers while columns 2-4 split the sample by whether the worker has no high school degree, a high-school degree or a college degree. The short-run effect is equal to the effect of privatization in the four years following privatization. The long-run effect is equal to the effect of privatization in years five through ten following privatization. Panel A includes all workers while Panel B restricts to establishment stayers which are defined as workers in time t who are in the same firm as they were prior to privatization (i.e. year $t^* - 1$). The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments). Standard errors are two-way clustered by worker and establishment.

Appendix Table A2: The Effect of Privatization on Log Monthly Wage by Race

	White Workers	Nonwhite Workers
	(1)	(2)
Panel A: Effect of privatization on all workers		
Post Short-run	−0.040	−0.099
	(0.016)	(0.015)
Post Long-run	−0.199	−0.283
	(0.022)	(0.026)
Avg. Outcome at $t = -1$	8.638	8.415
Observations	2,134,935	522,754
Panel B: Effect of privatization on stayers		
Post Short-run	0.004	−0.024
	(0.014)	(0.015)
Post Long-run	−0.104	−0.095
	(0.017)	(0.018)
Avg. Outcome at $t = -1$	8.638	8.415
Observations	1,285,093	311,339
Worker FE	Yes	Yes
Time FE	Yes	Yes
Demographics	Yes	Yes

Note: This table presents matched difference-in-differences estimates of the effect of privatization on log monthly wages by race. Column 1 reports estimates for white workers while Column 2 reports estimates for nonwhite workers. Following Gerard et al. (2018), nonwhite workers include Black and mixed race individuals. The short-run effect is equal to the effect of privatization in the four years following privatization. The long-run effect is equal to the effect of privatization in years five through ten following privatization. Panel A includes all workers while Panel B restricts to establishment stayers which are defined as workers in time t who are in the same firm as they were prior to privatization (i.e. year $t^* - 1$). Standard errors are are two-way clustered by worker and establishment.

Appendix Table A3: The Effect of Privatization on Log Monthly Wage by Sector

	Banking	Telecom.	Electricity
	(1)	(2)	(3)
Panel A: Effect of privatization on all workers			
Post Short-run	0.012	−0.023	−0.129
	(0.008)	(0.031)	(0.019)
Post Long-run	−0.172	−0.083	−0.338
	(0.013)	(0.043)	(0.029)
Avg. Outcome at $t = -1$	8.703	8.395	8.705
Observations	1,149,189	951,965	1,645,307
Panel B: Effect of privatization on stayers			
Post Short-run	0.075	−0.085	−0.034
	(0.007)	(0.021)	(0.019)
Post Long-run	−0.042	−0.182	−0.140
	(0.012)	(0.030)	(0.029)
Avg. Outcome at $t = -1$	8.703	8.395	8.705
Observations	770,601	545,536	1,121,460
Worker FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Demographics	Yes	Yes	Yes

Note: This table presents matched difference-in-differences estimates of the effect of privatization on log monthly wages by sector. Column 1 includes only workers in the banking sector at the time of privatization, column 2 includes only workers in the telecommunications sector at the time of privatization, and column 3 includes only individuals in the electricity sector at the time of privatization, and Panel A includes all workers while Panel B restricts to establishment stayers which are defined as workers in time t who are in the same firm as they were prior to privatization (i.e. year $t^* - 1$). Standard errors are are two-way clustered by worker and establishment.

Appendix Table A4: The Effect of Privatization on Formal Sector Employment by Sector

	Banking	Telecomm	Electricity
	(1)	(2)	(3)
Post Short-run	−0.102	−0.026	−0.021
	(0.002)	(0.002)	(0.002)
Post Long-run	−0.170	−0.078	−0.064
	(0.003)	(0.003)	(0.002)
Observations	1,159,260	1,089,630	1,900,050
Worker FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Demographics	Yes	Yes	Yes

Note: This table presents event-study estimates of the effect of privatization on the probability a worker is employed in the private sector by sector. Column 1 displays results for the banking sector, column 2 for the telecommunications sector, and column 3 for the electricity sector. The short-run effect is equal to the effect of privatization in the four years following privatization. The long-run effect is equal to the effect of privatization in years five through ten following privatization. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments). Standard errors are are clustered at the worker level.

Appendix Table A5: Robustness Results

	Private Control	Public Control	Winsorized	Match Earlier	5 digit Occ/Sec	Trade Exposure	Region + Estab Size	Region + Firm Size
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Effect of privatization on all workers								
Post Short-run	−0.032	−0.056	−0.048	−0.047	−0.050	−0.043	−0.047	−0.060
	(0.015)	(0.015)	(0.014)	(0.017)	(0.014)	(0.009)	(0.012)	(0.014)
Post Long-run	−0.154	−0.258	−0.203	−0.226	−0.219	−0.203	−0.207	−0.177
	(0.019)	(0.024)	(0.019)	(0.023)	(0.020)	(0.014)	(0.017)	(0.020)
Avg. Outcome at $t = -1$	8.436	8.732	8.620	8.649	8.624	8.603	8.663	8.668
Observations	1,965,433	2,686,613	3,746,461	3,101,673	3,127,904	1,705,130	1,347,930	625,208
Panel B: Effect of privatization on stayers								
Post Short-run	−0.000	0.002	−0.001	0.005	−0.012	−0.015	−0.015	0.007
	(0.012)	(0.014)	(0.010)	(0.017)	(0.012)	(0.007)	(0.011)	(0.016)
Post Long-run	−0.057	−0.120	−0.090	−0.109	−0.127	−0.133	−0.120	−0.061
	(0.019)	(0.019)	(0.014)	(0.021)	(0.016)	(0.013)	(0.017)	(0.026)
Avg. Outcome at $t = -1$	8.436	8.732	8.620	8.649	8.624	8.603	8.663	8.668
Observations	1,288,697	1,799,471	2,437,597	2,095,286	2,103,237	1,166,075	893,242	396,998
Worker FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table presents event-study estimates of the effect of privatization on workers wages. Column 1 matches workers in privatized firms to workers in private sector firms. Column 2 matches workers in privatized firms to workers in public sector firms that are never privatized. Column 3 uses the main control samples, which allows workers to be matched to either a private sector or a public sector firm, and winzorizes log earnings at the bottom 5 percentiles and the top 5 percentiles. Column 4 matches workers two years prior to privatization rather than one year prior to privatization. Column 5 matches on 5-digit sector and occupation classifications, rather than 2-digit as in the main specifications. Column 6 matches on deciles of regional trade liberalization exposure, where trade liberalization exposure is computed as in Dix-Carneiro and Kovak (2017). Column 7 adds microregion and bins for deciles of establishment size to the coarsened exact matching algorithm. Column 8 adds microregion and bins for deciles of firm size to the coarsened exact matching algorithm. The short-run effect is equal to the effect of privatization in the four years following privatization. The long-run effect is equal to the effect of privatization in years five through ten following privatization Panel A includes all workers while Panel B restricts to establishment stayers who are defined as workers in time t who are in the same firm as they were prior to privatization (i.e. year $t^* - 1$). Standard errors are are two-way clustered by worker and establishment.

Appendix Table A6: The Effect of Privatization on Formal Sector Employment

	All Workers	No HS Degree	HS Degree	College Degree
	(1)	(2)	(3)	(4)
Post Short-run	−0.073	−0.088	−0.058	−0.065
	(0.025)	(0.033)	(0.026)	(0.026)
Post Long-run	−0.100	−0.108	−0.091	−0.082
	(0.023)	(0.031)	(0.026)	(0.023)
Avg. Outcome	0.680	0.572	0.711	0.746
Observations	4,571,328	1,316,160	2,097,664	1,157,504
Worker FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes

Note: This table presents difference-in-differences estimates of the effect of privatization on the probability a worker is employed in the formal sector. Column 1 pools all workers while columns 2-4 split the sample by whether the worker has no high-school degree, a high school degree or a college degree. The short-run effect is equal to the effect of privatization in the four years following privatization. The long-run effect is equal to the effect of privatization in years five through ten following privatization. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments). Standard errors are clustered at the worker level.

Appendix Table A7: Probability of Within Occupation and Microregion Job Transition by Education

	No HS Degree (1)	HS Degree (2)	College Degree (3)
Within Occupation	0.309	0.408	0.488
Within Microregion	0.725	0.780	0.727
Number of Transitions	3,473,664	545,621	199,936

Note: This table displays the probability a job transition occurs within an occupation (Panel A) and within a microregion (Panel B) by education status, using all job transitions that occur between 1995 and 1996, where all of the privatization events studied in this paper occur in 1996 or after.

Appendix Table A8: Effect of Exposure to Privatization on Market-Level Wages: Robustness to Market Definition

	1 Digit Occ.	2 Digit Occ.	3 Digit Occ.	4 Digit Occ.
	(1)	(2)	(3)	(4)
Exposure	−0.025	−0.041	−0.012	−0.024
	(0.041)	(0.013)	(0.008)	(0.005)
Observations	83,492	365,683	804,337	1,363,475
Labor Market FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Trade Liberalization Control	Yes	Yes	Yes	Yes
Allow for Linear Trend	Yes	Yes	Yes	Yes

Note: This table presents OLS estimates of the effect of exposure to privatization on average log monthly market wage. The labor market is defined as an occupation-by-microregion cell. Columns 1-4 vary the level of aggregation for the occupation from one-digit (Column 1) to four-digit (Column 4). Standard errors are clustered at the labor market level.

Appendix Table A9: The Effect of Exposure to Privatization on Market-Level Wages: Robustness to Exposure Definition

	Baseline Estimate	Exclude Zero Exp.	Asinh Exp.	Exp. in Levels	Indicator for Positive Exp.
	(1)	(2)	(3)	(4)	(5)
Log Exposure	−0.019	−0.013			
	(0.005)	(0.007)			
Asinh Exposure			−0.017		
			(0.004)		
Exposure/100				−0.176	
				(0.104)	
Exposure>0					−0.055
					(0.011)
Observations	1,408,374	1,198,013	1,408,374	1,408,374	1,408,374
Labor Market FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Trade Liberalization Control	Yes	Yes	Yes	Yes	Yes
Allow for Linear Trend	Yes	Yes	Yes	Yes	Yes

Note: This table presents OLS estimates of the effect of exposure to privatization on the average log monthly market wage. The exposure for a given occupation-by-microregion cell depends on the fraction of jobs privatized in that occupation-by-microregion cell as well as the fraction of jobs privatized in closely related (in terms of job flows) occupation-by-microregion cells and goes from 0 to 100. In Columns 1-2 the dependent variable is log(exposure+1). In Column 3 the dependent variable is asinh(exposure). In Column 4 the dependent variable is exposure divided by 100. In Column 5 the dependent variable is an indicator which is equal to 1 if the occupation-by-microregion cell has a positive level of exposure. Standard errors are are clustered at the labor market level.

Appendix Table A10: The Effect of Exposure to Privatization on Market-Level Wages: Additional Robustness

	Broader Cluster	Occ by Year	Never Priv. Industries	Size Controls
	(1)	(2)	(3)	(4)
Log Exposure	−0.019	−0.021	−0.025	−0.019
	(0.010)	(0.005)	(0.010)	(0.005)
Change in Log Labor-Market Size				−0.000
				(0.000)
Observations	1,408,374	1,405,821	1,356,354	1,283,185
Labor Market FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	Yes
Trade Liberalization Control	No	Yes	Yes	No
Occupation by Year FE	No	Yes	No	No
Allow for Linear Trend	Yes	No	Yes	Yes

Note: This table presents OLS estimates of the effect of exposure to privatization on the average log monthly market wage. The exposure for a given occupation-by-microregion cell depends on the fraction of jobs privatized in that occupation-by-microregion cell as well as the fraction of jobs privatized in closely related (in terms of job flows) occupation-by-microregion cells and goes from 0 to 100. Colum 1 two-way clusters standard errors at the microregion and occupation level. Columns 2-3 cluster standard errors at the labor market level (i.e. interaction between microregion and occupation). Column 2 includes occupation-by-year fixed effects. Column 3 eliminates industries that experienced privatization events prior to 1996 (and therefore prior to the events studied in this paper). The primary industries that are dropped include metal industries, rubber production, chemicals, and petrochemicals.

Appendix Table A11: The Effect of Exposure to Privatization on Market-Level Wages: Sector-specific Exposure

	Banking	Telecomm	Electricity
	(1)	(2)	(3)
Banking Exposure	−0.030		
	(0.011)		
Telecom Exposure		−0.044	
		(0.017)	
Electricity Exposure			−0.127
			(0.015)
Observations	1,408,374	1,408,374	1,408,374
Labor Market FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Trade Liberalization Control	Yes	Yes	Yes
Allow for Linear Trend	Yes	Yes	Yes

Note: This table presents OLS estimates of the effect of exposure to privatization on log average market wage. The exposure for a given occupation-by-microregion cell depends on the fraction of jobs privatized in that occupation-by-microregion cell as well as the fraction of jobs privatized in closely related (in terms of job flows) occupation-by-microregion cells In column 1, only jobs privatized in the banking sector contribute to the exposure measure. In column 2, only jobs privatized in the telecommunications sector contribute to the exposure measure. In column 3, only jobs privatized in the telecommunications sector contribute to the exposure measure.

Appendix Table A12: Effect of Privatization on AKM Wage Premiums

	No HS Degree	HS Degree	College Degree
	(1)	(2)	(3)
Short-run	−0.045	−0.066	−0.076
	(0.002)	(0.002)	(0.002)
Long-run	−0.121	−0.101	−0.098
	(0.006)	(0.004)	(0.006)
Observations	80,786,003	23,844,432	9,242,929

This table estimates an AKM model with worker fixed effects, establishment fixed effects, a cubic in age interacted with year dummies, as well as in indicator for the short-run effect of privatization which is equal to one in the four years after privatization and a long-run effect of privatization which is equal to one if it has been more than four years since privatization. The sample includes all years between 1993-2004. Homoscedastic-standard errors are reported in parentheses.

Appendix Table A13: Informal Sector Wage Penalty

	All Sectors	Privatized Sectors
	(1)	(2)
Informal	−0.321	−0.267
	(0.002)	(0.013)
Union	0.103	0.134
	(0.002)	(0.009)
Education (Years)	0.046	0.080
	(0.000)	(0.002)
Average outcome	6.002	6.842
Informality Rate	0.429	0.145
Observations	611,087	16,019
R Squared	0.640	0.691
Year FE	Yes	Yes
Sector FE	Yes	Yes
Occupation FE	Yes	Yes
State FE	Yes	Yes
Demographics	Yes	Yes

Note: This table presents OLS estimates of the informal sector wage penalty. Data is from the Brazilian National Household Survey, years 2002-2007. Additional demographics covariates include gender, within-job tenure, number of jobs held, and a cubic in age. Heteroskedastic robust standard errors are presented in parentheses.

Online Appendix B: Data Appendix

A. Overview of RAIS data

The *Relação Anual de Informações Sociais* (RAIS) is an employer-employee matched dataset which includes information on all workers and establishments in the formal sector of Brazil. The main use of the RAIS is to compute federal wage-supplements (*Abono Salarial*). While not reporting can in theory result in fines, these fines are rarely issued in practice. However, workers and establishments are incentivized to provide accurate wage information given the federal public wage-supplement is based on the wage reported in the RAIS.

B. Definition of a Privatization Event

The privatization events in the paper are identified by observing changes in legal codes in the RAIS data. The variable used to identify legal changes is natureza jurídica or legal nature. There are a couple of different combinations of legal nature changes that will reflect privatization events. For purposes of defining state-owned enterprises, there are two types: (1) public company and (2) mixed capital company.

In a public company, all of the capital of the firm is owned by the government. In a mixed capital company, the capital comes from both the government and the private sector. However, to be classified as a mixed capital company, more than half of the voting shares must belong to the state. Both of these types of enterprises' labor contracts are governed by the same laws for private-sector firms, which differs from contracts of public administration workers.

Privatization events in the paper are defined as a company transferring from either a public company or mixed capital company to a private company. The key in both cases is that the voting shares of the company are transferred from the government to the private acquirer. However, it does not imply that the government sells all of the shares of the company. As long as the government relinquishes the majority control of voting shares, then this will be classified as a privatization event.

To understand the definition of privatization event, it is helpful to consider a few cases that would be considered a privatization event, along with cases that are not considered a privatization event for the purposes of this paper. First, the most common privatization event by far is going from a mixed company to a private-sector firm. The largest example of this in the data is the telecommunications company Telebrás. Before privatization, Telebrás was listed on the Sáo Paolo stock exchange. The government of Brazil had about a 21.5 percent stake in Telebrás, but over 50 percent of the voting shares, and therefore maintained majority control of the firm.[24]

In 1998, Telebrás was broken up and different parts of the company were sold to different private-sector companies. While the Brazilian government did not retain a stake in Telebrás, this does not need to be true to be defined as a privatization event. Another large privatization event took place in 1997, with the sale of Vale mining. In this privatization, the Brazilian government initially sold about a 41 percent stake in the company, relinquishing majority control, however still maintaining a minority stake in the company.

[24] See https://www.wsj.com/articles/SB898274445213587000 for a description of the privatization sale of a portion of Telebrás to the Spanish firm Telefonica.

However, given the state no longer has majority control and thus does not control the corporate governance of the firm, this is defined as a privatization event, and in the data Vale is no longer classified as a mixed capital company after 1997.

While the Brazilian government did sell minority shares of some companies during the privatization program, these will not be classified as a privatization event given that the ultimate control of the firm has not changed (and therefore no change in legal nature will occur in the data). Anuatti-Neto et al. (2003) report that between 1990-2000 the Brazilian government raised roughly 6 billion USD by selling minority shares in SOEs. In contrast, privatizations in which control of the firm was transferred raised about 76 billion USD.

C. Sample Selection

In the RAIS, workers are identified by an individual-specific PIS (Programa de Integração Social), a unique time-invariant worker identifier similar to a social security number. I follow Menezes-Filho and Muendler (2011) and drop workers with PIS identifiers less than 11 digits, as these are not valid identifiers. Errors in worker identifiers may be caused by (1) bad compliance and bookkeeping errors or (2) to allow workers to withdraw from their severance account through fake layoffs and rehires.

Most of the analysis (with the exception of the AKM models), is restricted to either state-owned enterprises or private-sector establishments. This eliminates public administration workers or workers for non-profits and charities. The distinction between SOEs and public administration is important given public administration workers are employed under different contracts than workers in SOEs or private-sector establishments. The public administration contract includes restrictions on firing employees while contracts for workers in SOEs do not.

D. Variable Definitions

PIS: A PIS is a worker identifier that is unique to a given worker over time.

CNPJ: The CNJP is an establishment-level identifier issued by the Brazilian tax authority which is unique to a given establishment over time. The first eight digits of the CNPJ corresponds to the firm of the establishment, while the last six correspond to the establishment within the firm.

Education: The RAIS records education at eight different categories. I recode these variables into three categories: (1) Less than High-School (2) High-School Graduate (3) College Graduate. Education for an individual worker is set to the modal value of education for the worker over the sample period.

Occupation: Occupations are defined by the Classificação Brasileira de Ocupações (CBO) into 2,355 distinct groups.

Sector: Sectors are reported under the *CNAE* four-digit classification (*Classificação Nacional de Atividade Econômica*) for 654 industries.

Wage: Wage refers to total payments, including regular salary payments, holiday bonuses, performance-based and commission bonuses, tips, and profit-sharing agreements, divided by total months worked during

the year for that employer. Payments that are not considered part of the wage include severance payments for layoffs and indemnity pay for maternal leave. Wages in the dataset are reported in terms of multiples the monthly minimum wage, which are then converted to real earnings using inflation adjustments available from Brazil's Institute of Applied Economic Research (IPEA).[25]

Microregion: Microregions in Brazil are defined by the Brazilian Statistical Agency (IBGE). Microregions group together clusters of contiguous municipalities with similar geographic and economic characteristics, similar to commuting zones in the United States.

E. Overview of PNAD

The Brazilian National Household Survey, Pesquisa Nacional por Amostra de Domicìlios (PNAD) is a survey conducted by the Brazilian statistical agency IBGE, since 1981. From 2001 onward, the Brazilian microdata from the PNAD is available online at the IBGE website.[26] Unlike the RAIS data, the PNAD contains information on earnings for workers in the informal sector, allowing one to estimate the informal-sector wage penalty controlling for characteristics of the worker.

Online Appendix C: Trade Liberalization

The privatization program instituted by Brazil was part of a larger economic reform. A particularly important reform is trade liberalization. As discussed in the main text, trade liberalization began in the early 1990s and ended in 1995, before the privatization events studied in this paper. During this time, tariffs fell from an average of 30.5 percent to 12.8 percent Dix-Carneiro and Kovak (2017). However, there was substantial variation across industries, implying that some regions faced large reductions while others faced small reductions, depending on the initial industry mix of the region. Dix-Carneiro and Kovak (2017) exploit this variation and find that regions that faced larger reductions experienced wage and employment declines relative to regions that faced smaller reductions.

Therefore, if privatization exposure is correlated with trade liberalization exposure, the spillover estimates could partially reflect effects due to liberalization. To understand whether this is the case, I use data from Dix-Carneiro and Kovak (2017) to control directly for exposure to trade liberalization. In particular, a region's exposure to trade liberalization is given by:

$$RTR_r = -\sum_i \beta_{ri} dln(1+\tau_i) \tag{16}$$

where RTR_r is the regional tariff reduction, i indexes industries, β_{ri} is a weight attached to industry i in region r, d represents the difference between 1995 to 1990, and τ_i is the tariff in industry i. The equation

[25] See http://ipeadata.gov.br/ for the minimum wage data as well as inflation data for Brazil. The early 1990s was a period of rapid inflation in Brazil. By 1995, price stabilization had succeeded due to the Plano Real reform which was implemented in August 1994.

[26] See website:https://ww2.ibge.gov.br. A package in STATA, called Data Zoom, is made available by the Department of Economics at PUC-Rio which standardizes questions across years.

for β_{ri} is:

$$\beta_{ri} \equiv \frac{\lambda_{ri}\frac{1}{\psi_i}}{\sum_j \lambda_{rj}\frac{1}{\psi_j}} \tag{17}$$

Where ψ_i is the cost share of nonlabor factors and λ_{ri} is the regional labor allocated to industry i. Therefore, if regional employment is high in an industry that faces large tariff reductions, then RTR_r will be relatively large. Dix-Carneiro and Kovak (2017) compute cost-shares from 1991 national accounts, employment shares from the 1990 census, and tariff changes from Kume et al. (2003). In order to understand how trade liberalization impacts the estimation of market-level effects, I estimate the specification in Equation (9), but with the additional term $\xi\left(RTR_r \cdot t\right)$, which allows a trend with respect to trade liberalization exposure.

Controlling for exposure to privatization does lead to a fall in the coefficient $\hat{\delta}$ from -0.041 to -0.032 (See columns 1 to 2 of Table 3). Indeed, there is a positive correlation between RTR_r and the exposure to privatized measure $Exposure_{it}$ (a 1 unit increase in RTR is associated with an increase in exposure levels equal to 3). The RTR variable varies from about -0.01 to 0.15, while exposure is defined from 1 to 100. Therefore, a 100 percent reduction in tariffs would theoretically be associated with about a 1 standard deviation in exposure. Nevertheless, even controlling for the exposure to trade liberalization, I still find a significant and economically meaningful impact of privatization. As discussed in the main text, the preferred estimate additionally allows for a pretrend in outcomes with respect to privatization exposure, which further reduces the elasticity of market wages with respect to privatization exposure to -0.019.

Online Appendix D: AKM Wage Premium Estimation

To understand differences in pay between SOEs and private-sector firms prior to privatization, I estimate a standard two-way fixed effects model with worker and establishment fixed effects following Abowd, Kramarz and Margolis (1999) (AKM) and Card, Heining and Kline (2013) utilizing data from the years prior to the privatization (1992-1995). Formally, I estimate the following regression model:

$$ln(w_{it}) = \alpha_i + \psi_{J(i,t)} + \gamma_t + x'_{it}\beta \tag{18}$$

where α_i is an individual fixed effect, ψ_j is an establishment fixed effect, γ_t is a year indicator, x'_{it} are time-varying covariates which include education and an age cubic interacted with year indicators, and $J(i,t)$ is a function which indicates the establishment individual i is employed at in time period t. Given the individual fixed effects, the establishment-specific wage premium ψ_j is interpreted as the premium paid by establishment j controlling for the quality of workers employed at establishment j.

As discussed in Card et al. (2013) and Card et al. (2018), ψ_j is only identified relative to an omitted establishment. In practice, I omit the largest private-sector establishment when estimating the AKM model.

For the estimation to yield an unbiased estimate of ψ_j, the "exogenous mobility" assumption must be satisfied. This assumption allows high-wage workers to sort to high-wage establishments but does not allow workers to sort based on idiosyncratic match effects. This implies firms offer a proportional wage premium to all workers regardless of their skill level and job. While a restrictive assumption in theory, it appears to

hold in many contexts (Card et al., 2013; Song et al., 2019), and in particular, in Brazil (Alvarez, Benguria, Engbom and Moser, 2018).

I estimate Equation (18) on the entire sample of establishments and collect the estimated $\hat{\psi}_j$ for all firms.[27] I then make two sample restrictions. First, I restrict the sample to the primary privatized industries which include banking, telecommunications, and electricity. Second, to focus the analysis on privatized SOEs, I exclude SOEs that are never privatized.[28] To estimate the wage premium associated with a privatized establishment prior to privatization, I estimate the following regression:

$$\hat{\psi}_j = \alpha + \omega Privatized_j + \xi_j + \varepsilon_j \tag{19}$$

Where ξ_j are industry fixed effects. Therefore, this regression compares the wage premium of SOEs that will become privatized at a later date to private-sector establishments that employ workers in the same industry. Following the AKM literature, the regression is performed at the worker level and therefore gives more weight to larger establishments. In this specification, $\hat{\omega}$ is equal to the wage premium associated with privatized establishments. The results of this specification are presented in Table 1 and discussed in Section B.

To understand how privatization impacts establishment AKM wage premia, I take an approach that controls flexibly for worker quality. This is particularly important given the large employment changes following privatization. In particular, I estimate the following regression model at the worker level:

$$ln(w_{it}) = \alpha_i + \psi_{J(i,t)} + \delta^{sr} Privatized_{J(i,t)} \cdot SR_t + \delta^{lr} Privatized_{J(i,t)} \cdot LR_t + x'_{it}\beta + \varepsilon_{it} \tag{20}$$

where $Privatized_{J(i,t)}$ is an indicator which is equal to one if establishment $J(i,t)$ has been privatized at some point before year t, SR_t is an indicator that is equal to one in the four years after privatization and zero otherwise, LR_t is an indicator equal to one if it has been more than four years since privatization and zero otherwise, x_{it} includes an age cubic interacted with year effects, and ε_{it} is an idiosyncratic error term.

The coefficients of interest are δ^{sr} and δ^{lr}, which captures on average how wages of workers within privatized establishments change in response to privatization, controlling for the worker and the establishment. I include both a short-run indicator of privatization as well as a long-run indicator to capture the empirical fact that wages respond slowly in response to privatization. By estimating the change in wage premia in an AKM framework with two-way fixed effects, the composition of the workers within an establishment is taken directly into account. For example, if a privatized establishment upgrades the skill-level of the workforce, but does not alter compensation policies, then both δs will be equal to zero, indicating that conditional on worker quality, privatization does not impact wages paid by the establishment. If workers' wages fall within privatized establishments, then we would expect both δs to be negative. Given the evidence from the worker-level results, we expect both δs to be negative, and that $\delta^{lr} < \delta^{sr}$, capturing the fact the effects gradually grow in magnitude.

[27] As discussed in length in Card et al. (2018), the AKM model is only identified on a set of establishments connected by labor mobility. In practice, I restrict the sample to the largest connected set.

[28] The wage premium associated with the never-privatized SOEs is similar to the wage premium associated with privatized SOEs.

To implement the estimation, I pool all years from 1993 to 2004 to estimate Equation (20). I also estimate the equation separately by education group, which allows establishment-specific wage premiums to vary by education group. Therefore, this allows high-educated workers to gain more from working at high-wage establishments and therefore relaxes the exogenous mobility assumption in the AKM literature along one dimension. Splitting by education also alleviates the computational burden of estimating the model. As can be seen in Appendix Table A12, privatization decreases wages in the short run by 4.4 percent for the no-high-school degree sample, by 6.3 percent for the high-school sample, and 7.3 percent for the college degree sample. Moving to the long-run effects, the effect of privatization on wage premiums is consistently larger in magnitude than the short-run effects, which is consistent with the worker-level results. In the long run, wages fall by 11 percent in the no high-school sample, 9.6 percent in the high-school sample, and 9.3 percent in the college sample. All effects are significant at the 1 percent level. Therefore, even conditional on worker quality, wages fall substantially in privatized establishments.

Online Appendix E: Decomposing the Direct Impact of Privatization

This section shows how to decompose the effect of privatization into two components: a within-establishment effect and a displacement effect. Let $T_i = 1$ if the worker is in a privatized establishment.

We can write the effect of privatization in a potential outcomes framework as:

$$\begin{aligned}\mathbb{E}[w_1 - w_0] =& Pr(\text{stayer}|T_i = 1) \cdot w_s^1 + (1 - Pr(\text{stayer}|T_i = 1)) \cdot w_m^1 \\ &- \left[Pr(\text{mover}|T_i = 0) \cdot w_s^0 + (1 - Pr(\text{mover}|T_i = 0) \cdot w_m^0\right]\end{aligned}$$

where w_1 denotes wage if privatized, w_0 denotes wage if not privatized, w_s^1 denotes wage if privatized conditional on staying in the same establishment, w_s^0 denotes wage if not privatized conditional on staying in the same establishment, w_m^1 denotes wage if privatized conditional on moving establishments, w_m^0 denotes wage if not privatized conditional on moving establishments. Adding and subtracting $Pr(\text{stayer}|T_i = 1)$ and rearranging the equation yields:

$$\begin{aligned}\mathbb{E}[w_1 - w_0] =& Pr(\text{stayer}|T_i = 1) \cdot (w_s^1 - w_s^0) + (Pr(\text{stayer}|T_i = 1) - Pr(\text{stayer}|T_i = 0) \cdot w_s^0 \\ &+ (1 - Pr(\text{stayer}|T_i = 1) \cdot w_m^1 - (1 - Pr(\text{stayer}|T_i = 0) w_m^0\end{aligned}$$

To make progress, it is helpful to assume that the wage for an individual that is displaced by a privatized SOE is the same as the wage for an individual that is displaced by a control firm. That is, $w_m^1 = w_m^0$. In this case, we can rewrite the impact of privatization as composed of two simple components: a within-establishment effect and a displacement effect:

$$\mathbb{E}[w_1 - w_0] = \underbrace{Pr(\text{stayer}|T_i = 1) \cdot (w_s^1 - w_s^0)}_{\text{within-establishment effect}} + \underbrace{(Pr(\text{stayer}|T_i = 1) - Pr(\text{stayer}|T_i = 0)) \cdot (w_s^0 - w_m^0)}_{\text{displacement effect}} \tag{21}$$

We can now use this decompose the long-run effect of privatization into two components. The fraction of

stayers changes over time as workers switch jobs. Therefore, I will perform the decomposition for both the short-run effects (i.e. the first column, first row of Appendix Table A1) and the decomposition for the long-run (i.e. the first column, second row of Appendix Table A1).

In the short-run, $Pr(\text{stayer}|T_i = 1) = 0.433$. In Panel B of Appendix Table A1, I find $w_s^1 - w_s^0 = -0.010$. The aggregate effect is $\mathbb{E}[w_1 - w_0] = -0.058$. Plugging these numbers into (21) and solving for the displacement effect in (21):

$$(Pr(\text{stayer}|T_i = 1) - Pr(\text{stayer}|T_i = 0)) \cdot (w_s^0 - w_m^0) = -0.058 - 0.433 * (-0.010) = -0.054.$$

Therefore, in the short-run, the within-establishment effect explains $\frac{0.004}{0.058} \times 100 = 7.5$ percent of the overall fall in wages, with the rest being explained by the displacement effect. Performing the same calculation for the long-run effect, I find the within-establishment effect is equal to -0.024 which explains $\frac{0.024}{0.223} \times 100 = 10.8$ percent of the aggregate effect.

Online Appendix F: Interpreting Estimates in the Presence of the Informal Sector

One important caveat to the worker-level analysis is that it is conditional on positive earnings. In Brazil, nearly 40 percent of the workforce is employed in the informal sector, therefore it would be incorrect to interpret an individual dropping out of the sample as unemployment or out of the labor force. However, given privatization increases the probability a worker is displaced, then estimates that condition on positive earnings likely understate the total negative impact of privatization on worker welfare, given that displacement is associated with large persistent earnings losses (Jacobson, LaLonde and Sullivan, 1993).

Panel A of Appendix Figure A2 plots the probability a worker is employed in the formal sector relative to the year of privatization. The probability of remaining in the sample decreases over time for both the privatized sample and the matched control group due to workers switching to informal jobs, unemployment, and retirement. In ten years, approximately half of all workers are no longer employed in the formal sector. However, the probability of remaining employed in the formal sector is higher for the control group. Panel B of Appendix Figure A2 plots δ^k from estimating Equation (6) with formal sector employment as the outcome. As can be seen in the figure, the probability of formal sector employment drops by 10 percentage points in the privatized SOEs relative to the control group two years after privatization. This effect remains relatively constant throughout the next ten years.

This gap has important consequences for the interpretation of the wage effects. If informal sector jobs pay lower on average, then the long-run wage effect in Figure 3 represents a lower bound, as it does not capture the fact that privatized workers are more likely to transition into the informal sector where they earn lower wages on average. While it is not possible to observe informal workers in the RAIS, the Brazilian National Household Survey, Pesquisa Nacional por Amostra de Domicìlios (PNAD) contains information on informal work as well as information on wages and sector. Therefore, it allows one to estimate formal-informal wage gaps while controlling for a variety of observables.[29]

[29]The PNAD microdata and documentation is available at the Brazilian Statistical Agencies website:https://ww2.ibge.gov.br. I am thankful to Data Zoom, developed by the Department of Economics at PUC-Rio, for providing the codes for accessing IBGE

To estimate the informal-formal wage gap, I estimate a regression of the following form:

$$ln(w_{it}) = \beta Informal_{it} + \gamma_t + \xi_s + \chi_o + \zeta_r + \pi X_{it} + \varepsilon_{it} \tag{22}$$

Where $Informal_{it}$ indicates that worker i is employed in the informal sector at time t, γ_t are year indicators, ξ_S are industry fixed effects, χ_o are occupation fixed effects, ζ_r are state fixed effects, and X_{it} is a vector of covariates which includes a cubic in age, tenure within the firm in months, union status, education in years, and number of jobs held. As can be seen in Column 1 of Appendix Table A13, when pooling all industries, I find an informal-sector wage penalty equal to -0.324.[30] Restricting to just the privatized industries dramatically reduces the sample, but I still find a statistically significant wage penalty equal to -0.264. Therefore, if privatization increases the probability a worker transitions to the informal sector, as Appendix Figure A2 finds, then this transition is associated with a large wage penalty which is not captured in the worker-level results, given the estimates are conditional on positive earnings in the formal sector. Therefore, the total impact of privatization on earnings for incumbents workers is likely larger than previously estimated. However, one caveat to this analysis is that I do not observe severance payments in the data, and therefore, the impact of job displacement may be overstated. Severance packages can be quite large in Brazil (Gonzaga, 2003), and therefore likely reduce the impact of job displacement on workers.

Online Appendix G: Market-level Effects Relative to Prior Literature

This section compares the size of the market-level effects in relation to prior work studying similar questions. One of the most-related papers is Beaudry et al. (2012) which studies the effect of industrial composition on wages in a general equilibrium search and matching model with many industries. In their model, reallocating employment across industries has two impacts on the average wage in a city. First, if one reallocates employment from a high-wage sector to a low-wage sector there is a direct impact due to the fact that the reallocated workers will earn lower wages. For example, in Beaudry et al. (2012), the authors consider the case of Pittsburgh, which lost the steel industry in the 1980s. The steel industry employed about 10 percent of the workforce and paid a 20 percent premium. Therefore, the "direct" impact (or accounting approach) would predict the loss of the steel industry would decrease the average wage in Pittsburgh by 2 percent.

However, there are also indirect effects on other industries. The high wages in the steel industry put pressure on other industries to increase wages. Therefore, the loss of steel will also lower wages in these industries. Their results imply that the aggregate impact of the reduction in steel employment would be about 6-7 percent, or 3-4 times the impact of the direct impact.

In terms of privatization, the private-sector and SOE sector can be conceptualized as two separate industries. I found that the "direct" impact on privatized workers implied an aggregate wage decline in the formal sector by about 1.0 percent. Incorporating indirect effects magnifies this to 3.1 percent. Therefore, the magnitudes here are similar to the magnitudes found in Beaudry et al. (2012).

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microdata.

[30] Estimating an earnings function without state, occupation and industry fixed effects yields an informal sector wage penalty equal to -0.422.

Another potentially relevant paper comes from Jofre-Monseny et al. (2018), which uses a similar search and matching model as Beaudry et al. (2012) to study how expansions of public sector jobs impact employment in Spain. In their calibration, moving from 0.026 percent of vacancies being offered by public sector to 0.039 causes a 1.5 percent increase in tradable wages and a 1.8 percent increase in non-tradable wages. Again, relatively small changes can be amplified in these models. The case of Spain is a reasonably good comparison, given similar wage premia associated with public employment as are associated with state-owned enterprises in Brazil. The elasticity in their model, again, is slightly higher, with a 1.3 percent decrease in percent public sector decreasing aggregate wages by between 1.5 and 1.8 percent. In Brazil, I find a 3 percent decrease in percent SOE leads to a 3.1 percent decline in the aggregate wage.

Lastly, Muralidharan et al. (2020) studies how public-sector work impacts private-sector earnings in an experiment in India. In their setting, some regions received a treatment that improved the implementation of a rural employment guarantee program. They find the reform raised earnings in low-income households by about 13 percent, despite the program only constituting about 7 percent of the labor market in their sample. They find that the direct impact of the program explains only 10 percent of the aggregate, with 90 percent stemming from increased earnings in the private sector. Therefore, in this paper as well, direct impacts are magnified leading to large changes in wages.

An alternative way to inform how these market-level effects compare to other settings is to provide a simple calibration of what the effects imply in terms of the model in Section III. To be clear, this calibration relies on a number of likely implausible simplifying assumptions, but it does provide a way to connect different parameters to the empirics in a transparent manner. To begin, we can write the firm wage in the model as:

$$w_m = \gamma R + (1 - \gamma)v \tag{23}$$

where R here is a measure of rents within the firm. The goal of this section will be to use estimates in the paper to calibrate the rent-sharing parameter γ and compare to recent work estimating the same parameter. For example, Abowd and Lemieux (1993) use foreign competition shocks to instrument for R while Kline et al. (2019) uses patents to instrument for R, both estimating a rent-sharing parameter of around 0.23. In this section I will place structure on v, and how privatization impacts v, which will allow me to to calibrate a value of γ consistent with the empirical results. To make the market-level results align with the firm-level model, I will assume firms within a market are identical, implying the firm-level wage is equal to the market wage.

I assume v^0 represents the outside option value before privatization and is given by:

$$v^0 = \mathbb{E}[w] - c = \lambda_u b + \lambda_p \mathbb{E}[w|j = p] + \lambda_{soe}\mathbb{E}[w|j = soe] + \lambda_{priv}\mathbb{E}[w|j = soe] - c \tag{24}$$

where λ_{priv} is the fraction of workers employed by firms that will be privatized, with all other terms defined as in the main text. Because privatization has not occurred, I assume privatized firms pay the same wages as an SOE that will never be privatized. After privatization, the value of the outside option is given by:

$$v^1 = \mathbb{E}[w] - c = \lambda_u b + \lambda_p \mathbb{E}[w|j = p] + \lambda_{soe}\mathbb{E}[w|j = soe] + \lambda_{priv}\mathbb{E}[w|j = p] - c \tag{25}$$

where now the privatized firms pay the wages of the private-sector firms and I assume $\mathbb{E}[w|j = p]$ and $\mathbb{E}[w|j = soe]$ do not change. Therefore, this calibration can be seen as a partial-equilibrium exercise, where I am assuming wages don't change at other firms. Note that there are additionally employment drops at privatized firms, which will change the fraction of workers in the private-sector firms and unemployment state. The bounds on the rent-sharing paramter can be found by assuming both extremes. For the purpose of illustration here, I will assume all of the drop in employment in privatized firms (22 percent drop in employment) will be made up by an increase in the flow to unemployment. The change in the outside option is therefore given by:

$$v^1 - v^0 = \lambda_{priv}(0.22)(b) + \lambda_{priv}(0.78)\mathbb{E}[w|j = p] - \lambda_{priv}\mathbb{E}[w|j = soe] \tag{26}$$

The first term stems from the reallocation of workers from privatized sectors to unemployment. For example, if 10 percent of workers are in privatized firms, then given the employment drops we would expect about 8 percent after the privatization. In this calibration, I am assuming that those 2 percent of workers laid off by privatized firms transfer into unemployment, in which case they receive b. Given unemployment and informal work are unable to be disentangled in the data, I will assume b is the expected wage in the informal sector.

In terms of how these parameters relates to the empirics, I assume $\lambda_{priv} = Exposure$. The wage in SOEs is estimated to be 1.2 times the wage in the private sector.[31] The wage in the informal sector is estimated to be 0.766 times the wage in the private sector, as seen in Appendix Table A13. Assuming no changes in the rents R at the private-sector firm following privatization, the change in wages can be computed as:

$$w_1 - w_0 = (1 - \gamma) \cdot Exposure \cdot \mathbb{E}[w|j = p](0.22 \cdot 0.766 + 0.78 - 1.2) \tag{27}$$

where I have substituted in $b = 0.766 \cdot \mathbb{E}[w|j = p]$, $E[w|j = SOE] = 1.2 \cdot \mathbb{E}[w|j = p]$, and $\lambda_{priv} = Exposure$. Given all private-sector firms are the same here, $\mathbb{E}[w|j = p] = w_0$. Dividing by w_0 yields:

$$\frac{w_1 - w_0}{w_0} = (1 - \gamma) \cdot Exposure \cdot (-0.251) \tag{28}$$

Taking the derivative of this expression with respect to Exposure and assuming the proportional changes are well approximated by log changes yields:

$$\frac{\partial log(w)}{\partial Exposure} = (1 - \gamma) \cdot (-0.251) \tag{29}$$

In column 4 of Appendix Table A10, I found that $\frac{\partial log(w)}{\partial Exposure} = -0.176$. Therefore, solving for γ yields:

[31] This comes from column 1 of Table 1, where the AKM wage premium for privatized SOE workers is estimated to be 0.182 log points, converting this to proportional effects yields an 20.0 percent wage premium for SOEs.

$$\gamma = 0.300 \tag{30}$$

This calibration relies on assuming employment drops in privatized firms increase employment in the informal sector. Assuming all employment is absorbed by the private sector yields a rent-sharing parameter of around 0.120. To compare, Kline et al. (2019) finds a rent-sharing parameter equal to 0.23, while Abowd and Lemieux (1993) find rent-sharing parameters between 0.152 and 0.392. In both cases, the identifying variation comes from shocks to rents, not shocks to the outside option of workers. While these two are similar, it is important to note the calibration here relied on a number of simplifying assumptions, and it is likely not appropriate to interpret the privatization event here as a firm-level shock, the required shock to cleanly identify the rent-sharing parameter.