The Politics of Pessimism: Turning Aspirational Voters into Populists

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Abstract

Higher-educated workers usually support mainstream parties. Lower-educated workers 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 among voters at high risk of losing their jobs. Using a staggered difference-in-differences event study approach, we estimate the causal 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 in states with high risk of downsizing. This is how a public health crisis has turned into a moment of heightened partisan polarization.

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1 Introduction

A growing literature argues 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" has contributed to the rise of populism (King and Rueda 2008; Emmenegger et al. 2012; Autor, Dorn, and Hanson 2013; Cavaille and Ferwerda 2017). Yet, existing empirical analyses find that economic variables are generally weak predictors of populist voting, and the poor or "outsiders" are not major constituencies for the radical right (Bornschier and Kriesi 2012; Kurer 2020; Häusermann et al. 2015; Kitschelt and Rehm 2019). In response, some studies have argued that the real drivers of populism are identity politics and cultural backlash (Inglehart and Norris 2016; Mutz 2018; De Vries and Hoffmann 2018). Others have shown relative economic decline triggers status anxiety and views that run counter to feminism, multiculturalism, and related values widely supported by the sociocultural elite (Gidron and Hall 2017; Burgoon et al. 2018; Smith and Pettigrew 2015; Kurer 2020).

The explanation we advance in this paper is consistent with arguments about relative status decline, but we point to a mostly overlooked factor that highlights 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 study, they argue that advanced capitalist democracies (ACDs) continuously create and re-create the foundations for material improvement for the majority, yet also induce technological upheaval and decline for large minorities tied to the "old" economy. ACDs are based on skill-intensive production, and those who acquire the necessary skills have reason to support policies and political parties that cater to the advanced sectors, thereby, reinforcing future access to quality jobs and prospect of rising incomes. Even those who are not in the advanced sectors may harbor reasonable expectations that they will one day benefit from the new economy, or at least that their children will. It is only when these aspirational voters see opportunities for themselves and their children diminish that they become amenable to populist appeals. They are not usually poor, nor are they political outsiders. Instead, they rationally fear losing out in the continuous transformation of capitalism, and they react defensively to these changes. Populist voters, we contend, are disappointed aspirational voters.

To test this argument, we leverage a quasi-experimental design that uses 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. The analysis compares the effects of business closures on political preferences for workers in areas with industries at low, compared to areas with high, risk of long-term job losses; what we refer to as the risk of downsizing. The U.S. provides an ideal setting for studying these effects because of the combination of the pandemic, a former 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 produced a rich trove of approval data for Donald Trump.

Specifically, we use a staggered difference-in-differences event study design to estimate the differential effect of state-level business closure mandates – conceptually, the 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' exposure to risk of downsizing.

The argument builds on two key assumptions: (i) that some individuals are at a higher risk of downsizing when an economic crisis hits than others, and (ii) that they perceive these differential risks and vote accordingly. We support these assumptions in two ways. First, we employ the approach proposed by Blanchard and Wolfers (2000) to show that economic shocks have larger effects on unemployment in states with high manufacturing employment, consistent with recent work on the "China shock." Second, we show that negative expectations about future economic opportunities are in fact strongly correlated with manufacturing employment. This evidence is purely correlational but it bolsters our assumption that voters in states with high risk of downsizing are likely to respond more negatively to business closures, and increase their support for Trump, which is an effect we can causally identify. Consistent with our argument, we discover a similar pattern when we compare states that were less prepared to transition to work online; i.e., states with low "teleworkable employment". We show these results are not driven by pre-existing support for Trump, differential initial unemployment rates, racial or ethnic composition, population density, or partisanship. Negative economic expectations, on the other hand, are strongly positively associated with support for Trump

To our knowledge, the only other study that explicitly considers the role of material aspirations is a recent paper by Häusermann et al. (2021). Using survey data for eight West European countries, they find that respondents with a combination of both i) low or medium SES and ii) pessimistic views of either their own or their children's economic future – our disappointed aspirational voters – are the ones who exhibit notably stronger support for radical right parties. Our survey evidence is fully consistent with theirs, but their analysis cannot settle concerns regarding reverse causality. We provide the most direct evidence yet that diminished economic prospects drive voters to the populist right, and we propose a general model to understand this effect.

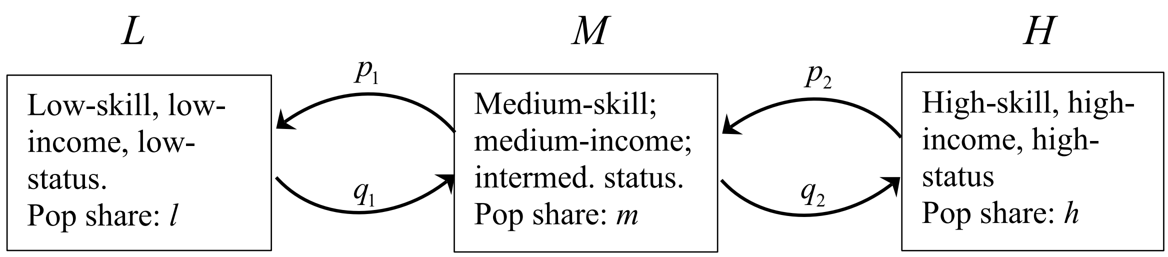
The broader implication of our study is to clarify the relationship between capitalism and democracy. Advanced capitalism is driven forward by a process of what Schumpeter called "creative destruction," which produces winners and losers with associated aspirations and fears about the future. In this dynamic process, it is among those who fear that they, or their children, will be left behind – those we have called disappointed aspirational voters — where we find the largest constituency for the populist right's appeals. During the pandemic that means a willingness to sacrifice public health in the hope of preventing the destruction of jobs that are unlikely to return. The contentious politics of lockdowns in the U.S. is, therefore, closely tied to material politics.

2. The General Argument

Using a standard setup in political economy, we divide the working-age population into three broad classes based on skills and income (assumed to be closely linked): *L* (low-skill, low-income), *M* (medium-skill, medium-income), and *H* (high-skill, high-income). Following a Weberian tradition, we furthermore assume that people associate status with class and confer higher status to higher classes (Chan and Goldthorpe 2007; Savage 2015). We also assume people want to preserve and improve their income and status.

Importantly for our argument, we allow mobility between the three groups, with transition probabilities given in Figure 1. These transition probabilities represent an average for each class, and if these averages are stable, so are the sizes of the three classes.[1] However, each average hides a distribution of probabilities that reflects the interaction of specific industries and skillsets with exogenous shocks such as new technology and economic recessions. Because some industries and workers are more affected by shocks than others, we can distinguish subgroups of *M* based on their combination of transition probabilities.

Figure 1. A three-class model of mobility.



In normal times, when transition probabilities are stable and the sizes of the three classes do not change, expected incomes over any period of time depends on the relative probabilities of upward and of downward mobility. For those in the *M* group with $p_1 < q_2$, expected income and status are both rising; for those with $p_1 > q_2$, both are declining. This distinguishes aspirational

[1] If the size of each class reaches a stable equilibrium, the number of people transitioning into one class must equal the number transitions out of that class. Expressed as population shares that add up to 1: $(p_1 + q_2) \cdot m = q_1 \cdot l + p_2 \cdot h;\ q_1 \cdot l = p_1 \cdot m,\ h = (1 - l - m)$. This implies the following stable group shares: $l = \dfrac{1}{\dfrac{q_1}{p_1} \cdot \left(\dfrac{q_2}{p_2} + 1\right) + 1}; m = \dfrac{1}{\dfrac{q_2}{p_2} + \dfrac{p_1}{q_1} + 1}; h = \dfrac{1}{\dfrac{p_2}{q_2} \cdot \left(\dfrac{p_1}{q_1} + 1\right) + 1}$.

If $p_1 = p_2 = q_1 = q_2 \Rightarrow l = m = h = 1/3$.

voters from their disappointed peers. A small but rapidly expanding literature has identified the latter as a core constituency for populist appeals, as status anxieties give rise to support for defensive policies and symbolic displays of belonging designed to preserve or restore status (Gidron and Hall 2017; Pettigrew 2017; Rooduijn and Burgoon 2018; Burgoon et al. 2019; Kurer and Gallego 2019; Kurer 2020). Aspirational voters, on the other hand, have reason to support institutions and policies that increase the prospects of joining the ranks of the well-off. The evidence in Häusermann et al. (2011) supports this interpretation in the European context by linking (low) individual expectations about the future to voting for the radical right. We confirm below that such a linkage also exists in the U.S. case.

Comparative statics, however, can only be used to establish correlation, not causation. We go one step further by drawing out the dynamic implications of the argument to consider the consequences of shocks that significantly raise the risk of loss of employment and income. The distinction is now between those for whom the loss of employment and income is believed to be transitory and short-term and those for whom it is believed to be more permanent or long-term. For the former, recovery is reflected in a temporary rise in q_1 during a post-shock recovery period, while for the latter, q_1 does not adjust upwards, or it does so only at a very slow rate. We say that the *risk of downsizing* is high in the latter case but low in the former.

3. The Formation of Expectations

The transition probabilities discussed in the previous section are not directly observable, but people can form beliefs about these probabilities based on past experience, notably exposure to unemployment. Modern macroeconomics and economic voting theory both assume that expectations are formed in large part retrospectively (see Carlin and Soskice 2015 Ch. 1.2.4 for a discussion of the economic literature; and Lewis-Beck 1988; Lewis-Beck and Stegmaier 2007; Duch and Stevenson 2008; Bratton 1994, Lockerbie 2008, and Murillo and Visconti 2017 on retrospective economic voting). We can interpret the formation of beliefs in a Bayesian framework in which people have priors about the likelihood of future events, but these priors are continuously updated as new information becomes available. In relatively stable settings where deviations from past experiences are small and transitory, priors will become well-established around a fairly stable equilibrium, and expectations about the future will not change much in the face of short-term deviations. When shocks are large and more enduring, on other hand, the priors would become less established and people will be primed to adjust their expectations more. To use a paradigmatic example from Economics, large and repeated price shocks will condition people to view *all* price increases as evidence of inflation, even if they are in fact transitory or merely relative price shifts (Lucas 1972; Chevalier and Ellison 1997; Choi et al. 2009; Fuster et al. 2010).

We assume that the transition probabilities in Figure 1 above are formed subjectively in a similar manner. Some workers will see economic fluctuations, especially rising unemployment in their workplace or community, as transitory and not radically change their expectations about future employment prospect. Others have been primed by past experiences to view rises in unemployment with greater alarm, likely amplified through social networks and local media (Ansolabehere, Meredith and Snowberg. 2014). A much-discussed case in point is the so-called China shock. Massive increases in imports from China significantly depressed employment in U.S. manufacturing industries exposed to foreign competition, and it took a decade or more for overall employment levels to be restored (Autor et al 2006). Acemoglu et al. (2014; 2016) likewise find that the Great Recession resulted in foreign, especially Chinese, competitors capturing market shares and turning layoffs into permanent job losses.[2] These recent past experiences prime those voters in the most-affected industries (i.e., those in manufacturing) to expect future employment shocks to have more enduring depressive effects.

We can confirm this assumption more generally using the approach proposed by Blanchard and Wolfers (2000). They use time dummies as indicators for "common shocks" across European countries, which they then interact with indicators for labor market institutions to account for cross-national differences in unemployment effects. We use the same nonlinear setup for states in the U.S., where the common shock assumption is arguably more realistic, but we use the share of employment in manufacturing as the conditioning variable rather than differences in institutions. We have state-level unemployment data from 1980-2020, and we use annual dummies as indicators for common shocks. The model is specified identically to Blanchard and Wolfers (2000) as follows:

$$u_{i,t} = c_i + d_t \cdot (1 + \beta \cdot M_i) + e_{i,t},$$

where i indexes states, c_i are state fixed effects, d_t are annual time dummies, and M_i is manufacturing employment as a share of the labor force. The equation is estimated using Stata's NL procedure with robust standard errors. The regression results are shown in online Appendix B. Because we do not have manufacturing employment data for all years, we split states into those with above-median and those with below-median employment based on manufacturing employment shares available from the Bureau of Labor Statistics (BLS) for the year 2000, the

[2] Many manufacturing jobs are also held by a somewhat older workforce with only high-school or vocational degrees who find it difficult to transition to other employment because their skills are very tied to particular industries or employers.

midpoint of our time series. The median split is not sensitive to changes in sector shares, and the results are substantively identical regardless of the year that is used to split the sample.

As expected, negative shocks show greater effects in states with above-median manufacturing employment. Specifically, a shock that raises unemployment by one percent is, on average, 30 percent larger in states with high manufacturing employment compared to states with low manufacturing employment (significant at a .001 level). Since manufacturing employment is always below 20 percent of the labor force, this is a large effect, and it suggests that manufacturing employment shocks have negative effects throughout the local economy. While these results do not speak to the longevity of shocks, manufacturing employment has been declining during most of the post-1980 period, consistent with the evidence cited above that when manufacturing jobs disappear, they tend to stay gone. Again, our expectation is that those who have experienced large and more or less permanent increases in unemployment are primed to interpret future shocks negatively and, therefore, turn more pessimistic in the face of future employment shocks, regardless of whether these new shocks prove to be permanent.

To confirm that manufacturing employment shapes voters' expectations, 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 county level.[3] The 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?"*

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

We use this "future ladder" question to capture aspirations for future economic betterment and test if they are related to manufacturing employment, which we have at the county-level from the Bureau of Economic Analysis (BEA). We include controls 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 and confirm that negative expectations, indeed, have a strong positive correlation with the local share of employment in manufacturing. Figure 2 below plots the correlation at both the county and state level.[4]

Table 1: Expectations and manufacturing shares

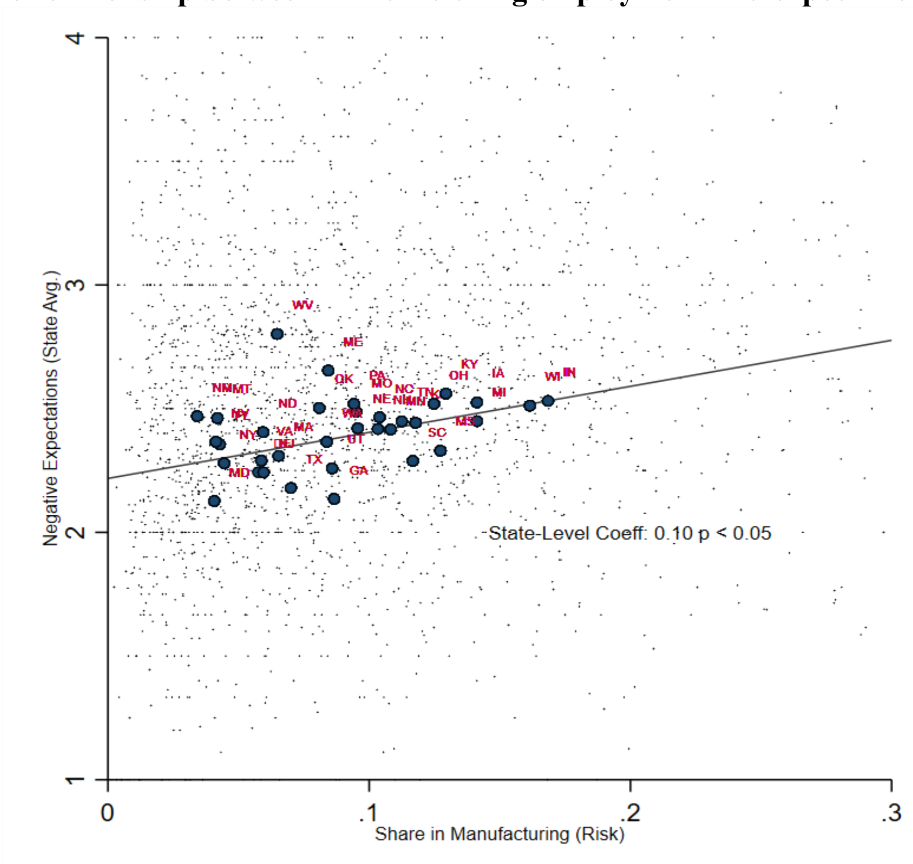
	Negative Expectations	
	Model 1	Model 2
County-level Manufacturing	0.345**	0.461***
	(0.143)	(0.159)
Age (Years)	0.031***	0.031***
	(0.001)	(0.001)
Gender	0.267***	0.268***
	(0.015)	(0.015)
Income	-0.116***	-0.118***
	(0.004)	(0.004)
Race Indicators	✓	✓
Occupation Indicators	✓	✓
Education Indicators	✓	✓

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

Marital Status Indicators	✓	✓
State Fixed-Effects	No	✓
Constant	1.606***	1.646***
	(0.064)	(0.088)
Observations	64,548	64,548
County-Level Clusters	2,388	2,388

Notes: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$ 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, 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.

Figure 2. 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].

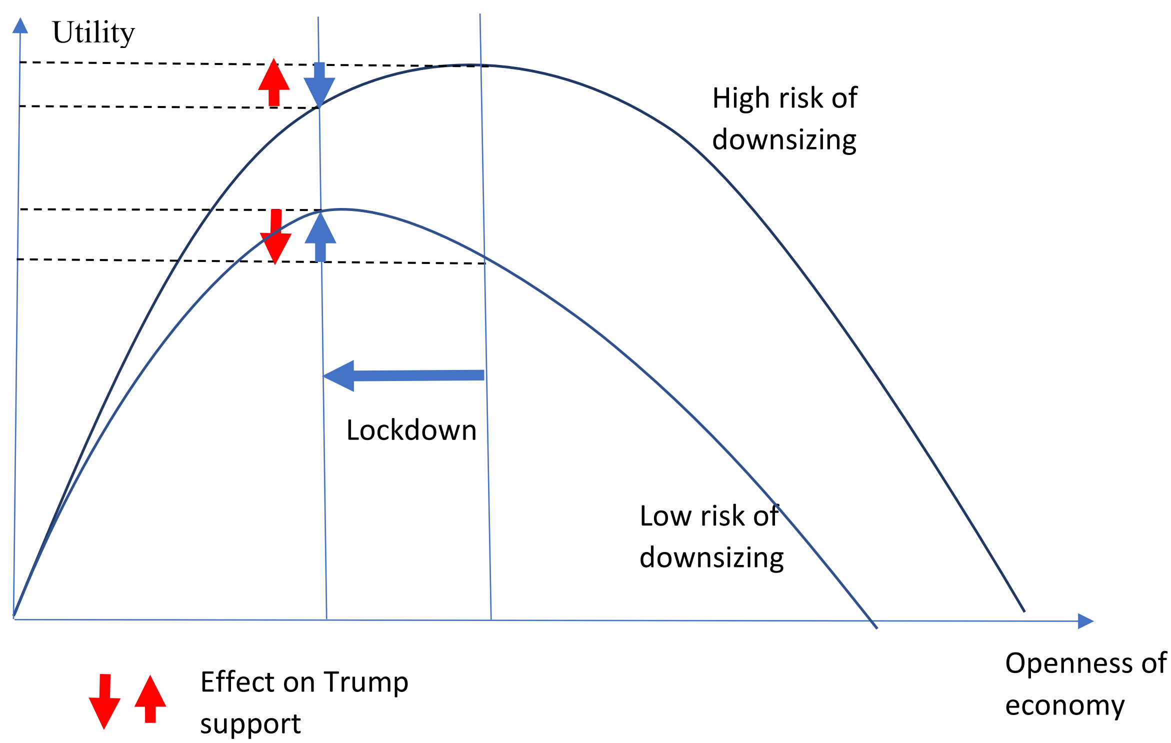
We use these results to support the assumptions underpinning the rest of our analysis, namely that high manufacturing employment leads to a greater risk of downsizing in response to employment shocks, and that such risks are correlated with negative expectations about the future. We have, of course, not shown that high employment in manufacturing *causes* larger shocks, but nor does Bayesian updating require such causality. We, instead, have provided evidence for our more modest claim is that people who are dependent on employment in manufacturing –directly or indirectly– tend to have more pessimistic expectations about the future. Our assumption that follows is that greater employment shocks in the past primes those at high risk of downsizing (i.e., employed in manufacturing) to treat future shocks with greater concern.

4. The Argument Applied to Trump Voting During the Pandemic

The business lockdowns in US states were motivated by public health concerns, but they were also costly in terms of loss of work and income. When the risk of downsizing is high, lockdowns undermine prospects for re-employment in the future, and aspirations for a better life consequently take a hit. We argue that this can tip support in the direction of populist politicians because workers in industries facing high risks of downsizing prioritize work and income over health *relative to* workers in industries with low risk of downsizing.

Figure 3 illustrates the logic in the US case during the Trump presidency. 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). The levels of the curves are arbitrary; only the shape and peaks matter. Voters with low risk prefer a lower level of openness during the crisis than those facing high risk because health concerns regarding COVID-19 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 employment or income. These "low risk" voters are, thus, likely to respond to lockdowns by reducing their support for Trump, who consistently exhorted state governments to keep the economy open ("liberate the states"). By contrast, voters at high risk of downsizing are looking for a president who leans against lockdowns, and their likelihood of supporting Trump, therefore, increases.

Figure 3. The differential effects of lockdowns on Trump vote



Note: The lines show the utility of people deping on the risk of downsizing. Starting from a relatively open economy, lockdowns improve the welfare of those with low risk of downsizing and reduce the welfare of those with high risk of downsizing (the levels of the curves are arbitratry). Trump wants to keep the economy open and lockdowns will therefore increase his support among people whose welfare is hurt by lockdowns. For others, who prioritize health relatively more, lockdowns are expected to reduce support for Trump.

There are two individual-level interpretations of this effect, which are both consistent with our argument. One is that lockdowns increase the pessimism of workers in states with high downsizing risks. This follows most directly from the Bayesian updating logic outlined in the previous section. A complementary logic is that lockdowns raise the salience of keeping jobs relative to concerns for health, a preference shaped by past experiences with downsizing. For both interpretations, the effect also depends on the severity of the health crisis because a steeper jobs-deaths tradeoff shifts the preferences of everyone towards less openness (i.e., the curves move to the left in Figure 3). In that case, even high-risk voters may prefer lockdowns, and the difference between the two groups in political responses should vanish. To account for this possibility the empirical analysis controls for different measures of the local impact of COVID19.

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

5. The Causal Effects of Lockdowns on Trump Support

In this section, we leverage quasi-experimental variation from the COVID19 outbreak. Policymakers around the world 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 were effective in limiting disease contagion, they came at a high economic cost to local businesses and also directly affected 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 3, driving down welfare for high-risk workers. We estimate the causal effect of this perceived drop in opportunities on support for Donald Trump, using a modified difference-in-differences design.

To gauge support for Trump, 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.).[5] 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). As with alternative studies that examine the effect of COVID19 policies (see Goodman-

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

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 had 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.[6] 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.[7]

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.[8] 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

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[6] 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.

[7] 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.

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

policies, and mandates for wearing masks). We calculate heteroskedasticity-robust standard errors using two-way clustering by state and week.

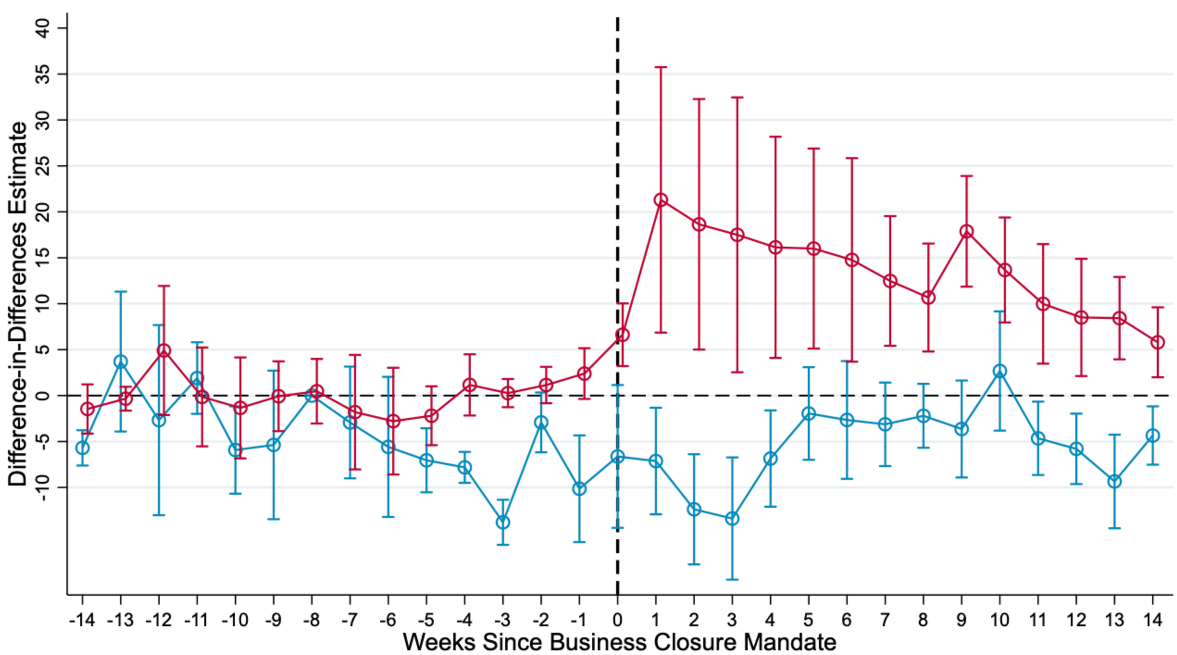
Finally, we consider a second-order difference between states with high manufacturing employment – our measure of high downsizing risk – and states with a low manufacturing employment. As discussed, our proxy for risk of downsizing is the percent share of employment in the manufacturing sector by state. Following the suggestion of Goodman-Bacon (2018), and consistent with our previous analysis of unemployment, 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, again 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 far from 100 percent. Hence the effect of downsizing risk is reduced by the proportion of low-risk workers in that “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 is that the difference between states with low and high manufacturing employment is not about differences in downsizing risk, but instead about some unobserved difference in another relevant variable. The most obvious alternative is different levels of pre-existing 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. We test for this, and show that is not the case. Specifically, if we use the vote for Trump in 2016 to divide states into high and low Trump support areas, we do not observe an effect for the lockdown policies at all (see results in online Appendix E).

In Equation (3), the coefficients, β_{-14} to β_{-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 4 below.

Figure 4. Effect of Business Closure Mandates on Support for Trump



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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 treatment 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. Had Trump simply been concerned with maximizing approval, he may therefore reasonably have supported 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 manufacturing. When business cannot easily be shifted online, business closures pose a greater threat to workers' future earnings and employment prospects. We may see this as functionally equivalent to high labor mobility, which in the past allowed every state to quickly return to full employment (Blanchard and Katz 1992). This conjecture is also strongly confirmed by the evidence (Figure 5).[9] 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 online Appendix E for details).[10]

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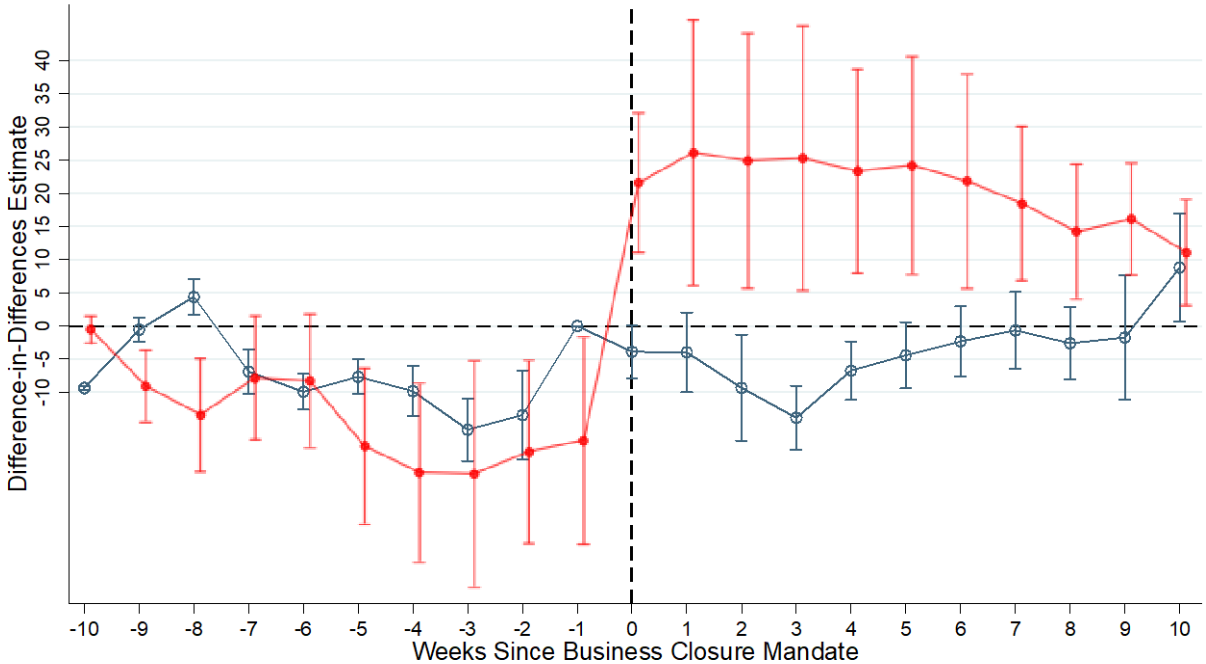
[9] As with the measure of employment risk, we estimate the effects in threshold-separated, characterizing the median share of non-teleworkability in our data sample as the threshold above which non-teleworkability is "high."

[10] If teleworkability is functionally equivalent to labor mobility, it is tempting to hypothesize that declining labor mobility has played a role in the rise of right populism in the US.

Figure 5. 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 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 mandates polarize the electorate. The negative effect of the policy for teleworkable (blue) states is more short-lived, as it is no longer statistically significant from Week 6 onwards. In contrast, the positive effect for non-teleworkable (red) states remains strong throughout the complete period of analysis.[11]

Our evidence assumes that exogenous employment shocks affect Trump voting by reducing expectations about the future among those living in states with high risk of downsizing (i.e., high manufacturing employment or low teleworkability). We cannot show causally that diminished expectations about the future is the mechanism, but we can support the plausibility of this mechanism by regressing the vote share for Donald Trump in the 2016 presidential election on the future aspirations for economic betterment in 2015, based on survey responses to Gallup's ladder question. We estimate the effects at the county-level with the same set of controls as those in the analysis of how manufacturing affects expectations (see Table 2 above). The results are shown in Table 2.

Table 2. 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			✓	✓

[11] As it is natural to observe with estimated policy effects, the public opinion response to the policy for non-teleworkable states reduces slightly over time in the later weeks, but what is critical is that the negative effect persists.

Education Indicators			✓	✓
Marital Status Indicators			✓	✓
State Fixed Effects	No	✓	No	✓
Constant	0.463*** (-0.009)	0.446*** (-0.026)	0.478*** (-0.009)	0.440*** (-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 shows that a standard deviation drop in aspirations is associated with a 7.96 percent increase in Trump voting, a substantively large effect that is significant at the $p < 0.01$ level. 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.

6. 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 5 and 6, 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 capture, to some extent, 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 online Appendix C, 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 A2 in online Appendix C. 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.

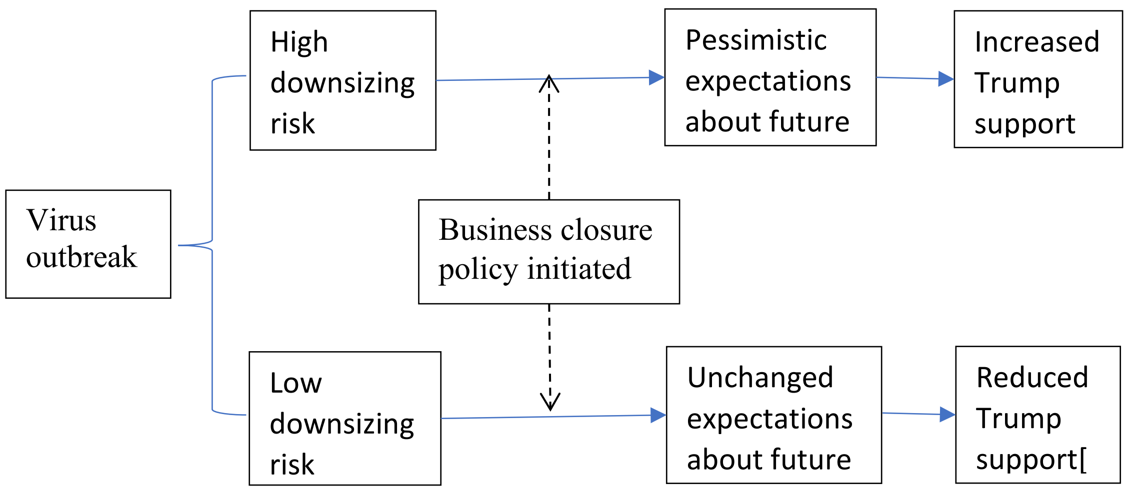
In particular, we run balance tests on a series of demographic, economic, and political characteristics, and on measure of the intensity of the COVID-19 pandemic. The results from the tests are presented in online Appendix D. As in the case of partisanship, reported above, we do not find any differential 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 $t < t_{0-14}$ and $t > t_{0+14}$. 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.

7. Summary of Argument and Findings

The implied causal model is summarized in Figure 6. We causally identify the effect of closures on Trump approval in low- and high-risk states. We use observational data to confirm that states with high manufacturing employment experienced larger employment shocks than other states, and we showed that those in high-risk districts and states have, on average, lower expectations about their future economic welfare *and* are more likely to vote Trump. If state closures did, indeed, change voter's expectations about the future, consistent with previous employment shocks, this is the 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).

8. 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 at high risk of downsizing –notably, those employed in 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, their hopes for a better future are dashed, 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 more radical 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. Donald 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 were implemented at the state-level during his presidency, voters facing high risks of downsizing, therefore, turned 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 in those with low capacity for teleworkability. We also find that dashed expectations about future economic prospects – our key mechanism -- is strongly correlated with Trump support in 2016. 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 U.S. 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 case of the U.S. because of the unique opportunities to causally test our theory, 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, hence closing the gap between the two lines in Figure 3 above. This is likely one reason why continental European countries have been generally more successful in confronting the coronavirus health crisis than the U.S. However, this remains a hypothesis to be tested.

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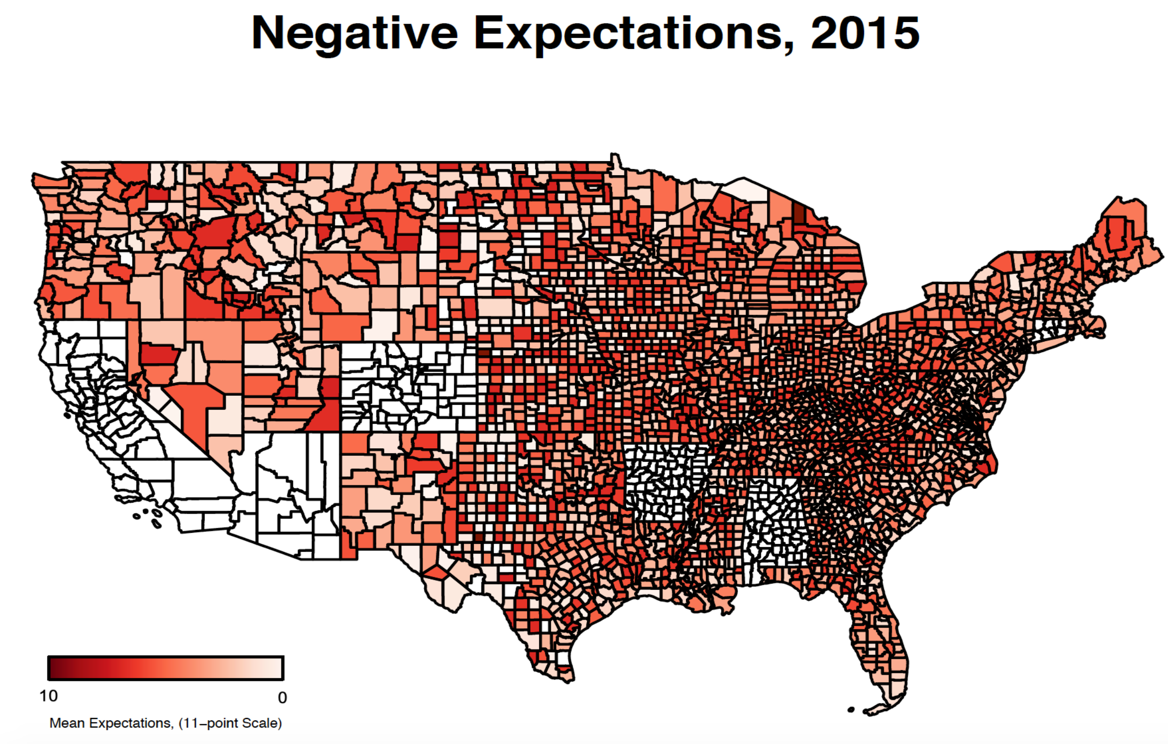
Online appendix A:

Figure A1. The geographical distribution of negative expectations about the future



Notes: The Gallup measure of negative expectations for the future mapped at the county-level.

Online appendix B: Blanchard and Wolfers' (2000) Non-Linear Analysis

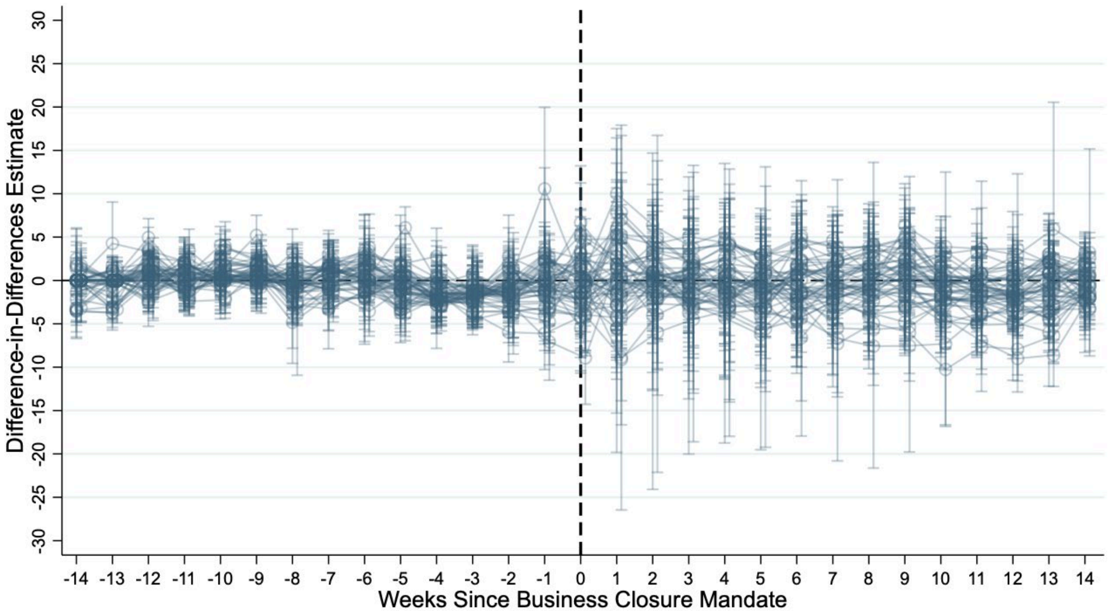
Table A1: Non-linear Analysis

	Unemployment Rate
High Risk	0.299***
	(0.047)
State Fixed-Effects	✓
Year Fixed-Effects	✓
Observations	2,091

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

Online appendix C: Randomization Inference Tests

Figure A2: 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.

Online appendix D: 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 A2: 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 A3: Tests for Balance of Baseline Covariates Between High and Low Non-Teleworkability

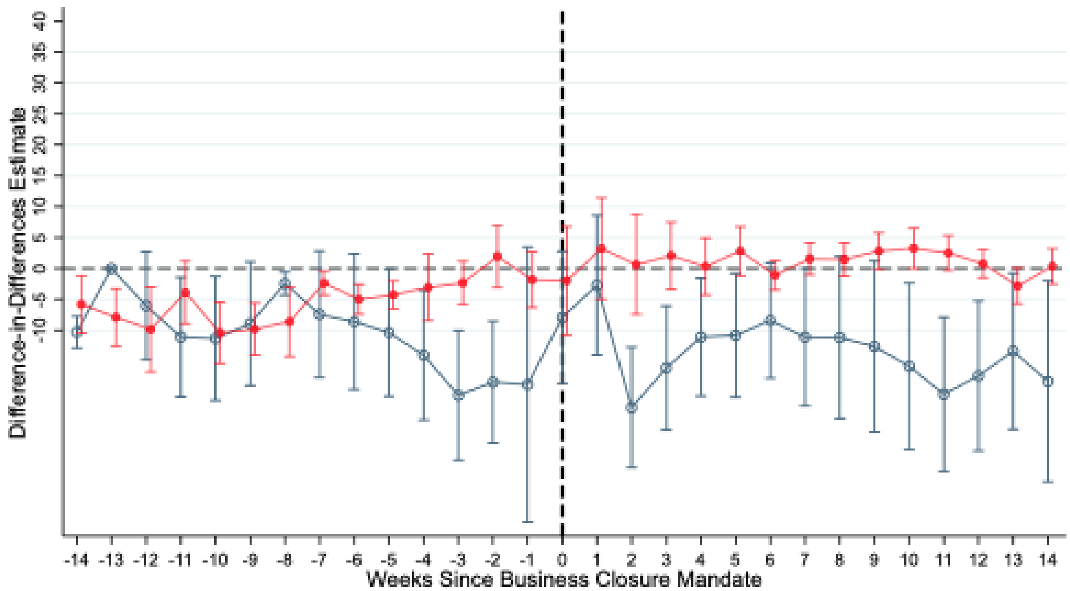
	High Risk			Low Risk			
	N	Mean	SD	N	Mean	SD	Difference
Weeks Since First COVID19 Case	725	-8.19	18.48	789	-11.75	22.71	-2.60
Confirmed COVID-19 Cases	725	21,068.09	63,825.53	789	13,249.35	27,389.24	-4072.74
2016 Trump Vote Share (%)	725	46.56	7.51	789	49.42	3.84	4.16***
State Population (2019) (Millions)	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.

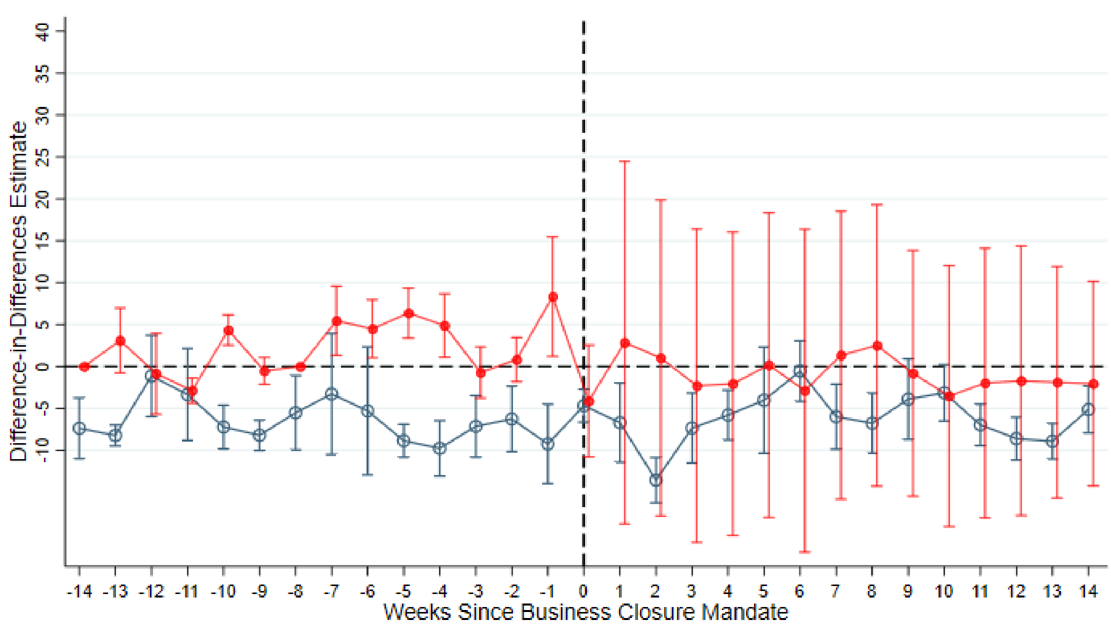
Online appendix E: Threshold-Separated Tests

Figure A3: Threshold-Separated Analysis for 2016 Trump Support



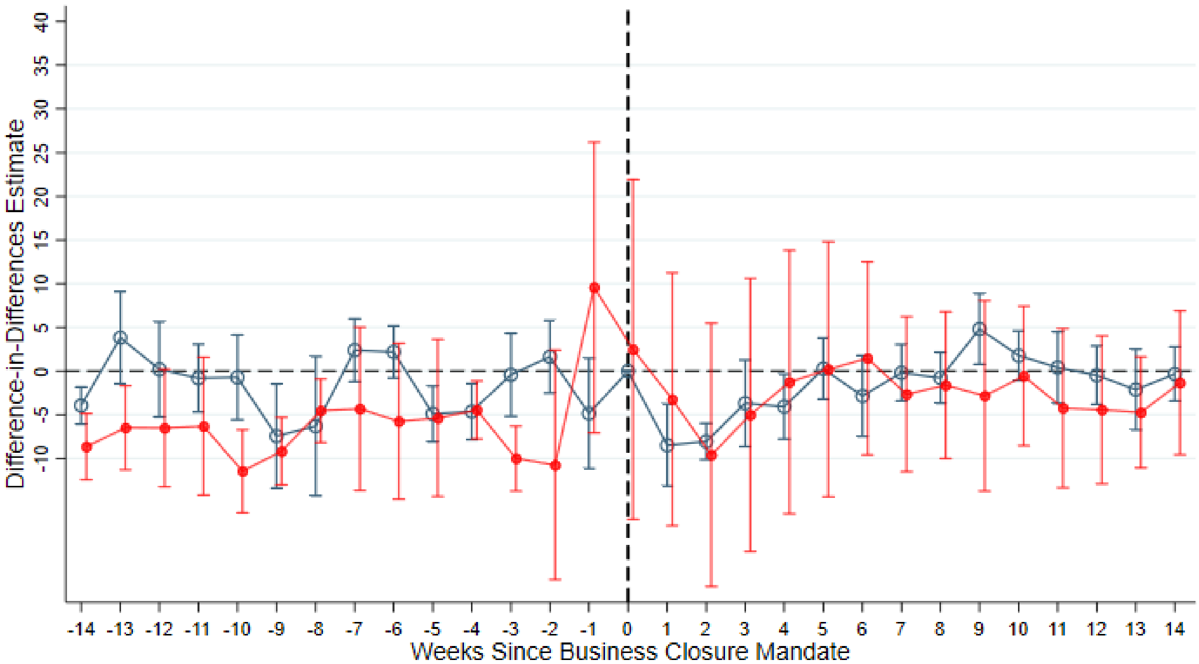
Notes: The figure plots the coefficient estimates with 95% confidence intervals for each week before and after the policy implemented at Week 0. In contrast to the split-sample analysis using high and low risk, the business closure policy mandates have no polarizing effect along pre-existing partisanship 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 A4: 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.

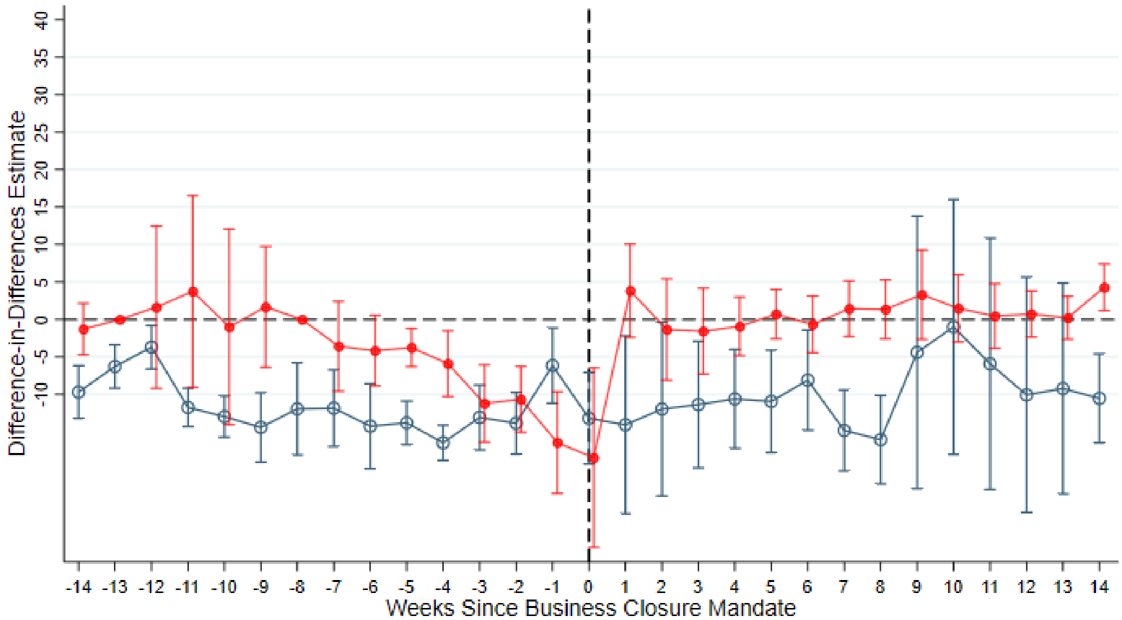
Figure A5: Threshold-Separated Analysis for GDP per capita (Thousands USD)



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.[12] 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.

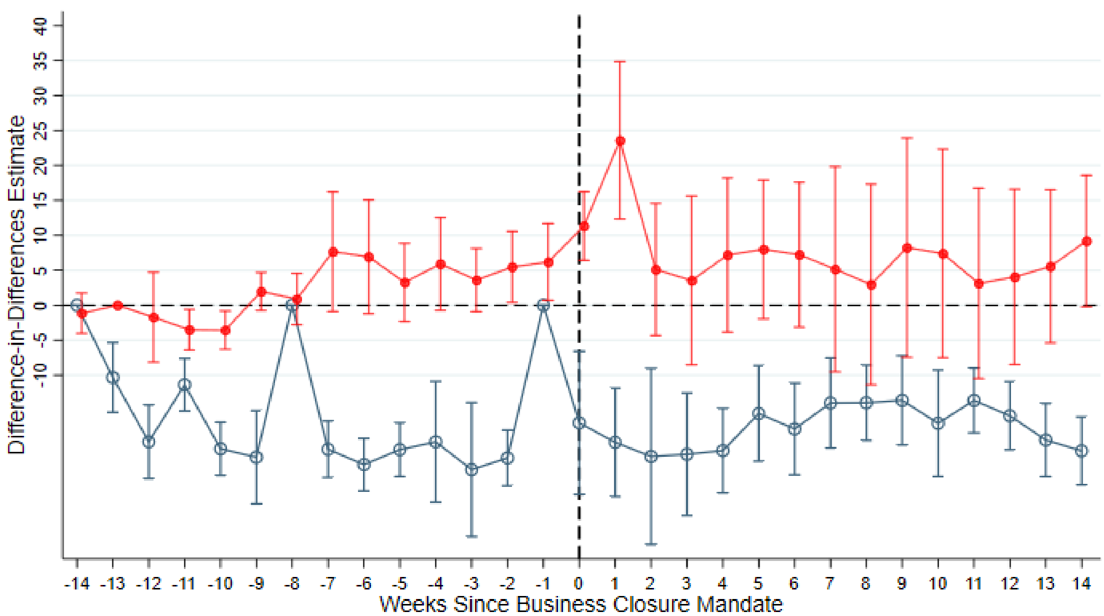
Figure A6: Threshold-Separated Analysis for Population Count

[12] 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/



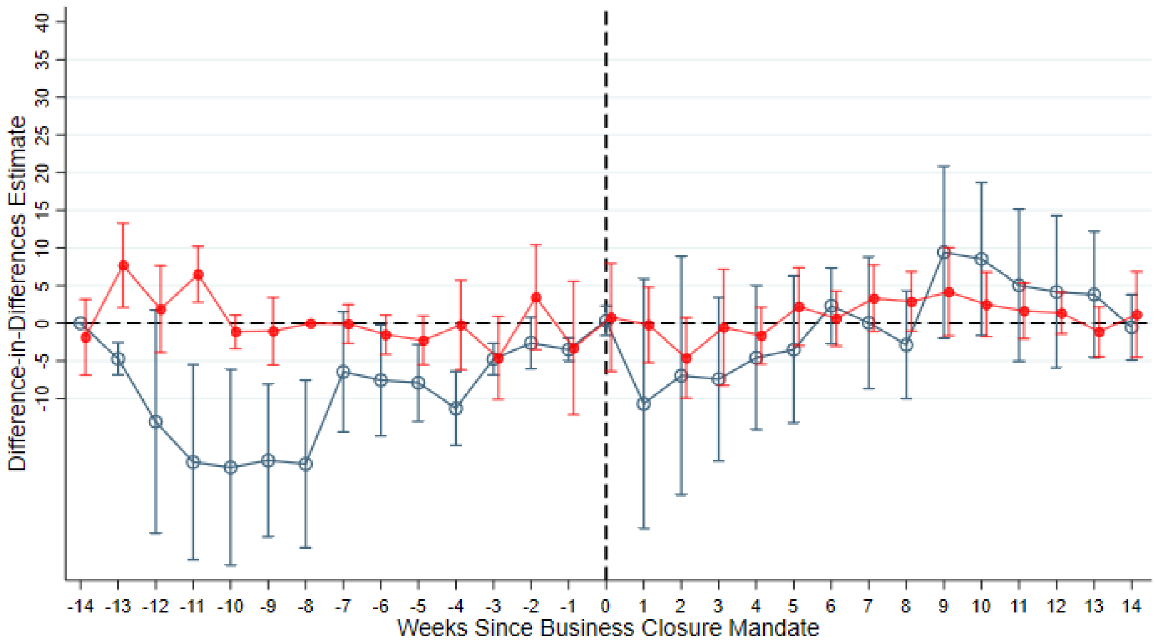
Notes: The figure plots the coefficient estimates with 95% confidence intervals for each week before and after the policy implemented at Week 0. In contrast to the split-sample analysis using high and low risk, the business closure policy mandates have no polarizing effect between high (blue) compared 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 A7: Threshold-Separated Analysis for Mean Age (Years)



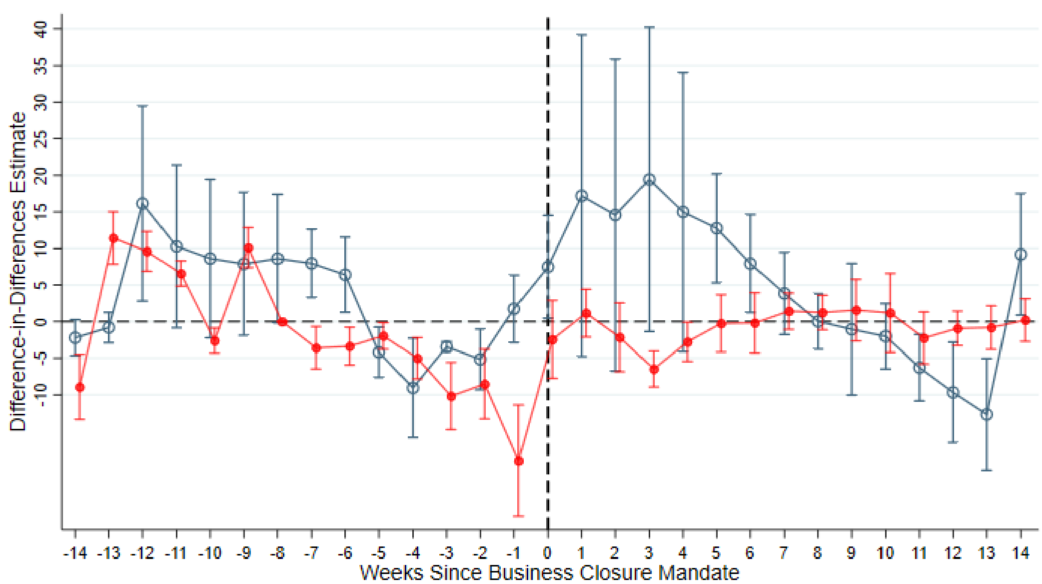
Notes: The figure plots the coefficient estimates with 95% confidence intervals for each week before and after the policy implemented at Week 0. In contrast to the split-sample analysis using high and low risk, the business closure policy mandates have no polarizing effect between different age groups. 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 A8: 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 A9: 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.

Online appendix F: Main Results in Table Form

Tables A4 and A5 below present the difference-in-differences estimates for the post- and pre-treatment period, respectively.

Table A4: 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 A5: 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: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Online appendix G: Summary Statistics of Dataset

Table A6: 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).