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Assessing the Political Aspects of Full Employment: Evidence from Strikes and Lockouts

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Abstract

Using monthly state-level data on work stoppages from the Bureau of Labor Statistics (BLS) and state-level labor market data from the Current Population Survey (CPS) this paper estimates the effect of state-level labor market conditions on strike activity from 1993 to 2023. Panel fixed-effects estimates suggest a one percentage-point increase in the unemployment rate reduces the number of work stoppages involving 1,000 or more workers (per million) by approximately 14%. The fixed-effects estimates are supported by a propensity-score based specification that exploits the differential timing of national recessions across US States. Entering a recession is negatively related to state-level strike activity as measured by both work stoppages and the share of employed workers reporting an absence from work due to a labor dispute. The results in this paper provide empirical support for Kalecki (1943)'s argument regarding the "political aspects of full employment": weak labor markets reduce direct action by labor, thereby providing a rationalization for capitalist opposition to full employment policy.

Keywords: Michal Kalecki, Strikes, Work Stoppages, Labor Relations, Business Cycles

JEL Codes: J52, D33, E11

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1 Introduction

In an oft-quoted passage in his 1943 essay on the "Political Aspects of Full Employment," Michal Kalecki asserts that political opposition to the maintenance of full employment by government spending is rooted in opposition to the social and political changes to which the maintenance of full employment would lead. In particular, Kalecki (1943) argues that under a full employment regime:

> [T]he 'sack' would cease to play its role as a disciplinary measure. The social position of the boss would be undermined, and the self-assurance and class-consciousness of the working class would grow. Strikes for wage increases and improvements in conditions of work would create great political tension. (Kalecki, 1943, p.326)

Although profits are higher in an economy operating at full employment, "discipline in the factories and political stability are more appreciated by business leaders than profits" (Kalecki, 1943, p.326). As a result, attempts to secure full employment via government spending are thwarted by capitalist collective action[1].

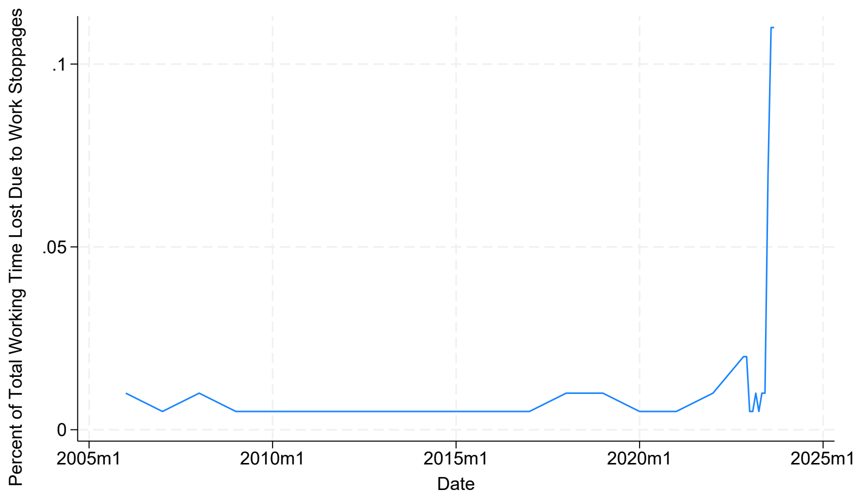
Despite the ubiquity of Kalecki (1943)'s essay (e.g., see Sawyer, 2023, for a review), little formal work has attempted to evaluate Kalecki's conclusions. The exception to this is perhaps the work of Nordhaus (1975) on political business cycles.[2] Thus, the seemingly positive association between strike activity and tight labor markets following the Covid-19 pandemic (see Figure 1) suggests that Kalecki (1943)'s argument may be due for a re-evaluation. In this

[1] Importantly, Kalecki's insight that a regime of full-employment creates a conflict of interest between workers and capitalists is contrary to the assertions of many defenders of free-market capitalism. For example, Knight (1940) writes that "[E]conomic depression is a phenomenon of the mechanics of money and presents an especially interesting and significant problem—a sort of test case on governmental action—*in that it does not rest upon a conflict of interest*. Practically everyone loses by depression and would gain from its abolition" (p.193).

[2] Nordhaus (1975) remarks that "[t]he only serious theory [of the political causes of the business cycle] is that of M. Kalecki" (p.181). However, Nordhaus (1975) is ultimately dismissive of Kalecki (1943), attributing Kalecki's conclusions to the fact that he assumes "business leaders and capitalists have a disproportionate control of the political mechanism" (p.182). If Kalecki is guilty of overstating the political influence of business leaders, Nordhaus (1975)—in adopting a social welfare function defined over the preference ordering of the median voter—stands condemned of the opposite sin: namely, assuming that capitalists have no influence at all.

paper, I provide an empirical assessment of one of the central premises of Kalecki (1943)'s argument: namely, that unemployment reduces labor disputes—including strikes and lockouts, thereby providing a rationale for capitalist opposition to full employment policy.

Figure 1: Percent of Total Working Time Lost Due to Work Stoppages Involving 1,000 or More Workers, 2005-2023



Notes: Data presents days of idleness from annaul work stoppages involving 1,000 or more workers as a % of total working time from January 2006 to September 2023. Data obtained from the Bureau of Labor Statistics (BLS) Work Stoppages series.

Using monthly data on work stoppages from the Bureau of Labor Statistics (BLS) and state-level labor market data from the Current Population Survey (CPS), I estimate the effect of state-level labor market conditions on strike activity from 1993 to 2023. Panel fixed-effects estimates suggest that a one percentage-point increase in the unemployment rate reduces the number of work stoppages involving 1,000 or more workers (per 1,000,000 employees) by approximately 14%. Using an alternative measure of strike activity based on the share of employed workers absent due to a labor dispute produces qualitatively similar results. The results are robust to the inclusion of a large suite of fixed-effects and controls. Application of the Oster (2019) adjustment for selection on unobservables suggests that—if anything—the OLS estimates are biased toward a null-effect.

To further address concerns about endogeneity in the panel fixed-effects estimates, I adopt an alternative identification strategy that exploits the differential timing of macroeconomic recessions across US states. In particular, I use the Sahm (2019)-rule to identify state-specific business cycle activity and apply an inverse-probability weighted regression adjustment (IPWRA) estimator (Wooldridge, 2007; Imbens and Wooldridge, 2009) to account for the fact that the timing of recession activity may not be randomly distributed across space. Results from this specification suggest entering a recession reduces work stoppages of 1,000 employees or more (per 1,000,000 employees) between 40% and 60%. Entering a recession similarly reduces the share of employed workers absent from work as a result of a labor dispute between 39% and 54%. These results are supported by placebo tests that suggest entering a recession has no effect on the fraction of employed workers absent from work for other reasons.

The rest of the paper is organized as follows. Section 2 reviews the literature on Kalecki (1943)'s essay as well as the literature on the macroeconomic drivers of strike activity. Section 3 introduces the data and describes the estimation strategy. Section 4 presents the results. Section 5 concludes.

2 Related Literature

Although widely cited (e.g., Sawyer, 2023; Toporowski, 2023; Spencer, 2024), little work has attempted to either theoretically formalize or empirically validate the insights of Kalecki (1943)'s famous essay. The literature on political business cycles—beginning with Nordhaus (1975)—is a notable exception. Unfortunately, the literature on political business cycles has tended to produce conclusions directly at odds with those suggested by Kalecki (1943). In particular, the political business cycles literature minimizes the influence of capitalist special interests on electoral outcomes, instead blaming myopic voters and self-interested politicians (see Dubois, 2016, for a review) for policy-driven fluctuations in unemployment and inflation. Thus, the first contribution of this paper is to provide empirical evidence in support of Kalecki (1943)'s argument that tight labor markets increase labor disputes (thereby providing a rationale for capitalist opposition to full employment).

The second contribution of this paper is to the broader literature on the macroeconomic determinants of strike activity. Although not in an explicitly Kaleckian context, a number of papers have attempted to assess either the determinants of strike activity or the consequences of strike activity for either consumer demand or product quality (Kaufman, 1982; Schor and Bowles, 1987; Card, 1990; Schmidt and Berri, 2004; Gruber and Kleiner, 2012; Massenkoff and Wilmers, 2024). In an important recent contribution, Massenkoff and Wilmers (2024) highlight the large decline in strike activity in the United States from 1970 to 2000 (an approximately 90% decline). Massenkoff and Wilmers (2024) attribute the decline in strike activity to the deterioration in strike outcomes for workers after 1981, providing evidence that—on average—strikes since 1981 have not been associated with improvements in wages, hours or benefits. Massenkoff and Wilmers (2024) argue that the ineffectiveness of strikes in the post-1981 period can be linked to anti-worker structural labor market shifts, particularly those stemming from the decline in labor relations following the 1981 air traffic controllers strike.

The findings in this paper most closely relate to the older work of Kaufman (1982) and Schor and Bowles (1987). Using annual data on strike activity for the United States from 1900 to 1977, Kaufman (1982) finds that strike activity increases in periods of rapid inflation and decreases during period of high unemployment. Similarly, Schor and Bowles (1987) show that strike activity is inversely related to the cost of job loss—measured as the difference between current earnings and expected income during the year following an employment termination—on an annual basis for the United States from 1955 to 1983. This study builds on these earlier papers, which feature small sample sizes based on annual time series data, by using monthly state-level panel data and an estimation strategy based on the differential timing of national recessions across US states to obtain well-identified estimates of the effect of labor market conditions on strike activity.

Finally, this paper contributes to the large empirical literature in Kaleckian macroeconomics. Much of the empirical literature in Kaleckian economics has focused on dimensions of the wage- versus profit-led demand (and/or growth) debate (Nikiforos and Foley, 2012; Rada and Kiefer, 2015; Blecker, 2016; Petach, 2020). More recently, scholars in the Kaleckian tradition have extended the empirical analysis of Kaleckian thought to a variety of contexts including estimating the effect of autonomous demand expansions (Girardi et al., 2020), assessing the validity of the Sraffian supermultiplier (Nikiforos et al., 2023), and exploring the regional implications of Kaleckian macroeconomics (Petach, 2021; Mendieta-Muñoz et al., 2022). By applying state-level panel data to assess the political aspects of full employment, this paper thus contributes to the growing empirical literature examining the regional dimensions of Kaleckian thought.

3 Data and Estimation Strategy

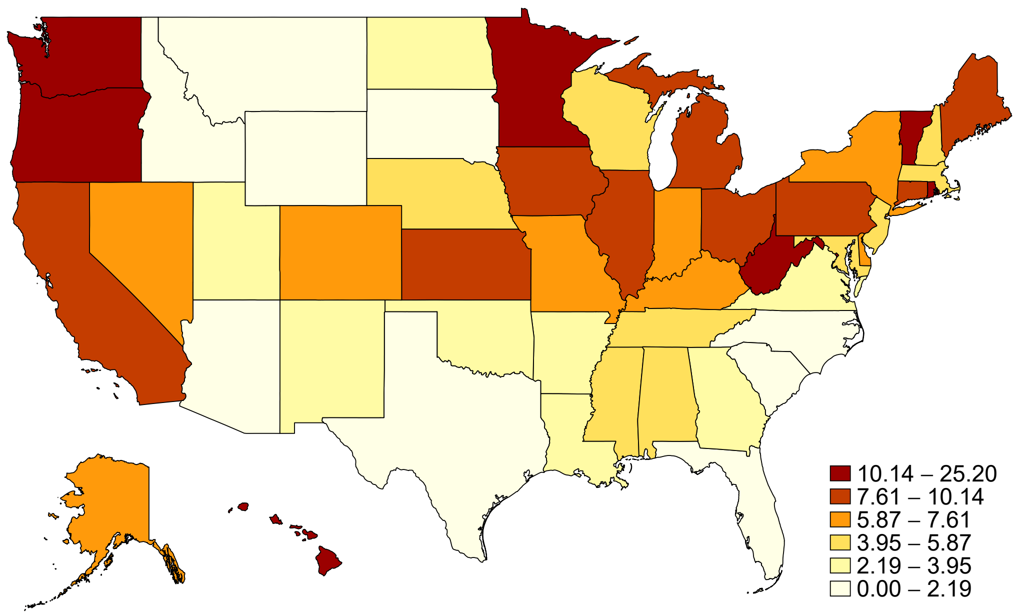
3.1 Data Description

Work Stoppage Data. Data on the state-level incidence of labor disputes are obtained from the Bureau of Labor Statistics (BLS) Work Stoppages program. The BLS Work Stoppages program provides monthly data on major work stoppages involving 1,000 or more workers lasting one full shift or longer. The monthly data indicate the establishment, union, and locations involved with the work stoppage. In particular, each entry in the data lists the organizations involved, the states where workers in those organizations are participating in the work stoppage, the beginning date of the work stoppage, the ending date of the work stoppage (if applicable), and the number of workers involved. The data is available on a monthly basis from 1993 onward[3]. Using this data, I construct state-level estimates of (A) the number of *new* work stoppages involving 1,000 or more workers in a given state in a given month, and (B) the number of total work stoppages (new and on-going) involving 1,000 or more workers in a given state in a given

[3] The earliest work stoppage beginning date in the data is February 16, 1988, for the Marine Towing and Transportation Employers' Association work stoppage in New York state that ended on December 20th, 1993.

month[4]. Figure 2 maps the total number of new work stoppages involving 1,000 workers or more (per million workers) between January 1993 and September 2023 across US states. Unsurprisingly, Figure 2 reveals that major work stoppage activity is concentrated in states with a historically significant union presence (e.g., Ohio, West Virginia, Michigan, Pennsylvania, and other states in the industrial Midwest) and/or states with a legal-institutional environment favorable to labor (e.g., California, Oregon, New York, Massachusetts, etc.).

Figure 2: Total Work Stoppages (per million workers), January 1993 to September 2023



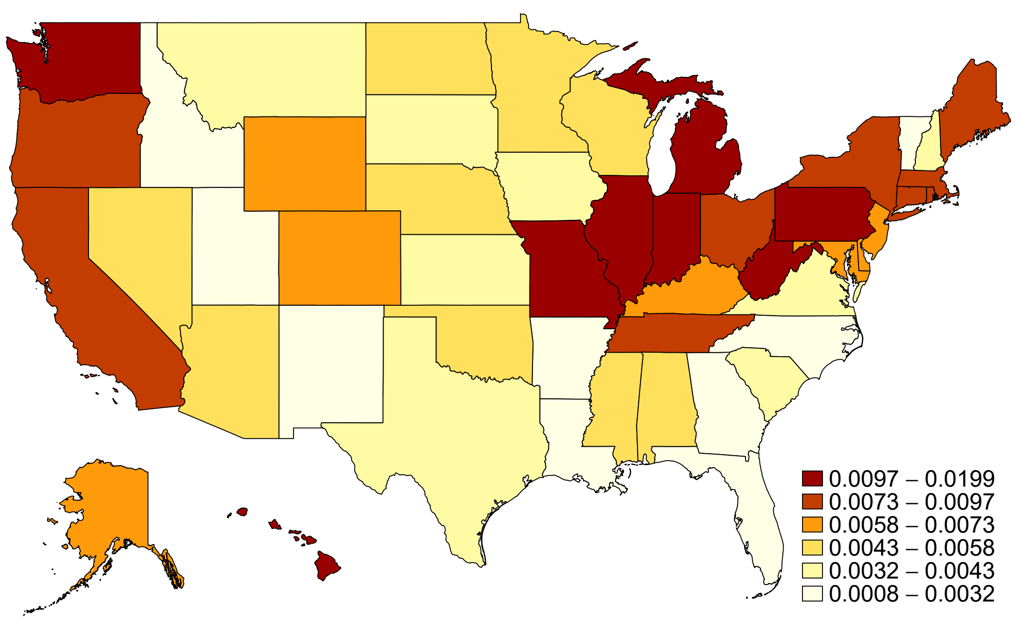
Notes: Figure maps the number of new work stoppages involving 1,000 or more workers (per 1,000,000 workers) that occurred between January, 1993 and September, 2023.

Labor Dispute Data. As an alternative measure of the state-level incidence of labor disputes, I obtain monthly data on the share of employed workers reporting an absence from work due to a labor dispute from the Current Population Survey (CPS) Integrated Public-Use Microdata Series (IPUMS) (Flood et al., 2023). Data on work absences is available on a monthly basis

[4]Because each entry in the BLS Work Stoppages program data constitutes a single work stoppage action, listing only the total number of workers involved and each of the states involved (but not the number of workers from each state), I do not make assumptions about how workers involved in the stoppage are allocated across states. Instead, the two measures of work stoppage constructed above simply indicate whether or not a stoppage involving 1,000 or more workers is affecting a particular state in a given month (regardless of what fraction of workers involved in the stoppage are employed in that state).

from January 1976 onward[5]. In particular, if an employed CPS respondent was absent from work in the past week, they are given a follow-up question asking them to specify "Why was [this person] absent from work last week?" I aggregate all respondents indicating a "labor dispute" as the reason for absence to the state-level to arrive at an estimate of the share of employed workers in each month who report missing work in the previous week as a result of a labor dispute. Figure 3 maps the average share of employed workers reporting an absence from work in the previous week due to a labor dispute between January 1993 and September 2023. The regional pattern of labor activity in Figure 3 is similar to that in Figure 2, although Figure 3 reveals the generally limited extent of labor direct action in the United States: even in the states where labor is most active, less than one one-hundredth of one-percent of employed CPS respondents report an absence from work in the previous week due to a labor dispute in an average month.

Figure 3: Percent of Employed Workers Absent in the Last Week due to a Labor Dispute, January 1993 to September 2023



Notes: Figure maps the average percent of employed workers absent from work in the previous week due to a labor dispute between January 1993 and September 2023.

[5]Although data on work absences is available from 1976 onward, monthly data on union membership—a key control variable in the regression specifications below—is only available from May 1983 onward. As such, I ultimately restrict the IPUMS CPS sample to the period after May 1983.

Additional State-level Characteristics. Data on additional state-level characteristics are obtained from the Current Population Survey (CPS) Integrated Public-Use Microdata Series (IPUMS) (Flood et al., 2023) and the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS) database. Data on state-level educational attainment, demographics, and union membership are obtained from the IPUMS CPS database. Data on state-level population, labor force participation, employment, and unemployment are obtained from the BLS LAUS database. I also obtain data on additional reasons for absences from work from the IPUMS CPS database. Table 1 presents summary statistics for all key variables.

Table 1: Summary Statistics

	Mean	Std. Dev.	N	Dates	Source
New Work Stoppages (per million)	.0167844	.0828426	18,819	1993m1-2023m9	BLS Work Stoppages
Total Work Stoppages (per million)	.0299758	.1108801	18,819	1993m1-2023m9	BLS Work Stoppages
Absent - Labor Dispute	.0001293	.0005375	24,735	1983m5-2023m9	IPUMS CPS
Unemployment Rate (%)	6.099204	2.202573	29,223	1976m1-2023m9	BLS LAUS
Employment-Population Ratio (%)	60.78366	3.953733	29,223	1976m1-2023m9	BLS LAUS
Population	9,447,224	7,697,794	29,223	1976m1-2023m9	BLS LAUS
Union Membership (%)	13.12734	7.028484	24,735	1983m5-2023m9	IPUMS CPS
White (%)	82.31636	8.960677	24,735	1983m5-2023m9	IPUMS CPS
College (%)	30.56761	7.775501	24,735	1983m5-2023m9	IPUMS CPS
Over 65 (%)	3.615972	1.457489	24,735	1983m5-2023m9	IPUMS CPS
Tradables Employment (%)	36.79025	6.317547	24,735	1983m5-2023m9	IPUMS CPS
Absent - Vacation	.0215762	.0197547	24,735	1983m5-2023m9	IPUMS CPS
Absent - Weather	.000978	.0031431	24,735	1983m5-2023m9	IPUMS CPS
Absent - Medical	.0084789	.0036954	24,735	1983m5-2023m9	IPUMS CPS

Notes: Observations weighted by average state population. “Tradables Employment (%)” measures the % of workers employed in agriculture, mining, manufacturing, retail, or wholesale industries. “Over 65(%)” captures the % of workers aged 65 or older.

3.2 Estimation Strategy

To estimate the effect of state-level labor market conditions on strike activity, I begin with the following panel fixed-effects specification:

$$Y_{it} = \beta_0 + \beta_1 Unemployment_{it} + \mathbf{X}_{it}^T \alpha + \gamma_i + \delta_t + \epsilon_{it} \quad (1)$$

Where Y_{it} is a measure of labor activity—either strikes or worker absences related to labor disputes—in state i, in month t, $Unemployment_{it}$ is the state-level unemployment rate (or an alternative measure of labor-market conditions, such as the employment-population ratio) in state i, in month t, $\mathbf{X}_{it}$ is a vector of state-level economic characteristics, γ_i is a state-fixed effect, δ_t is a time-fixed effect, and ϵ_{it} is an idiosyncratic error term. Because the various measures of strike activity used as dependent variables are highly skewed with many zero values, estimation of Equation 1 by ordinary least squares (OLS) is likely to be misleading[6]. In particular, a large literature suggests that in such cases Poisson regression is preferred to OLS, even when the dependent variable is not an integer (Silva and Tenreyro, 2004; Nichols, 2010; Gould, 2011; Cohn et al., 2022; Mullahy and Norton, 2023; Chen and Roth, 2023). In this case, I apply the Poisson pseudo-maximum likelihood fixed-effects estimator described by Correia et al. (2020).[7] The regression coefficients obtained from the Poisson pseudo-maximum likelihood estimator have the convenient property that they may be directly interpreted as semi-elasticities.

Although inclusion of a large suite of controls and fixed-effects is likely to absorb a significant amount of unobserved heterogeneity influencing the relationship between labor market conditions and strike activity, the panel fixed-effects specification described in Equation 1 is unlikely to capture all potential sources of bias. Thus, I also implement an alternative estimator based on the differential timing of national recessions across US states. In particular, I use the Sahm (2019)-rule to define state-specific recession dates for national economic downturns. Originally described by Sahm (2019) as a rule for triggering automatic stimulus payments during national recessions, the Sahm (2019)-rule suggests that the economy is in a recession when the three-month moving average of the unemployment rate is more than 0.5 percentage points above its minimum value over the previous twelve months. The Sahm (2019)-rule successfully

[6]To offer one example, suppose one was interested in the (semi-)elasticity of strike activity to the unemployment rate. In this case, a typical approach might be to regress the natural log of strike activity on the unemployment rate. However, as Silva and Tenreyro (2004) note, Jensen's inequality implies that $E(ln(y)) \neq lnE(y)$, such that the parameters of log-linearized models estimated by OLS are likely to be biased estimates of the true elasticity in the presence of heteroskedasticity. Silva and Tenreyro (2004) show that unbiased estimates can be obtained by the application of a Poisson pseudo-maximum likelihood estimator, and that—importantly—in such cases the data does not have to be Poisson and the dependent variable need not be an integer.

[7]I implement this estimator using Stata's `ppmlhdfe` command (Correia et al., 2020).

identifies every national recession between 1970 and 2014 with no false positives. To illustrate the usefulness of the Sahm (2019)-rule in identifying the differential timing of national recessions across states, Figure 4a plots the difference between the three-month moving average of the unemployment rate and its low over the prior twelve months for three states over the course of the Great Recession: Texas, Florida, and Nevada. Figure 4a indicates that for Florida and Nevada—states with labor markets that were severely impacted by the Great Recession—the Sahm (2019)-rule triggers several months ahead of the official NBER recession start date. In contrast, Texas—which was affected by the Great Recession to a much lesser degree—does not trigger until several months *after* the official NBER recession start date. Figure 4b plots the total number of states currently in a recession based on the Sahm Rule trigger over the course of the Great Recession. Figure 4b further illustrates the extent to which the timing of national recessions differs across states. It is not until December 2008—a full year after the national NBER recession start date—that every state in the country is in a recession according to the Sahm rule.

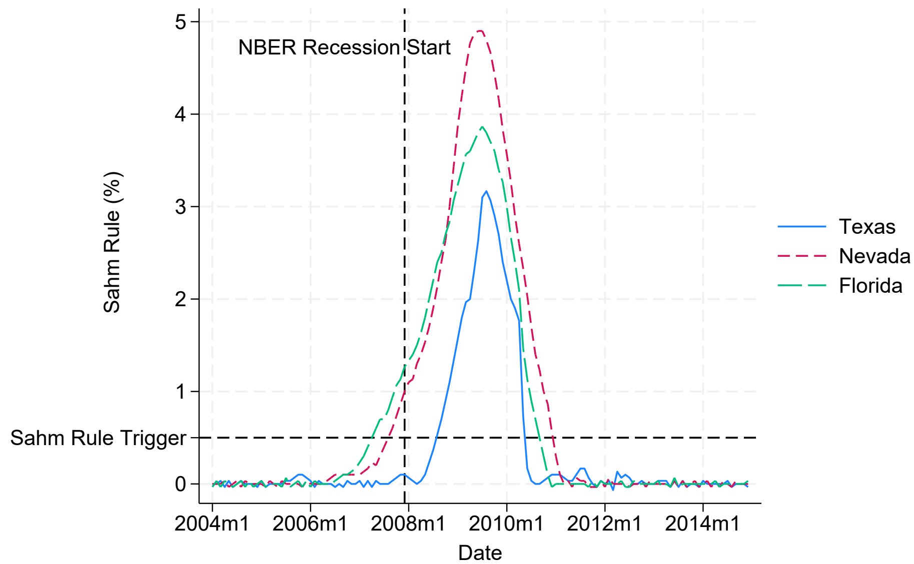
To exploit the differential timing of national recessions across US states using the Sahm rule, I estimate the following specification:

$$Y_{it} = \beta_0 + \beta_1 Sahm\_Rule\_Indicator_{it} + \mathbf{X}_{it}^T \alpha + \gamma_i + \delta_t + \epsilon_{it} \tag{2}$$

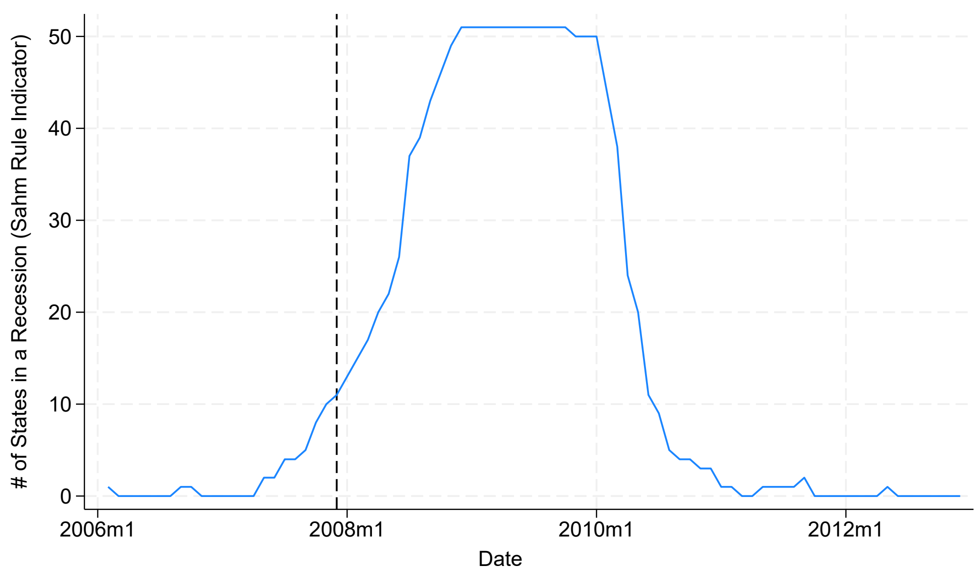
Where $Sahm\_Rule\_Indicator_{it}$ is an indicator variable for whether state i is in a recession at date t based on the Sahm rule trigger—i.e., a difference between the three-month moving average of the unemployment rate and the low over the prior twelve months greater than 0.5 percentage points. All other variables are defined as before. To address concerns that the regional severity of national recessions may not be randomly distributed across states, I estimate Equation 2 using propensity-score based methods. In particular, I employ an inverse-probability weighted regression adjustment (IPWRA) estimator (Wooldridge, 2007; Imbens and Wooldridge, 2009). The IPWRA estimator combines propensity score weighting with a regression adjustment, which uses regression analysis to arrive at estimates of counterfac-

Figure 4: The Sahm Rule for US States during the Great Recession

(a) Sahm Rule: Texas, Nevada, Florida



(b) # of States in a Recession (Sahm Rule Trigger)



Notes: Figure 4a plots the difference between the three-month moving average of the unemployment rate and the low over the prior twelve months for Texas, Nevada, and Florida between 2004 and 2014. The Sahm Rule Trigger is set at 0.5 percentage points. The official NBER recession start date is December 2007. Figure 4b plots the number of states currently in a recession based on the Sahm Rule trigger.

tual outcomes. The IPWRA estimator can be understood as a two-stage estimator[8]. In the first-stage, the Sahm-Rule indicator is regressed on lagged values of the Sahm-Rule, the unemployment rate, the employment-population ratio, as well as the contemporaneous controls to be included in the second-stage regression, and state- and time-fixed effects. The regression coefficients from the first-stage are used to generate a propensity score—e.g., the predicted probability of being in a recession—which is then used to weight observations in the second-stage regression. In the second stage, observations are re-weighted according to the inverse probability of treatment[9], such that higher weight is given to treated observations that appear unlikely to have been treated and to control observations that appear unlikely to have been controls. Effectively, this approach compares "treated" states (those in a recession) to a control group exhibiting similar dynamics, thereby addressing concerns about non-random selection into national recessions. An important feature of the IPWRA estimator is that it is doubly-robust, meaning that it is robust to mis-specification of either the treatment model (first-stage) or outcome model (second-stage) (Wooldridge, 2007). Appendix A presents regression results from the first-stage treatment model.

4 Results

4.1 Panel Fixed-Effects Estimates

Table 2 presents results from estimating Equation 1 via Poisson pseudo-maximum likelihood. Standard errors are clustered at the state-level to address the possibility of serial correlation over time within each state, the presence of which would erroneously shrink the confidence interval in the absence of clustering. Panels A and B use the unemployment rate as a measure of labor market slack, Panels C and D use the employment-population ratio. In Panel A and

[8]See Girardi et al. (2020) for an alternative application of the IPWRA estimator in the context of autonomous demand expansions.

[9]Specifically, treated units are assigned a weight of $\frac{1}{p}$ and control units are assigned a weight of $\frac{1}{1-p}$, where p is the predicted probability of treatment.

Panel C the dependent variable is *new* work stoppages involving 1,000 or more employees per million workers. In Panels B and D the dependent variable is *total* work stoppages (new and on-going) per million workers. Following Correia et al. (2020), I drop all observations that are separated[10] or singletons[11]. Finally, unless otherwise specified I weight all observations by average state population, such that the regression coefficients should be interpreted as the effect on the average person, rather than the effect on the average state.

The results in Table 2 suggest that labor market slack is inversely correlated with work stoppages. In a majority of regression specifications, an increase in labor market slack results in a reduction in work stoppages. The effect of the unemployment rate on both new- and total work stoppages is statistically and economically significant in every specification in Panels A and B except those reported in Column (4). Panel A suggests that a one percentage-point increase in the unemployment rate decreases new work stoppages involving 1,000 or more workers by up to 15%. Panel B suggests that a one percentage-point increase in the unemployment rate decreases total work stoppages involving 1,000 or more workers between 13.5% and 14.7% (the economic magnitude is actually larger in Column (4), although the coefficient is less precisely estimated). Panels C and D suggest the effect of the employment-population ratio is similar. In Panel C, the effect of the employment-population ratio on new work stoppages involving 1,000 or more workers is statistically insignificant in Column (3) and Column (4). However, in Panel D the effect of the employment-population ratio on total work stoppages is economically and statistically significant in every case. Columns (3) and (4) of Panel D suggest that a one percentage-point increase in the employment-population ratio increases total work stoppages involving 1,000 or more workers by up to 12.6%.

[10] A well-known problem with non-linear models is that maximum likelihood estimates are not guaranteed to exist. In particular, in the context of binary or count outcomes, "separation" occurs when one or more of a model's covariates perfectly predict the outcome variable (Zorn, 2005). Correia et al. (2021) show that—because any generalized linear model (GLM) can be nested within a "compactified" GLM where the conditional mean of each observation is allowed to go to its boundary values, and that observations with conditional means at the boundary are perfectly predicted observations—"separated" observations offer no information about the parameters with interior solutions, and as such can be dropped without affecting the consistency of the estimator.

[11] Correia (2015) shows that maintaining singleton observations—groups with only one observation—in regressions where fixed-effects are nested within clusters can overstate statistical significance and lead to incorrect inference, and therefore suggests dropping singleton observations.

Table 2: Panel Fixed-Effects Results — Work Stoppages Involving 1,000 or More Employees (January 1993 - September 2023)

	(1)	(2)	(3)	(4)
Panel A: New Stoppages (per 1,000,000)				
Unemployment$_{it}$	-0.151***	-0.152***	-0.0760**	-0.0243
	(0.0419)	(0.0413)	(0.0380)	(0.0510)
N	18,081	18,081	18,081	13,769
State Controls	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Month FE	N	Y	Y	N
Year FE	N	N	Y	N
Month $\times$ Year FE	N	N	N	Y
Panel B: Total Stoppages (per 1,000,000)				
Unemployment$_{it}$	-0.147***	-0.147***	-0.135*	-0.178
	(0.0469)	(0.0471)	(0.0792)	(0.112)
N	16,974	16,974	16,974	16,974
State Controls	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Month FE	N	Y	Y	N
Year FE	N	N	Y	N
Month $\times$ Year FE	N	N	N	Y
Panel C: New Stoppages (per 1,000,000)				
Employment-Population Ratio$_{it}$	0.115***	0.116***	0.0250	0.00378
	(0.0247)	(0.0247)	(0.0242)	(0.0289)
N	18,081	18,081	18,081	13,769
State Controls	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Month FE	N	Y	Y	N
Year FE	N	N	Y	N
Month $\times$ Year FE	N	N	N	Y
Panel D: Total Stoppages (per 1,000,000)				
Employment-Population Ratio$_{it}$	0.107***	0.107***	0.126**	0.126**
	(0.0363)	(0.0364)	(0.0521)	(0.0609)
N	16,974	16,974	16,974	16,974
State Controls	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Month FE	N	Y	Y	N
Year FE	N	N	Y	N
Month $\times$ Year FE	N	N	N	Y

Notes: Standard errors in parenthesis, clustered at the state-level. $p < 0.1$ * $p < 0.05$ ** $p < 0.01$ ***. Observations weighted by average state population. Estimates obtained via Poisson (pseudo-)maximum likelihood estimation using Stata's `ppmlhdfe` command (Correia et al., 2020). Time-varying state-level control variables include (log-) state population, the share of workers in a union, the share of the state population that is white, the share of the state population with at least four years of college, the share of the population over 65 years of age, the share of workers employed in tradables industries, and the rate of population growth.

Table 3: Panel Fixed-Effects Results — Share of Employed Workers Absent Due to a Labor Dispute (May 1985 - September 2023)

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.: Share Absent due to a Labor Dispute						
Unemployment$_{it}$	-0.0629*	-0.0642**	-0.0661**			
	(0.0325)	(0.0307)	(0.0311)			
Employment-Population Ratio$_{it}$				0.0542***	0.0459**	0.0364*
				(0.0208)	(0.0204)	(0.0210)
N	24,735	24,735	23,409	24,735	24,735	23,409
State Controls	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	N	Y	Y	N
Month FE	N	Y	N	N	Y	N
Month $\times$ Year FE	N	N	Y	N	N	Y

Notes: Standard errors in parenthesis, clustered at the state-level. $p < 0.1$ * $p < 0.05$ ** $p < 0.01$ ***. Observations weighted by average state population. Estimates obtained via Poisson (pseudo-)maximum likelihood estimation using Stata's `ppmlhdfe` command (Correia et al., 2020). Time-varying state-level control variables include (log-) state population, the share of workers in a union, the share of the state population that is white, the share of the state population with at least four years of college, the share of the population over 65 years of age, the share of workers employed in tradables industries, and the rate of population growth.

Table 3 presents results from an alternative specification featuring a dependent variable capturing the share of employed workers in a given month that report being absent in the last week due to a labor dispute, as reported in the Current Population Survey (Flood et al., 2023). In every case, an increase (decrease) in the unemployment rate (employment-population ratio) leads to a statistically significant decrease (increase) in the share of employed workers absent from work due to a labor dispute. Columns (1)-(3) suggest a one percentage-point increase in the unemployment rate decreases the share of employed workers absent due to a labor dispute between 6.3% and 6.6%. Similarly, Columns (4)-(6) suggest a one percentage-point increase in the employment-population ratio increases the share of the labor force absent due to a labor dispute between 3.6% and 5.4%. Taken together, the results in Tables 2 and 3 provide *prima facie* support for Kalecki (1943)'s claim that tight labor markets increase labor action, thus rationalizing capitalist opposition to the maintenance of full employment by government spending. However, despite the fact that the large suite of time-varying state-level controls and state- and time-specific fixed-effects are likely to absorb a significant amount of heterogeneity influencing the relationship between labor market conditions and strike activity, it is unlikely that the estimates presented in Tables 2 and 3 adequately capture all potential sources of bias. Thus, Section 4.2 implements the Oster (2019) adjustment for selection on unobservables to assess the extent to which omitted variable bias may influence the results in Tables 2 and 3. Section 4.3 then presents results from an alternative estimator exploiting the differential timing of national recessions across states via propensity-score based methods.

4.2 Oster (2019) Test for Selection on Unobservables

Oster (2019) derives a bias-correction procedure that can be used to adjust regression coefficients in the presence of selection on unobservables. In particular, Oster (2019) shows that under the assumption of equal selection—i.e., that selection on unobservable characteristics is proportional to selection on observable characteristics—the bias-corrected regression coefficient can be obtained as:

$$\beta_{BiasAdjsuted} = \beta_{long} - (\beta_{short} - \beta_{long})\frac{R^2_{max} - R^2_{long}}{R^2_{long} - R^2_{short}} \tag{3}$$

Where β_{short} is the regression coefficient obtained from a "short" regression of the dependent variable on the independent variable alone, β_{long} is the regression coefficient obtained from a "long" regression containing a full suite of controls, R^2_{short} is the R^2 value from the "short" regression, R^2_{long} is the R^2 value from the "long" regression, and R^2_{max} is the maximum value for R^2. Oster (2019) suggests $R^2_{max} = 1.3 \times R^2_{long}$ as a possible upper bound, in that only 45% of the non-randomized results analyzed by Oster (2019) survive the resulting bias adjustment.

Panel A of Table 4 presents bias-adjusted coefficients for specifications using the unemployment rate as the independent variable. Panel B of Table 4 presents bias-adjusted coefficients for specifications using the employment-population ratio as the independent variable. For the estimates from Table 2, the regression coefficient and R^2 from the "long" regression are obtained from the most saturated specification that remains statistically significant. For the estimates from Table 3, the coefficients and R^2 from the "long" regression is obtained from Columns (3)

Table 4: Oster (2019) Bias Adjustment

Panel A: Unemployment Rate			
Dep. Var.	New Strikes	Total Strikes	Absent (Labor Dispute)
Oster (2019) Coefficient	-0.08	-0.16	-0.15
Original Estimate	Table 2, Panel A, Column (3)	Table 2, Panel B, Column (3)	Table 3, Column (3)
Panel B: Emp-Pop Ratio			
Dep. Var.	New Strikes	Total Strikes	Absent (Labor Dispute)
Oster (2019) Coefficient	0.14	0.15	0.09
Original Estimate	Table 2, Panel C, Column (2)	Table 2, Panel D, Column (4)	Table 3, Column (6)

Notes: Table presents estimates of Oster (2019) bias-adjusted regression coefficients.

and (6), respectively.

In each case, the results in Table 4 suggest the original panel fixed-effects estimates obtained in Table 2 and Table 3 are biased toward zero. The Oster (2019) bias-adjusted estimates are larger in absolute magnitude for every regression specification. In specifications featuring the share of employed workers absent due to a labor dispute, the bias-adjusted coefficient is more than double the original estimate. Taken together, the results in Table 4 indicate that—to the extent that omitted variable bias influences the estimation results—the estimation results are likely to be influenced *away* from finding the effect suggested by Kalecki (1943).

4.3 Sahm (2019)-Rule Estimates

Table 5 presents results from the Sahm (2019)-rule based IPWRA estimation specification highlighted in Equation 2. In each regression specification observations are weighted by the inverse probability of treatment, following the discussion in Section 3. Columns (1) and (2) present results for new work stoppage activity, Columns (3) and (4) present results for total work stoppage activity, and Columns (5) and (6) present results for the share of employed workers absent from work due to a labor dispute. Equation 2 is estimated via Poisson pseudo-maximum likelihood, dropping all separated and singleton observations. Standard errors are clustered at the state-level.

The results in Table 5 suggest that the differential timing of national recessions across states exerts a negative and significant effect on direct action by labor. The results are economi-

Table 5: IPWRA Sahm (2019)-Rule Estimator

	(1) New Stoppages	(2) New Stoppages	(3) Total Stoppages	(4) Total Stoppages	(5) Absent(Labor Dispute)	(6) Absent(Labor Dispute)
Sahm Rule Indicator$_{it}$	-0.740***	-0.414	-0.645***	-0.407**	-0.386**	-0.260*
	(0.266)	(0.252)	(0.219)	(0.161)	(0.154)	(0.135)
N	16,852	12,982	15,820	15,728	22,844	21,724
State Controls	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
Month $\times$ Year FE	N	Y	N	Y	N	Y
Lagged Work Stoppages	N	Y	N	Y	N	Y

Notes: Standard errors in parenthesis, clustered at the state-level. $p < 0.1$ * $p < 0.05$ ** $p < 0.01$ ***. Observations weighted by inverse probability weights. Estimates obtained via Poisson (pseudo-)maximum likelihood estimation using Stata's `ppmlhdfe` command (Correia et al., 2020). Estimates exclude separated and singleton observations. Time-varying state-level control variables include (log-) state population, the share of workers in a union, the share of the state population that is white, the share of the state population with at least four years of college, the share of the population over 65 years of age, the share of workers employed in tradables industries, and the rate of population growth. Columns (2), (4), and (6) each include two lags of work stoppage activity.

cally meaningful and statistically significant in every Column except Column (2), which is marginally insignificant ($p = 0.10$). In particular, the results in Table 5 suggest that entering a recession reduces new work stoppages, total work stoppages, and the share of the labor force that is absent due to a labor dispute. Entering a recession reduces new work stoppages between 41% and 74%, reduces total work stoppages between 41% and 65%, and reduces the fraction of employed workers reporting an absence from work due to a labor dispute between 26% and 39%. In addition to adjusting for possible selection into national recessions via propensity-score based methods, the results are robust to the inclusion of state-, year-, and month-by-year fixed-effects as well as two lags of work stoppage activity. Table 5 provides empirical support for Kalecki (1943)'s claim that a regime of full employment would result in "[s]trikes for wages increases and improvements in conditions of work" (p.326), thereby rationalizing capitalist opposition to full-employment policy.

4.4 Placebo Tests

Table 6 presents results from placebo tests using alternative reasons for absences from work. Table 6 applies the Sahm (2019)-rule based IPWRA estimator to examine the impact of entering a national recession on absences from work due to vacations, medical issues, and weather. Intuitively, if Kalecki (1943) is correct such that the primary mechanism through which labor market slack influences work stoppages is via its effect on the relative bargaining power of workers, then the frequency of absences from work that are (largely) unrelated to worker bargaining power should be unaffected by a state entering a recession.

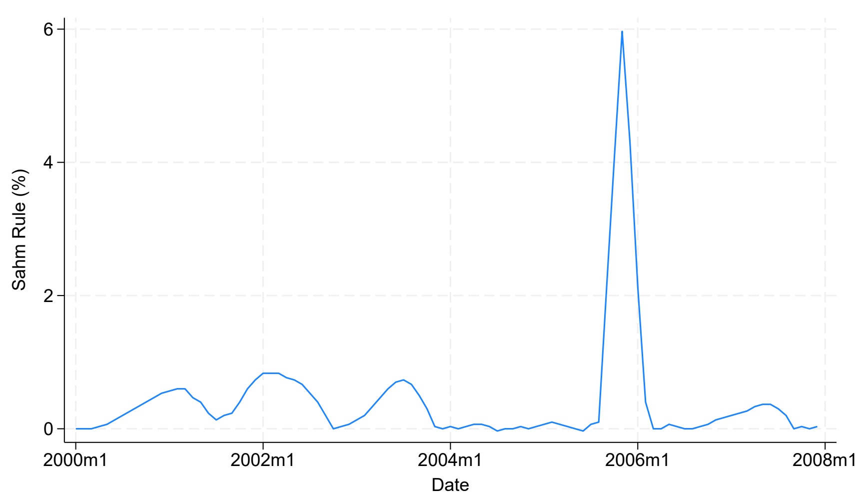
Table 6: Alternative Absence Placebo Tests

	(1)	(2)	(3)	(4)
	Vacation	Medical	Weather	Weather (excl. 2005, 2010)
Sahm Rule Indicator$_{it}$	-0.0207	0.0245	0.275***	0.125
	(0.0220)	(0.0186)	(0.0736)	(0.0803)
N	17,437	17,437	17,437	16,213
State Controls	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Month $\times$ Year FE	Y	Y	Y	Y
Lagged Work Stoppages	Y	Y	Y	Y

Notes: Standard errors in parenthesis, clustered at the state-level. $p < 0.1$ * $p < 0.05$ ** $p < 0.01$ ***. Observations weighted by inverse probability weights. Estimates obtained via Poisson (pseudo-)maximum likelihood estimation using Stata's `ppmlhdfe` command (Correia et al., 2020). Estimates exclude separated and singleton observations. Time-varying state-level control variables include (log-) state population, the share of workers in a union, the share of the state population that is white, the share of the state population with at least four years of college, the share of the population over 65 years of age, the share of workers employed in tradables industries, and the rate of population growth. All specifications include two lags of both the dispute absence share and total work stoppages.

The placebo tests presented in Table 6 support the main estimation results. The results in Columns (1) and (2) suggest that entering a recession based on the Sahm (2019)-rule indicator has no statistically significant effect on the fraction of employed workers reporting an absence from work due to a vacation or a medical reason, and the estimated regression coefficients in these columns are much smaller in magnitude than those found in Table 5. In Column (3), the estimated regression coefficient suggests that entering a recession *increases* the fraction of workers reporting an absence from work due to weather (the opposite sign one might expect to find if one were expecting the placebo test to invalidate the main results). This seemingly puzzling finding can be explained by noting that extreme weather shocks are themselves likely to cause both unemployment and absences from work. Figure 5 illustrates by plotting the Sahm rule (difference between the three month moving average of the unemployment rate and its low over the previous twelve months) for the state of Louisiana over the period including Hurricane Katrina. The state of Louisiana experiences a significant spike in the Sahm (2019)-rule during the aftermath of Hurricane Katrina, such that one might expect to find higher unemployment and (given the destruction caused by the hurricane) higher numbers of weather-induced work absences in this period. Excluding the year 2005 (Hurricane Katrina) and the year 2010 (which featured 21 total tropical cyclones, the most of any year in the 2010's[12]) alone is enough to render the regression coefficient in Column (4) statistically insignificant, and to reduce its magnitude compared to Column (3) by more than half. Thus, the combined results of the placebo tests presented in Table 6 suggest that entering a recession[13] has no impact on the share of employed workers reporting an absence from work for reasons *other* than a labor dispute, offering support for the main findings.

Figure 5: The Sahm (2019)-Rule in Louisiana during Hurricane Katrina



Notes: Figure 5 plots the difference between the three-month moving average of the unemployment rate and the low over the prior twelve months for Louisiana between January 2000 and December 2007. The figure highlights the spike in the Sahm (2019)-rule in Louisiana caused by Hurricane Katrina in 2005.

[12]e.g., see `https://www.nhc.noaa.gov/data/tcr/index.php?season=2010&basin=atl`.

[13]Or, more specifically, entering a recession not primarily driven by an extreme weather event.

5 Conclusion

The results in this paper provide empirical support for Kalecki (1943)'s argument regarding the "political aspects of full employment." In particular, this paper defends Kalecki (1943)'s assertion that the maintenance of full employment by government spending is likely to result in "[s]trikes for wage increases and improvements in conditions of work [which] would create great political tension" (p.326). Using monthly data on state-level work stoppages from the Bureau of Labor Statistics and data on state-level labor market conditions from the Current Population Survey, this paper estimates the effect of state-level labor market conditions on strike activity from 1993 to 2023. Panel fixed-effects estimates suggest that a one percentage point increase in the unemployment rate reduces the number of work stoppages involving 1,000 or more workers (per million) by approximately 14%. Using an alternative measure of strike activity based on the fraction of workers reporting an absence from work due to a labor dispute gives qualitatively similar results. I support the fixed-effects approach with a propensity-score based estimator that exploits the differential timing of national recessions across US states by application of a state-level Sahm (2019)-rule. Results from an inverse probability-weighted regression adjustment (IPWRA) estimator (Wooldridge, 2007; Imbens and Wooldridge, 2009) indicate that entering a recession reduces new work stoppages between 41% and 74%, reduces total work stoppages between 41% and 65%, and reduces the fraction of employed workers reporting an absence from work due to a labor dispute between 26% and 39%. Placebo tests examining the impact of entering a recession on absences from work for reasons *other* than a labor dispute support the main findings.

The negative effect of unemployment on strike activity rationalizes capitalist opposition to the maintenance of full employment. In contrast to claims by defenders of free market capitalism that the problem of economic depression "does not rest upon a conflict of interest" (Knight, 1940, p.192), the evidence presented here supports Kalecki (1943)'s claim that at the heart of full-employment policy lies a conflict of interest between capitalists and workers. Importantly, only by acknowledging this conflict of interest will it be possible to develop the new social and political institutions required to make “full-employment capitalism” (Kalecki, 1943, p.331) a reality. It is precisely in the success or failure of capitalism to adopt the fundamental reforms necessary to adjust itself to full employment that Kalecki (1943) locates the key to survival for the entire capitalist enterprise. According to Kalecki (1943), overcoming opposition to full employment is required if a descent into fascism is to be avoided: “[t]he fight of the progressive forces for full employment is at the same time a way of preventing the recurrence of fascism” (p.331). Only by first acknowledging the “political aspects” of full employment policy can this fight begin in earnest.

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A IPWRA: First-Stage Estimates

Table 7 presents results from the first-stage estimates used to generate the probability weights for the IPWRA regression results presented in Table 5.

Table 7: Probit Model for the Probability of Entering a Recession

	(1)
	Sahm Rule Indicator$_{it}$
Sahm Rule Indicator$_{i,t-1}$	3.406***
	(0.161)
Sahm Rule Indicator$_{i,t-2}$	-0.122
	(0.175)
Unemployment$_{i,t-1}$	0.813***
	(0.116)
Unemployment$_{i,t-2}$	-0.585***
	(0.111)
Emp-Pop$_{i,t-1}$	-0.404***
	(0.0992)
Emp-Pop$_{i,t-2}$	0.450***
	(0.102)
Ln(Population$_{it}$)	-0.123
	(0.283)
Union Membership$_{it}$	0.0121*
	(0.00706)
White Share$_{it}$	-0.00193
	(0.00758)
College Share$_{it}$	0.00367
	(0.00801)
Retired Share$_{it}$	0.0473
	(0.0300)
Tradables Share$_{it}$	0.000636
	(0.0103)
% Δ Population$_{it}$	5.526*
	(2.901)
N	22,899
Year FE	Y
State FE	Y

Notes: Standard errors in parenthesis, clustered at the state-level. $p < 0.1$ * $p < 0.05$ ** $p < 0.01$ ***.