The Politics of Pessimism:
The Effects of State Business Closures

Torben Iversen

Alice Xu

Department of government

Harvard University

Abstract

Highly-educated workers usually support mainstream parties. Low-skilled workers employed outside the leading sectors may do the same when enough opportunities exist for them or their children to improve their future economic standing. These "aspirational voters" can become anti-establishment populist voters, however, when their economic outlook dims. Rightwing populist voters are, we suggest, disappointed aspirational voters. We test this argument on US data where the sudden changes in economic outlook due to the coronavirus and associated lockdowns created a sharp rise in pessimism about the future among some voters. Using a staggered difference-in-difference approach, we causally identify the effect of lockdowns on Trump approval when employment downsizing risks are high, and when it is difficult to move labor online. We find that lockdowns increase support for Trump only among voters at high risk of downsizing. This is how a public health crisis has turned into a moment of heightened partisan polarization.

Word Count: 9,924 (8,828 without tables and appendix)

1 Introduction

A recent debate about the causes of the rise of right-wing populism pits materialist arguments (Autor, Dorn, and Hanson 2013; King and Rueda 2008; Cavaille and Ferwerda 2017) against explanations emphasizing culture or identity (Inglehart and Norris 2016; Mutz 2018; De Vries and Hoffmann 2018). One of the difficulties of adjudicating this debate is that economic and cultural divisions are correlated. In their seminal piece on the socioeconomic cleavages that gave rise to modern party systems, Lipset and Rokkan (1967) implied that the socioeconomic cleavages that gave rise to modern party systems have three elements: a structural division usually rooted in material conflict, social identities that link the need for self-identification with social divisions, and the formation of new political parties that organize around these cleavages. From this perspective, populism is a new cleavage that can manifest in multiple forms.

Nonetheless, we are still left with a puzzle. Although there is a consensus in the literature that the decline of industry and the growing economic divide between the well-educated in prosperous cities and the less educated in "left-behind communities" contributed to the rise of populism (Emmenegger et al. 2012), existing empirical analyses still find that economic variables are generally weak predictors of populist voting (Bornschier and Kriesi 2012; Kurer 2020). In fact, support for right-wing populism is not strong among the poor, nor among "outsiders" on the margin of the labor market (Emmenegger et al. 2012; Häusermann et al. 2015; Kitschelt and Rehm 2019). Gidron and Hall (2017) argue that the resolution is to not look at absolute deprivation but rather relative decline and "status anxiety," what Burgoon et al. (2018), following a mostly sociological literature, call "positional deprivation" (see also Smith and Pettigrew 2015 and Kurer 2020). The transition to a new economy leaves many, notably the male industrial worker, concerned about their relative position in society, triggering a reaction that typically runs counter to feminism, multiculturalism, and related values that are supported by sociocultural elites. This suggests a synthesis that decouples, to some extent, immediate material concerns from voting behavior.

The explanation we propose is consistent with this line of argument, but we point to an overlooked factor that suggests a more direct link between material interests and voting: expectations about the future. It takes the concept of aspirational voters proposed by Iversen and Soskice (2019) as the point departure. In their book, Iversen and Soskice (2019) argue that a distinct feature of advanced capitalist democracies (ACDs) is that they continuously create the foundations for material improvement. ACDs are based on skill-intensive production, and those who acquire the requisite skills have reason to support policies and political parties that cater to the advanced sectors, thereby reinforcing their own future access to quality jobs and prospect of rising incomes. Even those who are not in the advanced sectors may harbor expectations that they can one day benefit from the new economy, or at least that their children might, provided that they can hold onto their current jobs and gradually acquire new skills and competencies. It is only when these aspirational voters see opportunities for themselves and their children shut down that they turn to populist parties and politicians. These voters are not inherently poor nor are they political outsiders, but rather they fear losing out, as argued by Häusermann et al. (2015). Populist voters, we contend, are disappointed aspirational voters.

There is no single threshold for diminished aspirations above which populist candidates become more attractive than mainstream ones, but there are conditions that make such shifts in support to populism more likely. Secular industrial decline – deindustrialization – is a particularly negative omen for those whose skills are tied to the industrial economy. Yet, it is difficult to isolate the political effects of such decline because so many other variables tend to change at the same time, and because many will successfully transition into new jobs or retirement. Likewise, the financial crisis was a major shock that undermined the hopes of millions of workers, but the crisis evolved over the course of several years. While parties from the radical right might have benefitted (Hernández and Kriesi 2016), it is difficult to attribute a causal effect to the crisis on political preferences. Moreover, insofar as political attitudes change in response to the Great Recession, they tend to be short-lived (Margalit 2013).

In a recent paper, Häusermann et al. (2019) argue that instead of looking at objective economic measures, we should focus on subjective expectations. Using survey data for eight West European countries, they divide respondents according to whether they have positive or negative evaluations about the future as well as according to their socioeconomic status (SES) using a dichotomous measure. They find that survey respondents with a combination of both i) low SES and ii) pessimistic views of either their own or their children's economic future – our disappointed aspirational voters – are the ones who have notably stronger support for radical right parties. Their analysis relies on estimates of the correlation between different characteristics (i.e., survey questions) of each respondent within the same survey. As such, the study only provides suggestive evidence of future aspirations as a causal mechanism. It remains unclear whether pessimism drives populist voting or support for populism cultivates pessimism.

To give a concrete example, parties of the radical right may convince their supporters that their future economic prospects are limited since this is a narrative that justifies their own radical, anti-establishment position (e.g., Trump's dystopian image of reality). If radical right-wing parties are successful in advancing this narrative, it is not the case that pessimism about the future is what drives populist voting, but rather support for populism that drives pessimism.

In this paper, we adopt a different approach. We leverage a quasi-experimental design using the state-level policies for closing businesses in the U.S. in response to the public health crisis brought about by the SARS-CoV-2 coronavirus to estimate the differential effects of a decrease in perceived future economic prospects. The analysis compares the effects of this increase in economic pessimism for workers in areas with industries at low compared to high risk of downsizing. The U.S. provides an ideal setting for studying these effects, given the combination of the pandemic, a populist president with a clear anti-lockdown stance, the quasi-autonomy of states to decide how to respond to the virus, and the impending presidential election, which has produced a continuous flow of Trump approval data. Yet, we see the argument as generally applicable, and we will suggest factors that are likely to affect the comparative incidence of right populist responses to the crisis.

Specifically, we use a staggered difference-in-differences event study design to estimate the differential effect of state-level business closure mandates – conceptually, the causal effect of a perceived reduction in future economic prospects of workers – on support for a populist president. Our key hypothesis is that businesses closures cause workers who live in states with a high share of jobs at risk of downsizing to support Trump as a means to put pressure on states to reopen – or at least as a form of protest against closures. Essentially, the opening and closing of businesses is a policy switch that changes expectations about the future according to voters' risk of permanent downsizing.

2 The Micro-Logic

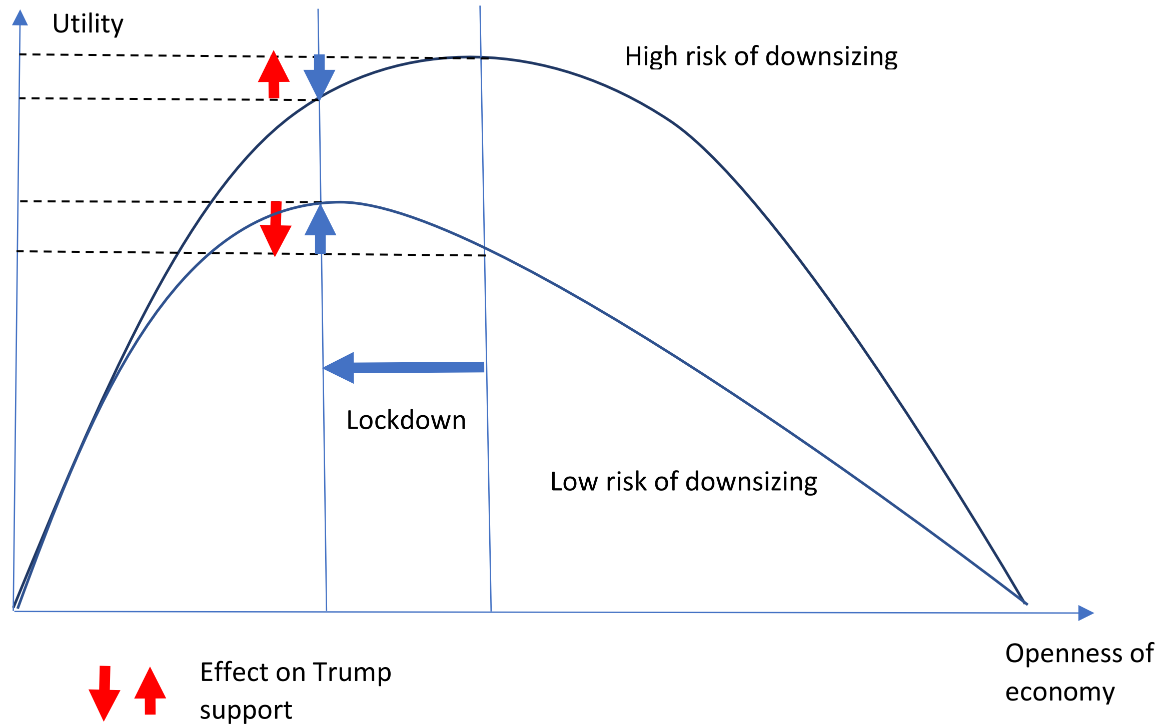
When there is a steep (short-term) tradeoff between work and health, lockdowns will be costly in terms of loss of work and income. Yet the costs vary according to the probability that layoffs become permanent – what we term the *risk of downsizing*. When the risk of downsizing is high, lockdowns undermine prospects for re-employment in the future, and aspirations for a better life consequently plummet. We argue that this can tip support in the direction of populist politicians, assuming that workers in industries facing high risks of downsizing prioritize work and income over health *compared to* workers who are in industries with low risk of downsizing.

Specifically, we assume that Trump prioritizes jobs over health and, therefore, puts pressure on states to keep the economy open. Trump is (or was), of course, inconsistent in his policies, but compared to Biden, he has clearly placed greater relative weight on the economy, and he has advocated for re-openings when states closed down ("liberate the states"). The majority of the American public has an interest in reducing health risks, and many, perhaps a majority, will support lockdowns for that reason. The net effect on Trump vote is, therefore, ambiguous, but the direct effect of forced business closures on the likelihood of Trump support among constituencies facing high risk of downsizing in employment should be unambiguously positive.

With this assumption in mind, Figure 1 illustrates the logic. It shows the utility of economic openness for different groups of voters, divided according to the level of downsizing risk they face: low (bottom curve) and high (top curve). Voters with low risk prefer a lower level of openness during the crisis than those facing high risk because health concerns weigh relatively more in their utility function. As a result, they are more supportive of forced closures of businesses as a policy instrument to contain the virus, expecting to recover their economic position over time even if they temporarily lose their employment. These "low risk" voters are, thus, likely to respond to lockdowns by reducing their support for Trump, who, again, is expected to counteract such policies. Voters at high risk of downsizing, by contrast, are looking for a president who leans against lockdowns. Given this is Trump's perceived stance, these "high risk" voters, therefore, increase their likelihood of voting for Trump. These voters likely would have remained optimistic about their future economic well-being without the shutdown of the economy. Instead, the forced business closures compel them to update their expectations.

For this effect to hold, it must be the case that lockdowns raise the salience of keeping jobs relative to concerns for health, while highlighting the difference in policy stance between Donald Trump and (implicitly) Joe Biden on jobs versus health. The effect likely also depends on the severity of the health crisis, where a steeper jobs-deaths tradeoff shifts the preferred openness downward for everyone. To account for this, the empirical analysis controls for different measures of the local impact of COVID19.

Figure 1. The differential effects of lockdowns on Trump vote



Note: The lines show the utility of people in different "sectors" of the economy. Preferences over work and health risks vary by occupation, education, etc. Lockdowns improve the welfare of some and reduce the welfare of others. Trump wants to keep the economy open and lockdowns will increase support among people whose welfare is hurt by lockdowns. For others, lockdowns will reduce support for Trump.

The empirical hypotheses to be tested are indicated by the red arrows. When downsizing risks are high, business closures mandates causally raise Trump support. Alternatively, when downsizing risks are low, mandates causally lower Trump support. In other words:

H1: $$\frac{\partial \text{Trump approval}}{\partial \text{Business closure}} > 0 \quad \text{if downsizing risk high}$$

H2: $$\frac{\partial \text{Trump approval}}{\partial \text{Business closure}} < 0 \quad \text{if downsizing risk low}$$

3 Data Sources

We conduct two separate analyses, constructing a separate data set for each analysis. The first seeks to causally estimate the effects of diminishing aspirations on support for Trump throughout the complete period of the COVID19 outbreak in the U.S. The second analysis explores the key mechanism that drives this estimated effect, using fine-grained proprietary data from the Gallup U.S. Daily Tracker ($N \approx 165{,}960$) on voter perceptions of future economic expectations. We also explore how such perceptions predict the vote for Trump in the 2016 presidential elections.

To causally test the effects of future economic aspirations on support for populism, we conduct an analysis of government policies in response to the SARS-CoV-2 coronavirus outbreak, constructing a panel dataset at the state-level. We use a collection of state-level public opinion polls compiled by FiveThirtyEight from a variety of local academic and news sources (e.g., Public Policy Institute of California, Roanoke College in Virginia, Marquette University Law School in Wisconsin). Each observation in our panel is a separate poll that is collected on a specific day between January 1st, 2020 and August 12th, 2020 in a particular state. Although the number of polls and dates measured for each state varies across states, creating an unbalanced panel, we are able measure public opinion attitudes towards Donald Trump on a continuous basis across states during the pandemic.

Next, we use the COVID-19 U.S. State Policy Database available from Raifman et al. (2020). Coded by a team at the Boston University School of Public Health, the data tracks the timing of a variety of state-level policies (e.g., business closures, mask mandates, etc.).[1] To measure voters' employment in industries at risk of downsizing, we use data on the share of the labor force employed in the manufacturing sector available from the Bureau of Labor Statistics (BLS). To be clear, the actual percent share of employment in manufacturing is never a majority share of total employment within any state. However, we expect that as long as states have a non-negligible share of employment in the manufacturing sector, the economic loss to voters in the sector also have considerable negative implications for the broader economy within the state.

We use manufacturing employment as a measure of downsizing risks for several reasons. First, manufacturing has been in secular decline for decades, and there is a widespread expectation that lost jobs will not come back. The Great Recession, for example, resulted in foreign, especially Chinese, competitors capturing market shares, turning layoffs into permanent job losses (Acemoglu et al. 2014; 2016). While the economy recovered after the crisis, employment in manufacturing did not. Second, many manufacturing jobs are held by an older population with high school or vocational (community college) school degrees who find it difficult to transition to other employment in part because most manufacturing jobs require skill sets that are specific to the industry or employer. In other words, re-employment is difficult, given the transferability of their skills is low. Third, while manufacturing is a modest share of total employment, it typically has many forward and backward linkages and is often a major source of income for particular localities. When manufacturing jobs disappear whole communities suffer, often for a long time.

[1] The database is available here: https://github.com/KristenNocka/COVID-19-US-State-Policy-Database.

As an alternative measure of downsizing risk, we employ Dingelman and Neiman's (2020) index of "teleworkability." Using a variety of survey data measures on the "physical and social factors that influence the nature of work," Dingelman and Neiman (2020) create a classification of feasibility of working from home for all occupations across U.S. counties. Merging the classification with detailed data on occupational employment counts available from the Bureau of Labor Statistics (BLS), they calculate a share of "teleworkable" employment by county. We use a state-level version of the measure and invert the index, such that it captures the share of "non-teleworkable" employment.[2] Employment in the manufacturing sector has very low teleworkability because production is physical in nature. However, the share of non-teleworkable employment likely captures various alternative industries in addition to those in manufacturing, and overall, such employment makes intensive use of onsite physical skills. The theorized effect of a business closures mandate on voters engaged in non-teleworkable employment is, thus, comparable to those facing high employment risk: perceived future economic loss.

To measure voters' aspirations, we use detailed individual-level polling data from the Gallup U.S. polls. Inclusive of over 150,000 observations per annum, the Gallup U.S. Daily Tracker data is the only dataset of its kind, allowing for representative sampling at the U.S. county level.[3] We use the 2015 Gallup wave, and pair it with data on county-level vote shares

[2] The "teleworkability" measure is available here: https://github.com/jdingel/DingelNeiman-workathome. The state-level measure was constructed by Ole Agarsnap: https://github.com/jdingel/DingelNeiman-workathome/blob/master/state_measures/output/state_workfromhome.csv

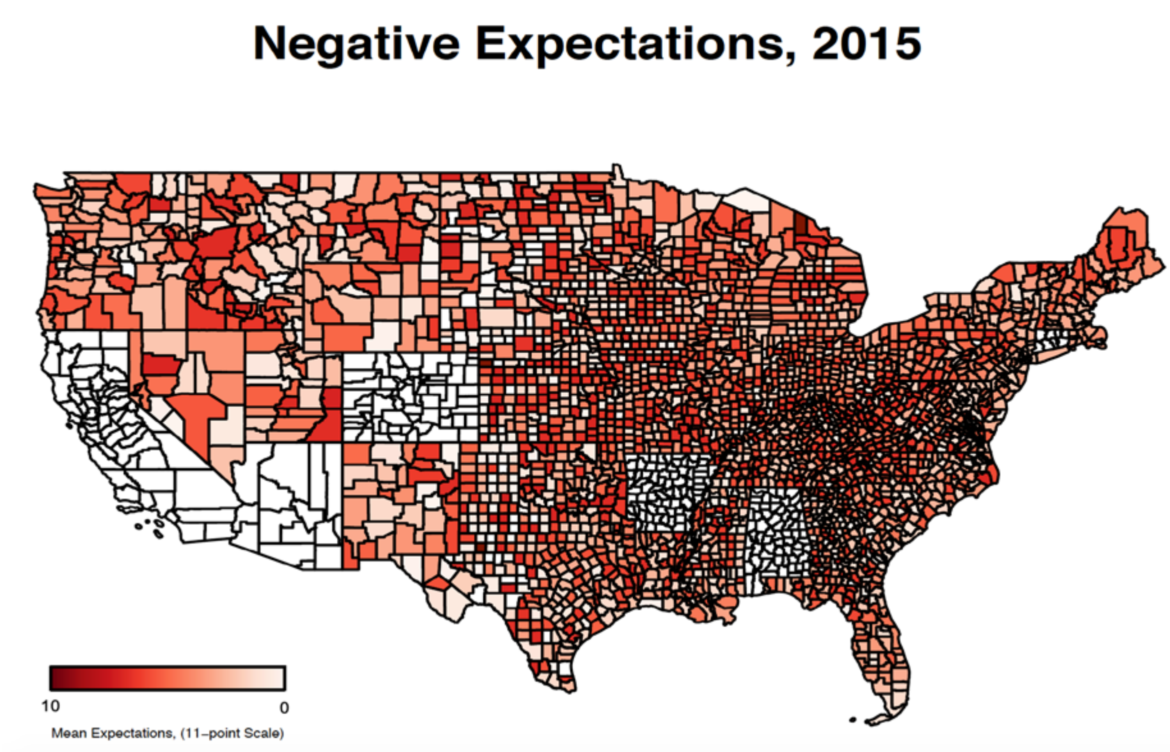
[3] This dataset is proprietary, requiring a subscription to Gallup Advanced Analytics for access.

by candidate in the 2016 presidential elections available from the MIT Election Lab.[4] The Gallup U.S. Daily Tracker survey includes a question that asks,

> *"Please imagine a ladder, with steps numbered from 0 at the bottom to 10 at the top. The top of the ladder represents the best possible life for you and the bottom of the ladder represents the worst possible life for you. Just your best guess, on which step do you think you will stand in the future, say about five years from now?"*

We use this "future ladder" question to capture aspirations for future economic betterment. The measure (i.e., averaged across respondents by county) is mapped at the county-level in Figure 2 below.

Figure 2. The geographical distribution of negative expectations about the future



[4] The MIT Elections Lab data can be accessed here: https://electionlab.mit.edu/data.

4 Causally Testing the Effect of the Business Closures

To causality test Hypotheses 1 and 2, we leverage quasi-experimental variation from the COVID19 outbreak. Policymakers around the world have placed unprecedented restrictions on its citizens in response to the novel SARS-CoV-2 coronavirus. We focus on one type of policy in particular: government mandates to close down businesses. Although these policies are effective in limiting disease contagion, they come at a high economic cost to local businesses and also directly affect future business expectations (Bartik, Bertrand, Cullen, Glaeser et al. 2020). We conceptualize government business closure mandates as a sharp plunge in perceived future prospects for economic betterment among those employed in industries at high risk of downsizing. This corresponds to the left "lockdown" shift in Figure 1, which drives down welfare for high-risk workers. We estimate the causal effect of this perceived drop in opportunities (i.e., in utility in Figure 1) on support for Donald Trump, using a modified difference-in-differences design.

Specifically, as with alternative studies that examine the effect of COVID19 policies (see Goodman-Bacon 2018, Abraham and Sun 2018, Brzezinski 2020b, Grossman et al. 2020, Wright et al. 2020), we use an event study design to estimate the differentially timed policies at the state-level. Compared to a standard "staggered" difference-in-differences estimation, which averages over heterogeneous treatment effects, this approach uses the differential timing of business closures policies across states to construct a control group comprised of states that have yet to experience the policy at each point in time. In other words, the "treated" group is always compared to a similar "untreated" control group. The estimating equation is:

$$Trump\ Support_{s,t} = \gamma + \alpha_s + \delta_t + \sum_{k=-14}^{14} \beta_k D_{s,t_0+k} + \psi z_{s,t} + \Omega x_{s,t} + \epsilon_{s,t}$$

where *Trump Support*$_{s,t}$ is the percentage of voters in each public opinion poll conducted in state *s* who support Donald Trump in week t. α_s and δ_t are state and week fixed-effects, respectively. The regressor, $D_{s,\, t0+k}$, is an indicator variable centered around the business closure mandate policy for each state *s* at time t_0, such that $D_{s,\, t0+k}$ equals 1 at time *t* if the state enacted the business closures policy *k* weeks ago. Our coefficient of interest is, therefore, β_k, which measures the net effect of closures on Trump support at a particular time.

Specifically, each coefficient compares the change in Trump approval in states with shutdowns from the pre- to the post-policy period to the change in Trump approval during the same period in states without shutdowns. We construct this indicator measure for each of the 14 weeks preceding as well as the 14 weeks following the week in which the business closures policy was first implemented.[5] The coefficients, β_{-13} to β_{-2} thus capture the pre-treatment period, hence they serve as placebo checks for the parallel trends identification assumption.[6]

---

[5] We chose an event window of 14 weeks (i.e., 3.5 months) before and after business closure mandates, capturing the period from January 2020 to July 2020. The statistical power of the t-tests increases with duration of event window. However, short event windows only capture the initial response to the policy are problematic if the response varies with time or if there is a lag period between the policy and voters' registered perceptions of its effect. Our main results are, however, robust to the choice of event window (e.g., 6, 8 or 12 weeks). Note also that the choice of window does not truncate the data because no closure happens less than 14 weeks prior to the last observation.

[6] An additional concern with having a staggered treatment in difference-in-differences estimation is that the event window for each staggered policy may not be consistent across the policies, since more recent mandates for closures may have fewer post-treatment weeks with data. We clarify that this is not of concern in our analysis, since all business closures policies occurred between March 19th and April 4th and we include data even for this more recent period of July and August 2020.

We also include a variable, $z_{s,t}$, which captures fixed-effects for *k* weeks since the first case of COVID19 in that state. In other words, $z_{s,t}$ equals 1 in period *t* if it has been *k* weeks since the first reported COVID19 case in that state and 0 otherwise, where $k \geq 0$. The inclusion of $z_{s,t}$, accounts for any time heterogeneity in the development of COVID19 across states.[7] Last, we add a vector, $x_{s,t}$, of state-level controls, such as cumulative COVID19 cases and alternative state-wide policies (i.e., state of emergency announcements, school closures, shelter-in-place policies, and mandates for wearing masks). We calculate heteroskedasticity-robust standard errors using two-way clustering by state and week.

Finally, we consider a second-order difference between states with high industrial employment – our measure of high downsizing risk – and states with a low industrial employment. As discussed, our proxy for risk of downsizing is the percent share of employment in the manufacturing sector by state. Following the suggestion of Goodman-Bacon (2018), we estimate the effects in threshold-separated tests via a split sample approach. Specifically, we compare the estimated effects for Equation (3) above for states above and below the median level of employment in manufacturing, assuming that manufacturing jobs are subjected to greater risk of downsizing. The split-sample approach allows for heterogeneous non-linear effects that are difficult to capture using interactions.

Note that because we do not have an individual-level measure of risk, the effect of, say, high risk is always an *average effect* across low- and high-risk workers in high-risk states. The share of high-risk workers will be higher in high-risk states, but it far from being 100 percent. Hence the effect of downsizing risk is reduced by the proportion of low-risk workers in that

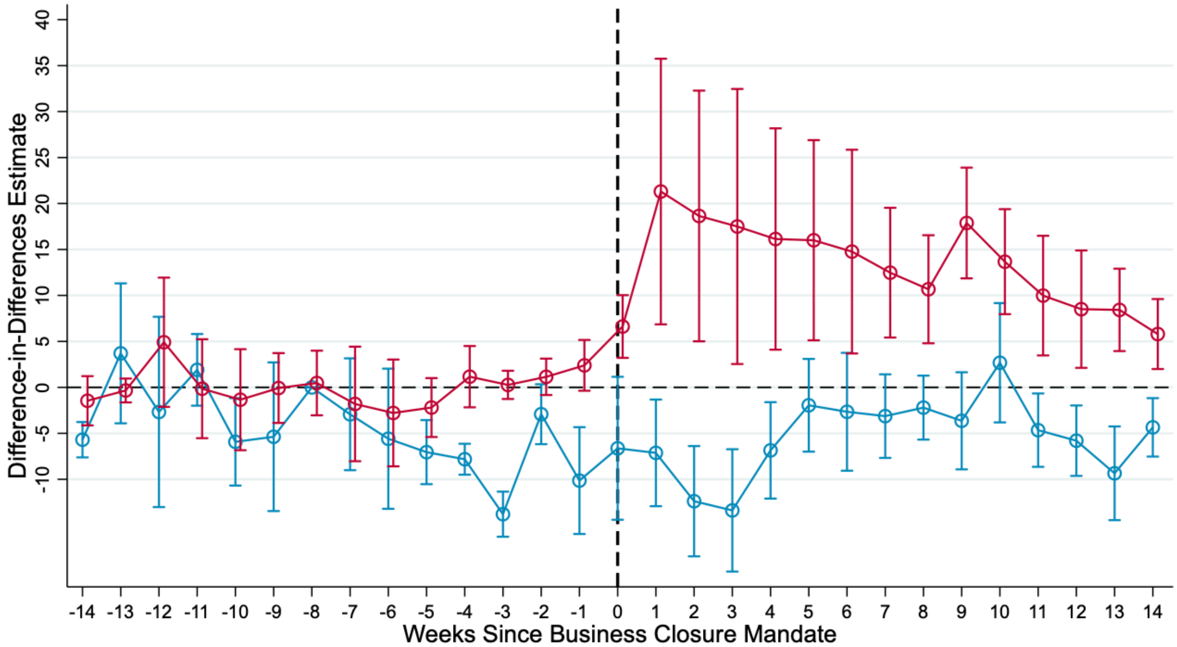
[7] Our results remain robust to excluding this variable.

"high risk" state. We do know, however, that since there are more high-risk workers in high-risk states, the difference in the effect of lockdowns will be positive. Still, the estimated effect will be downward biased because it is an averaged effect at the state-level.

A concern may be that the difference between states with low and high manufacturing employment is not about differences in downsizing risk, but some unobserved difference in another relevant variable. The most obvious alternative is different levels of preexisting partisanship. In areas with high Trump support, a particular interpretation of closedowns may come to dominate the public discourse and shape people's responses to actual closedowns. Yet, that is not the case. If we use vote in 2016 to divide states into high and low Trump support areas, there is no discontinuity around the lockdown at all (result are shown in Appendix A).

In Equation (3), the coefficients, β_{-14} to $\beta_{-1,}$ capture the pre-treatment period, hence serve as placebo checks. To be clear, although we are interested in comparing the estimates for states in the high relative to low downsizing risk group, the difference-in-differences estimates are always calculated relative to the true control group: states that have yet to experience a business closure mandate at that specific point in time. Our theory expects that the coefficients, β_{1} to β_{14}, capturing those for the post-treatment period will be positive and significant for the group of states classified as "high risk," while they will be negative for those in the "low risk" group – corresponding to H1 and H2. The difference-in-differences estimates are plotted in Figure 3 below.

Figure 3. Effect of Business Closure Mandates on Support for Trump



u

Notes: The figure plots the coefficient estimates with 95% confidence intervals for each week before and after the policy implemented at Week 0. The business closure policy mandates causally increases support for Donald Trump in states with a high share of employment in manufacturing (i.e., high risk of downsizing) (magenta), yet it temporarily decreases support for Trump in "low risk" states (blue). The two models control for cumulative COVID19 cases and other state-wide policies (e.g., mask mandates) and include state and week fixed-effects as well as fixed-effects for weeks since the first COVID19 case reported in the state. Robust standard errors are clustered by both state and week.

As the results illustrate, the business closures mandates have a statistically significant and large positive effect on support for Trump in states with a high share of voters employed in industries with a high risk of downsizing. In focusing on the effect of a policy mandate regarding business closures, we seek to isolate the perceived economic loss associated with the COVID19 outbreak. Conceptually, a business closure mandate directly captures a perceived plunge in future aspirations for economic betterment, potentially *even* among respondents who were not directly affected by the policy.

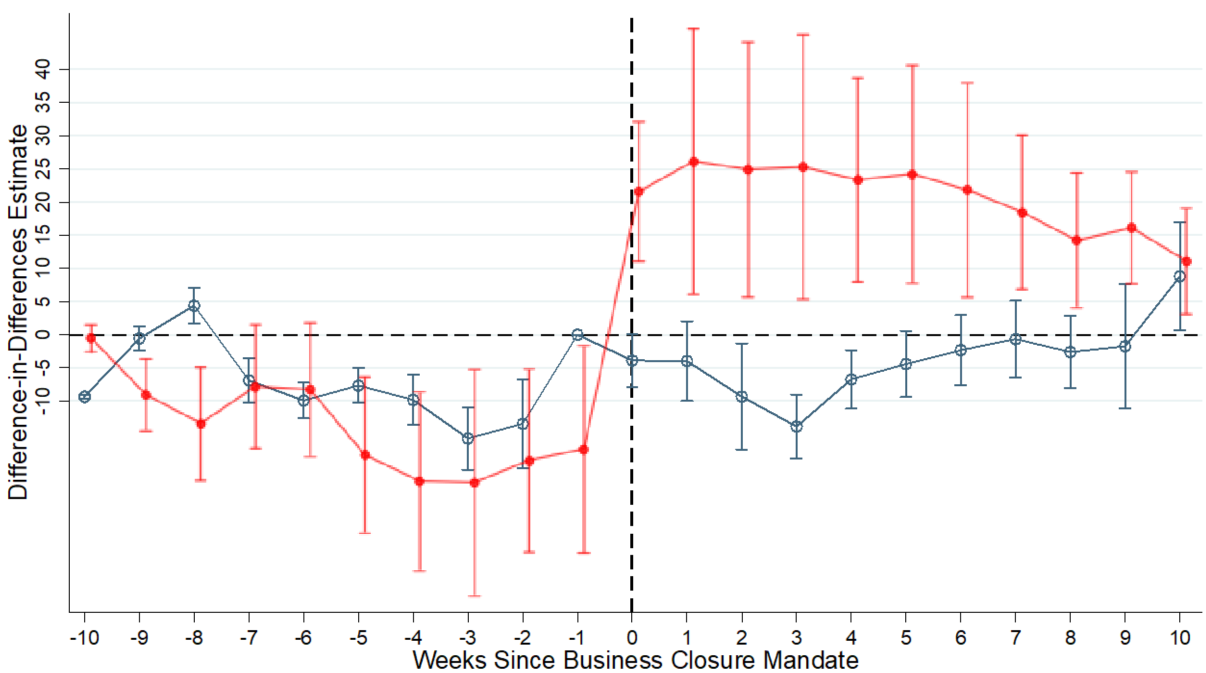
In contrast, the causal effect of the business closures mandate is negative for individuals in states with industries at low risk of downsizing. Many voters in these states see business closures as an insurance against getting ill, well worth the price in lost income. They therefore tend to express disapproval of Trump, who opposes all types of lockdowns. We measure business closures at the state level, and there is some evidence of pre-treatment effects before the policy. Still, the results make clear that while voters in both groups of states began with comparable levels of support for Trump, starting in Week -13, the business closure policy orders polarizes political preferences between states with high compared to low risk of downsizing.

The negative effect of the policy for low risk states is more transitory, lasting through the fourth week after the mandate. Conversely, the positive effect in high risk states is strong and persists throughout the complete period of analysis (i.e., beyond 14 weeks). As noted, the key is the *difference* between low- and high-risk states since all states have workers with low and high exposure to downsizing. What we are estimating is thus an average effect, and that effect shows a new boost in Trump support of about 25 percent in the first four weeks after a business closure mandate, comparing states with high downsizing risks to those with low.

It is noteworthy that the average effect across all states is negative, although it is not robustly significant. If Trump were simply concerned with maximizing approval, he would therefore reasonably support closures. However, it is widely accepted that voters in his base, many of whom live in high-risk states, count more than other voters in his political calculus, as is true for the Republican Party as a whole (Kitschelt and Rehm 2019).

We repeated the analysis using Dingelman and Neiman's (2020) measure of non-teleworkability in place of employment in the manufacturing. When business cannot easily be shifted online, business closures pose a greater threat to workers' future earnings and employment prospects. This is strongly confirmed by the evidence (Figure 4).[8] Again, Trump support in the 2016 election makes no difference for the effect of state closures. The difference between low-risk and high-risk, or low and high teleworkability, states is therefore not a spurious result of partisanship (see Appendix A).

Figure 4. Effect of business closure mandates on Trump support, depending on teleworkability



Notes: The figure plots the coefficient estimates with 95% confidence intervals for each week before and after the policy implemented at Week 0. The business closure policy mandates causally increases support for Donald Trump in states with a high share of employment in non-teleworkable industries (red), yet it has no effect on

[8] As with the measure of employment risk, we estimate the effects in threshold-separated, characterizing the median share of non-teleworkability in our data sample as the threshold above which non-teleworkability is "high."

support in states with low shares of non-teleworkable employment (blue). The two models control for cumulative COVID19 cases and other state-wide policies (e.g., mask mandates) and include state and week fixed-effects as well as fixed-effects for weeks since the first COVID19 case reported in the state. Robust standard errors are clustered by both state and week.

Before the mandated business closures, Trump support was steady or slightly declining across all states, but starting with the onset of the business closures mandate there is a sharp divergence with voters in the high non-teleworkability group increasing their support for Trump. The treatment-induced change is substantial, illustrating how the business closures mandate polarizes the electorate.

4.1. Tests of Model Assumptions

We performed a number of model tests to bolster the key causal argument:

First, the main identification assumption behind a difference-in-differences design is that of common trends: the timing of state business closure mandates is not correlated with changes in support for Trump for reasons other than the as-if random timing. An event study design directly addresses the common trends assumption, with the coefficients β_{-13} to β_{-2} capturing the pre-treatment period. As shown in the results in Figures 4, the effect of business closure mandates is statistically indistinguishable from zero for the "high risk" group prior to the onset of the mandates in Week 0 (i.e., blue dashed line). For states in the "low risk", there is evidence of a statistically significant and negative effect in certain weeks (i.e., -5 through -3) even in the pre-treatment period. We expect that these pre-treatment effects, to some extent, capture respondent reactions to Donald Trump's response to the COVID19 outbreak across the country. Our main focus is, however, on the effects for the “high risk” group (i.e., estimates in red), which definitively passes the common trends test.

To further test the validity of our design, we follow Hsiang and Jina (2014) and Brzezinski et al. (2020) in conducting a series of randomization inference tests. We randomly reassign the actual dates of policy adoption in certain states to others. The results are shown in Appendix A, and they all pass.

Specifically, comparing the estimated effects of the benchmark specification with the correct corresponding policy adoption dates with the estimates from the specification that uses randomly assigned dates, we expect the latter should not have statistically significant and comparable estimates to the former. The results from 50 of such randomization inference tests for the “high risk” group are plotted in Figure 5 in Appendix A. The results show that randomly assigned business closure policy dates have no effect on support for Donald Trump among the “high risk” group in any of the plotted tests. We run 1,000 iterations of this inference test, and the reassigned policy dates largely result in no effect. As discussed in Brzezinski et al. (2020), the results from this randomization inference test illustrate that two-way clustering by state and week yields highly consistent inferences about statistical precision.

Second, we used a test for balance of the baseline characteristics between the high and low risk group to discern potential alternative covariates that could be driving our main results. In particular, we run balance tests on a series of demographic, economic, and political characteristics, and on measure of the intensity of the COVID-19 pandemic. The results from the tests are presented in Appendix B. As in the case of partisanship, reported above, we do not find any differential effects between states in any of the covariates: population, GDP per capita, unemployment rate, confirmed COVID-19 cases, Hispanics, Asians, or mean age. Indeed, these tests largely reflect the average, slightly negative effect across all sub-samples.

Finally, we tried to include two binning indicators that equal 1 for all t at which t < t0 – 14 and t > t0 + 14. We do this as a robustness check, because Borusyak and Jaravel (2017) demonstrate that simply trimming the sample to exclude far-out periods would render the dynamic treatment effects to be a weighted combination of each other, with certain negative weights. Instead, absorbing any periods under and beyond 14 years from the policy treatment mitigates this under-identification problem. In addition, for fully dynamic event study designs with two-way fixed effects. Borusyak and Jaravel (2017) also recommend omitting the two most disparate placebo checks to use as reference periods. In our case, we leave out the placebo checks for the weeks k = -14 and k = -1. All our results are robust to excluding these two periods as reference periods (indeed, to reducing the period range all the way down to 6).

5 The Mechanism: Changed Expectations About the Future

If the differential effect of business closings is due to changes in expectations about future economic prospects, as we argue, we should observe that such expectations are directly related to voting behavior. We begin this analysis by first examining the effect of future aspirations for economic betterment in 2015 on the vote share for Donald Trump in the 2016 presidential election at the county-level. As discussed in Section 3, we estimate the effects controlling for a series of respondent-level demographic controls as well as state fixed-effects. All standard errors are clustered at the county-level. The results are provided in Table 1 below.

Table 1. Expectations about the economic future and Trump vote

	2016 Vote for Trump			
	Model 1	Model 2	Model 3	Model 4
Negative Expectations	0.061***	0.047***	0.009***	0.010***
	(0.004)	(0.003)	(0.003)	(0.003)
Age (Years)			0.000	0.000**
			(0.000)	(0.000)
Gender			-0.009***	-0.008***
			(-0.001)	(-0.001)
Race Indicators			✓	✓
Occupation Indicators			✓	✓
Education Indicators			✓	✓
Marital Status Indicators			✓	✓
State Fixed Effects	No	✓	No	✓
Constant	0.463***	0.446***	0.478***	0.440***
	(-0.009)	(-0.026)	(-0.009)	(-0.026)
Observations	165,960	165,960	160,591	160,591
County-Level Clusters	3,056	3,056	3,054	3,054

Note: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. This table presents the results for the estimated correlation between negative expectations regarding future economic standing measured in 2015 and the vote for Trump in 2016, using fixed-effects estimation and cluster-robust standard errors at the county-level. Negative expectations have a strong and positive association with support for Trump during the 2016 presidential elections. The measure of expectations is reversed, such that the higher numbers reflect lower expectations, and the variable is scaled to vary between 0 and 1.

The main result from this analysis is that lowered expectations about future well-being in 2015 is positively associated with the vote for Trump in 2016, and this result is significant at the $p < 0.01$ level. In particular, each unit increase in average perceived negative future expectations, averaged at the county-level is associated with around a 6.1% increase in share of support for Trump in that county. A standard deviation drop in aspirations is associated with a 7.96 percent increase in Trump voting, a substantively large effect. Respondents who placed themselves on the lower rungs of the perceived future ladder question are more likely to vote for Trump, even after controlling for income, education, and other covariates. Together, this set of results supports our theoretical claim that voters' subjective aspirations regarding the future are a key mechanism that predicts support for populism. It is also clear confirmatory evidence for Hausermann et al.'s (2019) findings.

Our argument also implies that voters employed in industries at high risk of downsizing are especially likely to harbor negative expectations for economic betterment in the future. To confirm this proposition, we estimate the correlation between county-level shares of employment in manufacturing available from the Bureau of Economic Analysis (BEA), analogous to the measure we used in the split-sample analysis at the state level, and individual-level responses for the Gallup negative expectations survey question. We include the same set of respondent-level controls as well as state fixed-effects. The results, presented in Table 2 below, confirm that negative expectations, indeed, have a strong positive correlation with share of employment in manufacturing.

Table 2: Expectations and manufacturing shares

	County-level Manufacturing Share	
	Model 1	Model 2
Negative Expectations	0.031**	0.031***
	(0.013)	(0.011)
Age (Years)	0.000**	0.000**
	(0.000)	(0.000)
Gender	-0.003***	-0.002***
	(0.000)	(0.000)
Income	-0.002***	-0.001***
	(0.000)	(0.000)
Race Indicators	✓	✓
Occupation Indicators	✓	✓
Education Indicators	✓	✓
Marital Status Indicators ✓	✓	
State Fixed-Effects	No	✓
Constant	0.085***	0.068***
	(0.003)	(0.008)
Observations	71,452	71,452
County-Level Clusters	2,400	2,400

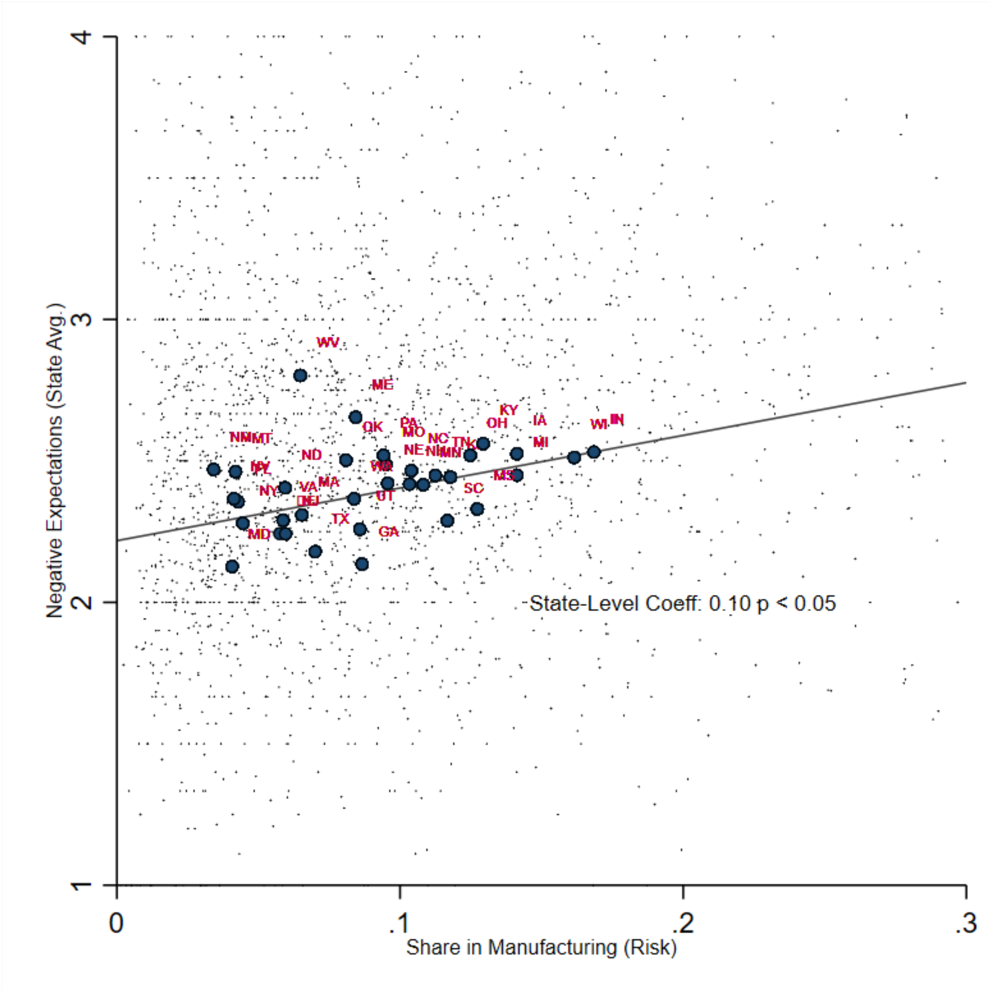
Note: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$ This table presents the results for the estimated correlation between negative expectations regarding future economic standing and the county-level share of employment in manufacturing, using fixed-effects estimation and cluster-robust standard errors at the county-level. Negative expectations have a strong and positive association with employment in manufacturing. The measure of expectations is reversed, such that the higher numbers reflect lower expectations, and the variable is scaled to vary between 0 and 1.

To be clear, our main event study analysis uses state-level manufacturing shares from the BLS, whereas the results presented in Table 2 above use county-level measures of employment in manufacturing from BEA.[9] Figure 5 below confirms that the correlation at the state-level using the BLS data from our main analysis is also strong.[10] The figure plots the correlation for both the county- and state-levels.

[9] To match the Gallup negative expectations measure, which is available at the county-level, we use county-level manufacturing shares available from the Bureau of Economic Analysis (BEA) here: https://apps.bea.gov/itable/iTable.cfm?ReqID=70&step=1

[10] We collapse individual-level survey responses to procure values for mean negative expectations at the state-level.

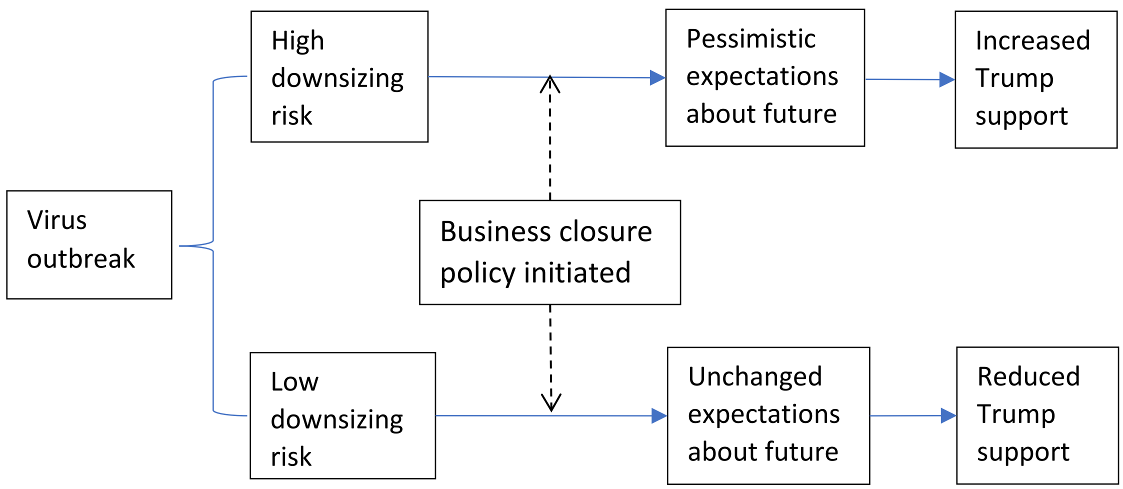
Figure 5. The relationship between manufacturing employment and expectations



Notes: Negative expectations for future economic betterment has a strong and positive association with the employment share in industries at high risk of downsizing (i.e., manufacturing) at both the state- and county-levels. The larger points (circles) are state-level observations, while the small points reflect county-level observations. The coefficient, 0.10, is significant at the p < 0.05 level with 95% CI [0.016, 0.184].

The implied causal model is summarized in Figure 6. We have causally identified the effect of closures on Trump approval in low- and high-risk states. We then used observational data to show that those in high-risk states have on average lower expectations about their future economic welfare *and* are more likely to vote Trump, as implied be the theoretical argument. If state closures did indeed change people's expectations about the future, this is the likely mechanism for the increased support for Trump in states with high downsizing risks.

Figure 6. Sketch of the causal argument



Notes: The virus outbreak and business closure policies affect workers differently. Those at high risk of downsizing will have diminished expectations about the future and vote Trump because he is expected to pressure states to reopen. Those at low risk of downsizing will not change their expectations about the future and vote against Trump because they want closures to remain in effect (until the virus is under control).

6 Conclusion

The coronavirus negatively affects employment everywhere, but the economic and political effects have been polarizing. Professionals in high-end services and other knowledge-intensive production have been able to mostly weather the storm by telecommuting while keeping their jobs or by having strong expectations that they will return to full on-site employment. These workers have supported business closures and general lockdowns to protect their health. For workers in industries with high downsizing risks – notably, manufacturing -- such policies have been devastating. They not only throw them out of jobs, but also dim their hope that the jobs will come back. Most jobs in manufacturing cannot be done from home, and layoffs tend to be permanent.

Although mandated businesses closures may not affect manufacturing directly, workers in industries at high risk of downsizing view such policies with alarm. For these workers they are likely ending their hopes for a better future, and these policies are, therefore, a great source of pessimism. While in the past, such voters may have viewed mainstream candidates and policies as a path to a better future, business closures turn them against such parties and policies in search of alternatives.

These alternatives are often found on the populist right, which promises to protect jobs, restore old industries, and quickly return the economy to normal. Trump is a prominent case in point, and he has consistently pushed against lockdowns and other policies designed to control the pandemic. When mandated business closure policies are implemented at the state-level, voters facing high risks of downsizing therefore turn to Trump for relief. We find this to be the case in our data, with a strong boost to Trump's support among workers in states with a large proportion of workers in industries at high risk of downsizing, or low capacity for teleworkability. We also find that pessimistic expectations about future economic prospects – our key mechanism -- are strongly correlated with Trump support in 2006. In the full sample of workers, however, business closure policies are actually associated with lower average Trump approval, suggesting that a majority of voters prioritize health over jobs.

We have used the American case to highlight what we believe is a general insight about the causes of populism: when workers no longer see a viable path to a better life for themselves or their children, they turn against mainstream candidates and parties. We have used the American case because of the unique opportunities to causally identify the logic, but we conjecture that it applies elsewhere. In a comparative analysis two additional variables would have to be considered: the social protection system and the educational system. Public health insurance and unemployment protection with high replacement rates make it less costly for workers to prioritize health over jobs, and access to good opportunities for skill-acquisition and retraining offer a path to a better life that does not depend on safeguarding existing industries and jobs, thus closing the gap between the two lines in Figure 1. Surely this is one reason why continental European countries have been generally more successful in confronting the coronavirus health crisis than the U.S. But it remains a hypothesis to be tested.

Bibliography

Abraham, Sarah and Liyang Sun (2018). “Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects”. Available at SSRN 3158747.< http://economics.mit.edu/files/14964>

Acemoglu, David Autor, David Dorn, Gordon Hanson, and Brendan Price. 2014. “The rise of China and the future of US manufacturing”. *VoxEU & CEPR*.

Acemoglu, David Autor, David Dorn, Gordon Hanson, and Brendan Price. 2016. “Import competition and the great US employment sag of the 2000s.” *Journal of Labor Economics,* 34(S1), S141-S198.

Acemoglu, Daron and Restrepo, Pascual. 2020. “Robots and Jobs: Evidence from U.S. Labor Markets.” *Journal of Political Economy* 123 (6): 2188 – 2244.

Autor, David, H., David Dorn, and Gordon H. Hanson. 2013. "The China syndrome: Local labor market effects of import competition in the United States." *American Economic Review* 103 (6): 2121-68.

Bartels, Larry M. and Cramer, K. J. 2018. The Political Impact of Economic Change: The Class of ’65 Meets the “New Gilded Age”. Manuscript.

Bartik, Alexander, Bertrand, Marianne, Cullen, Zoe, Glaeser, Edward, Luca, Michael, and Stanton, Christopher. 2020. “The Impact of COVID-19 on Small Business Outcomes and Expectations.” *Proceedings of the National Academy of Sciences* 117 (30): 17656-17666.

Betz, H.-G. (1993). The New Politics of Resentment: Radical Right-Wing Populist Parties in Western Europe. *Comparative Politics*, 25(4):413.

Bornschier, Simon, and Hanspeter Kriesi. 2012. "The populist right, the working class, and the changing face of class politics." In Jens Rydgren (ed.), *Class politics and the radical right*. London and New York: Routledge.

Borusyak, Kirill and Xavier Jaravel (2017). “Revisiting Event Study Designs”. Available at SSRN 2826228. < https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2826228>

Burgoon, Brian, van Noort, Sam, Rooduijn, Matthijs, and Underhill, Geoffrey. 2019. “Positional Deprivation and Support for Radical Right and Radical Left Parties.” *Economic Policy* 34 (January): 49–93.

Brzezinski, Adam, Deiana, Guido, Kecht, Valentin, and Van Dijcke, David. 2020a. “The COVID-19 Pandemic: Government vs. Community Action Across the United States”. COVID Economics: Vetted and Real-Time Papers 7: 115–156.
https://www.inet.ox.ac.uk/files/BrzezinskiKechtDeianaVanDijcke_18042020_CEPR_2.pdf

Brzezinski, Adam, Kecht, Valentin, and Van Dijcke, David. 2020b. “The Cost of Staying Open: Voluntary Social Distancing and Lockdowns in the U.S.” Economics Series Working Papers 910, University of Oxford, Department of Economics. https://www.economics.ox.ac.uk/department-of-economics-discussion-paper-series/the-cost-of-staying-open-voluntary-social-distancing-and-lockdowns-in-the-us

Cavaille, Charlotte, and Jeremy Ferwerda. 2017. "How distributional conflict over public spending drives support for anti-immigrant parties." Working Paper Series, Centre for Competitive Advantage in the Global Economy, University of Warwick.

Cramer, K. J. (2016). *The Politics of Resentment: Rural Consciousness in Wisconsin and the Rise of Scott Walker*. University of Chicago Press.

De Vries, C. and Homann, I. 2018. *The Power of Past. How Nostalgia Shapes European Public Opinion*. Bertelsmann Stiftung 2018/2.

Emmenegger, Patrick, et al., eds. 2012. *The age of dualization: the changing face of inequality in deindustrializing societies*. Oxford Univerity Press.

Gidron, Noam and Peter A. Hall. 2017. “The politics of social status: economic and cultural roots of the populist right.” *The British Journal of Sociology* 68: S57–S84.

Gitmez, Arda, Sonin, Konstantin, and Wright, Austin. 2020. “Political Economy of Crisis Response.” Becker Friedman Institute for Economics Working Paper No. 2020-68 < https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3604320>

Goodman-Bacon, Andrew. 2018. “Difference-in-Differences with Variation in Treatment Timing.” *NBER Working Paper No. 25018*.

Grossman, Guy, Kim, Soojong, Rexer, Jonah, and Thirumurthy, Harsha. 2020. “Political Partisanship Influences Behavioral Responses to Governors’ Recommendations for COVID-19 Prevention in the United States.” *Proceedings of the National Academy of Sciences of the United States of America* 117 (39): 24144-24153.

Häusermann, Silja, Kurer, Thomas., and Schwander, Hanna. 2015. High-skilled outsiders? Labor market vulnerability, education and welfare state preferences. *Socio-Economic Review* 13 (2): 235–258.

Häusermann, Silja, Kurer , Thomas and Zollinger, Delia. 2019. “Economic Opportunities and Electoral Preferences”. Paper prepared for "Populism and Capitalism" Workshop, Sept 26-27, 2019, University of Konstanz.

Hernández, Enrique and Hanspeter Kriesi. 2016. “The electoral consequences of the nancial and economic crisis in Europe.” *European Journal of Political Research* 55 (2): 203–224.

Inglehart, Ronald F., and Pippa Norris. 2016. "Trump, Brexit, and the rise of populism: Economic have-nots and cultural backlash." HKS Working Paper No. RWP16-026.

Iversen, Torben and David Soskice. 2019. *Democracy and Prosperity Reinventing Capitalism through a Turbulent Century*. Princeton University Press.

King, David and David Rueda. 2008. “Cheap Labor: The New Politics of "Bread and Roses" in Industrial Democracies.” *Perspectives on Politics* 6 (2): 279–297.

Kitschelt, Herbert P. and Philipp Rehm. 2019. "Secular partisan realignment in the United States: The socioeconomic reconfiguration of white partisan support since the new Deal era." *Politics & Society* 47 (3): 425-479.

Lipset, Seymour Martin, and Stein Rokkan, eds. 1967. *Party systems and voter alignments: Cross-national perspectives*. Vol. 7. Free press.

Kurer, Thomas. 2020. "The Declining Middle: Occupational Change, Social Status, and the Populist Right." *Comparative Political Studies* 53 (10-11): 1798–1835.

Margalit, Yotam. 2013. "Explaining social policy preferences: Evidence from the Great Recession." *American Political Science Review* 107 (1): 80-103.

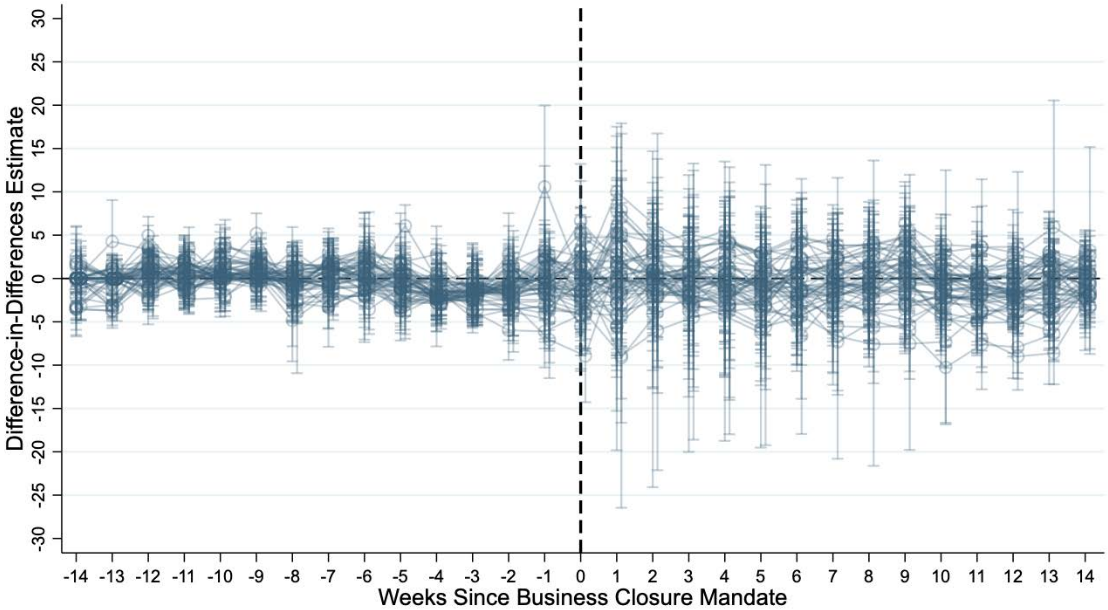
Mutz, Diana C. 2018. "Status threat, not economic hardship, explains the 2016 presidential vote." *Proceedings of the National Academy of Sciences* 115 (19): E4330-E4339.

Smith, Heather J. and Thomas Pettigrew F. 2015. “Advances in Relative Deprivation Theory and Research.” *Social Justice Research* 28 (1): 1–6.

Wright, Austin, Sonin, Konstantin, Driscoll, Jesse, and Wilson, Jarnickae. *Forthcoming*. “Poverty and Economic Dislocation Reduce Compliance with COVID-19 Shelter-in-Place Protocols.” *Journal of Economic Behavior and Organization*.

Appendix A: Randomization Inference Tests

Figure 5: Randomization Inference Tests



Notes: Randomly assigned business closure policy dates have no effect on support for Donald Trump among the "high risk" group in any of the plotted tests. We run 1,000 iterations of this inference test, and the reassigned policy dates largely result in no effect.

Appendix B: Balance Tests for High v. Low Employment Risk

Table 3 shows a balance test for a range of potentially relevant economic, political and demographic variables.

Table 3: Tests for Balance of Baseline Covariates Between High and Low Risk

	High Risk			Low Risk			
	N	Mean	SD	N	Mean	SD	Difference
Weeks Since First COVID19 Case	725	-8.19	18.48	789	-11.75	22.71	-3.57*
Confirmed COVID-19 Cases	725	21,068.09	63,825.53	789	13,249.35	27,389.24	-7,819*
2016 Trump Vote Share (%)	725	46.56	7.51	789	49.42	3.84	2.86***
State Population (2019) (Millions)	725	16.46	13	789	7.75	3.53	-8.31*
Share Female	725	0.5	0.01	798	0.51	0	0.003***
Share Black	725	0.12	0.08	798	0.12	0.07	0.005
Share Hispanic	725	0.25	0.13	798	0.07	0.02	-0.18***

Share Asian	725	0.06	0.04	798	0.03	0.01	-0.03***
Mean Age (Years)	725	38.58	2.15	798	39.87	0.74	1.29***
GDP per capita (Thousands USD)	725	62.47	10.86	798	58.78	7.73	-3.69***
Unemployment Rate (%)	724	3.63	0.72	788	3.68	1.05	0.05

Notes: Balance on covariates among observations in the high relative to low risk group. "Difference" column presents results from t-tests.

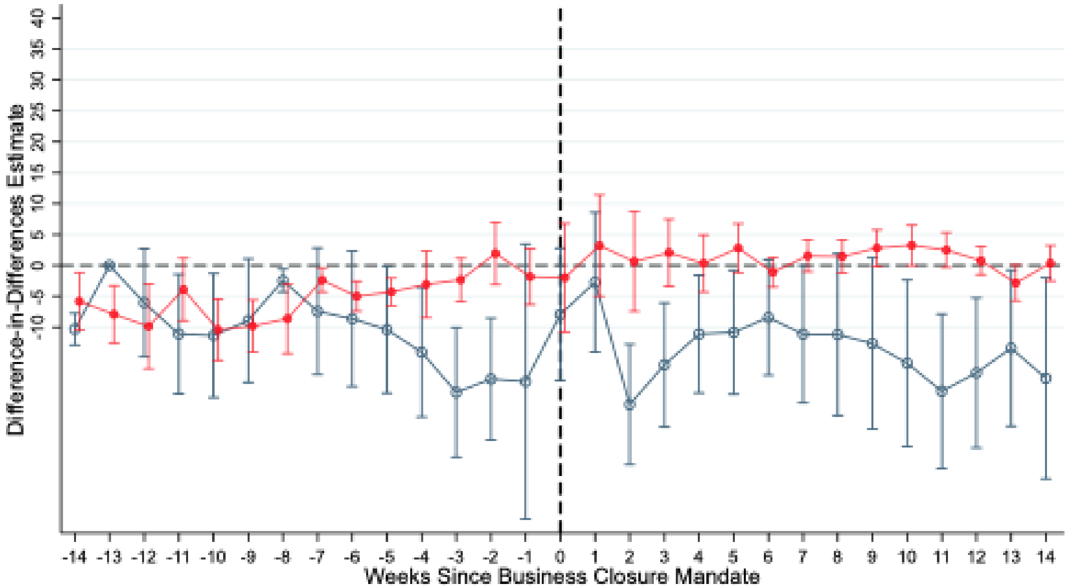
The results indicate that the baseline characteristics between the high and low risk groups are often not balanced. States facing high employment risk have higher 2016 vote count for Donald Trump, have lower population count, fewer confirmed COVID-19 cases, fewer Hispanics, fewer Asians, higher mean age, and lower GDP per capita. We therefore estimated the threshold-separated tests using, instead, each of these unbalanced covariates as the variable by which we split the sample. For example, in addressing the perhaps most plausible challenge to identification – that states with strong Trump support in 2016 are also the ones with strong reactions to lockdowns in 2020 --we split the sample by above and below median pre-existing partisan support for Trump in the 2016 elections. As can be seen from Figure 7, there are no differences in effect between pro- and anti-Trump states. We repeat this exercise for all the unbalanced covariates, and the results are plotted in Figures 8-12 below. All show that the politically polarizing effect of the business closure mandates is not observable for these alternative split-sample analyses.

Table 4: Tests for Balance of Baseline Covariates Between High and Low Non-Teleworkability

	High Risk			Low Risk			
	N	Mean	SD	N	Mean	SD	Difference
Weeks Since First COVID19 Case	725	-8.19	18.48	789	-11.75	22.71	-2.60
Confirmed COVID-19 Cases	725	21,068.09	63,825.53	789	13,249.35	27,389.24	-4072.74
2016 Trump Vote Share (%)	725	46.56	7.51	789	49.42	3.84	4.16***
State Population (2019) (Millions)	725	16.46	12.71	789	8.15	11.93	-7.04
Share Female	725	0.5	0.01	798	0.51	0	0.002***
Share Black	725	0.12	0.08	798	0.12	0.07	0.01
Share Hispanic	725	0.25	0.13	798	0.07	0.02	-0.13***
Share Asian	725	0.06	0.04	798	0.03	0.01	-0.03***
Mean Age (Years)	725	38.58	2.15	798	39.87	0.74	1.44***
GDP per capita (Thousands USD)	725	65.74	8.94	798	55.91	7.41	-9.83 ***
Unemployment Rate (%)	724	3.63	0.72	788	3.68	1.05	-0.05

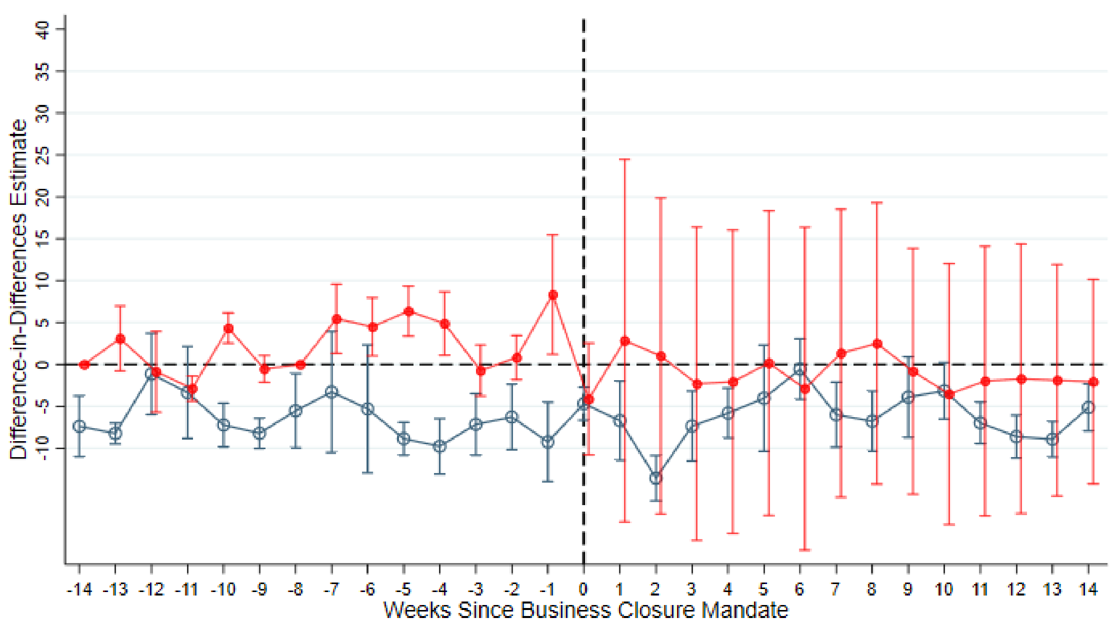
Notes: Balance on covariates among observations in the high relative to low risk group. "Difference" column presents results from t-tests.

Figure 7: Threshold-Separated Analysis for 2016 Trump Support



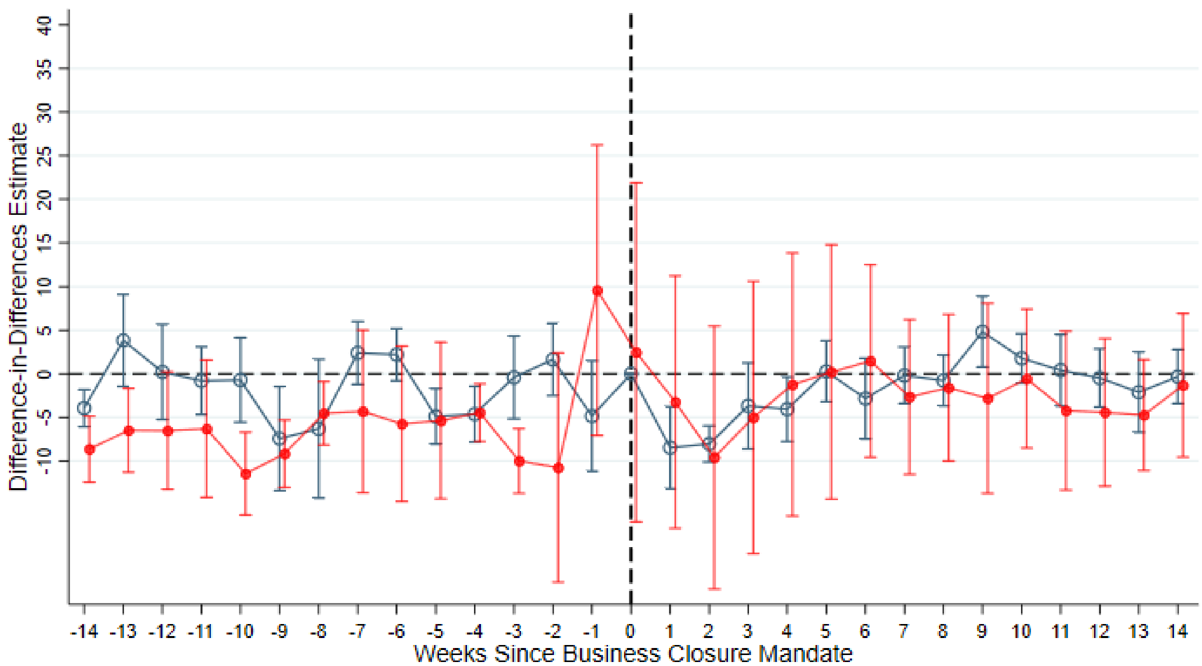
Notes: The figure plots the coefficient estimates with 95% confidence intervals for each week before and after the policy implemented at Week 0. In contrast to the split-sample analysis using high and low risk, the business closure policy mandates have no polarizing effect along pre-existing partisanship lines. Most notably, the policy mandates have no effect on support for Donald Trump in Trump stronghold states (i.e., according to the 2016 vote) (coefficients in red). The two models control for cumulative COVID19 cases and other state-wide policies (e.g., mask mandates) and include state and week fixed-effects as well as fixed-effects for weeks since the first COVID19 case reported in the state. Robust standard errors are clustered by both state and week.

Figure 8: Threshold-Separated Analysis for Unemployment Rate (%)



Notes: The figure plots the coefficient estimates with 95% confidence intervals for each week before and after the policy implemented at Week 0. In contrast to the split-sample analysis using high and low risk, the business closure policy mandates have no polarizing effect between states with high (blue) compared to low (red) unemployment rates. The two models control for cumulative COVID19 cases and other state-wide policies (e.g., mask mandates) and include state and week fixed-effects as well as fixed-effects for weeks since the first COVID19 case reported in the state. Robust standard errors are clustered by both state and week.

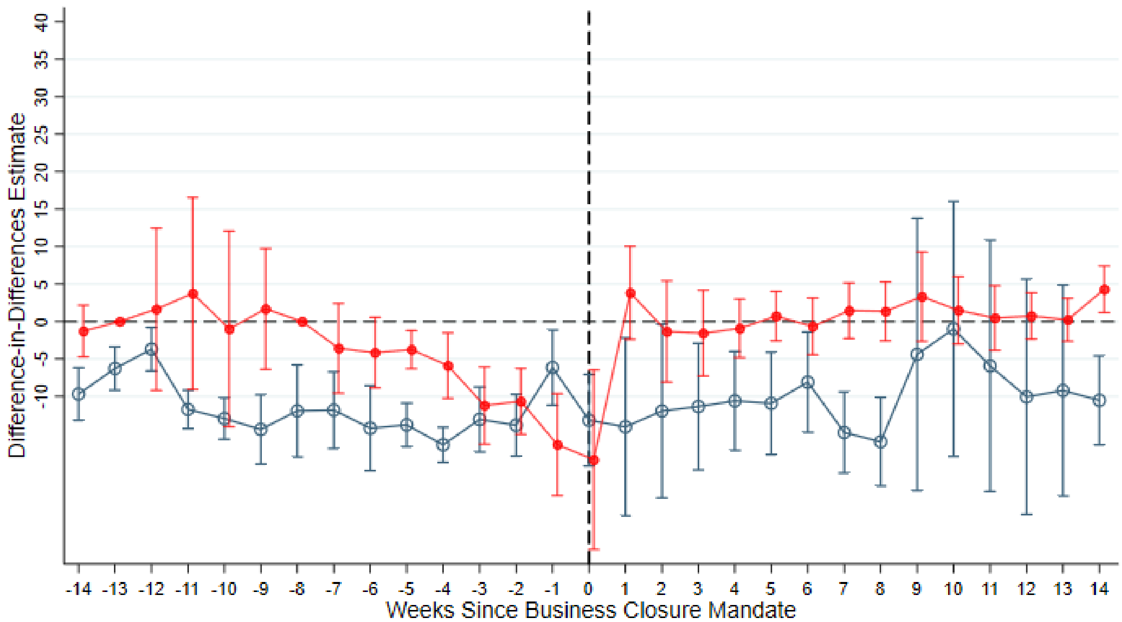
Figure 9: Threshold-Separated Analysis for GDP per capita (Thousands USD)



Notes: The figure plots the coefficient estimates with 95% confidence intervals for each week before and after the policy implemented at Week 0. In contrast to the split-sample analysis using high and low risk, the business closure policy mandates have no polarizing effect between states with high (blue) compared to low (red) GDP per capita (thousands USD) in 2019.[11] The two models control for cumulative COVID19 cases and other state-wide policies (e.g., mask mandates) and include state and week fixed-effects as well as fixed-effects for weeks since the first COVID19 case reported in the state. Robust standard errors are clustered by both state and week.

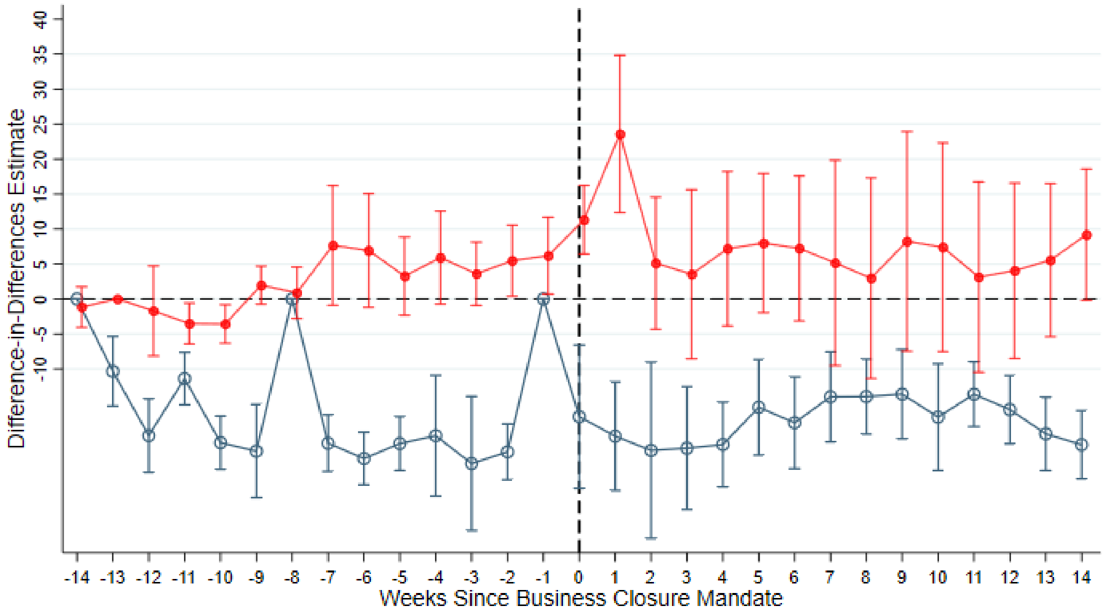
[11] Data for GDP by state in 2019 is available from Statista here: https://www.statista.com/statistics/248023/us-gross-domestic-product-gdp-by-state/

Figure 10: Threshold-Separated Analysis for Population Count



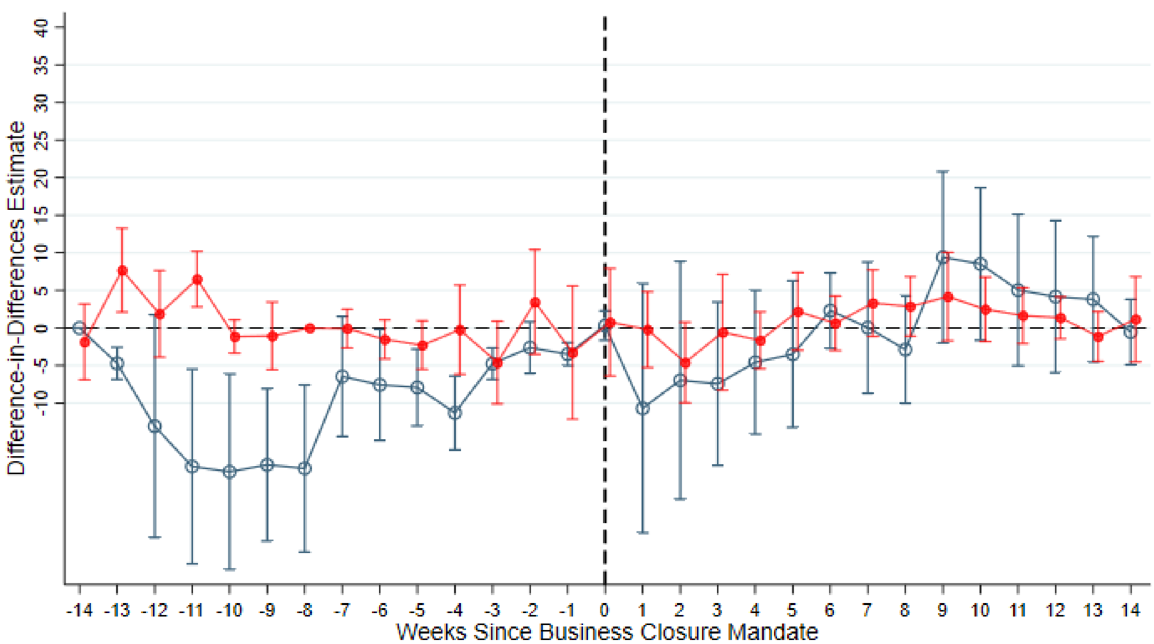
Notes: The figure plots the coefficient estimates with 95% confidence intervals for each week before and after the policy implemented at Week 0. In contrast to the split-sample analysis using high and low risk, the business closure policy mandates have no polarizing effect between high (blue) compared to low-population (red) states. The two models control for cumulative COVID19 cases and other state-wide policies (e.g., mask mandates) and include state and week fixed-effects as well as fixed-effects for weeks since the first COVID19 case reported in the state. Robust standard errors are clustered by both state and week.

Figure 11: Threshold-Separated Analysis for Mean Age (Years)



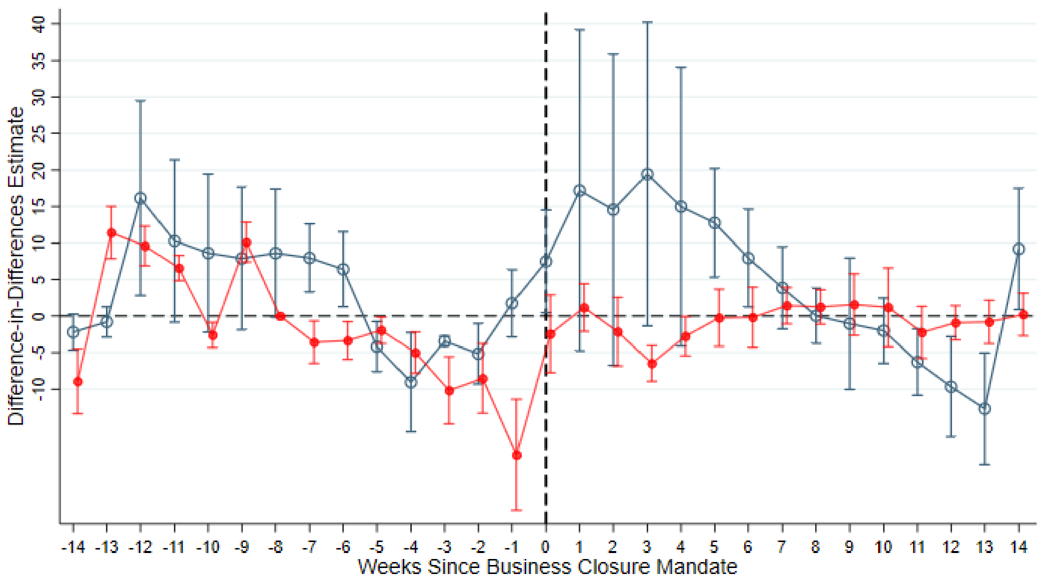
Notes: The figure plots the coefficient estimates with 95% confidence intervals for each week before and after the policy implemented at Week 0. In contrast to the split-sample analysis using high and low risk, the business closure policy mandates have no polarizing effect between different age groups. The policy mandates temporarily increase support for Trump in Weeks 0 and 1 for states with mean age above the median in the sample (red), but this effect does not persistent past Week 1. The two models control for cumulative COVID19 cases and other state-wide policies (e.g., mask mandates) and include state and week fixed-effects as well as fixed-effects for weeks since the first COVID19 case reported in the state. Robust standard errors are clustered by both state and week.

Figure 12: Threshold-Separated Analysis for Share Hispanic



Notes: The figure plots the coefficient estimates with 95% confidence intervals for each week before and after the policy implemented at Week 0. In contrast to the split-sample analysis using high and low risk, the business closure policy mandates have no polarizing effect between Hispanics and non-Hispanics. The two models control for cumulative COVID19 cases and other state-wide policies (e.g., mask mandates) and include state and week fixed-effects as well as fixed-effects for weeks since the first COVID19 case reported in the state. Robust standard errors are clustered by both state and week.

Figure 13: Threshold-Separated Analysis for Share Asian



Notes: The figure plots the coefficient estimates with 95% confidence intervals for each week before and after the policy implemented at Week 0. In contrast to the split-sample analysis using high and low risk, the business closure policy mandates have no polarizing effect between Asians and non-Asians. The two models control for cumulative COVID19 cases and other state-wide policies (e.g., mask mandates) and include state and week fixed-effects as well as fixed-effects for weeks since the first COVID19 case reported in the state. Robust standard errors are clustered by both state and week.

Appendix D: Main Results in Table Form

Tables 5 and 6 below present the difference-in-differences estimates for the post- and pre-treatment period, respectively.

Table 5: Post-Treatment Difference-in-Differences Estimates

	Support for Trump	
	High Risk	**Low Risk**
Week 14	6.621***	-6.626
	(1.740)	(3.966)
Week 15	21.303**	-7.125**
	(7.370)	(2.959)
Week 16	18.646**	-12.381***
	(6.956)	(3.060)
Week 17	17.499**	-13.381***
	(7.633)	(3.395)
Week 18	16.139**	-6.852**
	(6.142)	(2.675)
Week 19	16.005**	-1.947
	(5.554)	(2.572)
Week 20	14.770**	-2.654
	(5.653)	(3.271)
Week 21	12.477***	-3.126
	(3.600)	(2.320)
Week 22	10.673***	-2.200
	(2.997)	(1.775)
Week 23	17.881***	-3.637
	(3.072)	(2.693)
Week 24	13.673***	2.678
	(2.913)	(3.311)
Week 25	9.985***	-4.652**
	(3.320)	(2.036)
Week 26	8.509**	-5.797***
	(3.257)	(1.955)

Week 27	8.429***	-9.346***
	(2.286)	(2.600)
Week 28	5.800***	-4.346**
	(1.942)	(1.621)
Lockdown Policy	-2.398	-4.759***
	(1.470)	(1.187)
State of Emergency Declaration	-4.344***	2.306
	(0.645)	(2.203)
School Closures	-3.250***	4.713
	(0.618)	(3.044)
Mask Mandates	-1.316	-0.908
	(1.226)	(1.348)
Since First COVID19 Fixed-Effects	47.763***	50.890***
	(0.632)	(1.300)
Week Fixed-Effects	47.763***	50.890***
	(0.632)	(1.300)
Constant	47.763***	50.890***
	(0.632)	(1.300)
Observations	409	410

Note: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 6: Pre-Treatment Difference-in-Differences Estimates

	Support for Trump	
	High Risk	**Low Risk**
Week -14	-1.455	-5.692***
	(1.366)	(0.982)
Week -13	-0.338	3.699
	(0.661)	(3.883)

Week -12	4.909	-2.673
	(3.586)	(5.280)
Week -11	-0.146	1.906
	(2.745)	(1.988)
Week -10	-1.339	-5.924**
	(2.804)	(2.426)
Week -9	-0.069	-5.369
	(1.938)	(4.125)
Week -8	0.478	0.000
	(1.794)	(0.000)
Week -7	-1.802	-2.926
	(3.179)	(3.103)
Week -6	-2.779	-5.588
	(2.963)	(3.889)
Week -5	-2.195	-7.044***
	(1.634)	(1.779)
Week -4	1.165	-7.814***
	(1.702)	(0.859)
Week -3	0.269	-13.792***
	(0.782)	(1.252)
Week -2	1.149	-2.918*
	(1.010)	(1.663)
Week -1	2.395	-10.139***
	(1.409)	(2.964)
COVID19 Cases	0.000**	-0.000
	(0.000)	(0.000)
Lockdown Policy	-2.398	-4.759***
	(1.470)	(1.187)

State of Emergency Declaration	-4.344***	2.306
	(0.645)	(2.203)
School Closures	-3.250***	4.713
	(0.618)	(3.044)
Mask Mandates	-1.316	-0.908
	(1.226)	(1.348)
Since First COVID19 Fixed-Effects	-1.316	-0.908
	(1.226)	(1.348)
Week Fixed-Effects	-1.316	-0.908
	(1.226)	(1.348)
Constant	47.763***	50.890***
	(0.632)	(1.300)
Observations	409	410

Note: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Appendix E: Summary Statistics of Dataset

Table 7: Summary Statistics of Data

	Mean	Std. Deviation	Minimum	Maximum	Count
Current Support for Trump (%)	44.580	5.819	23	66	1,514
Manufacturing (Risk) (%)	9.630	3.758	2.108	16.847	1,514
Non-Teleworkability (%)	0.645	0.026	0.581	0.705	1,514
Weeks Since First COVID-19 Case	-10.045	20.861	-67	27	1,514
Post-Business Closures Mandate	0.240	0.427	0	1	1,514
Post-School Closures Policy	0.265	0.441	0	1	1,514
Post-State of Emergency Declaration	0.279	0.449	0	1	1,514
Post-Lockdown Policy	0.218	0.413	0	1	1,514

Post-Masks Mandate	0.083	0.275	0	1	1,514
Confirmed COVID-19 Cases	16,993	48,532	0	444,738	1,514
2016 Trump Vote Share (%)	48.049	6.060	30.03	68.5	1,514
State Population (In Millions)	11.9	10.1	0.732	39.5	1,514
Share Female	0.506	0.006	0.479	0.526	1,523
Share Black	0.120	0.077	0.006	0.460	1,523
Share Hispanic	0.156	0.131	0.017	0.493	1,523
Share Asian	0.046	0.034	0.008	0.477	1,523
Mean Age (Years)	39.256	1.706	33.715	42.878	1,523

Notes: Descriptive statistic of the sample. Statistics for current support for Trump, weeks since first COVID-19 case, confirmed COVID-19 cases, and post-policy mandates variables are aggregated for all observations across all weeks in the sample (i.e., from December 2019 through August 2020).