

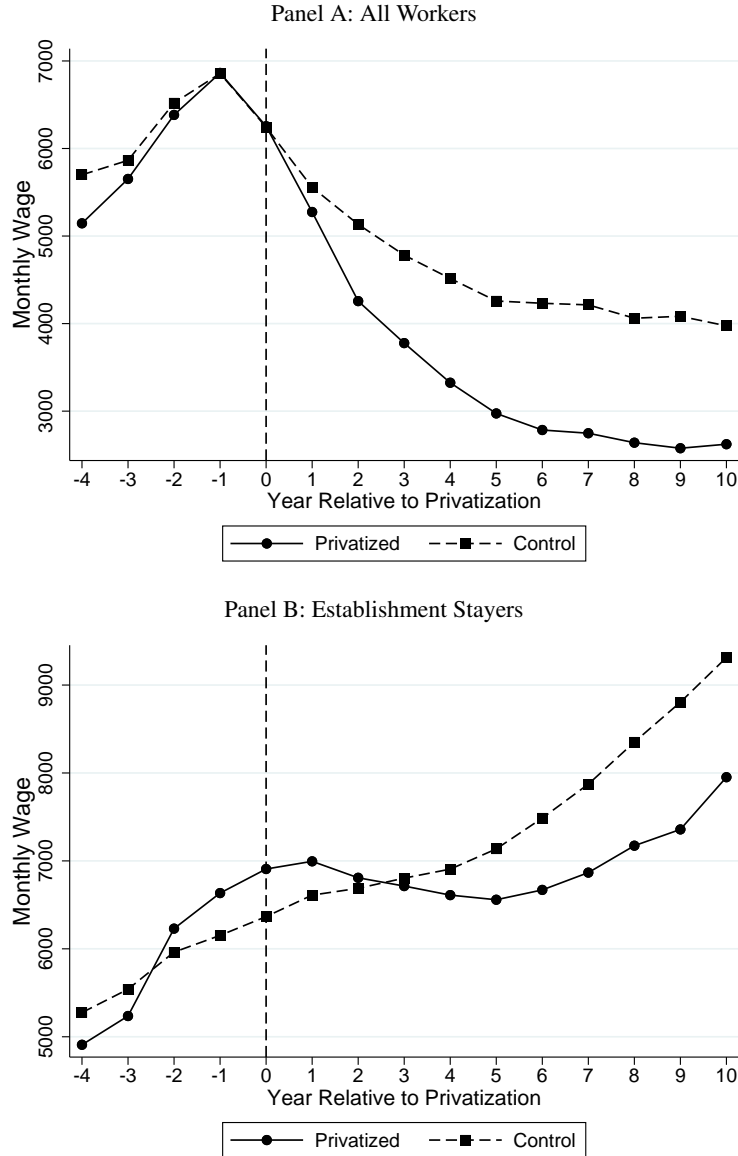
Online Appendix
The Impact of Privatization of State-Owned Enterprises on Workers

David Arnold*

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

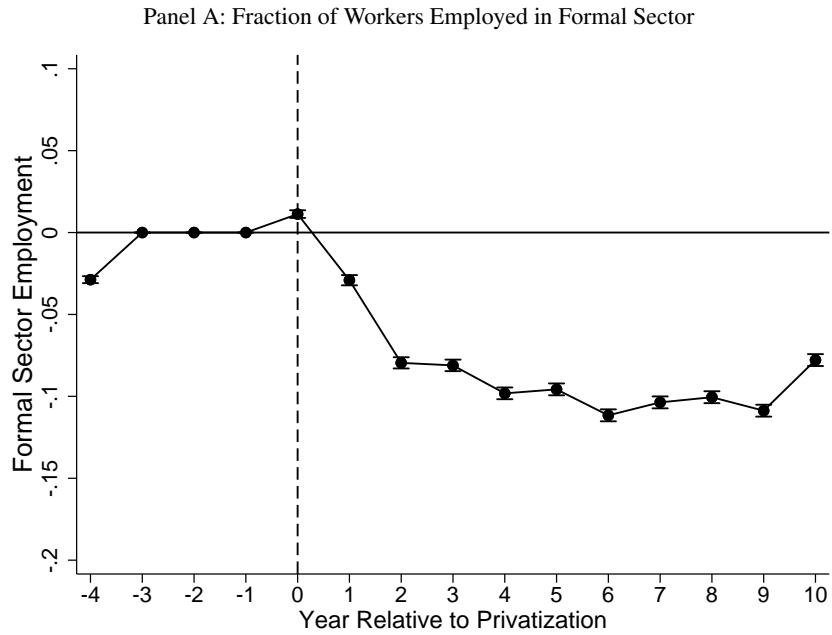
Online Appendix A: Additional Results

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

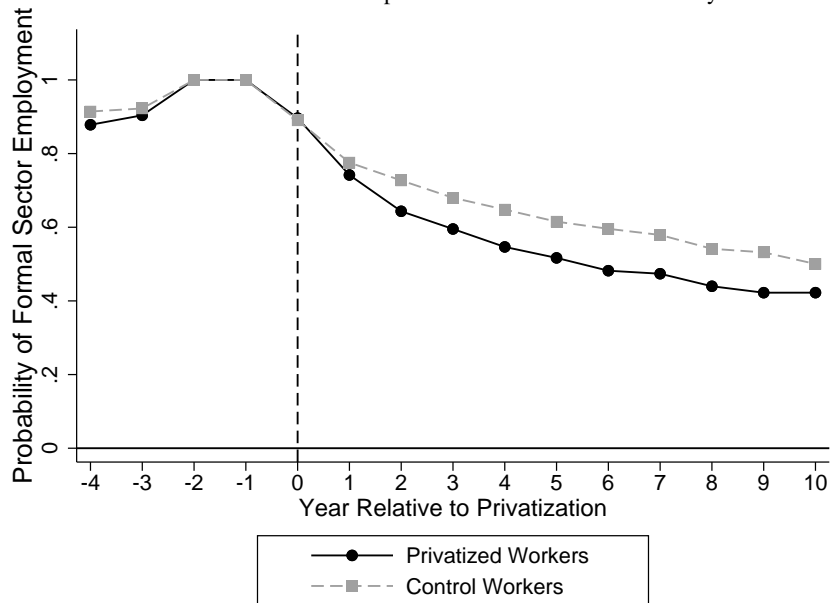


Note: This figure displays the average monthly wage for workers in privatized establishments compared to a matched control group. Panel A includes all workers while Panel B restricts to establishment stayers. Stayers are defined as workers in time t who are employed in the same establishment as they 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.

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

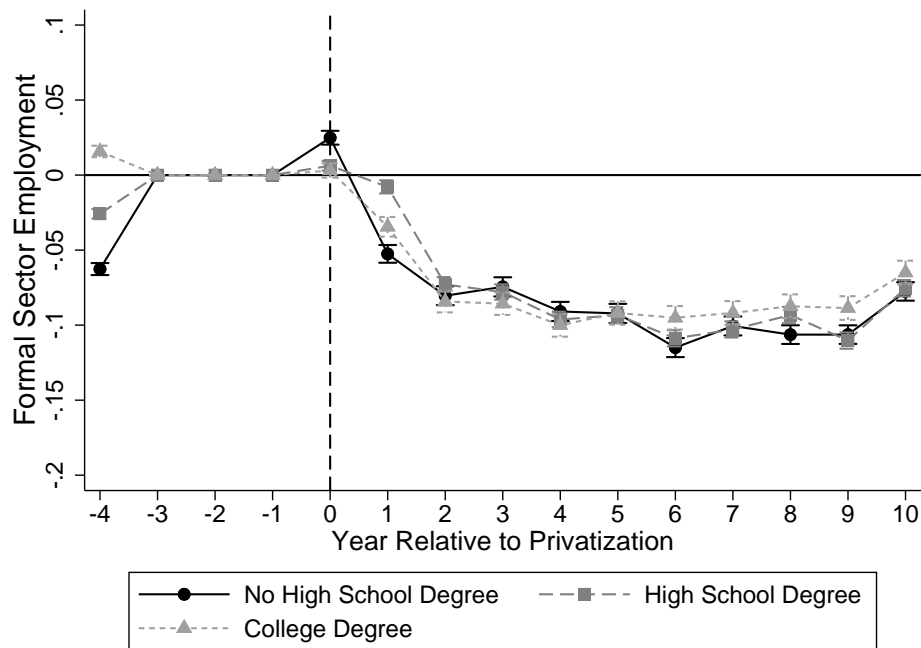


Panel B: Matched Difference-in-Differences Plot of the Impact of Privatization on Probability of Formal Sector Employment



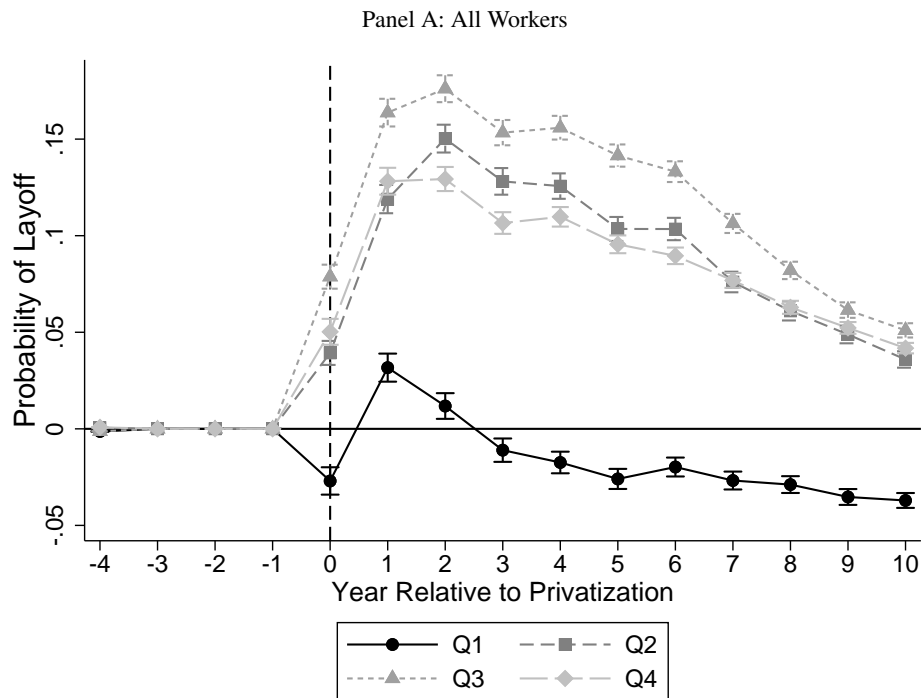
Note: This figure displays the fraction of the sample that is employed in the formal sector in each year relative to the privatization event in Panel A and matched difference-in-differences estimates of the effect of privatization on the probability of formal sector employment in Panel B. The omitted 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.

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



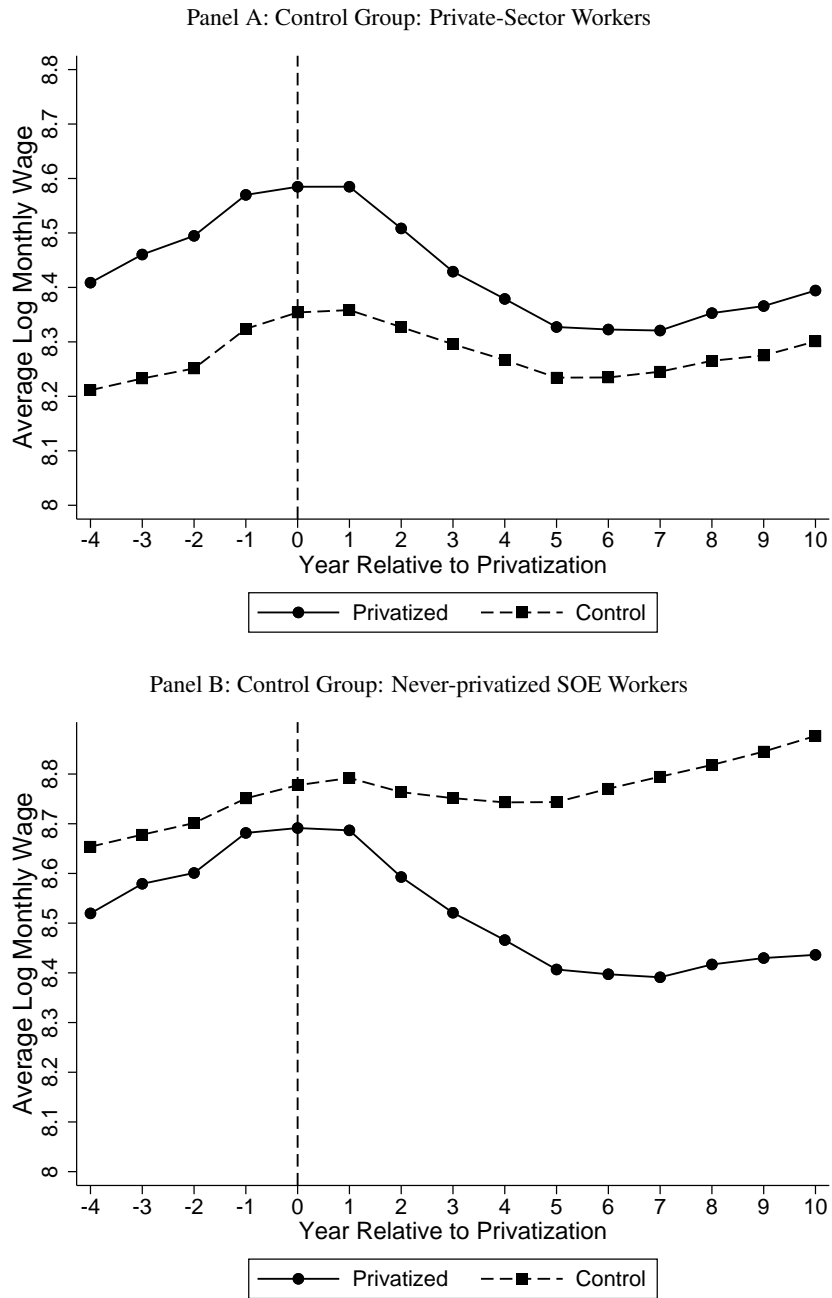
Note: This figure shows matched difference-in-differences estimates of the effect of privatization on the probability of formal sector employment. The omitted category is three years prior to the privatization event, given workers must be in the formal sector from years $t^* - 2$ to $t^* - 1$ to be in 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.

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



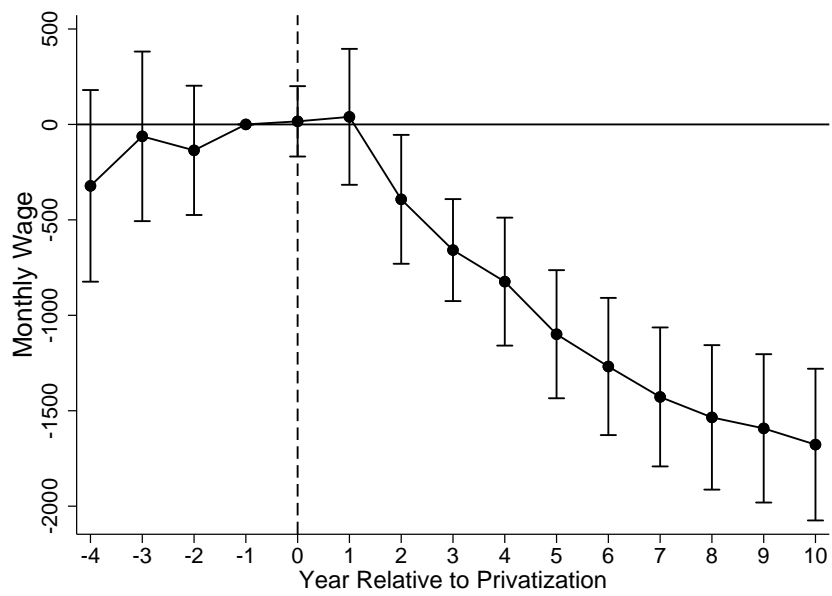
Note: This figure shows matched difference-in-differences estimates of the effect of privatization on probability of a worker is laid off by within-establishment tenure quartiles. A layoff is inferred from a change in establishment identifier or through a worker transitioning from the formal sector the 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

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



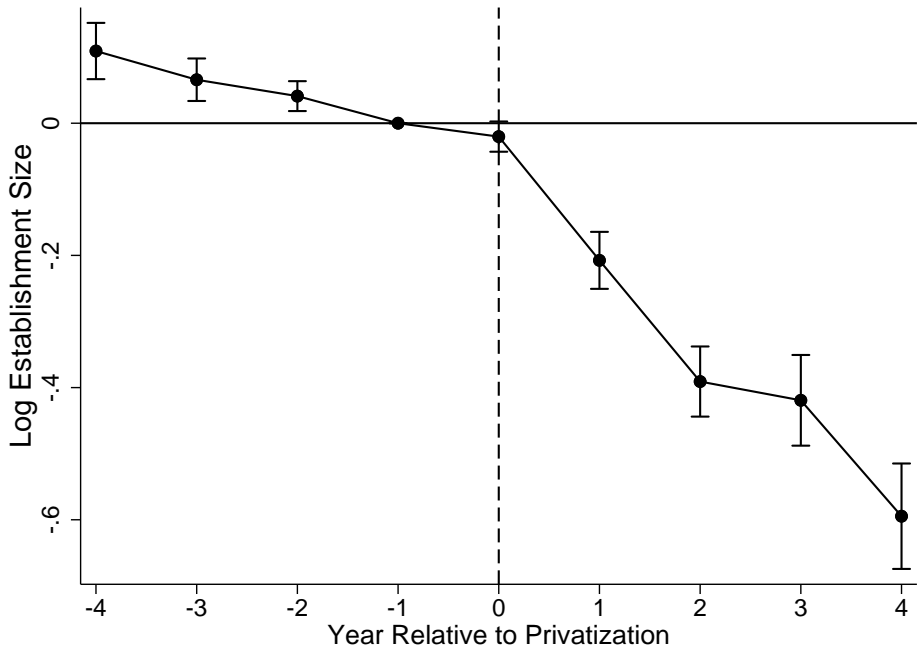
Note: This figure displays the average log monthly wage for workers in privatized establishments compared to a matched control group. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments (Panel A) or never-privatized SOEs (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.

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



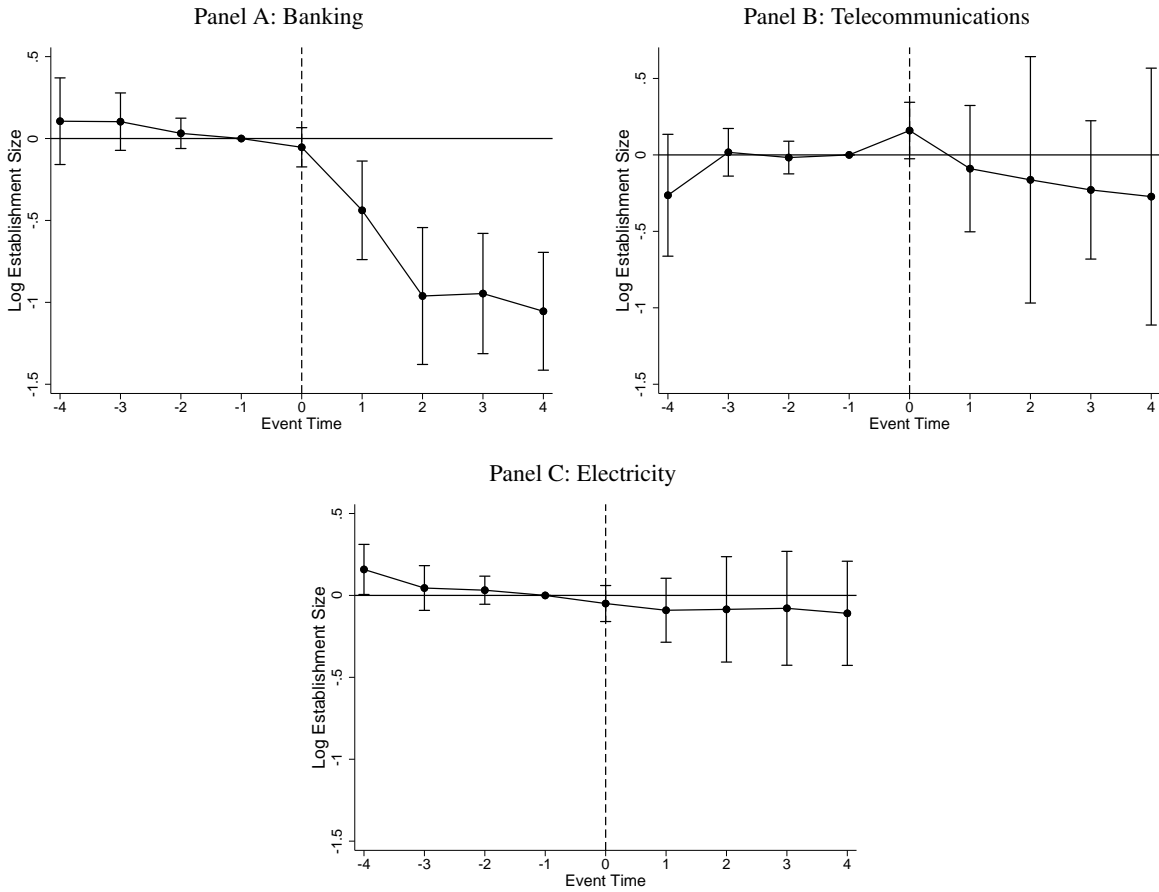
Note: This figure shows matched difference-in-differences estimates of the effect of privatization of monthly earnings in levels including zero earnings. The omitted category is the year prior to the privatization event. The regressions control for individual fixed effects, time effects, and a cubic 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.

Online 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.

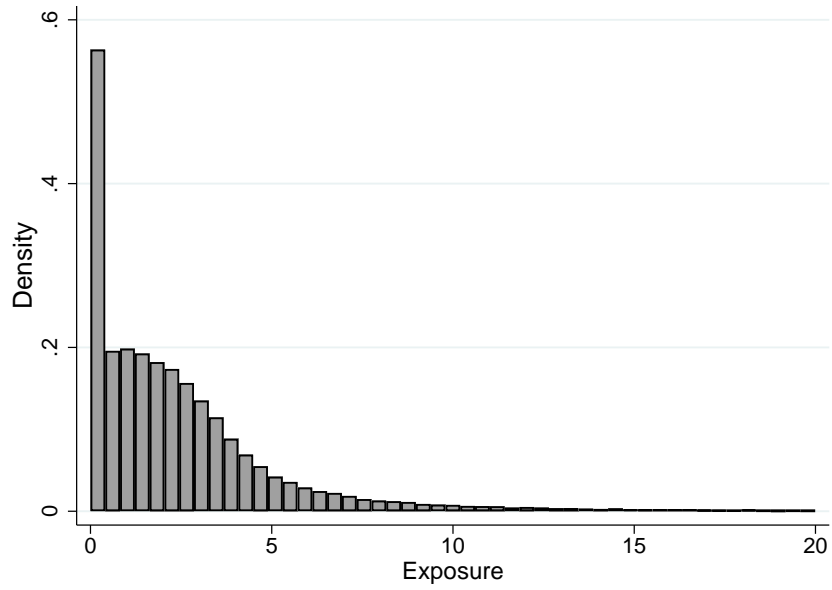
Online Appendix Figure A8: Matched Difference-in-Differences Estimates of the Effect of Privatization on Log Employment



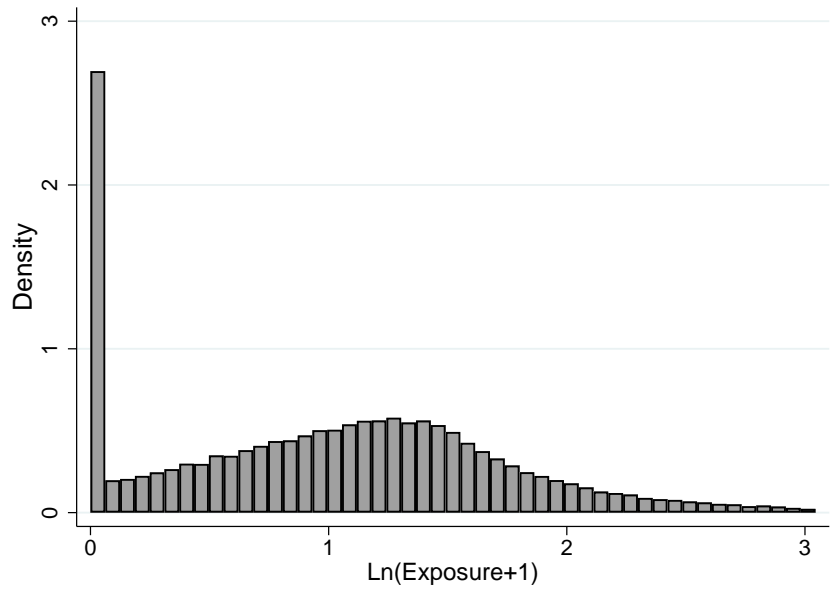
Note: This figure shows matched difference-in-differences estimates of the effect of privatization on log establishment employment. Panel A displays results for establishments in the banking sector, Panel B displays results for establishments in the telecommunications sector, and Panel C displays results for establishments in the electricity sector. The control 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.

Online Appendix Figure A9: Histograms of Exposure Measures

Panel A: Histogram of Exposure

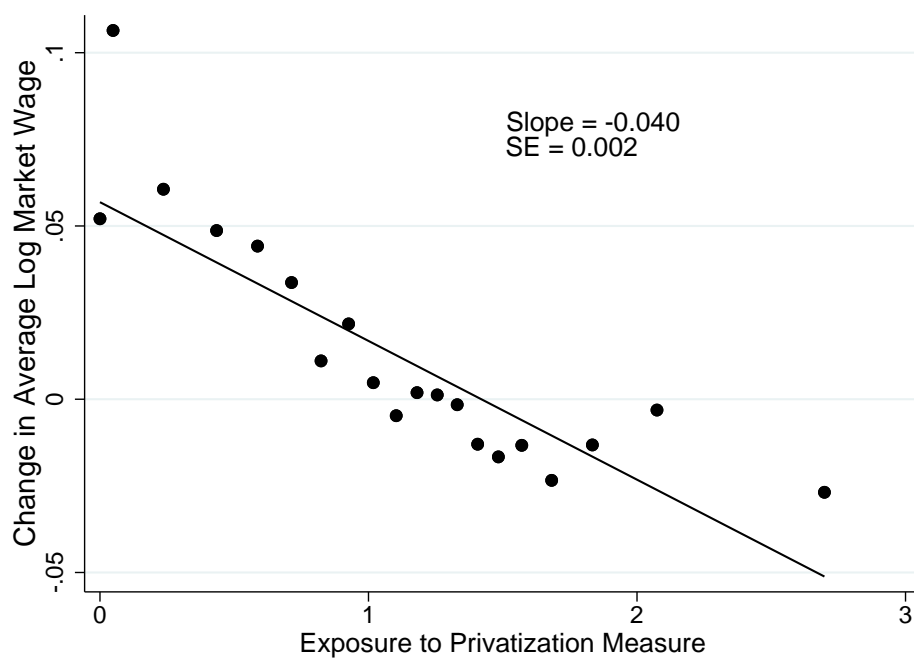


Panel B: Histogram of $\ln(\text{Exposure}+1)$



Note: This figure displays a histogram of exposure to privatization in Panel A and $\ln(\text{Exposure} + 1)$ 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.

Online 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 Online Appendix Figure A9.

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

	All Workers	No HS Degree	HS Degree	College Degree
	(1)	(2)	(3)	(4)
<i>Panel A: Effect of privatization on all workers</i>				
Post Short-run	-0.053 (0.014)	-0.101 (0.017)	-0.032 (0.011)	-0.052 (0.013)
Post Long-run	-0.215 (0.020)	-0.346 (0.026)	-0.185 (0.018)	-0.164 (0.020)
Avg. Outcome at $t = -1$	8.609	8.389	8.554	9.045
Observations	3,275,153	774,954	1,485,328	948,046
<i>Panel B: Effect of privatization on stayers</i>				
Post Short-run	-0.010 (0.011)	-0.030 (0.016)	-0.008 (0.010)	-0.014 (0.012)
Post Long-run	-0.119 (0.015)	-0.148 (0.026)	-0.118 (0.014)	-0.115 (0.018)
Avg. Outcome at $t = -1$	8.609	8.389	8.554	9.045
Observations	1,997,804	554,887	903,877	477,882
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 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.

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

	White Workers	Nonwhite Workers
	(1)	(2)
<i>Panel A: Effect of privatization on all workers</i>		
Post Short-run	-0.039 (0.015)	-0.093 (0.014)
Post Long-run	-0.196 (0.021)	-0.264 (0.029)
Avg. Outcome at $t = -1$	8.632	8.396
Observations	1,835,563	450,516
<i>Panel B: Effect of privatization on stayers</i>		
Post Short-run	0.005 (0.013)	-0.025 (0.012)
Post Long-run	-0.104 (0.015)	-0.102 (0.015)
Avg. Outcome at $t = -1$	8.632	8.396
Observations	1,027,134	247,054
Worker FE	Yes	Yes
Time FE	Yes	Yes
Demographics	Yes	Yes

Note: This table presents difference-in-differences estimates of the effect of privatization on workers 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 two-way clustered by worker and establishment.

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

	Banking	Telecom.	Electricity
	(1)	(2)	(3)
<i>Panel A: Effect of privatization on all workers</i>			
Post Short-run	0.017 (0.008)	-0.024 (0.029)	-0.124 (0.018)
Post Long-run	-0.166 (0.012)	-0.083 (0.040)	-0.327 (0.026)
Avg. Outcome at $t = -1$	8.703	8.394	8.705
Observations	969,475	803,958	1,370,349
<i>Panel B: Effect of privatization on stayers</i>			
Post Short-run	0.068 (0.007)	-0.072 (0.021)	-0.033 (0.018)
Post Long-run	-0.059 (0.012)	-0.158 (0.030)	-0.150 (0.026)
Avg. Outcome at $t = -1$	8.703	8.394	8.705
Observations	613,837	419,136	878,134
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 workers wages. 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 two-way clustered by worker and establishment.

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

	Banking	Telecomm	Electricity
	(1)	(2)	(3)
Post Short-run	-0.100 (0.002)	-0.025 (0.002)	-0.022 (0.002)
Post Long-run	-0.170 (0.003)	-0.078 (0.003)	-0.066 (0.003)
Avg. Outcome at $t = -1$	8.703	8.394	8.705
Observations	1,159,110	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 clustered at the worker level.

Online Appendix Table A5: Robustness Results

	Private Control (1)	Public Control (2)	Winsorized (3)	Match Earlier (4)	5 digit Occ/Sec (5)	Trade Exposure (6)	Region + Estab Size (7)	Region + Firm Size (8)
<i>Panel A: Effect of privatization on all workers</i>								
Post Short-run	-0.033 (0.013)	-0.056 (0.012)	-0.045 (0.012)	-0.044 (0.015)	-0.047 (0.013)	-0.042 (0.009)	-0.047 (0.012)	-0.060 (0.015)
Post Long-run	-0.138 (0.017)	-0.242 (0.019)	-0.195 (0.017)	-0.219 (0.021)	-0.214 (0.019)	-0.201 (0.014)	-0.206 (0.016)	-0.179 (0.020)
Avg. Outcome at $t = -1$	8.447	8.716	8.606	8.633	8.625	8.603	8.664	8.668
Observations	2,255,607	2,822,735	3,275,153	2,793,876	2,701,246	1,488,119	1,173,563	546,814
<i>Panel B: Effect of privatization on stayers</i>								
Post Short-run	-0.010 (0.011)	0.001 (0.011)	-0.006 (0.009)	-0.002 (0.015)	-0.012 (0.011)	-0.019 (0.007)	-0.019 (0.010)	0.002 (0.016)
Post Long-run	-0.069 (0.018)	-0.119 (0.014)	-0.106 (0.013)	-0.128 (0.019)	-0.130 (0.015)	-0.144 (0.014)	-0.131 (0.017)	-0.080 (0.027)
Avg. Outcome at $t = -1$	8.447	8.716	8.606	8.633	8.625	8.603	8.664	8.668
Observations	1,371,361	1,749,892	1,997,804	1,732,836	1,667,885	937,836	714,417	315,796
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 winsorizes log earns 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 two-way clustered by worker and establishment.

Online 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	-0.072 (0.001)	-0.075 (0.003)	-0.064 (0.002)	-0.076 (0.003)
Post Long-run	-0.100 (0.002)	-0.100 (0.003)	-0.097 (0.002)	-0.087 (0.004)
Avg. Outcome	0.674	0.607	0.704	0.710
Observations	4,602,848	1,485,104	2,098,320	1,018,592
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.

Online 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.

Online 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	-0.156 (0.040)	-0.021 (0.011)	-0.004 (0.007)	-0.017 (0.005)
Observations	83,549	408,947	803,580	1,361,893
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 log average 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.

Online 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	-0.018 (0.005)	-0.012 (0.007)			
Asinh Exposure			-0.016 (0.004)		
Exposure/100				-0.154 (0.103)	
Exposure>0					-0.054 (0.011)
Observations	1,406,850	1,194,674	1,406,850	1,406,850	1,406,850
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 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 and goes from 0 to 100. In Columns 1-2 the dependent variable is $\log(\text{exposure}+1)$. In Column 3 the dependent variable is $\text{asinh}(\text{exposure})$. In Column 4 the dependent variable is exposure. 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 clustered at the labor market level.

Online 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	-0.018 (0.009)	-0.020 (0.005)	-0.024 (0.010)	-0.019 (0.005)
Change in Log Labor-Market Size				-0.000 (0.000)
Observations	1,406,850	1,404,256	1,352,563	1,281,885
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 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 and goes from 0 to 1. Column 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.

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

	Banking	Telecomm	Electricity
	(1)	(2)	(3)
Banking Exposure	-0.029 (0.011)		
Telecom Exposure		-0.044 (0.017)	
Electricity Exposure			-0.127 (0.015)
Observations	1,406,850	1,406,850	1,406,850
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.

Online Appendix Table A12: Effect of Privatization on AKM Wage Premiums

	No HS Degree	HS Degree	College Degree
	(1)	(2)	(3)
Short-run	-0.055 (0.002)	-0.105 (0.002)	-0.104 (0.002)
Long-run	-0.149 (0.005)	-0.171 (0.003)	-0.132 (0.005)
Observations	104370422	32,384,996	12,676,617

This table estimates an AKM model with worker fixed effects, establishment fixed effects 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. The Baseline AKM premium compares the wages in privatized firms relative to private-sector firms in the same industry, where the AKM premium is allowed to vary by education group.

Online Appendix Table A13: Informal Sector Wage Penalty

	All Sectors (1)	Privatized Sectors (2)
Informal	-0.321 (0.002)	-0.267 (0.013)
Union	0.103 (0.002)	0.134 (0.009)
Education (Years)	0.046 (0.000)	0.080 (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 2002-2004. 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 São Paulo 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.¹

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. 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

¹ See <https://www.wsj.com/articles/SB898274445213587000> for a description of the privatization sale of a portion of Telebrás to the Spanish firm Telefonica.

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 CNPJ 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).²

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.

²See <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.

E. Overview of PNAD

The Brazilian National Household Survey, Pesquisa Nacional por Amostra de Domicílios (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 (PNAD, 2016).³ 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:

$$RTR_r = - \sum_i \beta_{ri} d \ln(1 + \tau_i) \quad (1)$$

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 for β_{ri} is:

$$\beta_{ri} \equiv \frac{\lambda_{ri} \frac{1}{\psi_i}}{\sum_j \lambda_{rj} \frac{1}{\psi_j}} \quad (2)$$

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, Piani and Souza (2003). In order to understand how trade liberalization impacts the estimation of market-level effects, I estimate the specification in Equation (??), but with the additional term $\xi(RTR_r \cdot t)$, 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 ??). 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

³See 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.

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 -0.018.

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 John M Abowd, Francis Kramarz (1999) (AKM) and David Card, Jorg Heining and Patrick Kline (2013) utilizing data from the years prior to the privatization (1992-1995). Formally, I estimate the following regression model:

$$\ln(w_{it}) = \alpha_i + \psi_{J(i,t)} + \gamma_t + x'_{it}\beta \quad (3)$$

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, Heining and Kline (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, Heining and Kline, 2013; Song et al., 2019), and in particular, in Brazil (Jorge Alvarez, Felipe Benguria, Niklas Engbom and Christian Moser, 2018).

I estimate Equation (3) on the entire sample of establishments and collect the estimated $\hat{\psi}_j$ for all firms.⁴ 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.⁵ To estimate the wage premium associated with a privatized establishment prior to privatization, I estimate the following regression:

$$\hat{\psi}_j = \alpha + \omega Privatized_j + \xi_j + \varepsilon_j \quad (4)$$

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 ?? and discussed in Section ??.

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:

$$\ln(w_{it}) = \alpha_i + \psi_{J(i,t)} + \delta^{sr} Privatized_{J(i,t)} \cdot SR_t + \delta^{lr} Privatized_{J(i,t)} \cdot LR_t + \varepsilon_{it} \quad (5)$$

⁴As 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.

⁵The wage premium associated with the never-privatized SOEs is similar to the wage premium associated with privatized SOEs.

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 δ s 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 δ s to be negative. Given the evidence from the worker-level results, we expect both δ s to be negative, and that $\delta^{lr} < \delta^{sr}$, capturing the fact the effects gradually grow in magnitude.

To implement the estimation, I pool all years from 1993 to 2004 to estimate Equation (5). 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 Online Appendix Table A12, privatization decreases wages in the short run by 5.4 percent for the no-high-school degree sample, by 10.0 percent for the high-school sample, and 9.9 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 13.8 percent in the no high-school sample, 15.7 percent in the high-school sample, and 12.4 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:

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

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:

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

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:

$$\mathbb{E}[w_1 - w_0] = \underbrace{Pr(\text{stayer}|T_i = 1) \cdot (w_s^1 - w_s^0)}_{\text{within-establishment effect}} + \underbrace{(Pr(\text{stayer}|T_i = 1) - Pr(\text{stayer}|T_i = 0)) \cdot (w_s^0 - w_m^0)}_{\text{displacement effect}} \quad (6)$$

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 Table A1) and the decomposition for the long-run (i.e. the first column, second row of Table A1).

In the short-run, $Pr(\text{stayer}|T_i = 1) = 0.441$. In Panel B of Appendix Table A1, I find $w_s^1 - w_s^0 = -0.010$. The aggregate effect including transitions (Panel A of Appendix Table A1) is $\mathbb{E}[w_1 - w_0] = -0.053$. Plugging these numbers into (6) and solving for the displacement effect in (6):

$$(Pr(\text{stayer}|T_i = 1) - Pr(\text{stayer}|T_i = 0)) \cdot (w_s^0 - w_m^0) = -0.053 - 0.441 * (-0.010) = -0.049.$$

Therefore, in the short-run, the within-establishment effect explains $\frac{0.053 - 0.049}{0.053} \times 100 = 7.5$ percent of the overall fall in wages, with the rest being explained by the displacement 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 (Louis S Jacobson, Robert J LaLonde and Daniel G Sullivan, 1993).

Panel A of Online 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 Online Appendix Figure A2 plots δ^k from estimating Equation (??) 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 ?? 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 Domicílios (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.⁶

⁶The 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 microdata.

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

$$\ln(w_{it}) = \beta \text{Informal}_{it} + \gamma_t + \xi_s + \chi_o + \zeta_r + \pi X_{it} + \varepsilon_{it} \quad (7)$$

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 Online Appendix Table A13, when pooling all industries, I find an informal-sector wage penalty equal to -0.324 .⁷ 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 Online

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, Green and Sand (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, Green and Sand (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 3.0 percent. Therefore, the magnitudes here are similar to the magnitudes found in Beaudry, Green and Sand (2012).

Another potentially relevant paper comes from Jofre-Monseny, Silva and Vázquez-Grenno (2018), which uses a similar search and matching model as Beaudry, Green and Sand (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,

⁷Estimating an earnings function without state, occupation and industry fixed effects yields an informal sector wage penalty equal to -0.422 .

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 3.0 percent decline in the aggregate wage.

Lastly, Muralidharan, Niehaus and Sukhtankar (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 ???. 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:

$$w_m = \gamma R + (1 - \gamma)v \quad (8)$$

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 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:

$$v^0 = \mathbb{E}[w] - c = \lambda_u b + \lambda_p \mathbb{E}[w|j = p] + \lambda_{soe} \mathbb{E}[w|j = soe] + \lambda_{priv} \mathbb{E}[w|j = soe] - c \quad (9)$$

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:

$$v^1 = \mathbb{E}[w] - c = \lambda_u b + \lambda_p \mathbb{E}[w|j = p] + \lambda_{soe} \mathbb{E}[w|j = soe] + \lambda_{priv} \mathbb{E}[w|j = p] - c \quad (10)$$

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 parameter 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 (21 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:

$$v^1 - v^0 = \lambda_{priv}(0.21)(b) + \lambda_{priv}(0.79)\mathbb{E}[w|j = p] - \lambda_{priv}\mathbb{E}[w|j = soe] \quad (11)$$

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 about 1.187 times the wage in the private sector.⁸ The wage in the informal sector is estimated to be 0.766 times the wage in the private sector, as seen in Online 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:

$$w_1 - w_0 = (1 - \gamma) \cdot Exposure \cdot \mathbb{E}[w|j = p](0.21 \cdot 0.766 + 0.79 - 1.187) \quad (12)$$

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:

$$\frac{w_1 - w_0}{w_0} = (1 - \gamma) \cdot Exposure \cdot (-0.236) \quad (13)$$

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

$$\frac{\partial \log(w)}{\partial Exposure} = (1 - \gamma) \cdot (-0.236) \quad (14)$$

In column 4 of Online Appendix Table A9, I found that $\frac{\partial \log(w)}{\partial Exposure} = -0.156$. Therefore, solving for γ yields:

$$\gamma = 0.339 \quad (15)$$

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.

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⁸This comes from column 1 of Table ??, where the AKM wage premium for privatized SOE workers is estimated to be 0.172 log points, converting this to proportional effects yields an 18.7 percent wage premium for SOEs.

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