

Reopening After Covid: A Replication of Chetty et al.’s “The Economic Impacts of COVID-19.”

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Abstract

During the Covid pandemic, many policies were implemented without knowledge of their economic impacts. In this paper, we investigate the efficacy of one policy—state-ordered reopenings—by replicating a portion of Chetty et al. (2020), who assemble a high-frequency database containing measures of economic health using anonymized data from private companies. They use an event study approach to compare the economic trajectory of the first several states to reopen with a set of controls, finding that the policy had only modest positive effects on spending, employment, business activity, and mobility. These results are broadly consistent with a growing literature on pandemic mandates’ economic impacts. We fail to precisely replicate the results of Chetty et al., finding discrepancies in magnitude, direction, and sample size. Despite these differences, our results are qualitatively similar and support their conclusion that mandates’ economic impacts were modest and that pandemic-driven changes in economic activity were more driven by consumers’ health concerns than policy restrictions, raising questions about stay-at-home orders’ use in future health crises.

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Introduction

The Covid pandemic was an unprecedented challenge for modern economic and political leaders. Rapid policy response was essential to aid recovery from the quick economic downturn. Reform was key as structural vulnerabilities were exposed. In many cases, though, guidelines were hastily implemented with their economic impacts unknown. As climate change and globalization make pandemics ever more likely, it is crucial that we learn from our reaction to the Covid crisis, studying the things that worked, and taking note of those that didn't.

In this paper, we replicate a portion of Chetty et al. (2020)¹, who investigated the pandemic response by constructing a real-time database that tracks key measures of economic health. They use these data to assess a wide range of legislation, including the 2020 stimulus payments, Paycheck Protection Program, and unemployment benefit increases. Our paper focuses on one of these measures: state-ordered reopenings and their heterogenous impact on consumer spending, employment, small business revenues, and how much time people spend away from home.

Chetty et al. find that state-level reopenings result in modest increases in spending, unemployment, and the number of open small businesses, concluding that an economic recovery (or lack thereof) is more affected by consumers' health concerns than government mandates. They situate these findings within a growing literature concerning the economic impacts of pandemic mandates, citing Bartik et al. (2020) and Goolsbee and Syverson (2020). Using private employment data, Bartik et al. find that layoffs largely took place before stay-at-home orders went into effect and that policy drove only a small share of overall job losses. Goolsbee and Syverson show that mobility declined well before lockdowns were enacted and their total effect was comparatively minor. They find that shutdowns also shifted consumer spending from nonessential to essential businesses, implying that mandate-driven changes in economic indicators may be heterogenous across industries.

We attempt to replicate the methodology of Chetty et al. to produce estimates of reopenings' impacts on broad measures of economic health. Chetty et al. have not provided replication materials, so we use their published data and stated methods to approximate their approach. Following their paper, our model is an event study with a difference-in-differences identification strategy. For data processing, we group the first five US states that reopened by their event date, identify control states with similar prior patterns in economic

¹Chetty et al. released an updated version of their working paper in October 2022. Our paper addresses the November 2020 version, which remains available on the Opportunity Insights website.

indicators (taken from Chetty et al. (2020)), and define time relative to this reopening date, stacking the five states’ series. We then perform our regressions, limiting our window of analysis to the two weeks before and after reopening. Our replication materials—including Stata code, tables, and figures—are available in a public repository whose link is in the Supplemental Materials section below.

We find that state-ordered reopenings cause uniform increases in consumer spending, employment, small business activity, and mobility; these results are robust to changes in analysis window and data aggregation techniques. Our results are broadly consistent with those of Chetty et al., but we fail to precisely replicate their numerical estimates and graphs. We speculate that these discrepancies may have arisen from the anonymization process—missing values being prevalent in the public database—or our own misinterpretation of methodology sections that we found ambiguous. These differences aside, we maintain the conclusion that state-mandated reopenings had modest positive economic effects, with businesses’ and consumers’ health preferences likely driving most of the changes in the indicators.

Our paper proceeds as follows. Section 2 summarizes the database developed by Chetty et al. and describes the specific series used in our replication. Section 3 describes our methodology and data processing. Section 4 reports our findings and discusses numerical discrepancies with Chetty et al. Section 5 concludes.

Data

Chetty et al. construct a public database that tracks key economic indicators like consumer spending, small business revenues, employment rates, and GPS mobility at a postal-code level. All series in the dataset are available from January 2020 to the time of writing (i.e., October 2022).

They create these series by aggregating data from several private companies. The consumer spending indicator uses internal data from Affinity Solutions, a company that “combines consumer credit and debit information to support financial service products, such as loyalty programs for credit card companies.” The employment rate data is aggregated from three companies: Paychex, Intuit, and Earnin. The former two facilitate payroll services for small and medium-sized businesses, while the latter is a financial management application that provides access to paychecks before deposit. They source small business data from Womply, a company that provides analytical services to small businesses by aggregating data from credit card processors. Finally, data on geographic mobility is drawn from Google’s Community Mobility Re-

ports, recording the amount of time people spend away from home at public spaces like parks, schools, and grocery stores. In addition to the indicators that we use, the database contains other series such as job postings, unemployment benefit claims, and Covid cases.

The data were processed and anonymized to protect confidential consumer information, remove seasonality, and minimize large daily fluctuations. This included using a seven-day moving average and transforming indices to reflect percentage changes from their mean in 4–31 January 2020. Chetty et al. have made the data publicly available on the Opportunity Insights website².

To study the effects of state-level reopening orders, the concern of this replication, Chetty et al. use their state-level data series on consumer spending, business revenues, employment rates, and geographical mobility. For all series except employment rates, we use the publicly available data. For employment, we identified missing values for Washington, D.C. in the public data, so we contacted Opportunity Insights for a more complete dataset, which we use instead.

Methods

We replicate Chetty et al. by using a difference-in-differences (DiD) event-study model to estimate the effect of reopening on consumer spending, employment, small businesses open, and time spent outside home.

We begin by defining our treatment group as the first five states which reopened in any capacity: South Carolina on 20 April 2020, Alaska and Georgia on 24 April 2020, and Minnesota and Mississippi on 27 April 2020. Each of these dates is associated with four sets of control states which had similar trends in each respective economic indicator of concern before reopening (see Chetty et al., Appendix Table 6).

For each event-indicator group, we drop states which are neither treated nor controls and code dummy variables for (1) whether the observation is from a treated or control state, (2) whether the observation is from past the reopening date, and (3) their interaction. Next, we define time relative to the date of reopening (e.g., $t = 0$ is 20 April 2020 in the first case) and stack the three events for each indicator. This allows us to regress the indicator against the three aforementioned dummy variables for a two week window about the event date, the estimated effect of reopening on the indicator being the coefficient on the interaction term. We also perform the same regression with a three week window for robustness.

²<https://opportunityinsights.org/data/>.

Results

We display our regression results in the second panel of Table 1. Using our DiD method, we estimate that reopening from a Covid lockdown results in increases of consumer spending by 0.33%, employment by 1.02%, small businesses open by 2.91%, and time spent outside home by 0.73%. These results are robust to expanding the regression window to three weeks, with all effects being identical in direction and greater in magnitude than the original estimates.

We plot the average value of the respective indicators for treatment and control states in Figures 1b, 2b, 3b, and 4³. As is visible, treated and control states follow approximately parallel trends before reopening and moderately diverge after reopening, commensurate with our DiD estimates. But there is a comparable—and in some cases even greater—divergence around the nadir of the series, roughly a month before the reopening. This difference between the treated and control states does not violate our parallel trends assumption because all windows of analysis exclude this trough; however, the divergence does suggest that varying individual responses to the Covid pandemic across states—independent of mandates—may have been a more significant determinant of economic activity between treated and control states than the lifting of restrictions. Further exploration of this question is left to future research.

The first panel of Table 1 and Figures 1a, 2a, and 3a display the original results of Chetty et al. in contrast with our own. While our regressions give qualitatively similar estimates to theirs, discrepancies exist in the precise magnitude of effects and in sample size. In some cases, these gaps are relatively small: with a two week analysis window, we estimate a 1.02% increase in employment as an effect of reopening while Chetty et al. estimate a 0.65% increase—both using 208 observations. However, in the case of open small businesses with a two-week window, our estimate of a 2.91% increase differs significantly from their estimate of a 0.30% decrease. Further, our two and three week windows used 244 and 366 observations respectively, while both of theirs use 248. These unchanging sample sizes are especially puzzling. We lack a sound explanation for many of these discrepancies, but there were some uncertainties during the replication that may have contributed.

First, while data were supplied at a daily frequency, Chetty et al. performed their analysis with weekly observations. Given that the data were provided as seven day moving averages, we constructed our weekly series by dropping all observations but Sundays; however, this operation was not directly specified by their paper.

Their graphs in Figures 1a, 2a, and 3a bring up another issue: the stacked

³Chetty et al. does not provide a graph for GPS mobility, so Figure 4 contains only one panel.

series all appear to be drawn from weekly data, the nearest point to the event being the day prior. For the opening dates 20 and 27 April 2020—both Mondays—, this corresponds to the data being Sunday-based, as we’d expect. However, their figures also imply that the series for the opening date 24 April 2020, a Thursday, is Wednesday-based. To reconcile this, we create our graphs using the Wednesday-based series for the 24 April date while we perform our regression analysis using the uniformly Sunday-based series. For robustness, we also ran our regressions using the alternative aggregating method, finding similar results.

Finally, there are several unclear specifications. Chetty et al. fail to describe whether they coded each treated state as open on the reopening date itself and they lack a complete definition for the analysis window; that is, “the two weeks before the reopening in the treated states and two weeks after” could mean $-14 \leq t \leq 14$, $-13 \leq t \leq 14$, or $-14 \leq t \leq -13$. Respectively, we code the reopening date as open and calculate the analysis window using the first variation, but our results are robust to all these choices.

Discussion

Despite what popular perception may imply, states’ decisions to reopen or remain closed appears to have had little impact on their economic health, at least for the first several states that reopened. We offer two potential explanations for this finding.

In line with our observation that most of the difference in economic performance between opening and closed states arose not during reopening but during the beginning of the pandemic, it is possible that health concerns about Covid persisted beyond reopening, driving consumers and businesses to maintain their habits even without mandate. Given the widespread national media coverage of the pandemic, it seems likely that individuals would remain cautious about social contact (e.g., purchasing goods or services) even without a state-level restriction and thus economic recovery would move parallel to consumers’ health concerns, rather than their absolute ability to be mobile and consume business’ products. That the greatest impact of reopening was on the number of open small business—actors without direct health concerns and with a countervailing profit motive—would seem to support this view.

On the other hand, reopenings may have had little effect because the closures had little impact in the first place. That we are studying only the *first* five states that reopened may have introduced a selection bias for those states with weak mandates or populations who were already unlikely to respect state-ordered closures. Our treated states are predominately rural and conservative

(e.g., Alaska, Georgia, and Mississippi), so it is not difficult to imagine their mandates being openly fouted and loosely enforced. In this sense, consumers' habits may have been unaffected by mandates to start, their behavior changing only with health concerns or social pressures, not removed restrictions.

Either conclusion in mind, a more thorough study including a broader range of states, sector disaggregation, and diverse identification approaches would provide stronger insights into the exact mechanism of reopenings' impacts on economic health.

We have already offered above several possible explanations for the discrepancies between our results and those of Chetty et al. (2020). While these were largely speculative, many arose from ambiguities and insufficient methodological detail in their paper. Potential resolutions to these issues could involve the publication of replication materials (i.e., Stata code) or the use of more precise language in their methodology.

Regardless, Chetty et al. offer a rich resource for future research into pandemic policies and economic behavior at large, and our conclusions vis-a-vis state-ordered reopenings are aligned with theirs. In light of these findings, policymakers should closely consider the necessity and usefulness of mandated closures, as well as the timing of reopenings, as they tackle future pandemics.

References

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Supplemental Materials

Our replication materials including datasets, figures, and Stata code are available at <https://www.github.com/samueleross/HERC-covid/>.

Table 1: OLS Regression results for state-level reopenings.

Dep. Var.:	Spending (%)		Employment (%)		Small Businesses Open (%)		Time Outside Home (%)	
Panel A: Chetty et al. (2020)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DiD Estimate of Effect	1.43*** (0.51)	1.37** (0.53)	0.65 (0.51)	1.04 (0.97)	-0.30 (0.85)	1.26 (0.88)	3.27** (1.26)	4.44** (1.85)
N	200	312	208	258	248	248	244	324
Analysis Window (Weeks)	2	3	2	3	2	3	2	3
Panel B: Replication	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DiD Estimate of Effect	0.33 (0.56)	0.95* (0.50)	1.02* (0.55)	1.57** (0.70)	2.91** (1.14)	3.91** (1.57)	0.73 (0.44)	1.32** (0.57)
N	200	300	208	312	244	366	112	168
Analysis Window (Weeks)	2	3	2	3	2	3	2	3

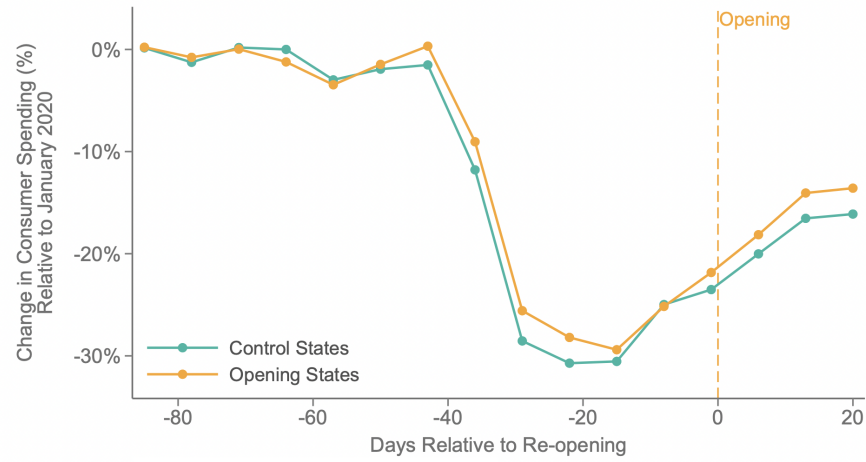
Standard errors clustered by state in parentheses.

Data source: Opportunity Insights Economic Tracker.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

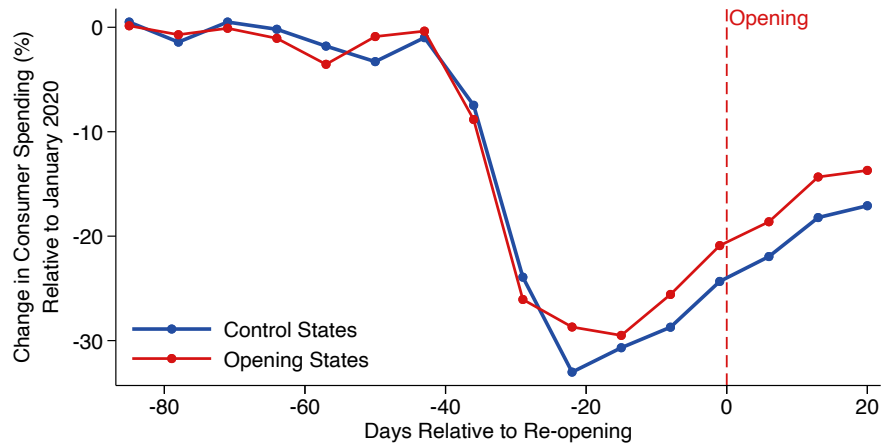
Figure 1

B. Re-Opened States vs. Control States: Consumer Spending



(a) Chetty (2020)

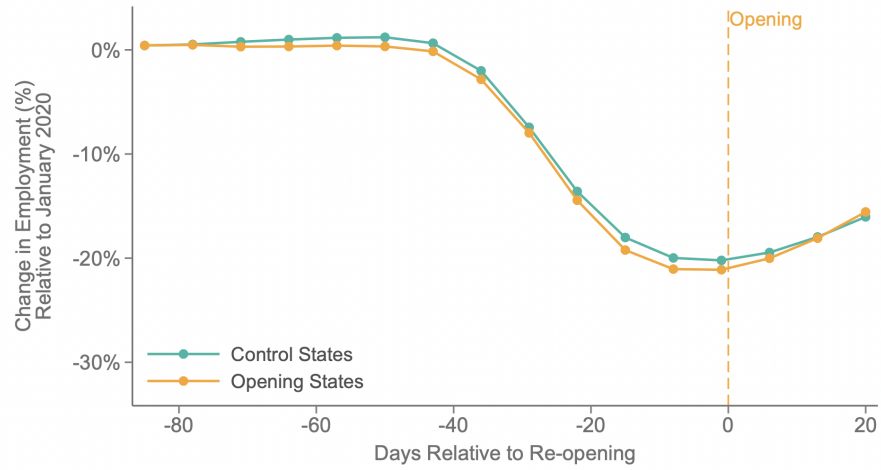
B. Re-Opened States vs. Control States: Consumer Spending



(b) Replication

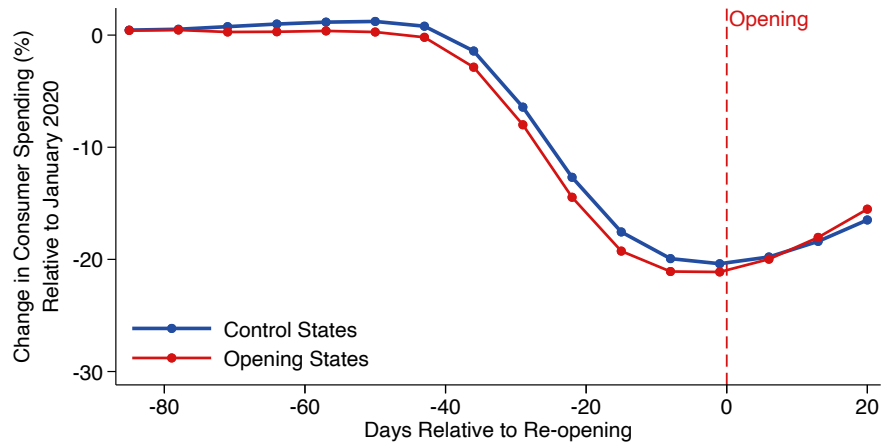
Figure 2

C. Re-Opened States vs. Control States: Employment



(a) Chetty (2020)

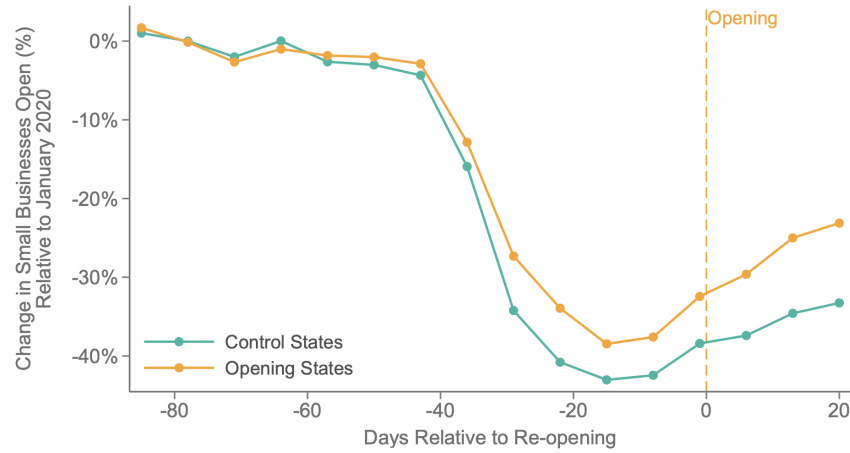
C. Re-Opened States vs. Control States: Employment



(b) Replication

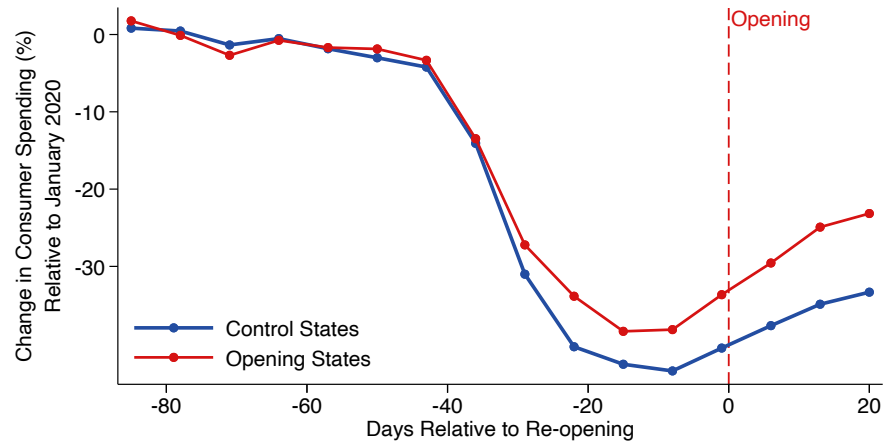
Figure 3

D. Re-Opened States vs. Control States: Small Businesses Open



(a) Chetty (2020)

D. Re-Opened States vs. Control States: Small Businesses Open



(b) Replication

Figure 4

