

Disentangling Policy Effects Using Proxy Data: Which Shutdown Policies Affected Unemployment During the COVID-19 Pandemic?*

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Abstract

We use high-frequency Google search data, combined with data on the announcement dates of non-pharmaceutical interventions (NPIs) during the COVID-19 pandemic in U.S. states, to disentangle the short-run direct impacts of multiple different state-level NPIs in an event study framework. Exploiting differential timing in the announcements of restaurant and bar limitations, non-essential business closures, stay-at-home orders, large-gatherings bans, school closures, and emergency declarations, we leverage the high-frequency search data to separately identify the effects of multiple NPIs that were introduced around the same time. We then describe a set of assumptions under which proxy outcomes can be used to estimate a causal parameter of interest when data on the outcome of interest are limited. Using this method, we quantify the share of overall growth in unemployment during the COVID-19 pandemic that was directly due to each of these state-level NPIs. We find that between March 14 and 28, restaurant and bar limitations and non-essential business closures can explain 6.0% and 6.4% of UI claims respectively, while the other NPIs did not directly increase own-state UI claims. This suggests that most of the short-run increase in UI claims during the pandemic was likely due to other factors, including declines in consumer demand, local policies, and policies implemented by private firms and institutions.

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1 Introduction

During a pandemic, governments may implement non-pharmaceutical interventions (NPIs) to slow the spread of disease. Examples of NPIs include shutting down businesses where social interactions take place, closing schools, ordering people to stay at home, and banning large gatherings. NPIs reduce the movement and social interactions of individuals during a pandemic ([Allcott et al., 2020](#); [Dave et al., 2020](#); [Friedson et al., 2020](#); [Gupta et al., 2020](#)) and slow disease spread ([Flaxman et al., 2020](#)), with varying degrees of effectiveness depending on the particular NPI. However, because many NPIs involve reductions in economic activity, there are concerns about the potential damage that NPIs may cause to the economy and labor markets. This led some elected officials to delay NPI introduction, introduce fewer NPIs overall, or consider the relaxation of NPI policies. Understanding the direct causal effects of specific state-level NPIs (vs. declines in consumer demand, local policies, and policies implemented by private firms and institutions) is of great importance to policy makers as states continue to face decisions about the specific sets of NPIs to relax or re-introduce. Disentangling the potentially heterogeneous effects of multiple policies that were introduced at similar time points is generally challenging because traditional data sources are rarely measured at high-enough frequencies. For example, administrative data on UI claims is available only at the weekly level, whereas different NPIs were often introduced within days of each other. As a result, most other work has considered a single representative NPI (e.g. stay-at-home orders). However, to assess the heterogeneous impacts of different NPIs, high-frequency data is necessary. To surmount this issue, we use a high-frequency proxy measure (daily Google search volumes) to separately estimate the short-run impacts of six different state-level policies that were introduced in close succession.

While our paper focuses on quantifying the role of state-level NPIs, pandemics may impact the economy through a number of channels. For example, they decrease consumer demand for particular goods and services, as individuals avoid public places, which then translates into decreased labor demand. In addition to a given state’s own NPIs, policies implemented by local governments, private firms and institutions, and spillovers from other states may also have additional economic impacts. Our paper does not directly quantify these alternative hypotheses, although other work has found evidence that voluntary decreases in consumer demand may play a large role ([Goolsbee and Syverson, 2020](#)). Thus, our estimates should be interpreted to pertain only to the specific state-level NPIs we outline below.

In this paper, we present an empirical framework for estimating a key policy parameter: the *share* of the short-run economic impact (as measured by UI claims) *directly* caused by a state’s decision to implement various NPIs. We use high-frequency Google search data, combined with data on the exact dates of NPI announcements during the COVID-19 pandemic in U.S. states, to isolate the impact of NPIs on UI claims in an event study framework. We consider six NPIs: restaurant and bar limitations, non-essential business closures, stay-at-home orders, large-gatherings bans, school closures, and emergency declarations. Exploiting the differential timing of the introduction of these NPIs across U.S. states, we analyze how Google searches for claiming unemployment responded to each policy.

We find that announcements of restaurant and bar limitations, non-essential business closures, and stay-at-home orders are associated with increases in the volume of Google searches for claiming UI on the day of the announcement as well as the following two days in the announcing state. At the same time, we find no such association for large-gatherings bans, school closures, and emergency declarations. The effect of stay-at-home orders disappears after controlling for restaurant and bar limitations and non-essential business closures, whereas the latter two policies have independent effects on Google searches that are robust to controlling for other policies.

We then introduce a method to translate our event study estimates into estimates of the share of own-state UI claiming caused by each NPI. Importantly, while our method uses a proxy measure of UI claims (the volume of Google searches for “file for unemployment”, first introduced and validated by [Goldsmith-Pinkham and Sojourner, 2020](#)), it does not rely directly on prior estimates of the relationship between the variable of interest (UI claims) and the proxy measure (Google searches). Instead, we require that the increase in Google searches caused by the NPIs is proportional to the increase in UI claims caused by the NPIs. In addition, we assume that the overall increase in Google searches during our period of interest (March 14 to March 28) can be mapped directly to the 10.2 million initial UI claims filed during those two weeks.

Under these two assumptions, we estimate that the combined causal effect of these NPIs directly accounts for 12.4% of the UI claims filed during this period. We emphasize that this share only encompasses own-state restrictions, and does not include effects from local policies, spillovers from other states, or voluntary changes in consumer and firm behavior. To account for one of these factors, we conduct an exploratory analysis on the role of cross-state spillovers from NPI announcements and

find evidence of modest effects. Our estimate is also subject to uncertainty arising from sampling error and imperfect correlation between search volume and UI claims. We address these concerns by performing bootstrap inference on our share estimate and empirically verify that search volume is a good predictor of UI claims.

Estimating the direct causal impact of state-level NPIs on the economy and the labor market is policy relevant as state governments must decide whether and when to implement or relax particular NPIs during pandemics, taking into account both their effects on the spread of disease as well as their effects on state economies and labor markets. We find that NPIs are heterogeneous in their effects: this heterogeneity is relevant for policy as it can inform trade-offs between the economic and public health impacts of policies when choosing which policies to implement or relax.

This paper makes several contributions. First, we provide estimates of the short-run causal effect of state-level NPI announcements on unemployment expectations (measured via internet search data). Second, we show how these effects can be translated into estimates of the contribution of NPIs to overall growth in UI claiming. Our method using high-frequency data on proxy outcomes to estimate policy effects could be useful beyond our particular setting. Third, this is the first study to simultaneously estimate the impacts of multiple NPI policies on UI claiming and to study variation in the magnitude of these effects.

Related Literature Our work contributes to the literature studying the impact of NPIs adopted during the COVID-19 pandemic on unemployment and other economic outcomes. Most closely related to our work, [Baek et al. \(2020\)](#) and [Lin and Meissner \(2020\)](#) use weekly UI claims data to study the effect of stay-at-home policies on UI claims. [Baek et al. \(2020\)](#) find a positive effect of stay-at-home orders, attributing 25% of the rise in UI claims between March 14, 2020 and April 4, 2020 to stay-at-home policies. In contrast, [Lin and Meissner \(2020\)](#) find that stay-at-home orders *decrease* UI claims. [Goolsbee and Syverson \(2020\)](#) study the effects of county-level shelter-in-place orders on consumption using cellphone tracking data, and attribute only 12% of the consumption decline to these legal restrictions. Other recent work examining the sources of economic decline during the pandemic include [Chetty et al. \(2020\)](#), [Allcott et al. \(2020\)](#), and [Murray and Olivares \(2020\)](#). All of these papers find that a relatively small share of the overall economic decline can be attributed to state-level

restrictions.¹ Our paper extends this existing work on NPIs in several ways. First, we offer evidence on the individual causal effects of a broader set of six different NPIs on unemployment. Second, we provide estimates using a daily outcome measure (the Google Trends data), which allows us to more precisely identify effects and better account for unobservable differences in the pandemic’s progression across different states. Third, we are able to use more granular timing variation in NPI announcements to estimate the effects of multiple NPIs jointly, which corrects for correlation in NPI announcement dates and reveals that individual NPIs have smaller effects on UI claiming than single-NPI analyses may suggest.

We also contribute to broader empirical work on labor market during COVID-19 pandemic. [Bartik et al. \(2020a\)](#) and [Kahn et al. \(2020\)](#) study work hours and job postings respectively, and find that employee hours and job postings were reduced over the course of the pandemic. [Dingel and Neiman \(2020\)](#) provide estimates of the share of jobs can be performed from home, [Mongey et al. \(2020\)](#) use SafeGraph data to show how workers’ ability to work from home affects their ability to practice social distancing. [Coibion et al. \(2020\)](#) find that job loss during the pandemic has been higher than implied by new UI claims and that many individuals who lost their jobs are not actively looking for work.² These empirical papers and ours complement a body of work that simulates the macroeconomic consequences of the pandemic and calibrates the effects of potential policies ([Atkeson, 2020](#); [Bethune and Korinek, 2020](#); [Eichenbaum et al., 2020](#); [Jordà et al., 2020](#); [Glover et al., 2020](#); [Guerrieri et al., 2020](#); [Krueger et al., 2020](#); [Ludvigson et al., 2020](#); [Rampini, 2020](#)). Our paper provides estimates of the labor-market effects of several of these policies and can be used to inform the parameter inputs of these models.

Lastly, we build on work that has used Google search data to study questions that are difficult to study with more traditional survey and administrative datasets. Our work is most closely related to [Goldsmith-Pinkham and Sojourner \(2020\)](#) who use Google search volumes to forecast UI claims during the COVID-19 pandemic.³ Our work is an example of how Google Trends data can be combined with policy variation to infer causal effects that are difficult to estimate using data from more traditional

¹In a related historical paper, [Correia et al. \(2020\)](#) study the 1918 Flu Pandemic and find that early and aggressive implementation of NPIs was not associated with negative economic effects and may have been associated with faster economic growth after the pandemic.

²Further work has studied the the relationship between the COVID-19 pandemic and short-term aggregate economic activity ([Lewis et al., 2020](#); [Mulligan, 2020](#)), consumption ([Baker et al., 2020b](#)), heterogeneity across firms ([Bartik et al., 2020b](#); [Hassan et al., 2020](#)), and economic uncertainty ([Baker et al., 2020a](#)).

³In earlier work by [Baker and Fradkin \(2017\)](#) estimate measures of job search intensity based on Google Trends and other data to study the consequences of UI policy changes.

sources. In addition, we introduce a method that augments the utility of high-frequency proxies for estimating the causal effects of policies. Our method could be employed in other contexts by policymakers and researchers to assess the real-time impacts of policies introduced in close succession.

The remainder of this paper proceeds as follows. We provide background information on the COVID-19 pandemic and NPI responses to the pandemic in Section 2. We then describe our data in Section 3. We describe our conceptual framework in Section 4 and our empirical strategy in Section 5. We present our results in Section 6. In Section 7, we conclude with a brief discussion of the interpretation of our results.

2 Background

2.1 The COVID-19 Pandemic in the U.S.

In January 2020, coronavirus disease 2019 (COVID-19), an infectious disease caused by severe acute respiratory syndrome coronavirus 2 (SARS-CoV-2) spread to the United States. COVID-19 is a highly infectious disease: most studies suggest that its basic reproduction number (R_0) is 2.2-2.7 (Du et al., 2020; Riou and Althaus, 2020; Wu et al., 2020); others report estimates as high as 5.7 (Sanche et al., 2020). Its symptoms include fever, cough, shortness of breath, difficulty breathing, chills, muscle pain, headache, sore throat, and new loss of taste or smell (Centers for Disease Control and Prevention, 2020c). COVID-19 can cause a wide spectrum of disease, including mild illness, moderate and severe pneumonia, respiratory failure, and death (Centers for Disease Control and Prevention, 2020b). To date, 1.19 million cases and 68,551 deaths have been reported in the U.S. (Centers for Disease Control and Prevention, 2020a).

2.2 Non-Pharmaceutical Interventions

Currently no vaccine or specific treatment exists for COVID-19 (Centers for Disease Control and Prevention, 2020d). U.S. states and cities have adopted NPIs to mitigate the spread of COVID-19. These include stay-at-home orders, mandatory quarantines for travelers, non-essential business closures, large gatherings bans, school closures, and restaurant and bar limitations. By April 20, 2020, all U.S. states with the exceptions of Arkansas, Iowa, Nebraska, North Dakota, South Dakota, and Wyoming have issued some form of a stay-at-home order. By the same time, all states with the exceptions of Arkansas, Minnesota, Nebraska, South Dakota, Texas, Utah, and Wyoming had announced some form of non-essential business closures. Strict restaurant and bar limitations had

been imposed in all states with the exception of South Dakota. All other states had closed restaurants and bars except for takeout and delivery, with the exceptions of Kansas and New Mexico which allowed limited on-site service and Oklahoma where restaurants and bars were only limited to takeout and delivery in affected counties (Kaiser Family Foundation, 2020).

Importantly for our analysis, while almost all states eventually announced these NPIs, initial announcements were staggered. For example, restaurants and bars were limited to takeout and delivery in 35 states by March 18, while 4 states still had restaurants and bars operating normally a week later. Likewise, 7 states announced non-essential businesses closures as early as March 20, whereas 16 states had not announced non-essential business closures by April 1st. Figure 1 shows the distribution of announcement dates for each NPI over time and the pairwise correlation across states of these dates. While announcement dates are positively correlated, the correlation is weak in most cases. (Appendix Table A1 shows the announcement date for each state and each NPI. Appendix Figure A1 provides information about the geographic distribution of announcement dates in heatmap form.)

3 Data

We combine data on internet searches from Google Trends, data on NPI announcement dates, as well as state economic data (e.g., industry composition) and data on the spread of COVID-19.

3.1 Google Search Data

We use data on Google searches for the term “file for unemployment” from February 1 to April 24, 2020.⁴ We download these data from Google Trends, which releases data on relative search intensities by search term, day, and geographic location. Because Google only releases relative search volumes, throughout our analysis we will normalize search volumes such that the highest volume day in California during our time period is set to 100.⁵ Because the Google Trends API draws a different sample of data for each request, we download and average 100 samples for each state to mitigate sampling variation. Appendix Figure A2 summarizes the overall evolution of Google searches for for

⁴88% of internet searches in the U.S. happen on Google (Statcounter GlobalStats, 2020).

⁵As explained by Google Trends, “Search results are normalized to the time and location of a query by the following process: Each data point is divided by the total searches of the geography and time range it represents to compare relative popularity. Otherwise, places with the most search volume would always be ranked highest. The resulting numbers are then scaled on a range of 0 to 100 based on a topic’s proportion to all searches on all topics.” (<https://support.google.com/trends/answer/4365533?hl=en>) A more specific explanation by Seth Stephens-Davidowitz reads: “It takes the percent of all searches that use that term; then divides by the highest number. So if 1 percent of searches include coronavirus in City A and 0.5 percent of searches include coronavirus in City B, city B will have a number half as high as City A.” (https://twitter.com/SethS_D/status/1238844534534045697) Stephens-Davidowitz and Varian (2015) provide a detailed description of how Google Trends data can be accessed and used for social science research.

claiming unemployment insurance during March and April, 2020.

We note that there are three benefits that arise from using a proxy outcome like the Google trends data. First, the search data are accessible in real time, whereas data on a target outcome (here, UI claims) is often only available with some delay. Second, the daily nature of the search data facilitates a more convincing research design, allowing us to trace a more detailed pattern of policy responses in both the pre- and post-periods, rather than just relying on a couple of points before and after each policy. Lastly, daily search data is useful for disentangling policy effects when the policies themselves are implemented at similar times. In our context, the daily search data allows us to disentangle the different effects of multiple policies that were announced and implemented within days of each other, to assess which policies have higher or lower costs in terms of unemployment, which can then be weighed against each policy’s public health effectiveness. To our knowledge, ours is the only study that disentangles the heterogeneous effects of as many as six different NPIs.

3.2 NPI Timing Data

We use data released by [Kaiser Family Foundation \(2020\)](#) to identify which states have announced each of the six NPIs we study: restaurant and bar limitations, non-essential business closures, stay-at-home orders, large-gatherings bans, school closures, and emergency declarations. For each state and NPI, we identify the precise date on which the NPI was first announced. In cases where multiple announcements pertained to the same NPI, we use the first recorded announcement. For a list of all NPI introduction dates by state, see [Table A1](#).

3.3 Other Data

We use confirmed COVID-19 cases and deaths from [Dong et al. \(2020\)](#) and [Johns Hopkins University \(2020\)](#). Total initial UI claims filed at a national level between March 14 and March 28 are derived from weekly news releases from the [U.S. Department of Labor \(2020\)](#). Industry employment shares at the national and state levels are computed from the Quarterly Census of Employment and Wages ([Bureau of Labor Statistics, 2020](#)) and from the 2013-2017 American Community Survey ([U.S. Census Bureau, 2020](#)). We use data on industry-level unemployment growth from March 14-28 from three states: Massachusetts ([Massachusetts Executive Office of Labor and Workforce Development, 2020](#)), New York ([New York State Department of Labor, 2020](#)), and Washington ([Washington State Employment Security Department, 2020](#)).

4 Conceptual Framework

Our empirical analyses are built on a conceptual framework where firms internalize the information contained in NPI announcements about their optimal employment level and workers have rational expectations of firm layoff decisions. Changes in workers' expectations of their layoff probability then lead to a rapid response in Google search behavior, which is the proxy outcome we measure. This conceptual model predicts that Google searches by workers not only respond to actual layoffs but also shifts in their *expectations* of impending layoffs.⁶

Our model requires that firms internalize the information contained in NPI announcements. Policies like restaurant and bar limitations and non-essential business closures directly reduce affected firms' future demand. In the presence of layoff/re-hiring costs, this leads to a reduction in affected firms' *current* optimal employment level. For example, a retailer may not lay off workers if demand may rebound in the following month, but may be willing to incur the adjustment costs of re-hiring workers later if demand were assuredly low due to a non-essential business closure policy.⁷

Workers who are not immediately laid off are assumed to anticipate the employment responses of their employers. For example, a waiter who hears the announcement of restaurant and bar limitations would seek out information on claiming UI. If some affected workers delay their search behavior, we may not be able to detect their responses depending on the length of our event study window.

One concern about this approach is that workers' expectations may not be correct: for example they under- or overestimate the change in their likelihood of unemployment when an NPI is announced and their internet search behavior may reflect such an under- or overreaction. This is only a problem for our approach to the extent that the response in internet search behavior around NPI announcements is biased in a way that is different from the bias associated with searches occurring for other reasons during our period. As long as workers are under- or over-reacting to NPIs and other economically relevant factors in the same way, our estimates remain unbiased.

⁶This focus of our model on worker and firm expectations helps to differentiate the effects of NPIs on layoffs from papers that show early reductions in hours and job postings (Bartik et al., 2020a; Kahn et al., 2020), outcomes that may be more responsive to short-run demand conditions.

⁷The importance of firms' demand expectations is magnified by the liquidity constraints faced by the typical small business: Bartik et al. (2020b) employ surveys of small businesses and find that 72% of business owners expect to re-open in December 2020 if the pandemic lasts 1 month, with this percentage dropping to 47% if the pandemic lasts 4 months.

5 Empirical Strategy

A characteristic of the economic downturn associated with the COVID-19 pandemic, and a common feature of many crises, is that the effects of particular policy responses are hard to isolate. We employ high-frequency proxy data from Google Trends to separately identify effects of policies released just days apart from each other and detect rapid changes in workers' behavior and expectations.

In addition to estimating causal effects of NPIs on Google searches, we also develop a new method to translate these estimates into causal effects on UI claims. In contrast to prior work using proxies for economic variables (e.g., [Goldsmith-Pinkham and Sojourner, 2020](#); [Baker and Fradkin, 2017](#)), our method only requires one data point on UI claims: the total number of claims filed between March 14 and 28. This is because we do not directly estimate the relationship between Google searches and UI claims. Instead, we employ alternative assumptions to first estimate the *share* of UI claiming caused by NPIs, which we multiply by the total March 14-28 change in UI claims to obtain the effect in level terms. Our method allows for policy effect estimation using proxy data where data on the variable of interest are limited (because of low-frequency measurement, small samples, or measurement error) but where the researcher can assume that causal effects satisfy certain assumptions.

5.1 Single-Policy Event Study

To quantify the impact of a given NPI on search volume, our baseline specification below is an event study regression that exploits differential NPI announcement dates across different states. Our main specification is of the form:

$$S_{it} = \sum_{\tau=-7}^6 \gamma_{\tau} \times 1\{r = \tau\} + \alpha_i + \alpha_t + \varepsilon_{it} \quad (1)$$

where S_{it} is Google search volume in state i and date t , r denotes the days relative to the date the policy was announced (which we define as day $r = 0$), and α_i and α_t are state and calendar date fixed effects. The coefficients of interest γ_{τ} estimate the differential increase in search volume for each day τ relative to the day prior to the announcement date ($r = -1$). We normalize $\gamma_{\tau=-1} = 0$ and cluster standard errors at the state level.

For periods $r > 6$ and $r < -7$, we assign $r = 6$ and $r = -7$ respectively. This follows from the assumption that the dynamic effects of the policy are constant 7 days after the policy announcement and prior to 7 days before the policy announcement. Estimation of the γ_{τ} coefficients for earlier pre-

periods would rely only on comparisons between states that announced the NPI early (the “treated” group for that pre-period) and states that announced the NPI at least one week later (the “control” group for that pre-period). An analogous logic holds for later post-periods. Given that the NPIs were introduced at very similar times⁸, states that announced policies more than a week apart are likely unobservably different from each other. Moreover, separately identifying calendar date fixed effects and the γ_τ coefficients for these earlier and later periods relies on an increasingly sparse (and selected) set of “treated” and “control” states.⁹

5.2 Multiple-Policy Event Study

The standard event study approach described above estimates the effect of a single NPI on search volume. However, during the COVID-19 pandemic, many states announced multiple NPIs simultaneously or in close proximity to each other. Correlation among NPIs may lead the single-NPI event studies to overstate the impact of each NPI (Figure 1 shows the correlation patterns between the six NPIs we consider). However, running an event study that includes all possible policies may not be feasible due to potential collinearity. To address both of these issues, we first estimate single-policy event studies for each of the six NPIs we consider. To account for correlated NPI announcements, we then estimate a multiple-policy event study that includes the subset of NPIs that exhibited significant effects in the single-policy estimation. This specification takes the form:

$$S_{it} = \sum_{p \in \mathcal{P}} \sum_{\tau=-7}^6 \eta_{p,\tau} \times 1\{r(p) = \tau\} + \alpha_i + \alpha_t + \nu_{it} \quad (2)$$

where S_{it} is Google search volume in state i and date t , \mathcal{P} denotes the set of included policies, $r(p)$ denotes the days relative to the date that policy p was announced (which we define as day $r = 0$), and α_i, α_t are state and calendar date fixed effects respectively. The coefficients of interest $\eta_{p,\tau}$ estimate, for each policy p , the increase in search volume for each day τ relative to the day prior to the announcement date of each policy ($r(p) = -1$), controlling for the time-varying effects of the other policies in \mathcal{P} . We normalize $\eta_{p,\tau=-1} = 0$ for all policies p and cluster standard errors at the state level. Under the multiple-policy specification, we can estimate each policy’s independent impact on search volume, controlling for the other policies that demonstrated an effect in the single-policy specification.

⁸The inter-quartile range of introduction dates is between 3 and 8 days for all of the policies we consider

⁹An alternative approach would be to drop data corresponding to $r < -7$ and $r > 6$, but while this “balances” the data in event time, the data becomes unbalanced in calendar time, certain calendar date fixed effects may no longer be separately identified from the γ_τ coefficients, and the reduction in sample size reduces statistical power.

5.3 Robustness

We discuss potential concerns and assess the robustness of our results in a number of ways.

First, certain states had a large number of cases early on (e.g., California and Washington) or were particularly strongly hit by the pandemic (e.g., New York). To address the concern that our results are driven by these states, we re-estimate the event study and the difference-in-differences specification excluding these three states.

Second, another concern is that our results may be driven by smaller states whose UI responses or economic trajectories may differ from larger states. To address this concern, we re-estimate our results weighting each state by its total employment.

Third, it is likely that NPI policy announcement dates are correlated with characteristics of the pandemic in each state. This would pose a problem to our identification strategy only if individuals modified their UI claiming behavior (and hence their Google search behavior) in response to their states' disease trajectory. To address this concern, we re-estimate our single-policy event study specification (Equation 1) with additional controls for case growth and deaths at the state-calendar date level, both interacted with state dummies:

$$S_{it} = \sum_{\tau=-7}^6 \tilde{\gamma}_{\tau} \times 1\{r = \tau\} + \tilde{\beta}_i \times \text{Case Growth}_{it} + \tilde{\delta}_i \times \text{Deaths}_{it} + \tilde{\alpha}_i + \tilde{\alpha}_t + \tilde{\varepsilon}_{it} \quad (3)$$

Case Growth_{it} is defined as the additional cases in state i in calendar date t relative to the previous day and Deaths_{it} is defined as the cumulative deaths in state i at calendar date t . Interacting both variables with state dummies allows the effects of case growth and deaths (captured by $\tilde{\beta}_i$ and $\tilde{\delta}_i$ respectively) to vary by state. This specification assesses whether our results are driven by differential case growth or deaths.

We also show that epidemiological outcomes are not changing rapidly around the exact timing of NPI announcements by replacing the outcome variable in our event study (Equation 1) with case growth and deaths.

Fourth, to further demonstrate that the NPI timing variation we use is not driven by the different epidemiological experiences of each state, we separate states into those that registered their first COVID-19 death early in the epidemic (on or before March 19) and those that registered their first COVID-19 death later (after March 19). We use March 19 as the cutoff date because it is the median

date of the first COVID-19 death across states. We then estimate the event study specification separately for both sets of states.

Fifth, one concern with event study approaches is that the same sets of states are used as “treated” and “control” states for various relative days. In Appendix D, we estimate an alternative difference-in-differences model, where we compare “treated” states that announced their first NPI early vs. “control” states that did not announce any NPI during the timeframe we use for estimation.

Sixth, we assess whether an industry-specific NPI (restaurant and bar limitations) differentially affected states with a higher share of employment in food services. We present the methods and results for this case study in Appendix E.

Seventh, we assess the robustness of our main estimates to controlling for spillovers from policies announced in bordering states. We present methods and results for this analysis in Appendix F.

Eight, also in Appendix F, we conduct exploratory analyses to assess the importance of potential cross-state spillovers in explaining UI claims. While own-state effects are the focus of our analysis, the magnitude of spillover effects across state borders may be of interest (for example, for informing coordination between states).

5.4 Quantifying the Impact of Individual NPIs on UI Claims

We rely on the event study specifications described above to partition the evolution of search volume into the causal effects of the NPIs and an aggregate time trend. We assume that, for each state, the (normalized) quantity of UI claims in a given period is proportional to the *area* under the curve defined by search intensity over the same period.¹⁰ We also assume that the Google search volume caused by factors other than the NPIs can be estimated by integrating the calendar date fixed effects in the event study. With these assumptions, the integral under the estimated NPI effect (given by the relative-time coefficients γ_τ in Equation 1 and $\eta_{p,\tau}$ in Equation 2) is proportional to the quantity of UI claims caused by the NPI. By comparing this integral to the integral under the time trend (given by the α_t coefficients), we can isolate the direct causal effect of the NPI. In Appendix B, we provide a formal discussion of these assumptions and describe how they allow proxy data to be used to estimate causal effects. In Appendix C, we provide evidence on the proportionality assumption for

¹⁰We do not require that the coefficient of proportionality be known or even estimated. Intuitively, the coefficient of proportionality cancels out in the numerator and denominator of the share expression we construct below. Moreover, the coefficient of proportionality is difficult to interpret, since over any requested time window, the Google Trends data are always normalized so that the maximum search intensity equals 100.

our context.¹¹

Consider the multiple-policy event study specification in Equation 2. Let I_{i,p,t_1,t_2} denote the integral under the $\eta_{p,\tau} \times 1\{r(p) = \tau\}$ terms for a given state i , NPI policy p , and $\tau \geq 0$. Further, let I_{p,t_1,t_2} denote the weighted average of $I_{i,p}$ over all states, using weights proportional to the average number of UI claims filed during the four weeks ending February 22, 2020 through March 14, 2020 and only counting the relative time periods that fall within $[t_1, t_2]$. For example, a non-essential business closure announced on March 25 will only contribute 4 days worth of effects to the share, due to the March 14-28 window. Let I_{α,t_1,t_2} denote the integral under the date fixed effects α_t between t_1 and t_2 (which estimates the pandemic effects not explained by the policies). The share of UI claims between t_1 and t_2 caused by the NPI can be estimated as:

$$\text{Share of UI claims caused by NPI } p = \frac{I_{p,t_1,t_2}}{I_{\alpha,t_1,t_2} + \sum_p I_{p,t_1,t_2}}. \quad (4)$$

Because NPIs can have industry-specific impacts, another quantity of interest is the share of UI claims in a given industry that was caused by the NPI. We describe how this share can be computed in the case of restaurant and bar limitations in Appendix Section E.

Defining the appropriate time window $[t_1, t_2]$ is challenging and will affect estimation of the shares defined above. We estimate the above shares for our six policies of interest using a window of $t_1 =$ March 14, when the first states began announcing NPIs, through $t_2 =$ March 28, approximately the time that the final states began announcing NPIs (see Figure 1 and Table A1). This also allows us to simply utilize two periods worth of the weekly UI claims data, avoiding the need for interpolation. Given the short period during which most states announced their first NPIs, we can evaluate all six policies using the same denominator for Equation 4.

In Appendix G we provide a detailed discussion of model uncertainty and inference on the shares of UI claims explained by NPIs. We report results from a cluster bootstrap procedure to generate confidence intervals for our share estimates and explore the implications of various modeling choices for the distribution of our estimates.

¹¹For more evidence on the ability of Google Trends data to predict UI claims, see [Goldsmith-Pinkham and Sojourner \(2020\)](#).

5.5 Identification

Our key identifying assumption is that the observed untreated outcomes of states that are not (yet) treated (states that announce their policy later) are a good counterfactual for the states announcing the policy on a given date. For example, this requires that states do not get an unobserved shock that impacts Google search behavior (e.g., new information on state-specific pandemic severity) at the time of the policy announcement. In support of this identifying assumption, we find flat pre-trends in each of our event studies (Figure 2), our results are robust to controls for epidemic severity (Figure 3), and epidemic severity does not change rapidly near NPI announcements (Appendix Figure A5).

Another source of bias would arise if workers anticipated the announcement and implementation of the NPIs. To the extent that anticipation led to consistently higher Google searches in the pre-period, our estimates of the causal policy effect will be biased toward zero. However, our results would still be policy-relevant: our estimates describe the effect of a policy *taking as given* firm and worker expectations. The policy-relevant treatment effect of the intervention accounts for the possibility that the policy results in a smaller increase in UI claims because firms had already laid off workers in anticipation of the policy. That said, the pre-trends of our event studies (Figure 2) suggest little anticipatory effect in Google searches before the policies are actually announced.

6 Results

6.1 Estimates from the Single-Policy Event Study

Figure 2 shows our event study estimates for each of the NPIs. The first columns of Appendix Tables A2-A7 show the corresponding event study coefficients. Figure 2 suggests that there is no differential trend in Google search volume for UI claiming prior to the announcement of any of the NPIs. For restaurant and bar limitations, there is an approximately 15.2 unit (24.3%) average increase in Google search volume on relative day 1 (the first day following the announcement date). For non-essential business closures the, increase is 29.9 units (48%), and for stay-at-home orders it is 22.8 units (34.9%). Percentage increases are computed relative to the mean search volume over the March 14-28 period.¹² After these initial increases, search volumes return to their pre-announcement levels. This may reflect an “impulse response” effect of announcements: workers affected by the NPIs may search online

¹²This is the most relevant normalization because it allows us to use a single period as a benchmark for different NPIs introduced at different times and also circumvents the issue that search volume for UI claiming is very low and sometimes not reported by Google in the preceding period.

intensively at first but then search less after they locate the appropriate resources for filing a UI claim. We see no comparable increase in search volume after the announcement of large-gatherings bans, school closures, and emergency declarations. Our interpretation is that these NPIs, announced in the same time frame, did not change unemployment expectations and did not directly increase UI claiming.

6.2 Estimates from the Multiple-Policy Event Study

Figure 4 and Table A8 report our event study results when we include multiple policies at the same time. Based on results reported in Section 6.1, we focus on the three NPIs that seem to have individual impacts: restaurant and bar limitations, essential business closures, and stay-at-home orders. Once we control for the presence and timing of the other policies, the impacts of restaurant limitations and non-essential business closures appear to be slightly smaller. Stay-at-home orders are no longer estimated to affect internet search volume because their timing is correlated with the timing of non-essential business closures. An insight from these results is that when estimating the contribution of individual policies, it is important to control for the presence and timing of other correlated policies.

6.3 Robustness

To assess the robustness of our results, we estimate our event study on alternative samples and using alternative specifications. We estimate our results (i) excluding California, New York, and Washington, three states hit hard or early on by the pandemic, (ii) weighting states by their total employment, (iii) controlling for case growth and the number of deaths, (iv) on the sample of states with early first deaths, (v) on the sample of states with late first deaths. Figure 3 summarizes our results, showing coefficient estimates and standard errors for day 1 from the event study, the first full day after each announcement date. Our results are very similar under these different specifications and when estimated on alternative samples, although they are sometimes noisier on smaller samples. (Appendix Figures A3 and A4 show full event studies for each of the six policies and each of the alternative specifications and samples. Columns 2-6 of Appendix Tables A2-A7 show the corresponding event study coefficients.)

To examine whether the exact timing of the introduction of NPIs coincides with epidemiological events that potentially provide information to the public about the spread of the pandemic, Appendix Figure A5 shows the evolution of case growth and the number of deaths relative to the announcement of NPIs. We find no evidence that the announcement of NPIs is preceded or followed by jumps in these

outcomes. (Appendix Tables A9 and A10 show the corresponding event study coefficients.) Note that this should not be taken as evidence that NPIs don't impact case growth or the number of deaths. Our estimates only show that controlling for overall time-trends, there is no *immediate* impact in our time frame; the effects of NPIs on cases and 7 deaths would be expected to emerge later.

We present difference-in-differences estimates comparing “early adopters” (first NPI announced between March 13-17) with “late or never adopters” (after March 1 or never) in Appendix D. Figure D1 and Appendix Table D1 show that trends for early and late adopters are identical until the first NPI announcement, at which point the early adopter states experience a jump in search volume that is sustained through additional announcements by early adopter states. The overall differential increase in search volume in early adopter states is 13%. Importantly, late adopter states also have increasing search volume: this underscores the idea that most UI claiming is not the direct effect of NPI adoption.

In our case study of the Accommodation and Food Services industry, presented in Appendix E, we show that the effects of restaurant and bar limitations are driven by states with high food services employment. Appendix Figure E1 and Appendix Table E1 show the event study estimates separately for states with high (above-median) and low (below-median) food service employment shares. The point estimates suggest that the effect of restaurant and bar limitation announcements is larger for states with a high share of their residents employed in food service, though this difference is not statistically significant. We estimate that the Accommodation and Food Services industry accounts for about 25% (2.5 million) of all initial UI claims filed between March 14 and 28. However, the policy of restaurant and bar limitations can account for only 24% of this effect (about 612,000 claims).

In Appendix F we provide preliminary estimates of cross-state spillovers due to NPI announcements. First, Appendix Figure F1 shows that our estimates of direct, own-state effects remain robust to the inclusion of border-state NPI announcements. Second, we provide suggestive evidence on the magnitude of spillover effects from NPIs on bordering states. We find that these effects are relatively modest, but could be of interest for coordination between states.¹³

6.4 Estimates of NPI Impacts on UI Claims

We use the method outlined in Section 5.4 to compute the share and number of UI claims caused by the six NPIs. The results from our main specification (see Figure 2) suggest that only restaurant and bar limitations, non-essential business closures, and stay-at-home orders have statistically significant

¹³In Appendix G and in particular Figure G4, we show that the implied shares of UI claims associated with each direct policy effect are also unchanged.

effects on search volume. To address the positive correlation between these policies (shown in Figure 1), we use the multiple-policy event study given by Equation 2 to estimate I_{p,t_1,t_2} , the area under the event study coefficients for each policy p and time window $[t_1, t_2]$, for the three policies above and $[t_1, t_2] = [\text{March 14}, \text{March 28}]$. Panels (a), (b), and (c) of Figure 4 graphically illustrate this calculation. Panel (d) shows I_{α,t_1,t_2} , the area under the time fixed effects.

Figure 5 shows the average direct, own-state NPI effects across all states, accounting for differences in states' NPI announcement timings and weighting by prior UI claims. The figure reflects the relative timing of NPI announcements across states: restaurant and bar limitations usually preceded non-essential business closures and stay-at-home orders. The figure also demonstrates that our chosen time window (March 14-28) captures the bulk of the impulse-response effects across states. There is some evidence of persistent effects of NPIs on search volume lasting into April, but these rely heavily on the event study coefficient for the last relative time period. Our estimates of $I_{p,\text{Mar14}, \text{Mar28}}$ are equivalent to integrating under this curve between March 14 and March 28.

We obtain $I_{p,\text{Mar14}, \text{Mar28}} = 58.1$ for restaurant and bar limitations, $I_{p,\text{Mar14}, \text{Mar28}} = 62.5$ for non-essential business closures, and $I_{p,\text{Mar14}, \text{Mar28}} = -0.5$ for stay-at-home orders, and we compute $I_{\alpha,t_1,t_2} = 851.5$. These values imply that restaurant and bar limitations, non-essential business closures, and stay-at-home orders account for 6.0%, 6.4%, and 0.0% of all UI claims between March 14 and March 28, respectively. Under the single-policy event study design, we would have mistakenly inferred that restaurant and bar limitations, non-essential business closures, and stay-at-home orders account for 6.7%, 7.6%, and 4.5% of all UI claims from March 14-28. We conclude that the six NPIs we consider directly account for 12.4% of the rise in own-state UI claims during this period and that failing to control for multiple correlated NPI introductions will tend to inflate the estimated importance of individual NPIs (in our case, by 6.4 percentage points or just over 50%).

In Appendix G, we use a cluster bootstrap procedure to assess the impact of sampling error on our share estimate. Once we account for sampling error, we are still able to rule out direct, own-state NPI effects above 38% of UI claims. We also find that the share estimate for restaurant and bar limitations exhibits significantly more variation than the other policies. Lastly, we find that the distribution of the direct, own-state share of UI claims due to NPIs is robust to whether or not we include spillovers and whether or not we exclude own-state stay-at-home orders (see Appendix Figures G5 and G6).

7 Discussion

In March 2020, as the COVID-19 pandemic spread through the U.S., state governments issued emergency declarations, limited business operations, closed schools, and imposed social distancing measures. At the same time, unemployment insurance claims skyrocketed and reached their highest levels since 1982. In this paper, we present the first estimates of the combined and individual effects of six NPIs on UI claims. We disentangle the effects of multiple NPIs using high-frequency Google search data to proxy for UI claims, increasing our ability to leverage small differences in policy timing. We describe a method and set of assumptions that allows proxies to be used for policy evaluation when data on the outcome of interest are limited. With the increasing need to measure policy effects in real time, we hope that our method will complement new sources of high-frequency proxy data, such as SafeGraph data for measuring mobility, Google Trends data for measuring online search, and even high-frequency survey data where the outcomes of interest may need to be proxied using survey questions. Lastly, our method can be employed in other contexts beyond COVID-19, where assessing the real-time impacts of multiple different frequently changing policies is of first-order importance.

Our results imply that most of the short-run increase in own-state unemployment was *not* directly due to the state-level NPIs that we consider. State-level restaurant and bar limitations, non-essential business closures, stay-at-home orders, large-gatherings bans, school closures, and emergency declarations collectively account for 12.4% of the increase in UI claims from March 14-28, 2020. Using high-frequency proxy data, we find that the different policies have heterogeneous effects. Restaurant and bar limitations and non-essential business closures account for 6.0% and 6.4% of the 10.2 million UI claims filed during this period. On the other hand, large-gatherings bans, school closures, and emergency declarations did not significantly impact UI claims. Stay-at-home orders had significant effects when considered in isolation, but their effect disappears after controlling for non-essential business closures. These results point to a large role played by direct pandemic effects, local policies, and spillovers.

At first, the idea that NPIs cannot explain most of the increases in UI claiming seems counter-intuitive. One may have thought that such policies would be responsible for most or even all of the economic decline experienced by U.S. states. However, the evidence presented here is consistent with a growing set of studies that find state restrictions do not explain a large share of this economic decline. In the U.S., a number of papers have documented that economic activity began its steep decline prior

to the introduction of NPIs ([Bartik et al., 2020b](#); [Goolsbee and Syverson, 2020](#); [Murray and Olivares, 2020](#)) and that it has not recovered in states that have relaxed their restrictions ([Chetty et al., 2020](#)). This body of evidence, taken together with the results of our paper, consistently point to a small role for NPIs in the economic decline that began in March, 2020 as a result of COVID-19. This suggests that other factors, including declines in consumer demand, local policies, and policies implemented by private firms and institutions, played a large role in the economic decline and the growth in UI claims. Our paper does not directly quantify these alternative hypotheses.

We caution against using our results to infer the impacts of NPIs in all contexts. At the time of introduction, the exact policy effects we estimate are conditional on pre-existing expectations and policies, including the policies of other states. Future relaxations or re-implementations of NPIs may occur in different contexts. For example, if consumer demand were otherwise high when NPIs were initially implemented, the constraints imposed by the NPIs may have been more binding with larger economic effects as a result. We also note that we are identifying short-run impacts of the policies, not their full impact over a longer time horizon. But our results can inform state policy decisions made against a similar economic backdrop, for example, as consumer demand remains low due to fears of contracting or spreading COVID-19.

Our results can be combined with work on the effectiveness of NPIs on slowing disease spread to identify NPIs that are effective but “inexpensive” from the standpoint of unemployment. For example, [Gupta et al. \(2020\)](#) find that informational NPIs like emergency declarations and school closures had the largest effects on social distancing behavior, whereas we find that these two NPIs had no detectable short-term effects on unemployment.

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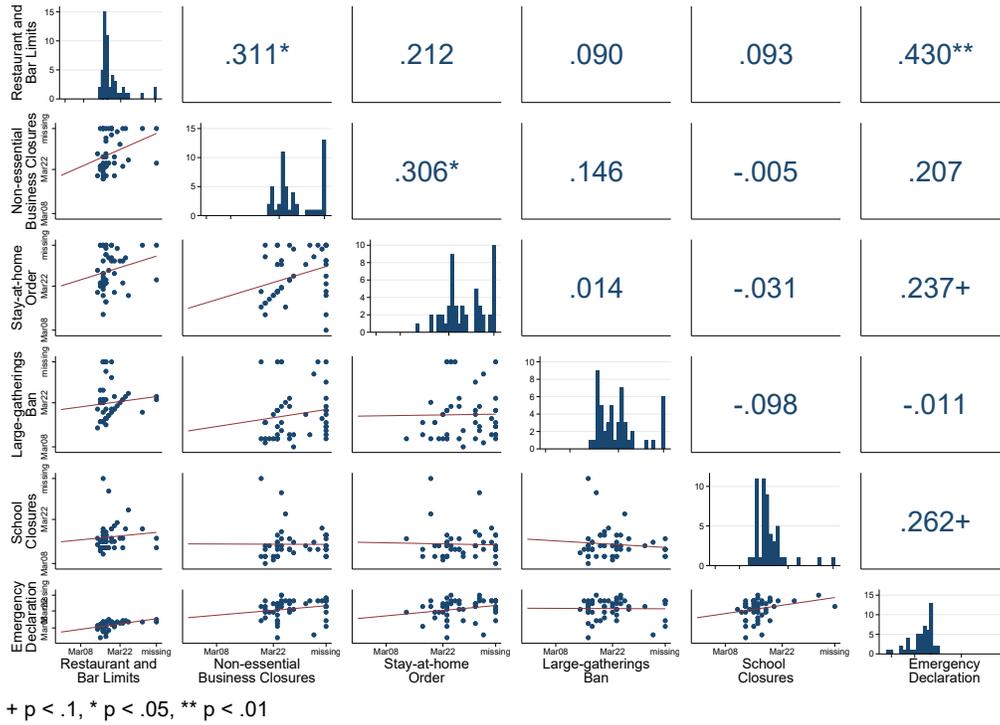
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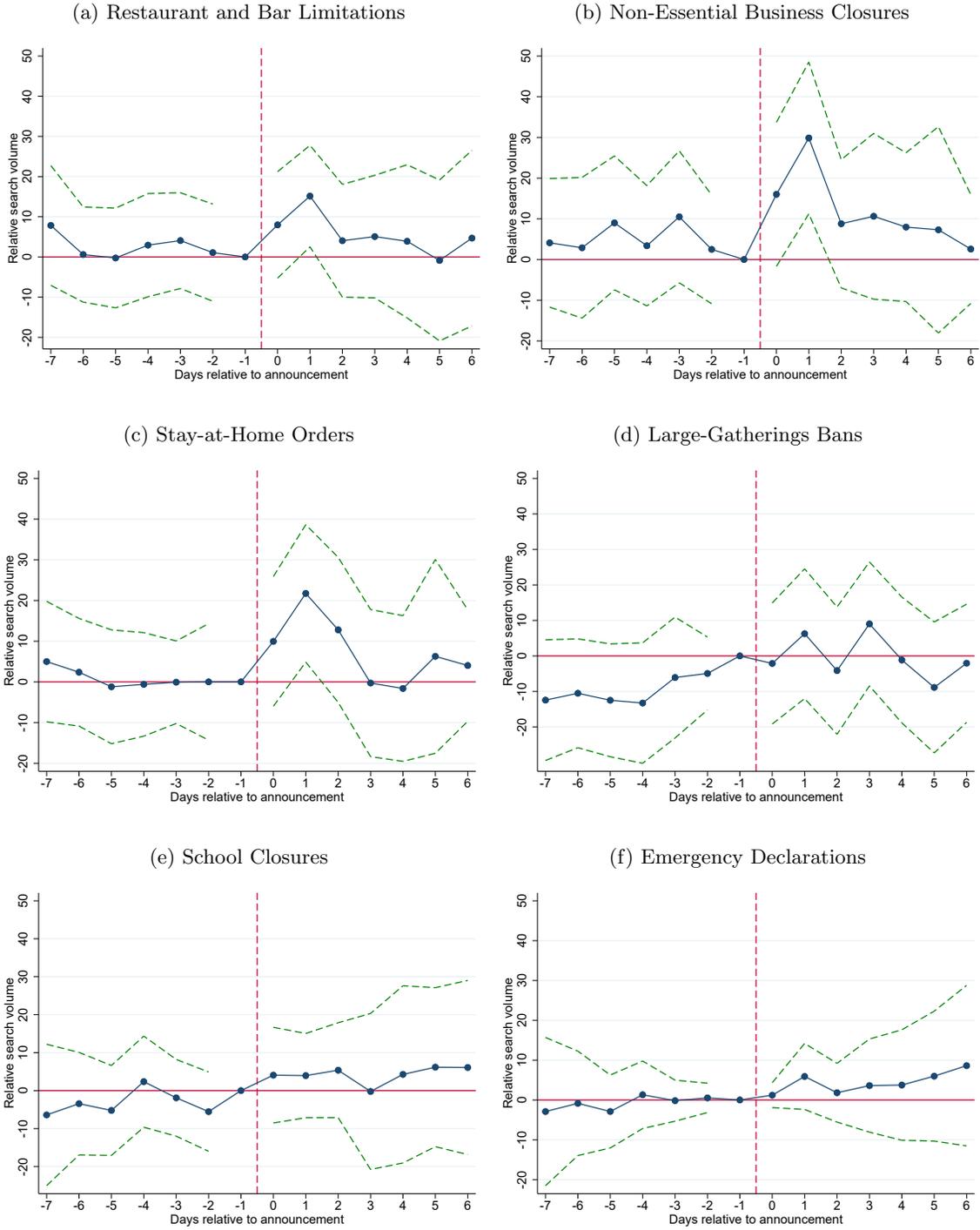
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Figure 1: Timing of NPIs



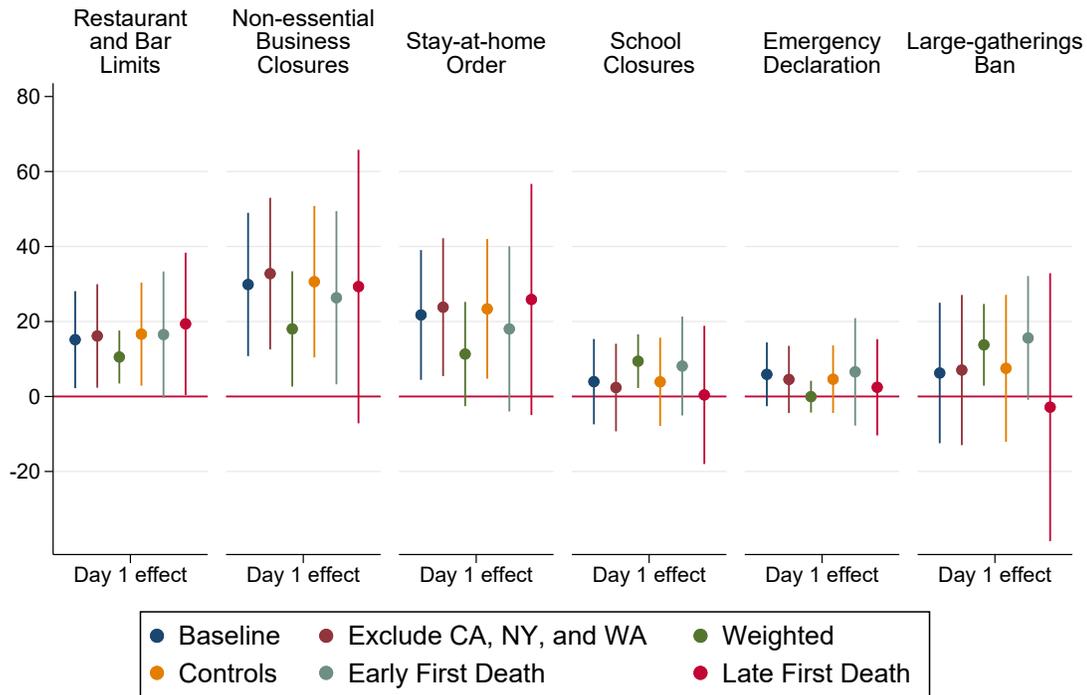
Note: Figure shows the distribution of the announcement dates of restaurant and bar limitations, non-essential business closures, stay-at-home orders, large-gatherings bans, school closures, and emergency declarations on the diagonal. The off-diagonal scatterplots show the cross-state pairwise relationship between the announcement dates for each pair of measures. The red lines are linear fits. The off-diagonal numbers are the corresponding correlation coefficient estimates. For more details, see Section 3.2.

Figure 2: Event Study Estimates



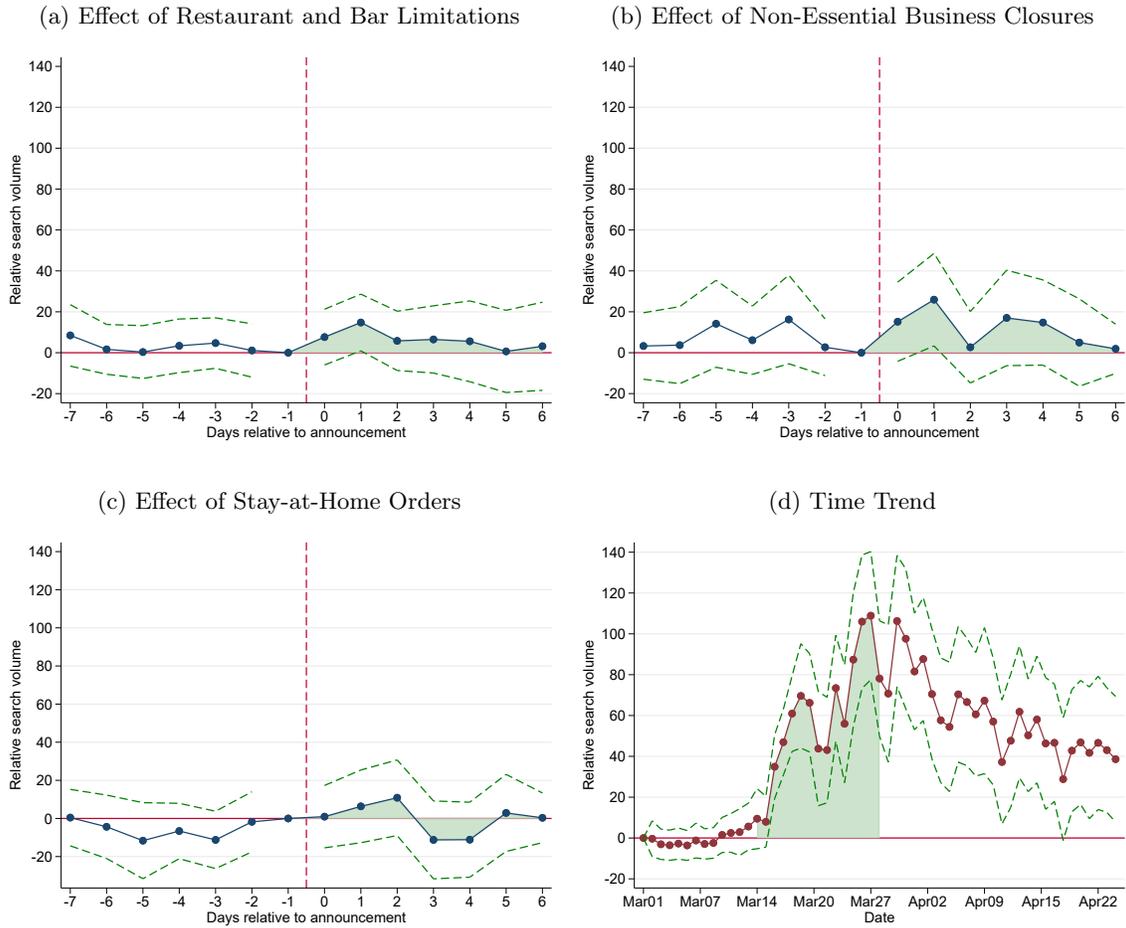
Note: Figure shows event study estimates of the impact of the introduction of restaurant and bar limitations (Panel (a)), non-essential business closures (Panel (b)), stay-at-home orders (Panel (c)), large-gatherings bans (Panel (d)), school closures (Panel (e)), and emergency declarations (Panel (f)), based on Equation (1). The day prior to the announcement is normalized to zero and standard errors are clustered at the state level. For more details, see Section 5.1.

Figure 3: Event Study Estimates: Robustness — Summary



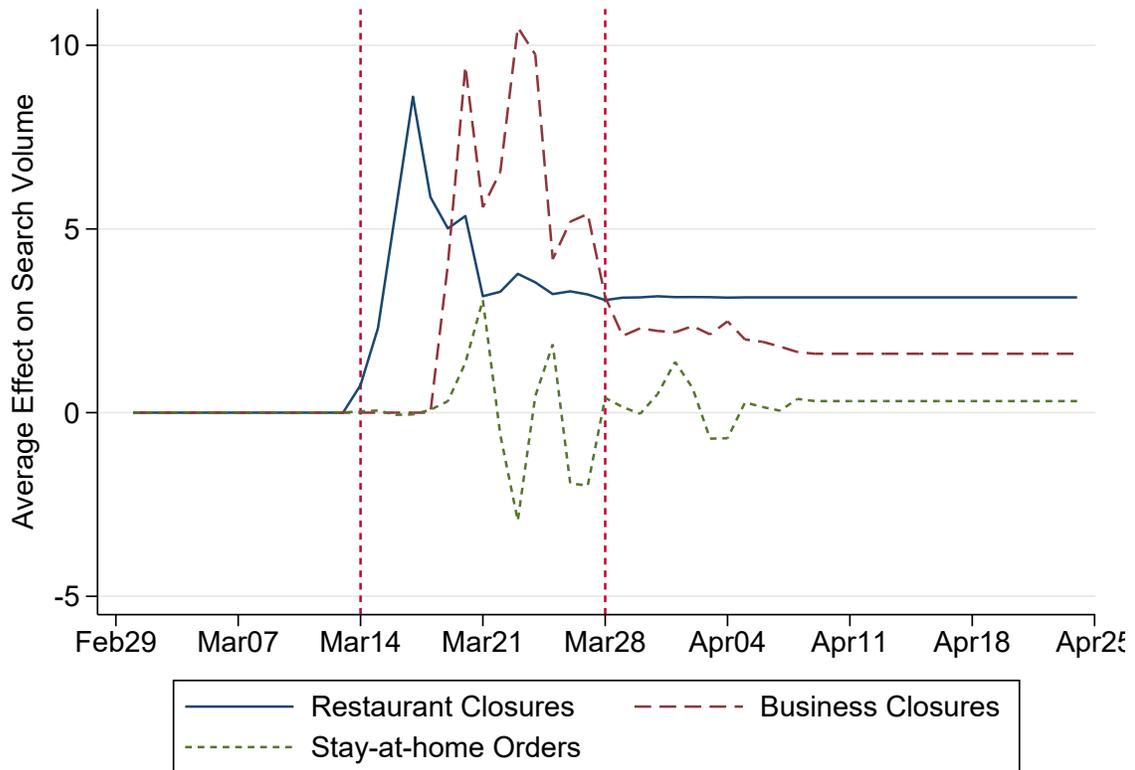
Note: Figure summarizes the results of our event study estimates using alternative samples and alternative specifications, based on Equation 1. We show the coefficient estimate for the day after the announcement date (day 1) from our event study for each of the NPIs (restaurant and bar limitations, non-essential business closures, stay-at-home orders, large-gatherings bans, school closures, and emergency declarations). For each NPI, we show our baseline result (in navy), as well as alternative results (i) excluding California, New York, and Washington, three states hit hard and/early by the pandemic, (ii) weighting states by their total employment, (iii) controlling for case growth and the number of deaths, (iv) on the sample of states with early first deaths, (v) on the sample of states with late first deaths. For more details, see Section 5.3.

Figure 4: Dis-aggregating Unemployment Effects by Policy and Pandemic Causes



Note: Figure shows event study estimates of the impact of restaurant and bar limitations (Panel (a)), non-essential business closures (Panel (b)), and stay-at-home orders (Panel (c)), based on Equation (2) which estimates the impact of the policies jointly. Panel (d) shows estimates of the overall time trend in UI search volume. The areas under the curves represent the share of the growth in UI claims that we attribute to the NPIs (Panels (a)-(c)) and other pandemic effects (Panel (d)). The day prior to the announcement is normalized to zero and standard errors are clustered at the state level. For more details, see Sections 5.2 and 5.4.

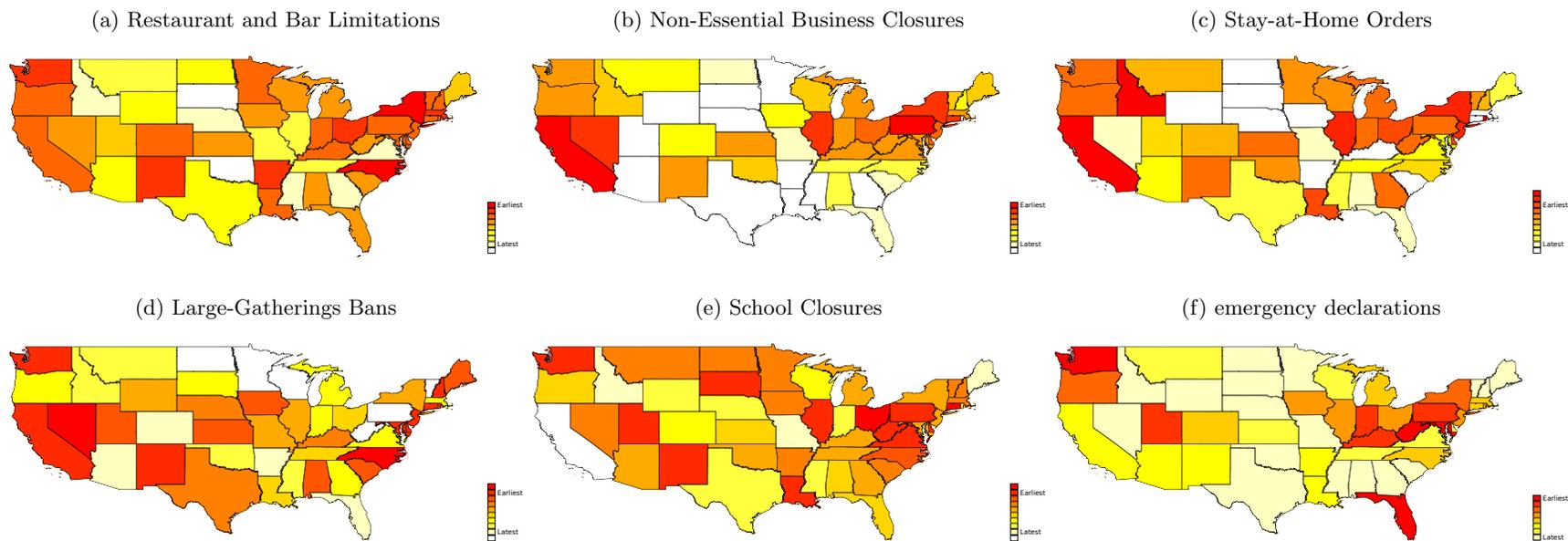
Figure 5: Effects of NPIs Over Time



Note: Figure shows the average effects of each NPI over time, weighted across states using each state's average UI claims prior to our time period of interest (March 14 through March 28, denoted by the vertical dashed lines). Weights are proportional to the average number of UI claims from February 22, 2020 through March 14, 2020 in each state. See Section 5.2 for more details on the underlying event study specification.

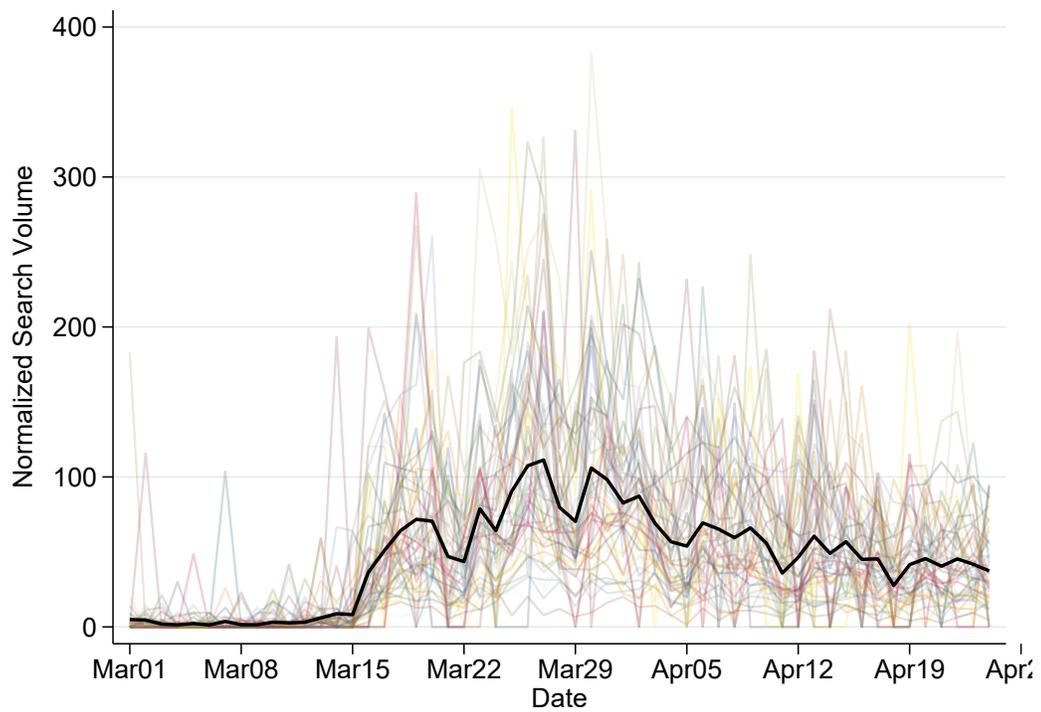
A Additional Figures and Tables

Appendix Figure A1: Geographic Distribution of NPI Adoption



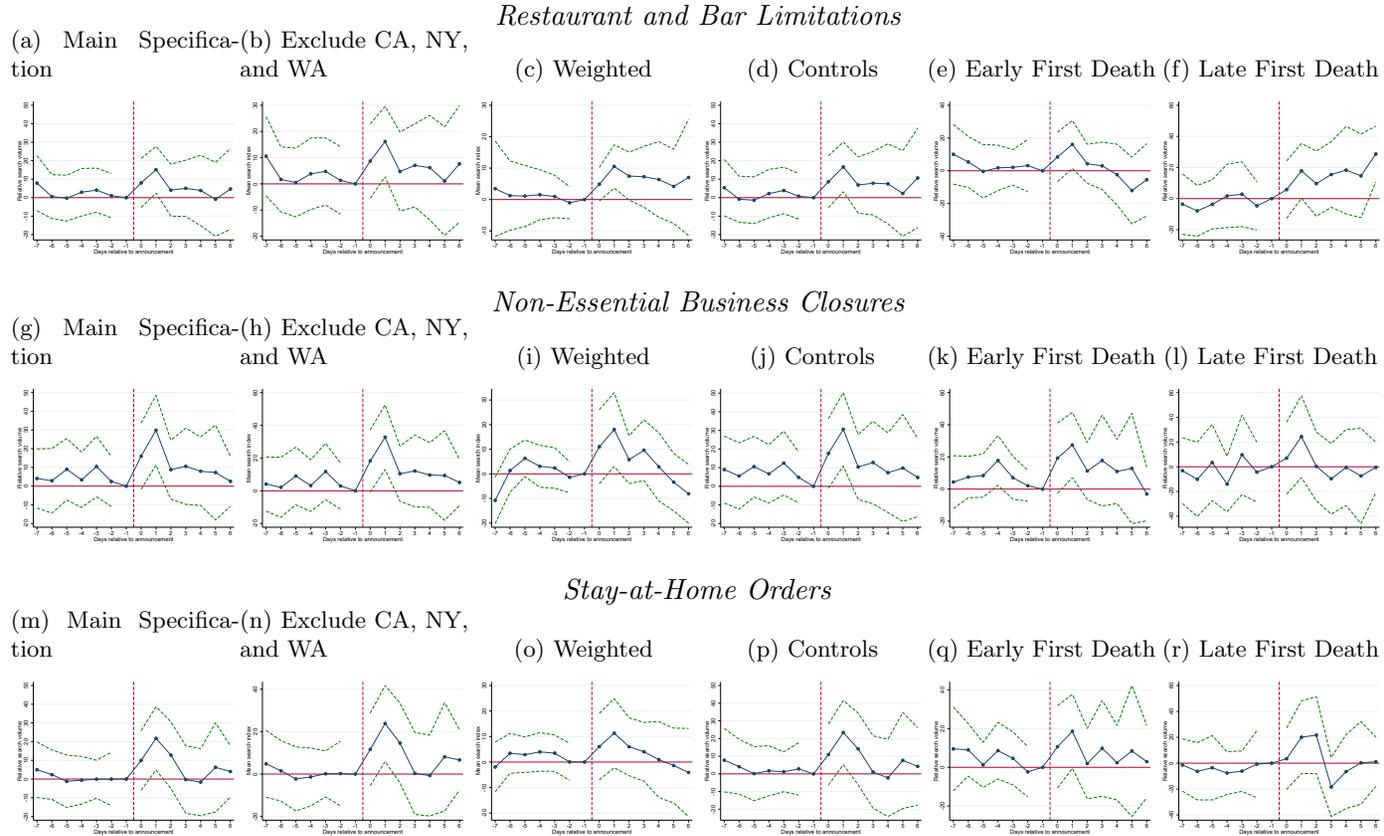
Note: Figure shows heatmaps of the distribution of the announcement dates of restaurant and bar limitations (Panel (a)), non-essential business closures (Panel (b)), stay-at-home orders (Panel (c)), large-gatherings bans (Panel (d)), school closures (Panel (e)), and emergency declarations (Panel (f)) across states. Darker colors indicate an earlier announcement date, lighter colors indicate a later announcement date, and white indicates that the policy was not announced by April 3 in the state. For more details, see Section 3.2.

Appendix Figure A2: Evolution of Google Search Volume for Claiming Unemployment Insurance in March and April, 2020



Note: Figure shows normalized Google search volumes for claiming unemployment insurance for U.S. states between March 01 and April 24, 2020. Each light colored line represents one state and the black line represents the national average. For more details, see Section 3.1.

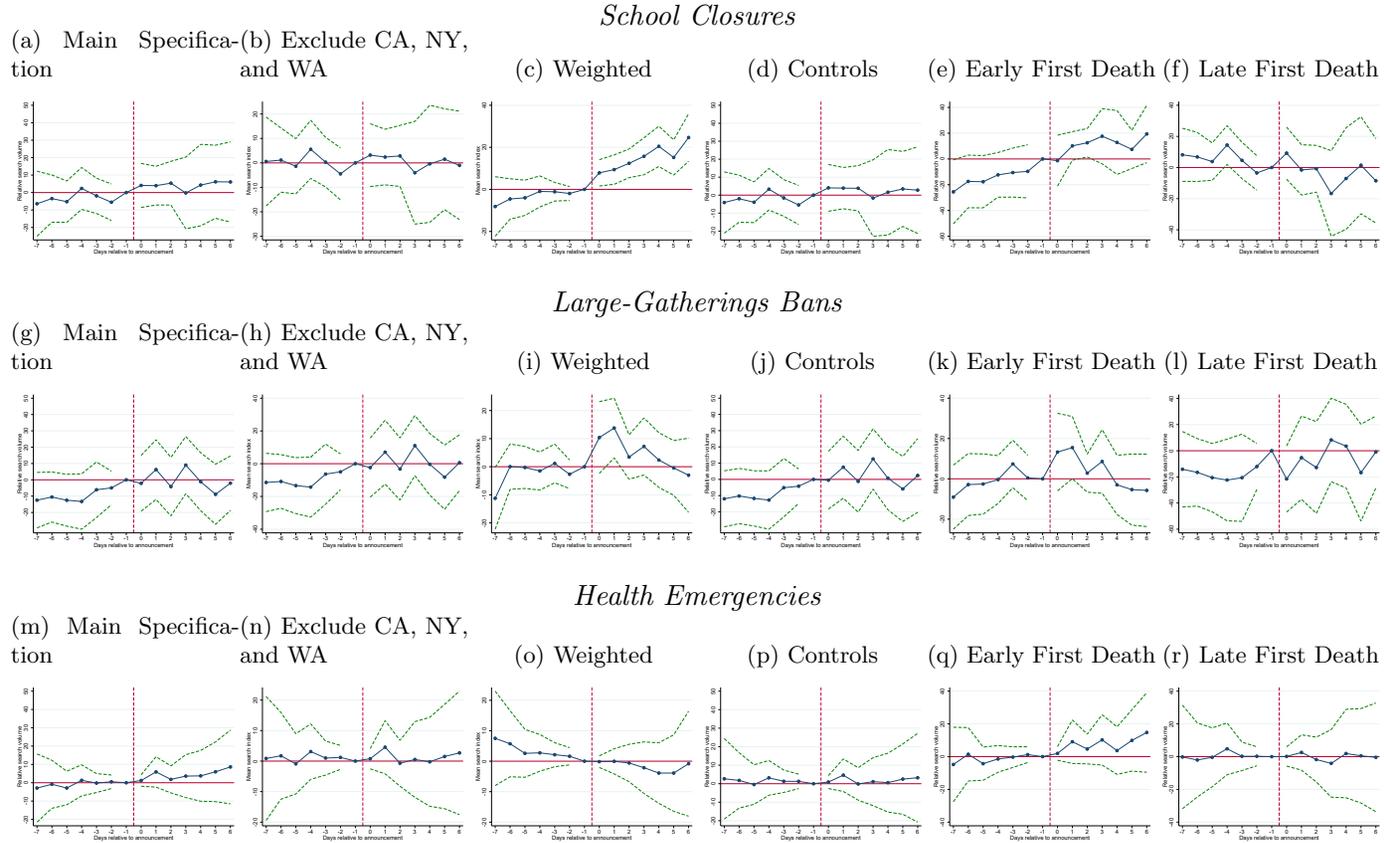
Appendix Figure A3: Event Study Estimates: Robustness



4

Note: Figure shows event study estimates of the impact of the introduction of restaurant and bar limitations (Panels (a)-(f)), non-essential business closures (Panels (g)-(l)), and stay at home orders (Panels (m)-(r)), based on Equation (1). Replicating Figure 2, Panels (a), (g), and (m) show our main specification. Panels (b), (h), and (n) show estimates excluding California, Washington, and New York. Panels (c), (i), and (o) show estimates weighted by total employment in the state. Panels (d), (j), and (p) show estimates including controls for case growth and number of deaths, both interacted with state dummies to allow the effect of case growth and deaths to vary by state. Panels (e), (k), and (q) show estimates limiting to the 26 states that registered their first COVID-19 death on or before March 19. Panels (f), (l), and (r) show estimates limiting to the 23 states that registered their first COVID-19 death after March 19. The day prior to the announcement is normalized to zero and standard errors are clustered at the state level. For more details, see Sections 5.1 and 5.3.

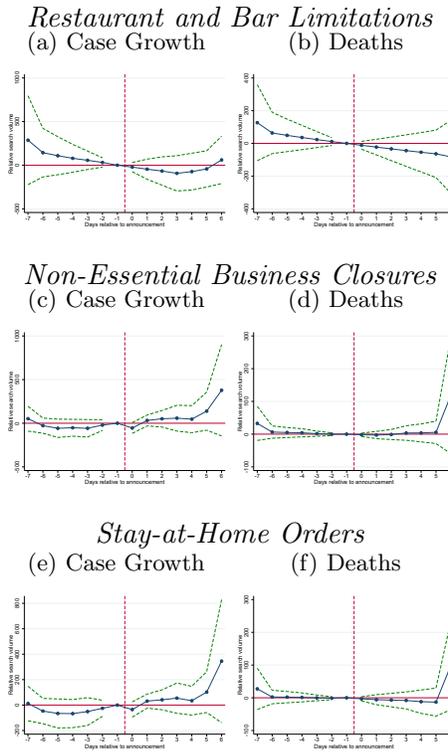
Appendix Figure A4: Event Study Estimates: Robustness



5

Note: Figure shows event study estimates of the impact of the introduction of school closures (Panels (a)-(f)), large-gatherings bans (Panels (g)-(l)), and emergency and public health emergency declarations (Panels (m)-(r)), based on Equation (1). Panels (a), (g), and (m) show our main specification. Panels (b), (h), and (n) show estimates excluding California, Washington, and New York. Panels (c), (i), and (o) show estimates weighted by total employment in the state. Panels (d), (j), and (p) show estimates including controls for case growth and number of deaths, both interacted with state dummies to allow the effect of case growth and deaths to vary by state. Panels (e), (k), and (q) show estimates limiting to the 26 states that registered their first COVID-19 death on or before March 19. Panels (f), (l), and (r) show estimates limiting to the 23 states that registered their first COVID-19 death after March 19. The day prior to the announcement is normalized to zero and standard errors are clustered at the state level. For more details, see Sections 5.1 and 5.3.

Appendix Figure A5: Event Study Estimates: Epidemiological Outcomes



Note: Figure shows event study estimates of the relationship of the introduction of restaurant and bar limitations (Panels (a) and (b)), non-essential business closures (Panels (c) and (d)), and stay-at-home orders (Panels (e) and (f)) and epidemiological outcomes (case growth and deaths) based on Equation (1). Panels (a), (c), and (e) show estimates for case growth. Panels (b), (d), and (f) show estimates for deaths. The day prior to the announcement is normalized to zero and standard errors are clustered at the state level. For more details, see Sections 5.1 and 5.3.

Appendix Table A1: NPI Adoption by State

State	State	Restaurant and Bar Limitation	Essential Business Closure	Stay- at- Home Order	Public Health Emergency	Large- Gatherings Ban	School Closure	First Case	First Death	Early First Death
AK	Alaska	3/17	3/20	3/17	3/11	3/20	3/13	3/13	3/25	
AL	Alabama	3/17	3/27	4/3	3/13	3/17	3/17	3/13	3/25	
AR	Arkansas	3/15			3/11	3/26	3/15	3/15	3/24	
AZ	Arizona	3/19		3/30	3/11	3/30	3/16	3/1	3/21	
CA	California	3/16	3/19	3/19	3/11	3/16		3/1	3/4	✓
CO	Colorado	3/16	3/26	3/26	3/10	3/26	3/18	3/6	3/13	✓
CT	Connecticut	3/16	3/20		3/10	3/16	3/11	3/10	3/18	✓
DC	District of Columbia	3/20	3/24	3/30	3/11	3/20	3/20	3/16	3/20	
DE	Delaware	3/16	3/22	3/22	3/12	3/16	3/13	3/11	3/26	
FL	Florida	3/17	4/1	4/1	3/1	4/1	3/17	3/2	3/8	✓
GA	Georgia	3/23		3/23	3/14	3/23	3/16	3/3	3/12	✓
HI	Hawaii	3/17		3/17	3/4	3/17	3/15	3/7	3/24	
IA	Iowa	3/17	3/26		3/9	3/17	3/15	3/9	3/25	
ID	Idaho	3/25	3/25	3/19	3/13	3/25	3/25	3/13	3/26	
IL	Illinois	3/20	3/20	3/20	3/9	3/20	3/13	3/1	3/17	✓
IN	Indiana	3/16	3/23	3/23	3/6	3/23	3/19	3/6	3/16	✓
KS	Kansas	3/17	3/23	3/23	3/12	3/17	3/17	3/8	3/13	✓
KY	Kentucky	3/16	3/23		3/6	3/19	3/16	3/6	3/16	✓
LA	Louisiana	3/16		3/22	3/11	3/22	3/13	3/11	3/14	✓
MA	Massachusetts	3/15	3/23		3/10	3/23	3/15	3/1	3/20	
MD	Maryland	3/16	3/23	3/30	3/5	3/16	3/16	3/6	3/19	✓
ME	Maine	3/18	3/24	3/31	3/15	3/18	3/31	3/12	3/27	
MI	Michigan	3/17	3/23	3/23	3/10	3/23	3/16	3/11	3/18	✓
MN	Minnesota	3/16		3/25	3/13		3/15	3/6	3/21	
MO	Missouri	3/21	4/3	4/3	3/13	3/21	3/21	3/8	3/18	✓
MS	Mississippi	3/24		3/31	3/14	3/24	3/19	3/12	3/19	✓
MT	Montana	3/20	3/26	3/26	3/12	3/24	3/15	3/13	3/27	
NC	North Carolina	3/14	3/27	3/27	3/10	3/14	3/14	3/3	3/25	
ND	North Dakota	3/19	4/2		3/13		3/15	3/12	3/27	
NE	Nebraska	3/30			3/13	3/19	3/19	3/6	3/28	
NH	New Hampshire	3/16	3/26	3/26	3/13	3/16	3/15	3/2	3/23	
NJ	New Jersey	3/16	3/21	3/21	3/9	3/16	3/16	3/5	3/10	✓
NM	New Mexico	3/15	3/23	3/23	3/11	3/16	3/13	3/11	3/25	
NV	Nevada	3/17	3/20	4/1	3/13	3/15	3/15	3/5	3/16	✓
NY	New York	3/14	3/20	3/20	3/7	3/20	3/16	3/2	3/14	✓
OH	Ohio	3/15	3/22	3/22	3/9	3/22	3/12	3/10	3/20	
OK	Oklahoma		3/24	3/24	3/15	3/24	3/16	3/7	3/19	✓
OR	Oregon	3/16	3/23	3/23	3/8	3/23	3/17	3/1	3/15	✓
PA	Pennsylvania	3/16	3/19	3/23	3/6		3/13	3/6	3/18	✓
RI	Rhode Island	3/16		3/13	3/9	3/16	3/18	3/1	3/28	
SC	South Carolina	3/17	3/31		3/13	3/17	3/15	3/7	3/16	✓
SD	South Dakota				3/13	3/23	3/13	3/12	3/11	✓
TN	Tennessee	3/22	3/30	3/30	3/12	3/22	3/16	3/5	3/20	
TX	Texas	3/19		3/31	3/13	3/19	3/19	3/5	3/17	✓
UT	Utah	3/18		3/27	3/6	3/18	3/13	3/7	3/22	
VA	Virginia	3/23	3/23	3/30	3/12	3/23	3/13	3/8	3/14	✓
VT	Vermont	3/16	3/24	3/24	3/13		3/15	3/8	3/19	✓
WA	Washington	3/15	3/23	3/23	2/29	3/16	3/13	3/1	3/1	✓
WI	Wisconsin	3/17	3/24	3/24	3/12		3/18	3/10	3/20	
WV	West Virginia	3/17	3/23	3/23	3/4		3/13	3/18	3/30	
WY	Wyoming	3/19			3/13	3/20	3/19	3/12	4/13	

Note: Table shows for each state: the day the state announced each NPI, the day the state registered its first death from COVID-19, the day the state registered its first confirmed case of COVID-19, and whether we categorize the state as a state with an early first death (first death by 3/19). The source of these data is [Kaiser Family Foundation \(2020\)](#). For more details, see Section 3.2.

Appendix Table A2: Event Study Estimates: Restaurant and Bar Limitations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
-7	7.851 (7.603)	3.460 (7.727)	10.55 (7.708)	9.104 (8.014)	5.401 (7.771)	2.184 (5.847)	10.08 (9.080)	-7.012 (8.122)
-6	0.603 (6.039)	1.256 (5.618)	1.688 (6.326)	4.081 (6.222)	-0.842 (6.350)	0.136 (4.818)	4.528 (8.371)	-9.131 (7.994)
-5	-0.251 (6.329)	1.111 (4.953)	0.501 (6.687)	3.086 (5.716)	-1.376 (6.445)	0.166 (4.341)	-1.179 (8.735)	-4.694 (7.515)
-4	2.941 (6.549)	1.545 (4.053)	3.843 (6.964)	3.566 (4.636)	2.216 (6.682)	0.846 (3.678)	1.209 (7.551)	0.946 (10.17)
-3	4.077 (6.080)	0.955 (3.445)	4.756 (6.533)	1.958 (4.010)	3.893 (6.403)	0.530 (3.527)	1.970 (6.114)	2.713 (10.74)
-2	1.080 (6.165)	-1.019 (2.601)	1.326 (6.572)	-1.019 (3.088)	0.781 (6.293)	-1.100 (2.618)	3.115 (8.535)	-5.240 (7.878)
-1	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)
0	7.979 (6.760)	4.873 ⁺ (2.725)	8.682 (7.234)	6.146 ⁺ (3.278)	8.638 (7.167)	5.827* (2.900)	8.043 (7.921)	7.231 (10.08)
1	15.15* (6.440)	10.53** (3.521)	16.14* (6.861)	12.33** (4.063)	16.64* (6.850)	11.71** (3.530)	16.51 ⁺ (8.168)	19.37* (9.197)
2	4.043 (7.154)	7.468 ⁺ (3.884)	4.722 (7.676)	8.895* (4.242)	6.859 (7.781)	9.097* (3.808)	4.318 (7.234)	13.56 (11.27)
3	5.075 (7.781)	7.266 (4.945)	7.038 (8.097)	11.46* (5.023)	7.809 (8.734)	9.175 ⁺ (4.682)	2.191 (8.924)	19.13 ⁺ (10.99)
4	3.899 (9.725)	6.379 (6.140)	6.193 (10.19)	9.383 (6.853)	7.507 (11.05)	9.203 (5.637)	-1.004 (11.53)	21.86 (15.47)
5	-0.866 (10.21)	4.181 (6.013)	1.073 (10.58)	6.690 (6.248)	2.285 (11.82)	6.469 (5.857)	-13.01 (14.00)	18.81 (14.14)
6	4.693 (11.15)	7.023 (9.444)	7.635 (11.34)	13.16 (9.893)	10.67 (13.72)	12.76 (8.580)	-3.455 (16.30)	33.22** (11.50)
<i>N</i>	2805	2805	2640	2640	2244	2244	1144	1100
<i>R</i> ²	0.580	0.682	0.579	0.684	0.654	0.769	0.727	0.600
Employment Weights	No	Yes	No	Yes	No	Yes	No	No
Drop WA, CA, NY	No	No	Yes	Yes	No	No	No	No
Case Growth & Death Ctrls	No	No	No	No	Yes	Yes	No	No
Early or Late First Death	Both	Both	Both	Both	Both	Both	Early	Late

Standard errors in parentheses

⁺ $p < .1$, * $p < .05$, ** $p < .01$

Note: Table shows event study coefficients of the impact of the introduction of restaurant and bar limitations on search volume, based on Equations (1) and (3). Column (1) shows our main specification. Column (2) shows estimates weighted by total employment in the state. Column (3) shows estimates excluding California, Washington, and New York. Column (4) shows estimates weighted by total employment in the state and excluding California, Washington, and New York. Column (5) shows estimates including controls for case growth and number of deaths, both interacted with state dummies to allow the effect of case growth and deaths to vary by state. Column (6) shows estimates weighted by total employment in the state and including controls for case growth and number of deaths, both interacted with state dummies to allow the effect of case growth and deaths to vary by state. Column (7) shows estimates for states with an early first death. Column (8) shows estimates for states with a late first death. The day prior to the announcement is normalized to zero and standard errors are clustered at the state level. For more details, see Sections 5.1 and 5.3.

Appendix Table A3: Event Study Estimates: Non-Essential Business Closures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
-7	4.100 (8.058)	-10.76* (4.817)	4.111 (8.446)	-10.53+ (5.486)	8.973 (9.166)	-2.987 (6.411)	12.28 (10.85)	-0.695 (14.00)
-6	2.879 (8.820)	1.328 (4.475)	2.143 (9.363)	1.393 (5.121)	5.542 (9.084)	5.795 (5.380)	13.16 (8.293)	-10.90 (14.68)
-5	9.003 (8.393)	6.303 (3.800)	9.048 (8.965)	7.245+ (4.238)	10.57 (8.314)	10.50* (4.424)	13.01+ (7.275)	1.390 (14.61)
-4	3.377 (7.535)	3.076 (4.350)	3.174 (8.061)	2.343 (4.833)	6.702 (7.984)	6.623 (4.737)	21.83* (8.297)	-11.65 (12.21)
-3	10.49 (8.269)	2.387 (4.191)	11.77 (8.772)	5.027 (4.634)	12.47 (8.744)	5.559 (4.572)	9.937 (7.209)	10.70 (17.64)
-2	2.486 (6.814)	-1.392 (3.214)	2.973 (7.198)	-0.881 (3.959)	4.778 (6.960)	0.697 (3.469)	5.198 (4.959)	-2.219 (12.83)
-1	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)
0	16.02+ (9.030)	11.04 (7.664)	18.26+ (9.686)	16.38+ (9.346)	17.64+ (9.519)	12.66 (8.150)	19.22 (12.01)	9.848 (15.35)
1	29.87** (9.517)	18.02* (7.657)	32.77** (10.05)	26.75** (8.452)	30.62** (10.05)	17.91* (8.513)	26.34* (11.21)	29.31 (17.67)
2	8.786 (8.039)	5.755 (4.966)	10.45 (8.585)	11.44* (5.375)	10.37 (8.917)	5.597 (6.114)	8.923 (9.830)	7.181 (16.38)
3	10.62 (10.39)	9.611 (6.302)	12.13 (11.18)	14.71+ (8.125)	12.77 (11.30)	10.23 (7.471)	15.68 (15.55)	-1.005 (15.50)
4	7.961 (9.324)	2.865 (7.057)	9.641 (10.07)	8.805 (8.372)	7.330 (10.92)	1.681 (8.042)	5.412 (10.99)	7.441 (18.95)
5	7.297 (12.94)	-3.375 (5.965)	9.344 (14.00)	1.675 (7.676)	9.816 (14.65)	-3.160 (6.969)	7.924 (18.16)	5.644 (24.37)
6	2.562 (6.818)	-8.106 (6.095)	5.120 (7.185)	-0.620 (5.411)	4.737 (10.74)	-8.246 (9.055)	-11.36 (11.47)	18.03 (18.42)
<i>N</i>	2805	2805	2640	2640	2244	2244	1144	1100
<i>R</i> ²	0.582	0.694	0.581	0.696	0.656	0.776	0.732	0.601
Employment Weights	No	Yes	No	Yes	No	Yes	No	No
Drop WA, CA, NY	No	No	Yes	Yes	No	No	No	No
Case Growth & Death Ctrl	No	No	No	No	Yes	Yes	No	No
Early or Late First Death	Both	Both	Both	Both	Both	Both	Early	Late

Standard errors in parentheses

+ $p < .1$, * $p < .05$, ** $p < .01$

Note: Table shows event study coefficients of the impact of non-essential business closures on search volume, based on Equations (1) and (3). Column (1) shows our main specification. Column (2) shows estimates weighted by total employment in the state. Column (3) shows estimates excluding California, Washington, and New York. Column (4) shows estimates weighted by total employment in the state and excluding California, Washington, and New York. Column (5) shows estimates including controls for case growth and number of deaths, both interacted with state dummies to allow the effect of case growth and deaths to vary by state. Column (6) shows estimates weighted by total employment in the state and including controls for case growth and number of deaths, both interacted with state dummies to allow the effect of case growth and deaths to vary by state. Column (7) shows estimates for states with an early first death. Column (8) shows estimates for states with a late first death. The day prior to the announcement is normalized to zero and standard errors are clustered at the state level. For more details, see Sections 5.1 and 5.3.

Appendix Table A4: Event Study Estimates: Stay-at-Home Orders

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
-7	4.995 (7.554)	-1.939 (4.972)	4.825 (8.028)	-0.944 (6.077)	7.740 (9.155)	2.906 (6.737)	16.35 (13.85)	-4.344 (11.03)
-6	2.373 (6.748)	3.439 (3.992)	1.535 (7.280)	2.345 (4.389)	4.050 (7.897)	6.044 (5.364)	13.71 (9.157)	-8.111 (12.35)
-5	-1.203 (7.136)	2.894 (3.551)	-2.294 (7.703)	0.938 (3.884)	0.148 (7.810)	5.608 (4.800)	5.136 (6.653)	-4.777 (13.54)
-4	-0.608 (6.486)	3.976 (3.856)	-1.362 (6.954)	1.776 (4.439)	1.760 (7.224)	6.527 (4.383)	12.43 (8.944)	-7.109 (9.226)
-3	-0.0792 (5.172)	3.492 (3.631)	0.133 (5.517)	4.768 (4.165)	1.186 (5.750)	6.618 (4.235)	7.506 (7.248)	-6.558 (8.978)
-2	0.00188 (7.300)	0.0527 (3.713)	0.243 (7.777)	0.462 (4.814)	2.818 (7.556)	2.584 (3.809)	0.495 (7.068)	2.491 (13.79)
-1	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)
0	9.952 (8.134)	6.073 (6.634)	11.62 (8.727)	9.269 (8.171)	10.93 (8.807)	8.090 (7.588)	12.13 (12.24)	4.054 (12.79)
1	21.75* (8.616)	11.32 (6.920)	23.82* (9.145)	17.02* (8.371)	23.37* (9.272)	11.96 (7.896)	18.02 (10.69)	25.87+ (14.94)
2	12.79 (9.093)	6.015 (5.793)	14.62 (9.591)	11.23+ (6.662)	14.16 (10.34)	6.890 (7.620)	1.547 (10.57)	25.49 (17.13)
3	-0.288 (9.220)	4.030 (5.865)	0.284 (9.859)	7.251 (7.838)	1.047 (10.44)	5.057 (7.564)	9.166 (13.90)	-13.79 (13.66)
4	-1.622 (9.134)	0.969 (7.588)	-0.699 (9.705)	6.360 (9.095)	-2.242 (11.16)	1.433 (9.495)	-1.053 (11.31)	-3.126 (18.03)
5	6.273 (12.15)	-1.361 (7.485)	8.126 (13.02)	4.206 (9.524)	7.615 (13.86)	-0.794 (9.081)	5.786 (18.11)	6.428 (19.15)
6	4.021 (6.989)	-4.090 (8.800)	6.641 (7.281)	5.944 (8.593)	4.226 (11.19)	-3.490 (11.20)	-4.847 (11.75)	10.02 (16.04)
<i>N</i>	2805	2805	2640	2640	2244	2244	1144	1100
<i>R</i> ²	0.581	0.685	0.580	0.685	0.656	0.770	0.728	0.604
Employment Weights	No	Yes	No	Yes	No	Yes	No	No
Drop WA, CA, NY	No	No	Yes	Yes	No	No	No	No
Case Growth & Death Ctrls	No	No	No	No	Yes	Yes	No	No
Early or Late First Death	Both	Both	Both	Both	Both	Both	Early	Late

Standard errors in parentheses

+ $p < .1$, * $p < .05$, ** $p < .01$

Note: Table shows event study coefficients of the impact of stay-at-home policies on search volume, based on Equations (1) and (3). Column (1) shows our main specification. Column (2) shows estimates weighted by total employment in the state. Column (3) shows estimates excluding California, Washington, and New York. Column (4) shows estimates weighted by total employment in the state and excluding California, Washington, and New York. Column (5) shows estimates including controls for case growth and number of deaths, both interacted with state dummies to allow the effect of case growth and deaths to vary by state. Column (6) shows estimates weighted by total employment in the state and including controls for case growth and number of deaths, both interacted with state dummies to allow the effect of case growth and deaths to vary by state. Column (7) shows estimates for states with an early first death. Column (8) shows estimates for states with a late first death. The day prior to the announcement is normalized to zero and standard errors are clustered at the state level. For more details, see Sections 5.1 and 5.3.

Appendix Table A5: Event Study Estimates: Large-Gatherings Bans

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
-7	-12.46 (8.671)	-11.25 ⁺ (5.616)	-11.46 (9.081)	-6.647 (5.576)	-11.97 (8.797)	-3.342 (5.190)	-4.629 (6.845)	-17.54 (15.31)
-6	-10.53 (7.820)	0.0200 (4.118)	-10.85 (8.367)	2.791 (4.590)	-10.25 (8.675)	4.130 (5.140)	-0.843 (9.544)	-18.40 (14.14)
-5	-12.50 (8.106)	-0.275 (3.814)	-13.38 (8.722)	0.851 (4.778)	-11.73 (8.677)	3.637 (4.490)	-0.482 (8.870)	-21.51 (14.08)
-4	-13.28 (8.655)	-1.510 (3.462)	-14.29 (9.365)	-1.017 (4.661)	-12.74 (9.160)	1.581 (4.262)	1.323 (7.083)	-23.88 (16.47)
-3	-6.089 (8.691)	1.163 (3.510)	-6.358 (9.359)	2.799 (3.898)	-5.126 (9.172)	3.971 (4.038)	8.627 (7.050)	-20.73 (17.60)
-2	-4.952 (5.239)	-2.680 (2.605)	-4.964 (5.591)	-2.869 (3.173)	-4.205 (5.424)	-1.086 (2.801)	1.265 (5.724)	-11.79 (9.423)
-1	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)
0	-2.138 (8.699)	10.39 (6.518)	-2.377 (9.203)	12.85 (7.872)	-0.484 (9.072)	12.08 ⁺ (6.666)	14.53 (10.10)	-20.46 (13.39)
1	6.263 (9.325)	13.77* (5.422)	7.052 (9.952)	18.94** (5.834)	7.512 (9.755)	13.17* (5.800)	15.61 ⁺ (8.015)	-2.852 (17.31)
2	-4.143 (9.145)	3.451 (4.028)	-3.239 (9.671)	8.308* (4.000)	-1.210 (9.680)	3.553 (4.573)	2.887 (5.010)	-7.421 (19.15)
3	9.016 (8.904)	7.220 (5.192)	11.03 (9.365)	13.55* (5.651)	12.55 (9.461)	6.966 (5.920)	9.028 (8.663)	14.82 (17.50)
4	-1.147 (9.041)	2.382 (4.974)	-0.355 (9.592)	4.799 (5.983)	0.747 (9.953)	0.246 (5.244)	-5.209 (8.388)	8.797 (17.63)
5	-8.878 (9.409)	-0.435 (4.968)	-8.213 (10.01)	3.096 (5.420)	-5.882 (10.22)	-1.875 (5.260)	-6.801 (10.19)	-9.722 (20.42)
6	-2.063 (8.494)	-3.026 (6.734)	0.607 (8.703)	5.954 (5.060)	2.385 (11.58)	-3.680 (9.079)	-4.885 (13.03)	6.729 (18.97)
<i>N</i>	2805	2805	2640	2640	2244	2244	1144	1100
<i>R</i> ²	0.582	0.689	0.581	0.689	0.656	0.772	0.729	0.604
Employment Weights	No	Yes	No	Yes	No	Yes	No	No
Drop WA, CA, NY	No	No	Yes	Yes	No	No	No	No
Case Growth & Death Ctrls	No	No	No	No	Yes	Yes	No	No
Early or Late First Death	Both	Both	Both	Both	Both	Both	Early	Late

Standard errors in parentheses

⁺ $p < .1$, * $p < .05$, ** $p < .01$

Note: Table shows event study coefficients of the impact of large-gatherings bans on search volume, based on Equations (1) and (3). Column (1) shows our main specification. Column (2) shows estimates weighted by total employment in the state. Column (3) shows estimates excluding California, Washington, and New York. Column (4) shows estimates weighted by total employment in the state and excluding California, Washington, and New York. Column (5) shows estimates including controls for case growth and number of deaths, both interacted with state dummies to allow the effect of case growth and deaths to vary by state. Column (6) shows estimates weighted by total employment in the state and including controls for case growth and number of deaths, both interacted with state dummies to allow the effect of case growth and deaths to vary by state. Column (7) shows estimates for states with an early first death. Column (8) shows estimates for states with a late first death. The day prior to the announcement is normalized to zero and standard errors are clustered at the state level. For more details, see Sections 5.1 and 5.3.

Appendix Table A6: Event Study Estimates: School Closures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
-7	-6.410 (9.505)	-8.142 (7.219)	0.574 (9.278)	-2.148 (11.37)	-4.109 (8.692)	-7.249 (5.556)	-20.12 ⁺ (11.18)	8.108 (8.874)
-6	-3.444 (6.896)	-4.556 (4.908)	1.149 (6.695)	0.0881 (7.417)	-1.996 (6.681)	-4.205 (4.115)	-14.05 (9.804)	6.763 (8.904)
-5	-5.225 (6.042)	-4.008 (4.275)	-1.367 (5.756)	0.148 (6.240)	-3.947 (5.783)	-3.756 (3.687)	-14.83 (9.995)	3.837 (6.329)
-4	2.357 (6.124)	-0.970 (3.771)	5.555 (6.062)	2.759 (5.332)	3.295 (5.892)	-0.871 (3.288)	-10.14 (8.591)	14.23* (6.410)
-3	-1.896 (5.161)	-1.120 (2.247)	0.298 (5.180)	1.713 (2.940)	-1.565 (5.204)	-1.378 (2.147)	-9.128 (9.806)	3.882 (6.311)
-2	-5.542 (5.330)	-1.955 (1.698)	-4.490 (5.432)	-0.408 (1.855)	-5.443 (5.544)	-2.083 (1.709)	-8.761 (10.85)	-4.301 (6.264)
-1	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)
0	4.074 (6.430)	7.866* (3.193)	3.180 (6.552)	6.783 ⁺ (3.599)	4.058 (6.646)	7.629* (3.543)	-2.195 (10.51)	10.00 (9.049)
1	3.955 (5.664)	9.412* (3.565)	2.384 (5.800)	8.336* (4.030)	3.908 (5.859)	9.468* (4.027)	8.124 (6.407)	0.415 (8.929)
2	5.376 (6.379)	12.39** (3.474)	2.853 (6.360)	10.03* (4.274)	3.828 (6.414)	11.48** (4.081)	9.756 (6.389)	-0.695 (8.022)
3	-0.227 (10.48)	15.71** (4.487)	-4.033 (10.71)	11.37 ⁺ (5.736)	-1.617 (10.84)	14.72** (5.353)	14.07 (12.25)	-15.79 (14.92)
4	4.276 (11.91)	20.47** (4.889)	-0.381 (12.22)	14.81* (6.501)	1.610 (12.14)	19.19** (5.482)	8.451 (14.10)	-7.344 (17.70)
5	6.167 (10.68)	15.18** (4.279)	1.502 (10.51)	9.611 (5.854)	3.454 (10.64)	13.79* (5.578)	2.269 (9.575)	2.544 (16.63)
6	6.086 (11.70)	24.65** (5.746)	-1.028 (11.32)	17.39* (8.100)	2.774 (12.30)	24.26** (8.731)	13.06 (14.79)	-5.140 (15.92)
<i>N</i>	2805	2805	2640	2640	2244	2244	1144	1100
<i>R</i> ²	0.579	0.696	0.577	0.683	0.653	0.774	0.728	0.598
Employment Weights	No	Yes	No	Yes	No	Yes	No	No
Drop WA, CA, NY	No	No	Yes	Yes	No	No	No	No
Case Growth & Death Ctrls	No	No	No	No	Yes	Yes	No	No
Early or Late First Death	Both	Both	Both	Both	Both	Both	Early	Late

Standard errors in parentheses

⁺ $p < .1$, * $p < .05$, ** $p < .01$

Note: Table shows event study coefficients of the impact of school closures on search volume, based on Equations (1) and (3). Column (1) shows our main specification. Column (2) shows estimates weighted by total employment in the state. Column (3) shows estimates excluding California, Washington, and New York. Column (4) shows estimates weighted by total employment in the state and excluding California, Washington, and New York. Column (5) shows estimates including controls for case growth and number of deaths, both interacted with state dummies to allow the effect of case growth and deaths to vary by state. Column (6) shows estimates weighted by total employment in the state and including controls for case growth and number of deaths, both interacted with state dummies to allow the effect of case growth and deaths to vary by state. Column (7) shows estimates for states with an early first death. Column (8) shows estimates for states with a late first death. The day prior to the announcement is normalized to zero and standard errors are clustered at the state level. For more details, see Sections 5.1 and 5.3.

Appendix Table A7: Event Study Estimates: Public Health Emergencies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
-7	-2.904 (9.492)	7.493 (7.909)	0.861 (10.36)	8.533 (9.000)	2.649 (11.09)	9.776 (8.556)	5.368 (14.43)	1.180 (18.22)
-6	-0.847 (6.690)	5.734 (5.536)	1.724 (7.248)	6.616 (6.071)	1.766 (7.624)	6.600 (5.927)	6.582 (9.785)	-1.998 (12.76)
-5	-2.891 (4.673)	2.576 (4.006)	-0.911 (5.026)	2.804 (4.213)	-0.455 (5.447)	3.661 (4.454)	0.895 (6.629)	-0.879 (10.01)
-4	1.305 (4.324)	2.743 (3.072)	3.113 (4.630)	3.428 (3.348)	3.159 (4.792)	3.582 (3.436)	2.667 (5.271)	4.269 (8.460)
-3	-0.176 (2.630)	2.130 (2.012)	0.995 (2.842)	2.553 (2.184)	1.302 (3.080)	2.679 (2.286)	2.597 (4.040)	0.206 (4.976)
-2	0.528 (1.882)	1.622 (1.444)	1.205 (1.993)	2.118 (1.585)	1.258 (1.995)	1.854 (1.536)	2.681 (2.704)	0.264 (3.116)
-1	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)
0	1.208 (1.593)	-0.147 (0.978)	0.772 (1.692)	-0.0865 (1.280)	0.890 (1.790)	-0.199 (1.347)	1.244 (2.449)	0.219 (3.230)
1	5.909 (4.229)	-0.0425 (2.107)	4.550 (4.447)	-0.413 (2.040)	4.605 (4.483)	-0.768 (2.424)	6.570 (6.959)	2.457 (6.220)
2	1.819 (3.762)	-0.588 (3.135)	-0.704 (3.830)	-1.606 (3.125)	-0.122 (4.466)	-1.667 (3.547)	1.183 (5.753)	-2.289 (7.911)
3	3.612 (5.966)	-2.133 (4.318)	0.518 (6.325)	-3.501 (4.714)	1.102 (6.615)	-3.408 (4.914)	5.331 (8.615)	-4.270 (11.78)
4	3.752 (7.076)	-3.894 (5.070)	-0.237 (7.433)	-6.334 (5.757)	0.569 (8.108)	-5.815 (5.881)	-2.068 (8.636)	1.358 (15.40)
5	5.984 (8.322)	-3.896 (6.403)	1.500 (8.718)	-5.621 (6.945)	2.484 (9.680)	-5.309 (7.206)	3.570 (11.74)	-0.0343 (16.32)
6	8.619 (10.28)	-0.825 (8.787)	2.687 (10.32)	-1.906 (7.514)	3.217 (12.24)	-3.662 (9.126)	3.837 (15.96)	-0.277 (18.88)
<i>N</i>	2805	2805	2640	2640	2244	2244	1144	1100
<i>R</i> ²	0.579	0.682	0.577	0.683	0.653	0.769	0.725	0.594
Employment Weights	No	Yes	No	Yes	No	Yes	No	No
Drop WA, CA, NY	No	No	Yes	Yes	No	No	No	No
Case Growth & Death Ctrls	No	No	No	No	Yes	Yes	No	No
Early or Late First Death	Both	Both	Both	Both	Both	Both	Early	Late

Standard errors in parentheses

+ $p < .1$, * $p < .05$, ** $p < .01$

Note: Table shows event study coefficients of the impact of emergency declarations on search volume, based on Equations (1) and (3). Column (1) shows our main specification. Column (2) shows estimates weighted by total employment in the state. Column (3) shows estimates excluding California, Washington, and New York. Column (4) shows estimates weighted by total employment in the state and excluding California, Washington, and New York. Column (5) shows estimates including controls for case growth and number of deaths, both interacted with state dummies to allow the effect of case growth and deaths to vary by state. Column (6) shows estimates weighted by total employment in the state and including controls for case growth and number of deaths, both interacted with state dummies to allow the effect of case growth and deaths to vary by state. Column (7) shows estimates for states with an early first death. Column (8) shows estimates for states with a late first death. The day prior to the announcement is normalized to zero and standard errors are clustered at the state level. For more details, see Sections 5.1 and 5.3.

Appendix Table A8: Event Study Estimates: Multiple-Policy Estimation

	(1)	(2)	(3)
	Restaurant and Bar Limitations	Essential Business Closures	Stay- at- Home Orders
-7	8.477 (7.662)	3.283 (8.293)	0.461 (7.554)
-6	1.640 (6.211)	3.709 (9.605)	-4.368 (8.478)
-5	0.324 (6.583)	14.16 (10.85)	-11.64 (10.18)
-4	3.391 (6.688)	6.113 (8.528)	-6.584 (7.410)
-3	4.714 (6.293)	16.27 (11.05)	-11.25 (7.673)
-2	1.099 (6.660)	2.674 (7.072)	-1.767 (8.041)
-1	0 (.)	0 (.)	0 (.)
0	7.642 (6.952)	15.16 (9.874)	0.958 (8.345)
1	14.77* (7.077)	25.94* (11.54)	6.354 (9.731)
2	5.797 (7.389)	2.676 (8.910)	10.87 (10.12)
3	6.501 (8.384)	17.01 (11.91)	-11.24 (10.42)
4	5.587 (10.08)	14.77 (10.62)	-11.14 (10.03)
5	0.638 (10.25)	4.978 (10.91)	2.905 (10.31)
6	3.164 (11.00)	1.912 (6.143)	0.345 (6.643)
<i>N</i>	2805	2805	2805
<i>R</i> ²	0.586	0.586	0.586

Standard errors in parentheses

+ $p < .1$, * $p < .05$, ** $p < .01$

Note: Table shows event study coefficients of the relationship between NPIs and search volume when we include multiple policies at the same time, based on Equation (2). Column (1) shows estimates for restaurant and bar limitations. Column (2) shows estimates for essential business closures. Column (3) shows estimates for stay-at-home orders. For more details, see Section 5.2.

Appendix Table A9: Event Study Estimates: Case Growth

	(1)	(2)	(3)	(4)	(5)	(6)
	Restaurant and Bar Limitations	Essential Business Closures	Stay- at- Home Orders	Large- Gatherings Bans	School Closures	Public Health Emergencies
-7	286.6 (259.8)	51.78 (72.45)	12.48 (69.17)	149.3 ⁺ (79.13)	617.8 (413.9)	-116.7 (106.4)
-6	143.7 (141.9)	-27.57 (44.22)	-47.14 (50.74)	84.29 ⁺ (42.90)	295.3 (204.3)	-83.14 (69.36)
-5	108.9 (111.2)	-58.01 (53.56)	-65.79 (58.23)	68.56 ⁺ (35.23)	244.6 (172.3)	-87.03 (69.76)
-4	79.08 (81.40)	-51.75 (49.27)	-67.19 (56.28)	53.48 ⁺ (26.92)	180.1 (129.6)	-74.12 (60.58)
-3	56.37 (53.74)	-58.09 (51.12)	-50.79 (55.74)	33.16 ⁺ (17.87)	131.6 (89.00)	-69.34 (52.74)
-2	31.51 (27.34)	-20.74 (31.60)	-25.30 (32.23)	20.60* (8.737)	65.11 (43.83)	-35.26 (29.69)
-1	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)
0	-21.58 (26.73)	-54.81 ⁺ (31.39)	-35.08 (30.60)	-11.01 (14.25)	-57.03 (36.89)	-24.06 (27.02)
1	-45.81 (58.89)	31.99 (31.80)	31.19 (28.09)	-28.04 (24.71)	-111.7 (76.38)	30.96 (29.57)
2	-66.73 (82.78)	51.07 (48.28)	41.48 (39.78)	-28.62 (44.27)	-172.3 (117.3)	31.15 (38.83)
3	-92.93 (103.0)	58.05 (75.42)	54.60 (60.63)	-22.50 (66.80)	-237.3 (160.5)	39.01 (56.06)
4	-73.45 (107.5)	46.77 (79.12)	34.22 (57.74)	-45.03 (71.00)	-301.4 (201.3)	23.06 (61.53)
5	-42.53 (106.0)	138.5 (111.3)	102.2 (81.92)	-45.51 (86.70)	-360.1 (243.1)	53.71 (68.55)
6	60.70 (138.2)	377.7 (267.0)	345.3 (246.9)	-57.93 (138.9)	-582.9 (381.4)	116.4 (147.9)
<i>N</i>	2244	2244	2244	2244	2244	2244
<i>R</i> ²	0.563	0.569	0.568	0.560	0.574	0.561

Standard errors in parentheses

⁺ $p < .1$, * $p < .05$, ** $p < .01$

Note: Table shows event study coefficients of the relationship between NPIs and case growth, based on Equation (1). Column (1) shows estimates for restaurant and bar limitations. Column (2) shows estimates for essential business closures. Column (3) shows estimates for stay-at-home orders. Column (4) shows estimates for large-gatherings bans. Column (5) shows estimates for school closures. Column (6) shows estimates for public health emergencies. The day prior to the announcement is normalized to zero and standard errors are clustered at the state level. For more details, see Sections 5.1 and 5.3.

Appendix Table A10: Event Study Estimates: Deaths

	(1)	(2)	(3)	(4)	(5)	(6)
	Restaurant and Bar Limitations	Essential Business Closures	Stay- at- Home Orders	Large- Gatherings Bans	School Closures	Public Health Emergencies
-7	126.6 (118.5)	32.77 (26.54)	27.28 (32.18)	53.27 ⁺ (28.38)	237.0 (178.4)	-32.13 (26.03)
-6	63.85 (64.34)	6.345 (9.547)	2.352 (10.54)	30.29* (14.25)	110.5 (86.57)	-19.34 (13.67)
-5	49.09 (50.66)	4.964 (7.833)	2.018 (8.807)	25.06* (11.71)	93.44 (73.10)	-16.65 (11.59)
-4	36.02 (37.85)	4.129 (6.353)	1.154 (6.944)	18.84* (8.743)	69.12 (54.67)	-13.10 (9.215)
-3	23.30 (24.87)	1.460 (4.042)	-0.295 (4.464)	12.34* (5.743)	50.37 (37.81)	-9.479 (6.509)
-2	11.24 (12.27)	-0.247 (2.097)	-1.189 (2.244)	6.053* (2.765)	24.98 (18.64)	-5.570 (3.684)
-1	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)
0	-11.45 (12.19)	-1.912 (2.499)	-3.143 (3.296)	-6.552 ⁺ (3.264)	-21.24 (16.57)	4.114 (3.492)
1	-22.62 (24.38)	-2.679 (5.651)	-5.708 (7.775)	-12.62* (6.044)	-44.24 (31.75)	9.781 (6.459)
2	-33.44 (36.65)	-1.132 (7.871)	-7.045 (10.53)	-19.08* (9.107)	-67.90 (49.33)	14.93 (10.45)
3	-44.09 (49.20)	3.478 (11.33)	-8.053 (13.30)	-24.91* (12.26)	-91.95 (66.80)	20.32 (14.30)
4	-54.11 (61.39)	3.764 (13.99)	-11.88 (18.23)	-31.00* (15.35)	-115.1 (83.80)	25.06 (17.32)
5	-63.79 (73.94)	5.460 (17.43)	-13.21 (21.84)	-36.03 ⁺ (18.01)	-136.8 (99.61)	29.84 (20.76)
6	-82.16 (113.8)	118.8 (91.04)	98.98 (68.99)	-47.85 (33.61)	-232.3 (167.2)	83.35 (61.01)
<i>N</i>	2244	2244	2244	2244	2244	2244
<i>R</i> ²	0.345	0.345	0.345	0.343	0.350	0.344

Standard errors in parentheses

⁺ $p < .1$, * $p < .05$, ** $p < .01$

Note: Table shows event study coefficients of the relationship between NPIs and deaths, based on Equations (1) and (3). Column (1) shows estimates for restaurant and bar limitations. Column (2) shows estimates for essential business closures. Column (3) shows estimates for stay-at-home orders. Column (4) shows estimates for large-gatherings bans. Column (5) shows estimates for school closures. Column (6) shows estimates for public health emergencies. The day prior to the announcement is normalized to zero and standard errors are clustered at the state level. For more details, see Sections 5.1 and 5.3.

B Estimating Policy Effects Using Proxy Data

One important challenge of using proxy data to estimate policy effects is relating the causal effects of the policy on the proxy to the causal effect of the policy on the outcome of interest. One straightforward solution is to perform an initial estimation step that relates the outcome of interest to the proxy. Once this relationship is known, the causal impact of the policy on the proxy can be “fed through” the model relating the proxy and the outcome of interest to obtain the causal impact of the policy on the outcome of interest.

B.1 Method 1: Estimating Policy Effects Using Data on the Outcome of Interest

To formalize this first method, consider a data set with units $i \in I$ and time periods t . Denote the set of policies of interest by \mathcal{P} . Denote the dummy variable describing whether an individual policy $p \in \mathcal{P}$ is active by P_{it} , the outcome of interest by U_{it} , and the proxy variable by S_{it} . The objective is to explain the relative contribution of each policy p on U_{it} . For this example, we will focus on estimating how policies p affect the average time trend of U_{it} between t_1 and t_2 :

$$\tilde{U} = \frac{1}{N_I} \sum_{i \in I} \sum_{t \in \{t_1, \dots, t_2\}} U_{it}$$

First, assume that S_{it} is a *relevant* proxy for U_{it} . That is, variation in S_{it} predicts variation in U_{it} . Note that this assumption must hold for any method that uses proxy variables for outcomes of interest. This first method directly tests the relevance condition in Equation 5, while our method imposes relevance as an assumption (ideally verified in prior studies). Additionally, we assume \tilde{U} is known and a causal effect γ_p of P_{it} on S_{it} can be obtained for each policy p . For example, in a linear regression:

$$S_{it} = \hat{\gamma}_p P_{it} + \omega_{it}$$

Then, the relationship between the proxy S_{it} and outcome of interest U_{it} is parameterized by $\theta_{U,S}$ and directly estimated. In the linear regression case, this is:

$$U_{it} = \hat{\theta}_{U,S} S_{it} + \zeta_{it} \tag{5}$$

Finally, parameters from the above regressions are combined to translate the causal effect of P_{it} on S_{it} into a causal effect of P_{it} on U_{it} by combining the above two relationships. In the linear case, this is simply:

$$\hat{\beta}_p = \hat{\theta}_{U,S} \times \hat{\gamma}_p$$

The share of \tilde{U} explained by policy p is then simply

$$\pi_p \equiv \hat{\beta}_p / \tilde{U}.$$

This approach is valid for *any subset of policies* for which $\hat{\gamma}_p$ can be estimated, but requires compiling enough data on U_{it} to estimate $\hat{\theta}_{U,S}$. The precision of the $\hat{\beta}_p$ estimate depends on the ability of the estimated model in Equation 5 to predict U_{it} .

B.2 Method 2: Using Proxies to Estimate Policy Effects With Limited Outcome Data

In cases where data on U_{it} are limited, our alternative method allows for estimation of the share of \tilde{U} when several additional assumptions hold.

Assumption 1: the effect of S_{it} on U_{it} must be proportional, that is, the relationship between the proxy and outcome has the form $U_{it} = \theta_{U,S} S_{it}$ for some $\theta_{U,S}$, which does not need to be estimated.

Assumption 2: the researcher must be able to specify *all* of the policies that affect \tilde{U} and estimate causal effects γ_p for all of them. Given these assumptions, the *share* of \tilde{U} caused by any policy $p \in \mathcal{P}$ can be estimated by:

$$\pi_p = \frac{\gamma_p}{\sum_{p \in \mathcal{P}} \gamma_p} \quad (6)$$

Note, the share of any subset of policies $\mathcal{P}_s \subseteq \mathcal{P}$ can be computed similarly as:

$$\pi_p = \frac{\sum_{p \in \mathcal{P}_s} \gamma_p}{\sum_{p \in \mathcal{P}} \gamma_p}. \quad (7)$$

Note that the expression for π_p no longer requires estimation of $\hat{\theta}_{U,S}$, and only depends on the γ_p parameters, which can be estimated using only data on the proxy variables S_{it} and the policies P_{it} . To recover the effect of p in units of U_{it} , we simply compute

$$\hat{\beta}_p = \pi_p \times \tilde{U}.$$

Hence, we have directly recovered the causal impact of p on U_{it} , without having to estimate the relationship between the proxy S_{it} and U_{it} .

This method uses the simple idea that the effect of a policy can be estimated as a *share* of a total known quantity of the outcome variable (e.g. total UI claims in a given period), if the researcher can account for all policies that would affect this total quantity. In our context, we define \tilde{U} as the total UI claims between March 14 and March 28, and Assumption 2 is satisfied by defining the set of policies as the NPIs plus the direct effects of the pandemic. The causal effects of each NPI are estimated using an event study approach, and the direct pandemic effects are estimated as the time trend in Google searches that remains after netting out the effect of the NPIs. For this interpretation to hold, this assumes that the only reason that Google search volume for “file for unemployment” was elevated from March 14 to March 28 relative to March 1st is due to direct pandemic effects. For more details, see Sections 5.1 and 5.2 above.

C Verifying the proportionality assumption

In order to implement our method of using a proxy (internet search volume) to estimate policy effects, we require that the outcome variable (UI claims) be proportional to the proxy (Assumption 1, see Section B.2). In this section, we demonstrate that the proportionality assumption between search volume and UI claims is reasonable. Although not all proxies can be verified in this way, we note that in some cases (such as ours), the proportionality assumption can be directly verified. Moreover, in cases where proportionality appears to fail, there may be a transformation of the proxy variable such that the resulting relationship between outcome and transformed proxy is proportional. In such cases, the transformed proxy could be used, and our method remains valid.

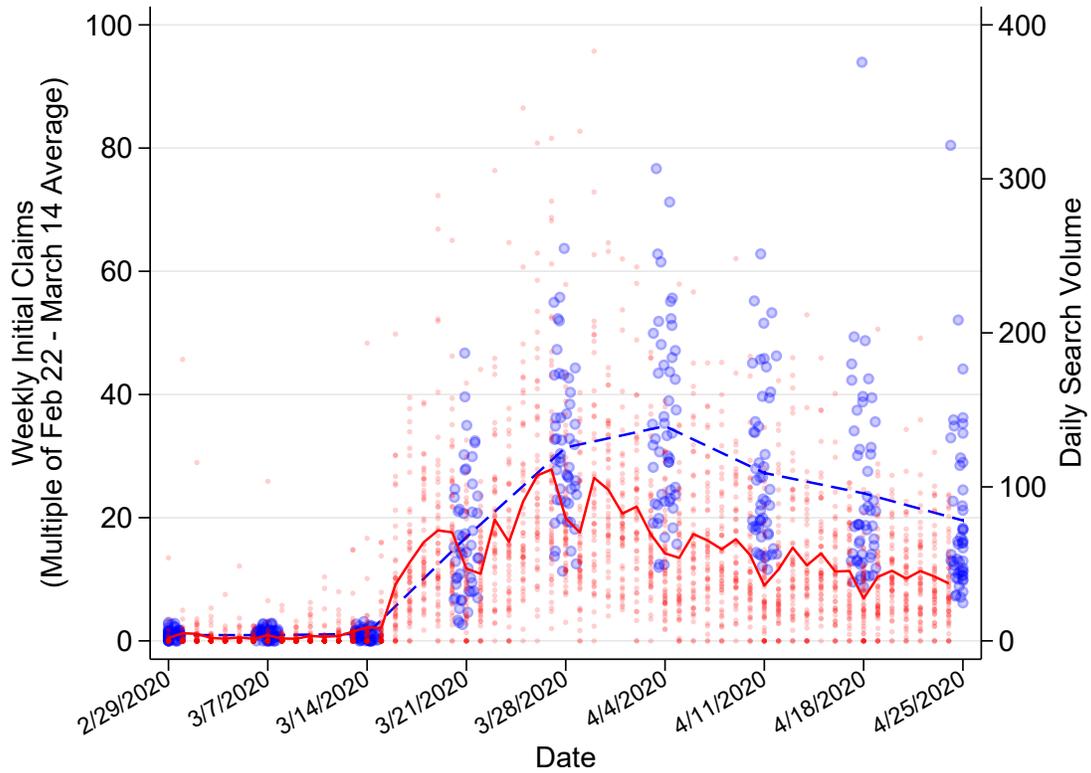
The following analysis is similar to the estimation procedure used by [Goldsmith-Pinkham and Sojourner \(2020\)](#), except we conduct our estimation using state-by-week UI claims from the Department of Labor ([U.S. Department of Labor, 2020](#)), rather than report-level claims. In addition, rather than using the change in Google search intensity, we use the average search volume in each week (which corresponds to the logic of integrating under the search volume curve, as in Section 5.4). We construct a similar measure of UI claims: for a given week, we normalize each state’s initial UI claims by the average number of claims filed during the four weeks ending February 22, 2020 through March 14, 2020. This accounts for variance in the sizes of different states. We construct this normalized measure of weekly UI claims for weeks ending March 21, March 28, April 4, and April 11. We are especially concerned with proportionality during the weeks ending March 21 and March 28, which are the two weeks to which we apply our share estimation procedure.

Figure C1 shows the raw data of weekly normalized UI claims plotted alongside search volume. Initial UI claims rose beginning in the weeks ending March 21 and March 28, peaked around the first week of April, and remained elevated (with a slight downward trajectory) throughout the rest of April. A very similar pattern is evident in the pattern of search volumes over time. Note that the search volume time series starts increasing at the same time as UI claims but peaks about a week earlier, potentially reflecting a time lag between when workers search for UI claims information online and when their claims are actually filed and processed. If this is the case, then the share estimate we obtain is still valid but should be multiplied by a different number of total UI claims, since some of the claims corresponding to searches conducted between March 14-28 may have been eventually filed in the following week. Because the lag is not very large, we will continue to match each week’s UI claims with the search volumes in the prior week. In addition, the proportionality results we present below are robust to lagging the search volumes by up to 7 days to account for the lag between searching and filing.

Figure C2 demonstrates that normalized UI claims are roughly proportional to contemporaneous search volumes, especially for the March 14-21 and March 21-28 periods, where a linear fit between the two variables has an R^2 of 0.67 and 0.79, respectively. We use a linear fit (omitting the constant term) to demonstrate that the relationship is indeed proportional. The estimated coefficient of proportionality k is quite stable between March 14 and March 28. Moreover, the strong relationship between UI claims and search volume persists into April as well. These results suggest that we can leverage search volumes as a valid proxy for initial UI claims in our main analyses. Recall that because the actual UI

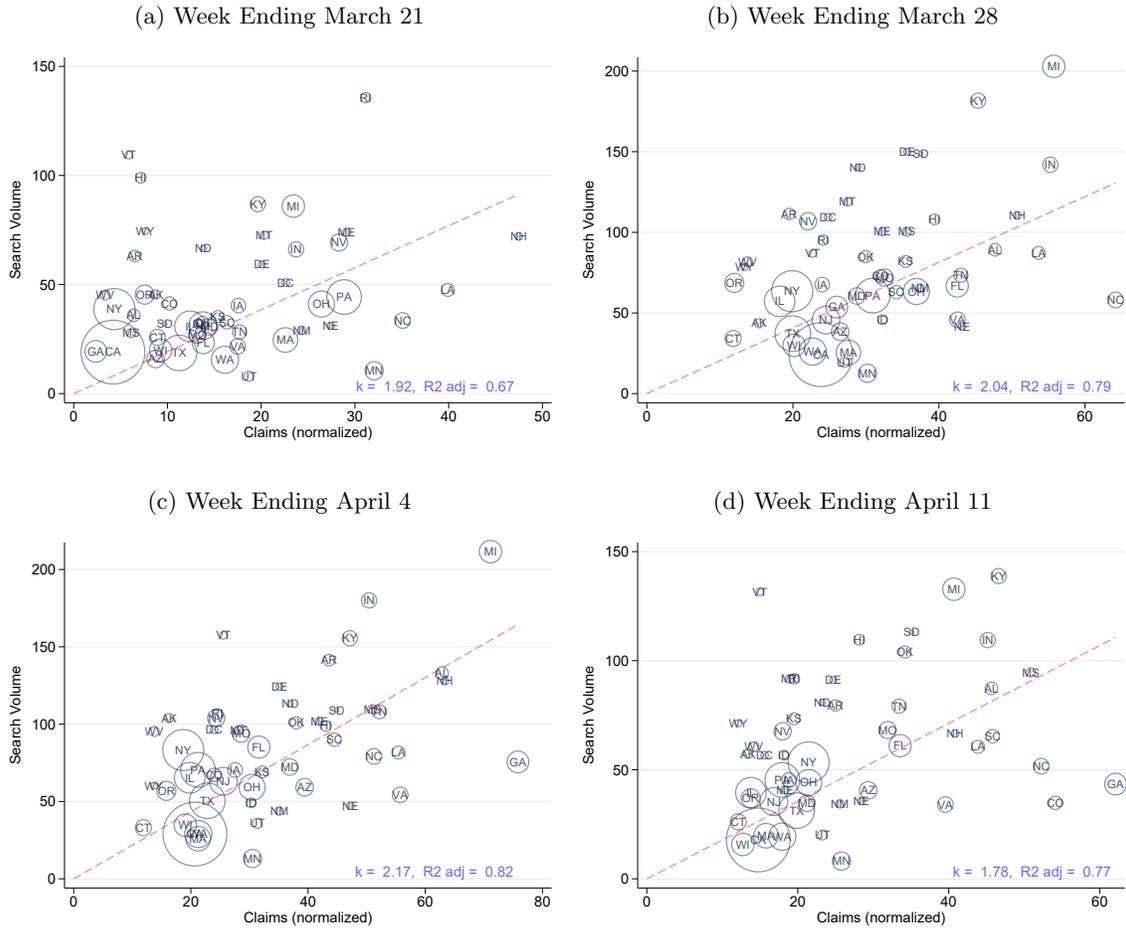
claims are only released on a weekly basis, we would not be able to use them to disentangle the effects of contemporaneous NPI announcements.

Appendix Figure C1: Time Series of Initial UI Claims and Search Volume



Note: Figure shows the time series of daily search volume (red dots, right Y-axis) and weekly initial claims (blue dots, left Y-axis) for each state. Initial UI claims are expressed as a multiple of each state's average UI claims for the weeks ending February 22, 2020 through March 14, 2020. The dashed blue lines indicate the average UI claims filed each week, and the red line indicates the average search volume each day.

Appendix Figure C2: Assessing Proportionality Between Search Volume and Initial UI Claims



Note: Figure shows scatter plots of average search volume (on the Y-axis) against initial UI claims (on the X-axis) at the state-week level. The measure of search volume averages over the 7 days up to and including the date of the UI claims. For example, for the week ending March 21, each state's search volume is an average of March 15 through 21. The number of UI claims in each state is normalized by each state's average initial claims in the prior period (weeks ending February 22, 2020 through March 14, 2020). The size of the circles reflects this average of prior-period UI claims. Panels (a) and (b) show this relationship during the weeks we consider when constructing our share estimates (see Section 5.4). The dashed red line indicates the line of best fit from a uni-variate regression of search volume on normalized claims that omits a constant term. The bottom right of each panel contains text reporting the coefficient from this regression (k) and the adjusted R^2 .

D Difference-in-Differences

We also estimate a difference-in-differences event study specification where we compare “early adopters” and “late or never adopters” of NPIs. We label states as “early adopters” if they announced their first NPI¹⁴ within a week of the first state (March 13-17). We label states as “late adopters” if they announced an NPI on or after March 18, or not at all. (See Figure 1, Appendix Figure A1, and Appendix Table A1 for details on when each state announced its policies.) We estimate a regression of the form:

$$S_{it} = \sum_{\tau=March7}^{March21} \delta_{\tau} \times 1 \{Early\ Adopter, t=\tau\} + \beta \times 1 \{Early\ Adopter\} + \xi_t + \mu_{it}, \quad (8)$$

where the δ_{τ} coefficients describe the differential evolution of search volume in “early adopters” relative to “late adopters.” We normalize $\delta_{\tau=March12} = 0$, so β captures the average difference in S_{it} between early and late adopters on March 12th. The ξ_t denote date fixed effects which control for the time trend in search behavior for the late adopters. We limit to the period of March 7 to March 17, which allows for 6 days where no states have announced restaurant and bar limitations, followed by 5 days where the early adopters began announcing limitations but the late adopters did not. The late adopters are thus never treated during our estimation window. We also estimate a version of the difference-in-differences regression where we pool all dates before March 13 into a single pre-period and all dates on or after March 13 into a single post-period:

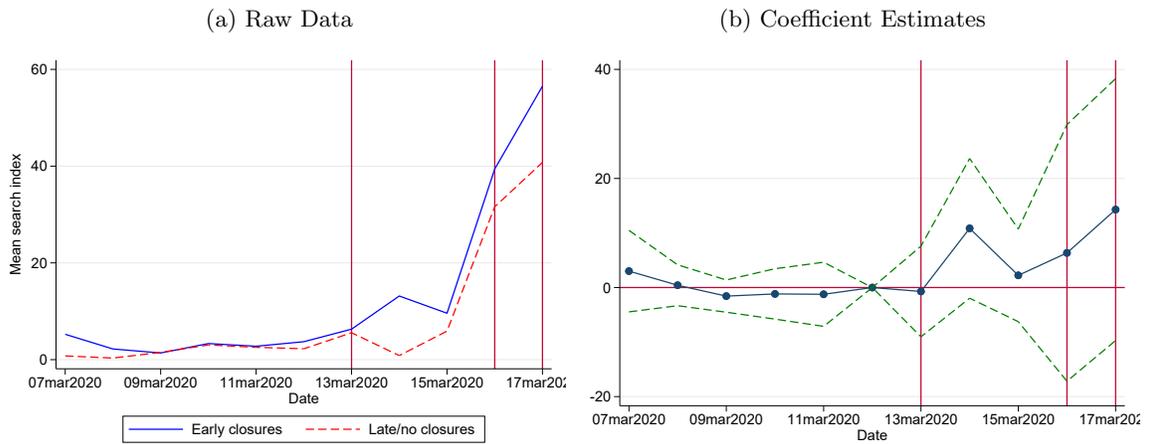
$$S_{it} = \alpha + \delta \times 1 \{Early\ Adopter\} \times 1 \{Post\} + \beta \times 1 \{Early\ Adopter\} + \xi_t + \mu_{it}, \quad (9)$$

where the single δ coefficient measures the differential change between the pre-period and the post-period for the early adopters.

This difference-in-differences approach has some advantages and disadvantages relative to our main event study approach. It is a transparent approach that where control states are those that did not announce any NPI during the timeframe we use for estimation. On the other hand, these late-adopter states are more likely to be different on unobservable dimensions. We include this approach to offer additional evidence for our finding that NPIs increase search activity, but our quantitative estimates of UI claiming rely on our event study approach.

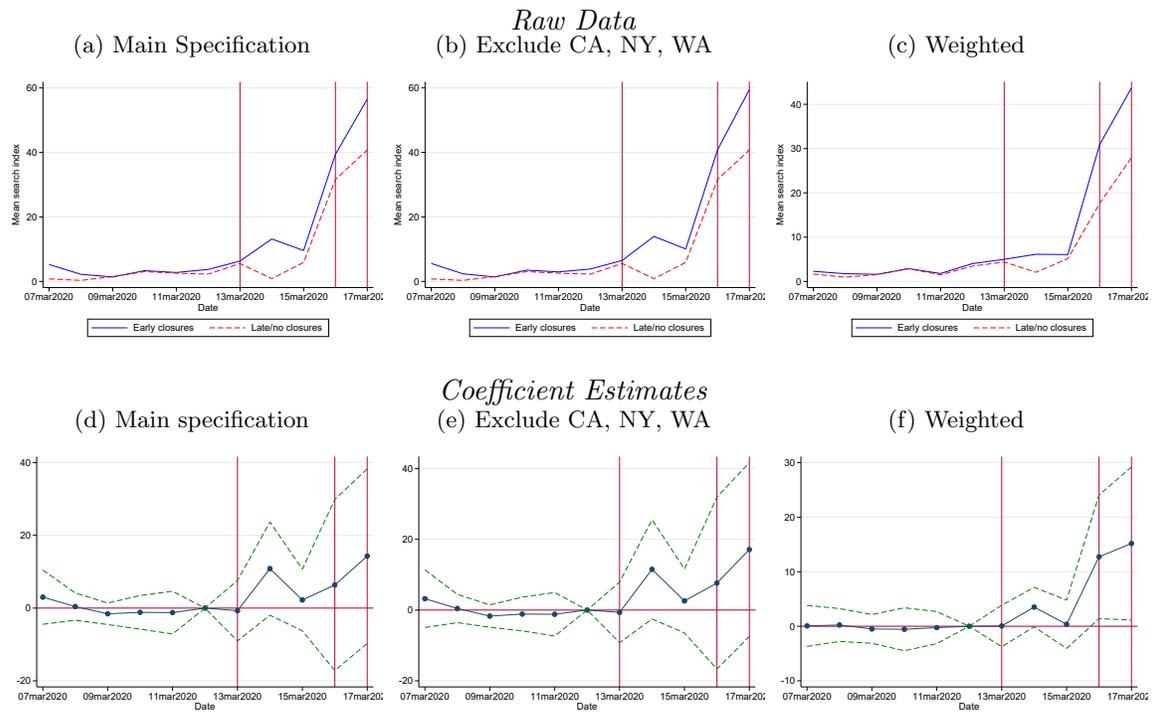
¹⁴Most states’ first NPI was restaurant and bar limitations.

Appendix Figure D1: Difference-in-Differences Estimates — Early vs. Late Adopters



Note: Figure shows difference-in-differences estimates of the impact of NPI announcements, comparing “early adopters” (March 13-17) with “late or never adopters” (after March 17 or never). We divide states based on the announcement date of their first NPI they adopt. Panel (a) shows raw data and Panel (b) shows regression coefficients, based on Equation (8).

Appendix Figure D2: Difference-in-Differences Estimates — Early vs. Late Adopters: Robustness



Note: Figure shows difference-in-differences estimates of the impact of NPI announcements, comparing “early adopters” (March 13-17) with “late or never adopters” (after March 17 or never). We divide states based on the announcement date of their first NPI they adopt. Panels (a), (b), and (c) show raw data and Panels (d), (e), and (f) show regression coefficients, based on Equation (8). Panels (a) and (d) show our main specification. Panels (b) and (e) show estimates excluding California, Washington, and New York. Panels (c) and (f) show estimates weighted by total employment in the state.

Appendix Table D1: Difference-in-Differences Estimates — Early vs. Late Adopters

	(1)	(2)	(3)	(4)
Post March 12	14.20** (3.333)	9.274** (2.017)	14.20** (3.335)	9.274** (2.018)
Early Closure States	-0.0781 (0.917)	-0.0655 (0.426)	0.0138 (0.936)	0.211 (0.451)
Post X Early Closure	8.139+ (4.815)	6.987* (2.904)	9.211+ (5.028)	9.029** (3.142)
Constant	2.737** (0.817)	2.159** (0.304)	2.737** (0.817)	2.159** (0.304)
N	867	867	816	816
R^2	0.162	0.215	0.164	0.214
Employment Weights	No	Yes	No	Yes
Drop WA, CA, NY	No	No	Yes	Yes

Standard errors in parentheses

+ $p < .1$, * $p < .05$, ** $p < .01$

Note: Table shows difference-in-differences estimates of the impact of NPI announcements, comparing “early adopters” (March 13-17) with “late or never adopters” (after March 17 or never), based on Equation (8). We divide states based on the announcement date of their first NPI they adopt. Column (1) shows our main specification. Column (2) shows estimates weighted by total employment in the state. Column (3) shows estimates excluding California, Washington, and New York. Column (4) shows estimates weighted by total employment in the state and excluding California, Washington, and New York. Standard errors are clustered at the state level.

E Case Study: Restaurant and Bar Limitations and Food Service Employment

Our empirical framework relies on the assumption that firms and individuals internalize how the information contained in NPI announcements affects firms’ optimal labor-force size and individuals’ employment probabilities. One test that could help validate this mechanism is to examine how Google searches change for individuals employed in industries directly affected by the NPI versus individuals employed in unaffected industries. In particular, we would like to assess whether the NPI of restaurant and bar limitations disproportionately affected employment expectations for food service workers. This is not possible to examine directly, since Google Trends data does not provide industry characteristics of searchers.¹⁵ Instead, we implement this test by using variation in states’ 2013-2017 employment shares in food service (measured using the American Community Survey and defined as the share of individuals employed in 2-digit NAICS code 72). We define high-food-service states as those with above-median employment in food service and run the following event study specification:

$$S_{it} = \sum_{\tau=-7}^6 (\hat{\gamma}_{\tau} \times 1\{r = \tau\} + \hat{\xi}_{\tau} \times 1\{r = \tau\} \times 1\{\text{High Food Service}\}) + \hat{\alpha}_i + \hat{\alpha}_t + \hat{\varepsilon}_{it} \quad (10)$$

where the $\hat{\gamma}_{\tau}$ coefficients now measure the response of Google search volume to restaurant and bar limitations for states with a below-median food service share and the sum $\hat{\gamma}_{\tau} + \hat{\xi}_{\tau}$ measures the response for states with an above-median food service share. We focus on the food service industry because it clearly corresponds to the policy of restaurant and bar limitations, which should have lowered firms’ expected need for labor (e.g. waitstaff). Non-essential businesses would have been another candidate for an industry-level analysis, but definitions of “essential” were often unclear or varied across states. In lieu of a representative set of “non-essential” industry codes, we focus our attention on the food service industry.

E.1 Computing the industry-specific share of UI claims caused by an NPI

This section extends the method introduced in 5.4 to compute the industry-specific share of UI claims caused by a particular NPI. If the NPI p (here, restaurant and bar limitations) targets *only* industry s (here, Accommodation and Food Services) and $\rho_s \in [0, 1]$ is the industry s share of the overall increase in UI claims, then the share of UI claims for s that was caused by the NPI can be estimated as:

$$\text{Share of UI claims in industry } s \text{ caused by NPI } p = \frac{I_{p,t_1,t_2}}{\rho_s \times (I_{\alpha,t_1,t_2} + \sum_p I_{p,t_1,t_2})}. \quad (11)$$

E.2 Event Study Results by Share of Food Service Employment

Figure E1 and Table E.3 report event study results separately for states with high (above-median) and low (below-median) food service employment shares, estimated from Equation 10. The point estimates suggest that the effect of restaurant and bar limitation announcement is larger for states with a high share of their residents employed in food service. However, we are not able to detect a statistically significant difference between the coefficients due to low statistical power.

¹⁵Search volumes for industry-specific terms such as “restaurant jobs” are too low to analyze at the state-day level

E.3 Share of UI Claims in Accommodation and Food Services Caused by Restaurant and Bar Limitations

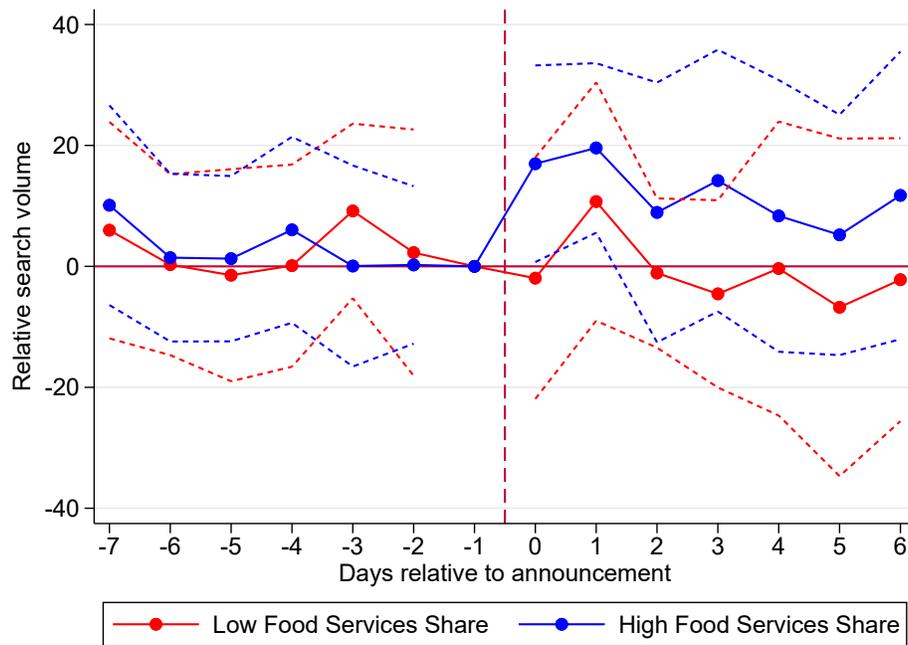
Finally, we calculate the number of UI claims filed as a result of restaurant and bar limitations as a share of the total UI claims filed in the Accommodation and Food Services industry between March 14 and March 28. This analysis assumes that restaurant and bar limitations only affected UI claiming in the Accommodation and Food Services industry.

First, we estimate the share of initial claims filed between March 14 and March 28 in the Accommodation and Food Services industry using data from three states (Massachusetts, New York, and Washington) that have released UI data by industry (Table E2).¹⁶ We estimate the UI-claims-weighted average of this share to be 24.7% (about 2.5 million claims), where the weights we use are shown in Row 1 of Table E2.

As we report in Section 6.2, we estimate that 6.0% of all UI claims between March 14 and March 28 were caused by restaurant and bar limitations (about 612,000 claims). Assuming that all of these claims occurred in Accommodation and Food Services, we conclude that 24.3% of the claims filed in Accommodation and Food Services were caused by restaurant and bar limitations.

¹⁶Reliable national-level estimates of the contribution of individual industries to UI claims during the COVID-19 pandemic have not been released to date. Estimates using national-level data would differ if the Accommodation and Food Services share of new UI claims were different at the national level relative to Massachusetts, New York, and Washington.

Appendix Figure E1: Event Study Estimates by Share Employed in Food Service



Note: Figure shows event study estimates of the impact of the introduction of restaurant and bar limitations separately for states with below-median food service employment shares (in red) and above-median food service employment shares (in blue), based on Equation (10). The day prior to the announcement is normalized to zero and standard errors are clustered at the state level.

Appendix Table E1: Event Study Estimates: Low vs. High Food-Service Share

	(1)	(2)
-7	5.984 (-9.129)	10.107 (-8.422)
-6	0.284 (-7.635)	1.445 (-7.075)
-5	-1.457 (-8.948)	1.274 (-6.973)
-4	0.122 (-8.526)	6.044 (-7.843)
-3	9.168 (-7.363)	0.058 (-8.481)
-2	2.281 (-10.384)	0.237 (-6.643)
-1	0 0	0 0
0	-1.945 (-10.186)	16.971* (-8.296)
1	10.715 (-10.056)	19.584** (-7.154)
2	-1.101 (-6.299)	8.924 (-10.955)
3	-4.549 (-7.899)	14.185 (-11.044)
4	-0.356 (-12.399)	8.351 (-11.464)
5	-6.77 (-14.239)	5.206 (-10.143)
6	-2.2 (-11.936)	11.738 (-12.133)
<i>N</i>	2,805	
<i>R</i> ²	0.583	

Standard errors in parentheses

⁺ $p < .1$, * $p < .05$, ** $p < .01$

Note: Table shows event study coefficients of the relationship between restaurant and bar limitations and Google search volume separately for states with a below-median share of employment in food service and for states with an above-median share of employment in food service, based on Equation (10). Column (1) shows estimates for states with a low food-service share. Column (2) shows estimates for states with a high food-service share. The day prior to the announcement is normalized to zero and standard errors are clustered at the state level.

Appendix Table E2: Employment and unemployment statistics

	U.S.	Massachusetts	New York	Washington
Total UI Claims, March 14-28	10,174,000	328,967	449,778	291,854
UI Claims from Accommodation and Food Services		70,286	129,252	64,876
Share of UI Claims from Accommodation and Food Services, March 14-28		21.4%	28.7%	22.2%
Share Employed in Accommodation and Food Services	11.1%	10.1%	9.7%	10.0%

Note: Table shows employment and unemployment statistics for the U.S. and for three states (Massachusetts, New York, and Washington) for which industry-level unemployment claims are available. The source of national-level UI claims data is [U.S. Department of Labor \(2020\)](#). The source of Massachusetts UI claims data is [Massachusetts Executive Office of Labor and Workforce Development \(2020\)](#). The source of New York UI claims data is [New York State Department of Labor \(2020\)](#). The source of Washington UI claims data is [Washington State Employment Security Department \(2020\)](#). The source of national and state employment shares in the Food and Accommodation Services industry is [Bureau of Labor Statistics \(2020\)](#).

F Spillovers

Our main results only capture the direct effects that NPIs have on the states that implement them. This partial effect is policy-relevant for governors and other state policy makers who want to predict the short-run consequences of NPIs for their own state. However, NPIs in one state may have spillover effects on nearby states through decreased demand or disruption of local production networks. After taking spillovers into account, the direct *and indirect* effects of state-level NPIs may jointly account for a larger share of the increase in UI claims during our period of consideration (March 14-28).

Below, we present a supplementary analysis that provides a preliminary measure of the impact of spillovers. In an extension of our event study framework that includes both own-state and border-state NPI announcements, we find that 17% of UI claims (coming from an average of 4.6 bordering states) during our period can be associated with NPIs announced in bordering states, compared to 11.5% of UI claims associated with own-state NPI announcements, broken down into non-essential business closures (8.5%) and restaurant and bar limitations (2.9%). Due to a paucity of statistical power and additional assumptions that must be imposed, we caution against drawing strong inferences on the nature and magnitude of spillovers from this analysis. We do note that our point estimates of direct, own-state effects remain robust to the inclusion of border-state NPI announcements. Finally, even if spillovers from state NPIs could be fully accounted for, our analysis would still omit the effects of NPIs implemented by other levels of government (i.e. local or federal policies) as well as NPIs imposed by private firms, institutions, and individuals.

F.1 Methods for Modeling Spillovers

To model spillovers in our event study framework, our preferred specification estimates the following:

$$S_{it} = \sum_{p \in \mathcal{P}} \sum_{\tau=-7}^6 \tilde{\eta}_{p,\tau} \times 1\{r(p) = \tau\} + \sum_{p \in \mathcal{P}_b} \tilde{\beta}_p \frac{1}{|B(i)|} \sum_{s \in B(i)} 1\{r(p) \in [0, 6]\} + \tilde{\alpha}_i + \tilde{\alpha}_t + \tilde{\nu}_{it} \quad (12)$$

where S_{it} is Google search volume in state i and date t , \mathcal{P} denotes the set of included own-state policies, $r(p)$ denotes the days relative to the date that policy p was announced (which we define as day $r = 0$), \mathcal{P}_b denotes the set of included border-state policies, $B(i)$ denotes the set of states bordering index state i , and $\tilde{\alpha}_i, \tilde{\alpha}_t$ are state and calendar date fixed effects respectively. The coefficients of interest are the $\tilde{\eta}_{p,\tau}$'s (which capture direct NPI effects) and the $\tilde{\beta}_p$'s (which capture spillover effects from bordering states) for each policy p . We cluster standard errors at the state level and exclude Alaska and Hawaii as they border no other states. Under this specification, we can estimate each policy's direct effect on search volume, controlling for potential spillover effects from policies in other states.

The above specification makes several tacit assumptions that are necessary for modeling spillovers in a parsimonious way.

First, we assume that the magnitude of spillovers is uniform across all bordering states, and that the effects from each border state are additive. Second, because each index state may have a different number of border states (Figure F6, Panel (a)), we normalize the spillover effects by the number of

bordering states $|B(i)|$. Third, because we have limited power to estimate a full set of relative-time coefficients, we estimate a specification that captures border-state effects using a single dummy that equals one in the first 7 days following the border state’s NPI announcement and zero otherwise. We call this the “Week One” specification; we test alternative functional forms below.

Thus, the coefficients $\tilde{\beta}_p$ are interpreted as the effect of increasing, from 0 to 1, the share of border states that announced policy p sometime in the past week. One could imagine myriad modifications to these parametric assumptions, which we view as a source of uncertainty due to model specification. We will test several modifications, listed below, to trace out part of this potential uncertainty.

Modifications varying the set of border-state policies included: We explore differences in the set of border-state policies included in \mathcal{P}_b . We start by including all three NPIs that exhibit significant own-state effects when considered in isolation (Section 6.1). Next, we exclude stay-at-home orders, which do not have significant own-state effects after controlling for the other two policies (Section 6.2). Finally, we exclude non-essential business closures, which have significant own-state effects but do not appear to have significant border-state effects (see Figure F1, Panel (d)).

Modifications varying the functional form of spillover effects: In addition, we consider two additional functional forms for the spillover effects, which take the place of $1 \{r(p) \in [0, 6]\}$ in Equation 12. The first modification uses $1 \{r(p) \geq 0\}$, which assumes that border-state spillovers have persistent effects on the index state (we call this the “Announced Share” specification). This is the simplest functional form one might consider, but does not match the impulse-response nature of the event study coefficients that we find in practice. The second modification uses $\sum_{\tau=-7}^6 \tilde{\beta}_{p,\tau} \times 1 \{r(p) = \tau\}$, which is akin to the event-study coefficients we use to estimate the direct, own-state NPI effects. We call this the “Relative Time” specification. The downside of this functional form is that it adds a substantial number of parameters ($7 \times 3 = 21$ versus 3 parameters when all three border-state policies are included). This will result in increased noise, which we detect in Figure G5.

We estimate all $3 \times 3 = 9$ combinations of these modifications. We discuss the point estimates in the following section and inference in Section G.

F.2 Event Studies with NPI Announcements in Bordering States

The first set of figures below demonstrates that per-state spillover effects from bordering states are modest. Figure F1 shows event study estimates when NPIs in bordering states are also included in the regression using the “Relative Time” functional form described above. First, we note that our results for the three own-state policies (Panels (a), (c), and (e)) are almost identical to our baseline estimates, and the coefficients for the first full day after NPI announcement (relative day 1) remain significant for bar and restaurant closures and non-essential business closures. Panels (b), (d), and (f) show that the only statistically significant border-state effect comes from bar and restaurant closures in neighboring states. The event study coefficients are suggestive of a similar impulse-response pattern, and the pooled post-period coefficient (from the Week One specification) is significant. The effects of other NPI announcements in bordering states (non-essential business closures and stay-at-home orders) are not significant, with relatively flat point estimates.

The results suggest that bar and restaurant closures may have some spillover effects on neighboring states, whereas non-essential business closures and stay-at-home orders do not. One possible interpretation is that announcements of bar and restaurant closures (which were the earliest NPIs announced in all but 3 states) in bordering states affected own-state expectations regarding future NPIs or the regional progression of the pandemic. Like own-state stay-at-home orders, later NPI announcements in bordering states may not have provided much additional information. That being said, these results are too noisy to allow us to draw strong conclusions about the nature of NPI spillovers. Moreover, this analysis requires a number of additional assumptions (outlined in Section F.1), and we are less well-powered to detect border-state effects compared to the own-state effects in our main specifications.

In addition to border-state NPI announcements, we also explored whether NPIs announced in non-bordering states had effects on search volume in the index state. We found very limited evidence for these longer-range spillovers (see Figure F2), although this analysis was also relatively under-powered.

F.3 Quantifying the Impact of Spillovers

With the above caveats and considerations in mind, we extend our share-estimation procedure from Section 5.4 to translate our estimates of border-state effects into a share of UI claims associated with these effects. We do not interpret this as a quantification of all possible spillover effects, but rather as an exploratory exercise to determine the share of UI claims that would be explained by spillovers of similar magnitude to the border-state effects that we find.

To perform this calculation, we adopt a main specification that includes two own-state policies (restaurant and bar limitations and non-essential business closures) and the same two border-state policies. We adopt the “Week One” functional form for border effects. Because we find limited evidence of spillovers from distant states, we will omit NPI policies from non-bordering states to avoid over-fitting. To illustrate the uncertainties underlying this analysis, we conduct sensitivity analyses to the modifications outlined in the previous section as well as bootstrap inference to account for sampling error (see Section G for more details and results).

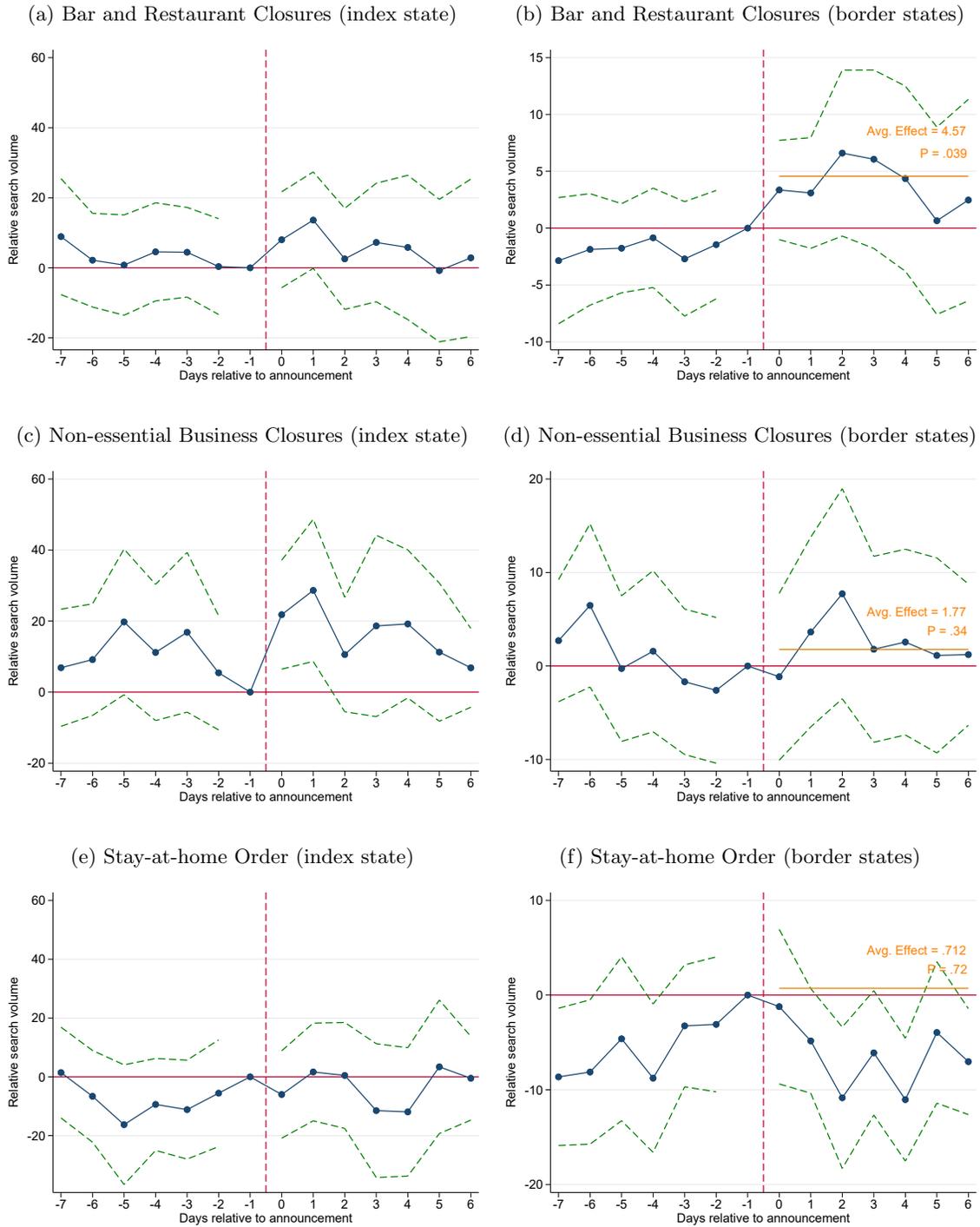
Appendix Figure F3 shows the event study results for our preferred specification with spillover effects. The own-state policies (Panels (a)-(c)) show similar results to our main baseline specification without spillover effects (see Figure 2). For NPIs announced in border states, we find significant effects for restaurant and bar limitations (when post-period coefficients are pooled) but not for non-essential business closures. The magnitude of the time trend (Panel (f)) is noticeably smaller than in our main specification without spillover effects (see Figure 4).

Appendix Figure F4 shows the average contribution of spillover effects when either the “Relative Time” or “Week One” functional form assumptions for border-state NPIs are used. Both versions show that spillovers from border states make modest contributions to the overall increase in search volume. Appendix Figure F5 shows the contributions of individual own-state policies (Panel (a)) and the relative contributions of the time trend, spillover effects, and own-state policies (Panel (b)). We see that own-state and spillover effects are comparable in magnitude, and that the effects of spillovers seem to peak earlier (similar to own-state restaurant and bar limits).

Using our preferred specification, we find that direct, own-state NPI effects account for 11.5% of UI claims during our period (only slightly less than the 12.4% we find in our main specification

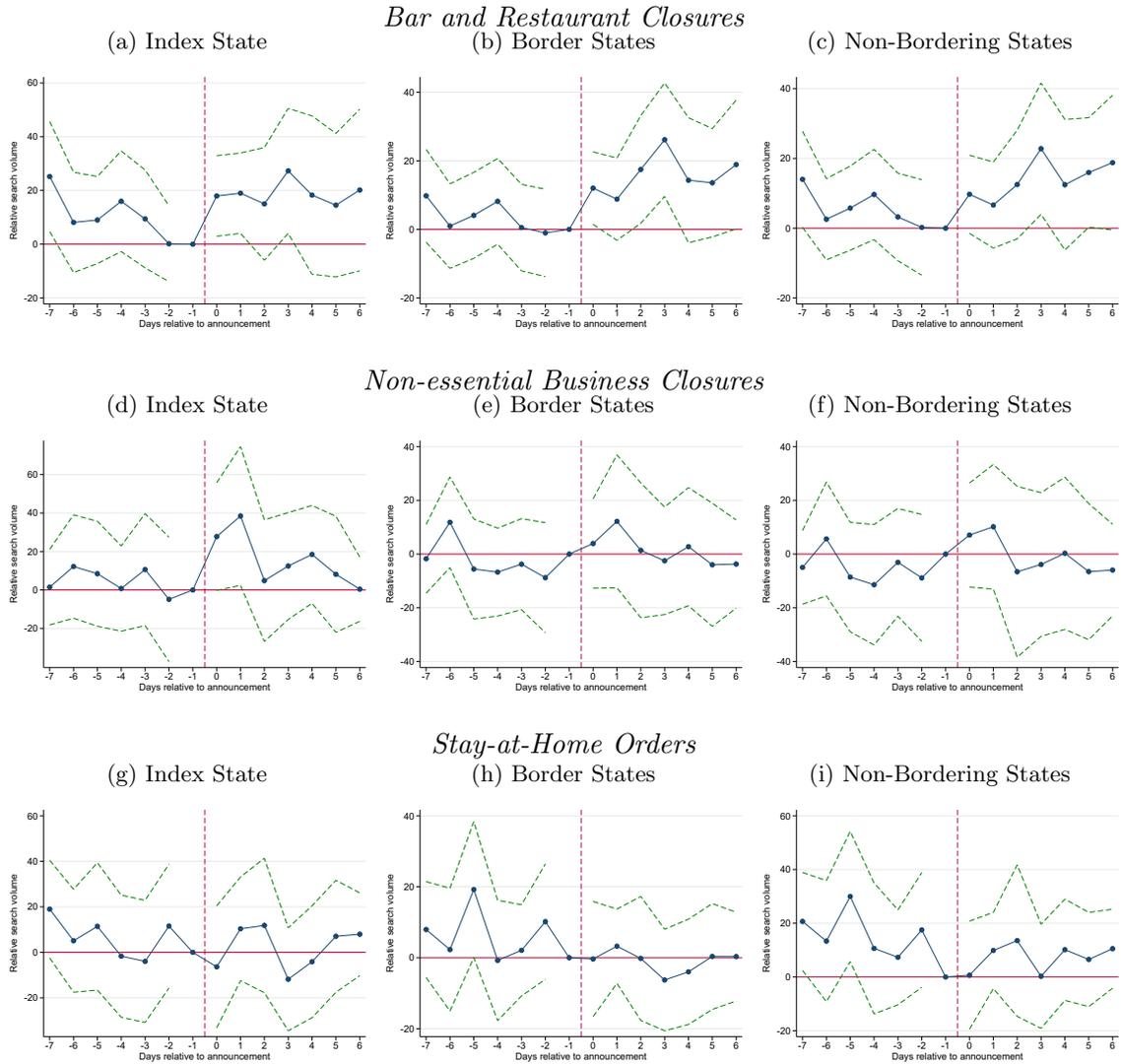
that excludes border effects). Restaurant and bar limitations account for 2.9% (6.0% in our main specification that excludes border effects), and non-essential business closures account for 8.5% (6.4% in our main specification that excludes border effects). We estimate that spillovers account for an additional 16.6% of UI claims. Given an average of 4.6 bordering states (see Figure F6), this translates to approximately a 3.6% spillover effect for each border state that announces both NPIs. This suggests that the spillover effect from NPIs in a bordering state is about a third the size of the corresponding direct, own-state effect. Combining direct and spillover effects, we estimate that 28% of UI claims can be attributed to state-level NPIs. Due to a paucity of statistical power and additional assumptions that must be imposed, we caution against drawing strong inferences on the nature and magnitude of spillovers from this analysis. Finally, even if spillovers from state NPIs could be fully accounted for, our analysis would still omit the effects of NPIs implemented by other levels of government (e.g. local or federal policies) as well as NPIs imposed by private firms, institutions, and individuals.

Appendix Figure F1: Border State Event Studies



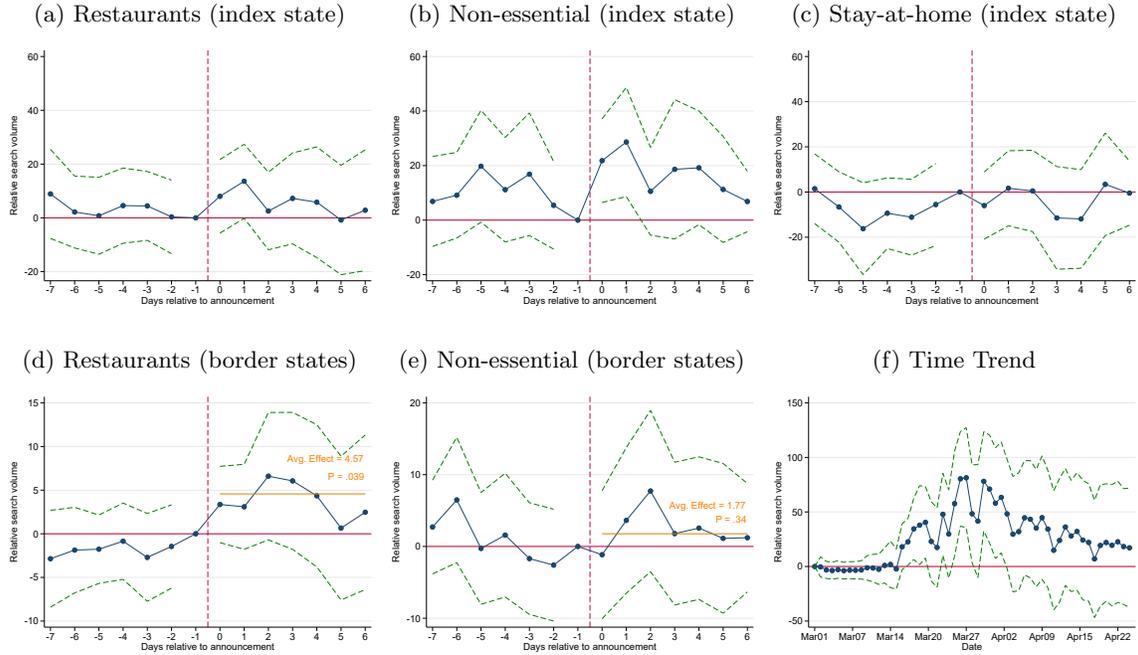
Note: Figure shows event study estimates of effects of NPIs announced in index states (left panels) vs. bordering states (right panels). The regression includes index-state and border-state NPI announcements for the following policies: bar and restaurant closures (Panels (a) and (b)), non-essential business closures (Panels (c) and (d)), and stay-at-home orders (Panels (e) and (f)). For comparability to the direct NPI effects of own-state NPI announcements, the effects for border-state NPIs are scaled by the average number of border states. The event study coefficients thus represent the average change in search volume associated with an NPI announcement in a single border state. The orange lines in the post-period indicate the estimate of the pooled post-period coefficients. See Appendix Section F for more details.

Appendix Figure F2: Adding Non-Bordering State NPIs



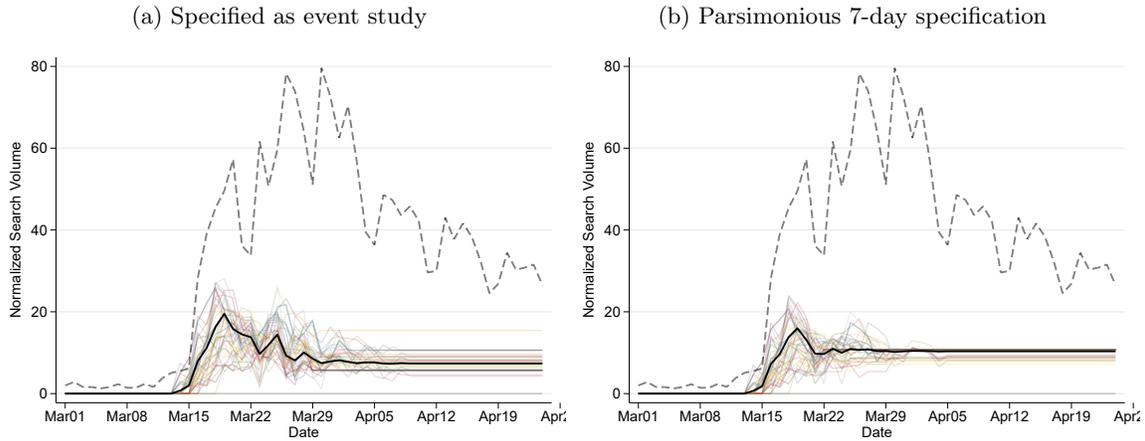
Note: Figure shows event study estimates of effects of NPIs announced in non-bordering states, when these announcements are added to the event study specification in Figure F1. The regression includes index-state, border-state, and non-bordering state NPI announcements for the following policies: bar and restaurant closures (Panel (a)), non-essential business closures (Panel (b)), and stay-at-home orders (Panel (c)). For comparability to the direct NPI effects of own-state NPI announcements, the effects for border-state and non-bordering state NPIs are scaled by the average number of border states and non-bordering states respectively. The event study coefficients thus represent the average change in search volume associated with an NPI announcement in a single bordering/non-bordering state. See Appendix Section F for more details.

Appendix Figure F3: Main Border State Specification



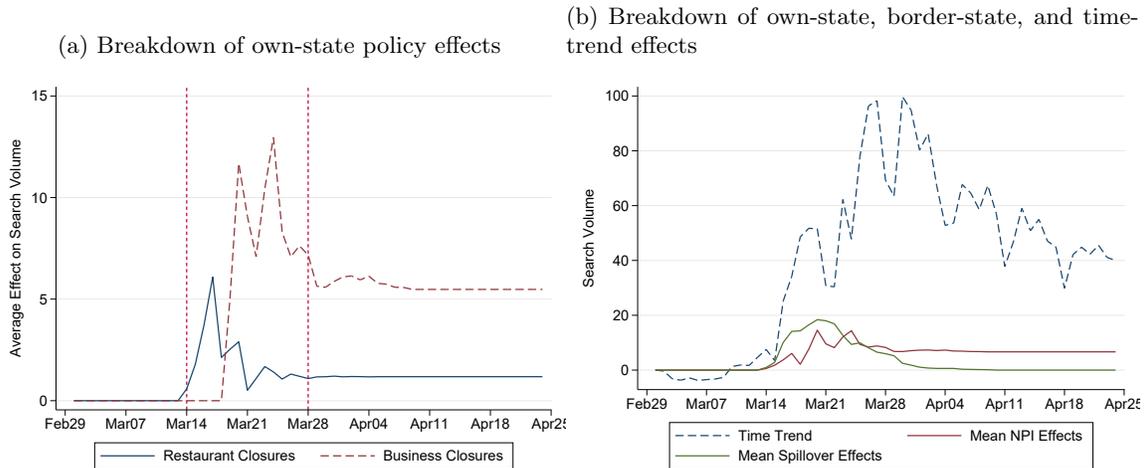
Note: Figure shows event study estimates of effects of direct, own-state NPI effects as well as spillover effects from bar and restaurant closures in bordering states. The regression includes own-state and border-state NPI announcements for the following policies: bar and restaurant closures (Panels (a) and (d)), non-essential business closures (Panels (b) and (e)), and stay-at-home orders (Panel (c), border-state version not significant and not shown). For comparability to the direct NPI effects of own-state NPI announcements, the effects for border-state and non-bordering state NPIs are scaled by the average number of border states and non-bordering states respectively. The event study coefficients thus represent the average change in search volume associated with an NPI announcement in a single bordering/non-bordering state. See Appendix Section F for more details.

Appendix Figure F4: Border Effects From Preferred Specification



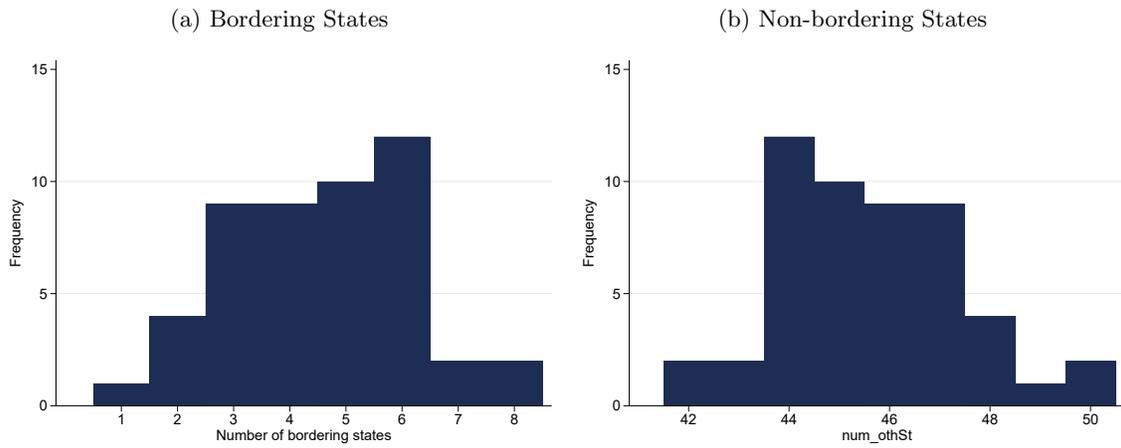
Note: Figure shows border effects from our preferred specification that includes own-state and border-state announcements of two policies: restaurant and bar limitations and non-essential business closures. Border effects for each index state are plotted, with the black, solid line representing the weighted average of the border effects. Weights are proportional to the number of UI claims in each state for the period Feb 22, 2020 through March 14, 2020.

Appendix Figure F5: Breakdown of Effects on Search Volume



Note: Figure shows how the overall time trend of search volume is decomposed over time. The underlying event study regression includes two own-state and border-state policies: restaurant and bar limitations and non-essential business closures. The “Week One” functional form assumption is used for the border effects. Panel (a) shows the contribution of each direct, own-state NPI policy to the overall time trend of search volume. Panel (b) shows how the weighted average search volume is decomposed into an overall time trend, weighted-average direct NPI effects, and weighted-average spillover effects. The weights are proportional to the number of UI claims in each state for the period Feb 22, 2020 through March 14, 2020.

Appendix Figure F6: Number of Bordering and Non-Bordering States



Note: Figure shows the distribution of the number of bordering states for each index state and Washington, D.C. (Panel (a)), and the number of non-bordering states for each index state (Panel (b)). Alaska and Hawaii have no bordering states and are not included in Panel (a).

G Inference and Model Uncertainty

Our share estimator incorporates all of the post-announcement coefficients in our event study specification, regardless of whether the individual coefficients are statistically significant. This is necessary for our share estimator to be unbiased; conditioning on a particular level of significance for each of the relative-time coefficients would result in a share estimator that is biased toward zero, especially because the shape of the response in search volume tends to be an impulse response that reaches a peak before returning to zero.

In our framework, the standard errors of the event study coefficients are used to determine whether particular policies have statistically significant effects on search volume in any post period, hence informing the set of policies that we include in our joint estimation procedure (Section 5.2) and share estimator. Conditional on a set of policies identified to have significant effects, we are no longer concerned with determining the significance of particular relative-time coefficients. That said, we would like to have some way to incorporate sampling error and model uncertainty into our share estimate.

We incorporate uncertainty into our share estimates in two ways. First, we use a cluster bootstrap procedure to generate confidence intervals for each of our share estimates. This procedure automatically accounts for the non-linearity of the share estimator as well as within-state correlation in the error terms. Second, we also explore how modeling choices (e.g., the set of NPIs included in the estimation and the functional form of spillover effects) affect the distribution of our share estimates. One source of uncertainty that is important, but that we do not explicitly address, is the uncertainty due to an imperfect correlation between UI claims and search volume. That said, we demonstrate that search volume has high predictive power for UI claims (Appendix C).

Rather than reporting a single point estimate for the share of UI claims associated with NPIs, our approach translates the sampling error in the data and inherent uncertainty regarding model specification into a distribution of share estimates and clearly delineates between these sources of uncertainty. See below for more details.

Inference Methods: We use a cluster bootstrap procedure to assess the impact of sampling error on our share estimate. For each bootstrap iteration, we draw, with replacement, a sample of 51 states. We then run our joint estimation procedure (Section 5.2) and compute share estimates (Section 5.4). We repeat this for 1,000 bootstrap iterations to generate the joint distribution of the share estimates for each own-state policy, the total share associated with direct (own-state) NPI effects, and the additional share associated with spillovers (for specifications that include spillovers; see Appendix F for more details).

We construct 95% confidence intervals of the form $[\mu - 1.96 \times SD, \mu + 1.96 \times SD]$, where μ denotes the mean of the bootstrap estimates and SD denotes the standard deviation of the bootstrap estimates. This relies on an assumption of asymptotic normality for the sampling distribution of the share estimates, which we show is approximately true by fitting normal density functions in the figures below. We also show graphically the confidence intervals that would result from using the 2.5 and 97.5 percentiles of the distribution of bootstrapped estimates.

Inference Results (version without spillovers): Figure G1 shows bootstrap results for our main specification that does not include spillover effects, described in Section 6.2. The figure shows the marginal distributions of share estimates for each own-state policy and the distribution of the total share associated with direct NPI effects. We observe that the share estimate for restaurant and bar limitations is quite noisy, with a 95% CI between -14.3% and 29.4%, centered on an original point estimate of 6.0%. The distribution for the other two policies are much less variable: non-essential business closures are associated with a 6.4% (95% CI -2.6% - 15.4%) share of UI claims and stay-at-home orders are associated with a 0.0% (95% CI -8.3% - 7.9%). The effect of all three NPIs has a point estimate of 12.4% and a 95% CI of -10.8% - 38.4%, where the variability is primarily driven by restaurant and bar limitations.

One reason for the greater uncertainty surrounding restaurant and bar limitations could be timing: this NPI was announced in mid-March at the same time that search volumes and UI claims began to spike upward. This may make it particularly difficult to disentangle this NPI effect from the aggregate time trend. A further reason for additional variance for all three NPIs could be heterogeneity in treatment effects. Our model estimates the average NPI effect across states, but it's possible that a given NPI may have stronger effects in certain states. Indeed, we document a heterogeneous response to restaurant and bar limitations (based on states' baseline employment in food services) in our case study on the Food and Accommodation Services industry (Appendix Section E).

Lastly, while the 95% confidence intervals for the bootstrapped share estimates do not always reject the null hypothesis of no effect, we interpret these distributions as visualizing uncertainty in our policy-relevant parameter (the share of UI claims caused by NPIs), rather than a statistical test for whether these NPIs increased UI claims at all (indeed, this test is already satisfied by our event study results that show statistically significant effects of these NPIs on search volumes). Because the share estimator requires using all of the post-announcement event study coefficients (including the ones that do not reach statistical significance), we do not necessarily expect the bootstrapped share estimates to always reject zero at the 5% level. On the other hand, the bootstrapped confidence intervals do suggest that we can rule out direct effects *exceeding* 38%. Indeed, the "significance" of our estimates depends on the nature of the null hypothesis (which may reasonably be either 0, 1, or an intermediate value).

Our bootstrap procedure also allows us to visualize the joint distribution of the share estimates. Figure G2 shows the joint distributions of pairs of share estimates. These results show negligible correlations between the restaurant and bar limitations share and those of the other two policies. However, the non-essential business closures share is negatively correlated with that of stay-at-home orders, suggesting that these two policies exhibit some collinearity that affects estimation. This provides a rationale for excluding stay-at-home orders from our specifications (both in terms of own-state and border-state policies). We assess the impact of this exclusion below.

Inference Results (version with spillovers): Figure G3 shows results from our main specification that includes spillovers. This specification includes restaurant and bar limitations and non-essential business closures for both the index state and bordering states. It models spillovers in each border

state via an indicator variable that takes value 1 in the 7 days after NPI announcement. The share estimate for restaurant and bar limitations is still quite noisy and estimated to be 2.9% (95% CI -16.1% - 25.3%). Non-essential business closures are associated with a 8.5% (95% CI 1.0% - 15.9%) share of UI claims. The total direct effect is 11.5% (95% CI -10.1% - 36.1%), and the spillover effect is 16.6% (95% CI 1.2% - 30.7%). The sum of the direct and spillover effects is 28.0% (95% CI 0.1% - 57.7%). Note that while the total direct effect does not reject zero, this is due to the restaurant and bar limitations NPI; the non-essential business closures NPI has a much less variable share that does reject zero. The spillover share is large in magnitude and rejects zero.

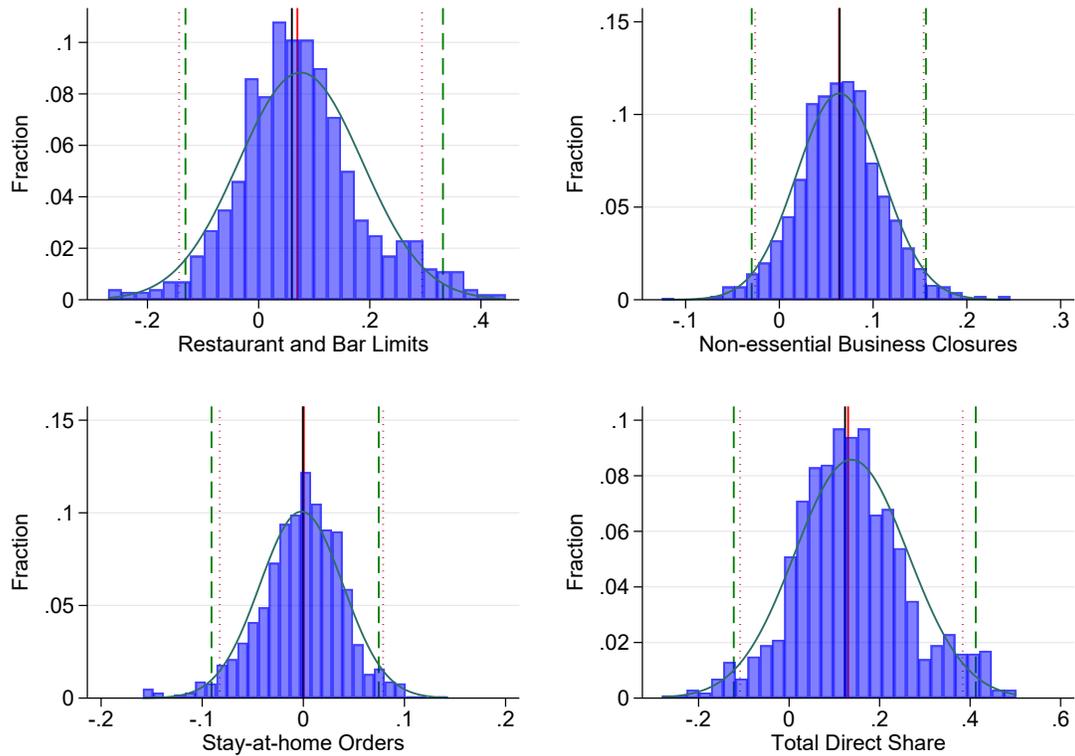
Figure G4 shows the impact of adding spillover effects and varying the functional form of these spillover effects on the direct (own-state) share estimates. We find that the distributions of direct effects are robust to whether or not we account for spillovers and the particular functional form we specify for these spillovers. This suggests that our estimates of the share of UI claims due to direct NPI effects are valid, whether or not we are able to model spillovers correctly.

Although the direct policy effects are robust to alternative functional forms for spillovers, varying the functional form of spillovers does affect our estimates of the share of UI claims associated with spillovers. Figure G5 shows the impact of varying the spillover functional form on the magnitude of the spillover share.¹⁷ Assuming spillover effects persist after border states make announcements (“Announced Share” version) suggests a larger role for spillovers, whereas modeling spillovers as relative-time effects (“Relative Time” version) suggests a smaller role for spillovers. Our preferred functional form allows spillovers to act in the week after border-state NPI announcements (“Week One” version); the resulting spillover share lies between the other two functional forms, and also exhibits the least variable distribution. This may be because the “Week One” version is parsimonious (only requiring one coefficient per included NPI) but also reflects our previous finding that changes in search volume tend to diminish after one week. Lastly, Figure G6 demonstrates that our share estimates are robust to excluding stay-at-home orders (which do not exhibit significant effects in the event studies after controlling for non-essential business closures).

Taken together, these results demonstrate that our share estimates contain uncertainty from a number of sources. First, there is a fair amount of sampling error (as evidenced by the bootstrapped distributions). Second, the functional form assumed for spillovers has modest effects on the share of UI claims associated with spillovers, but not on the share of UI claims associated with direct NPI effects. Third, varying the sets of own- and border-state policies (by removing policies that do not have significant effects in the event studies) does not affect the distributions of share estimates.

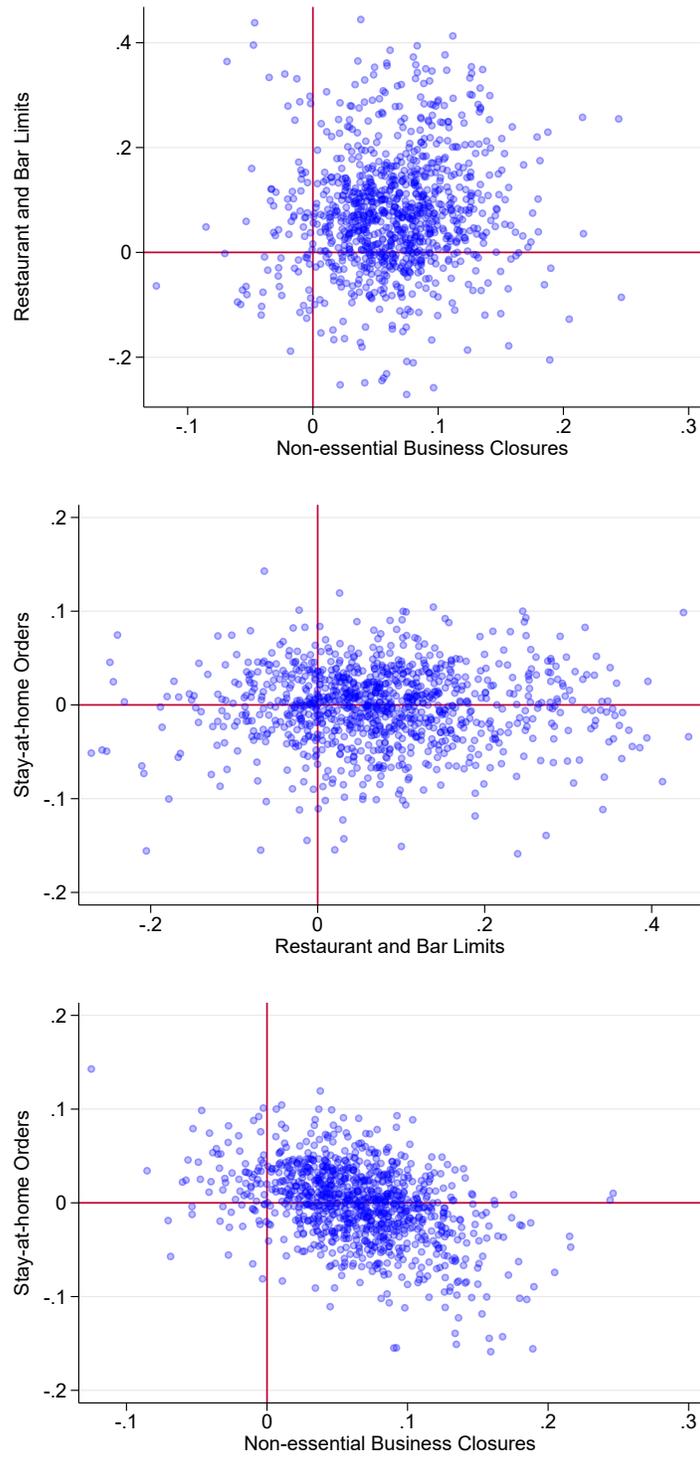
¹⁷This figure and Figure G6 both average over the number of border-state NPIs included, since Figure G4 suggests that the number of border-state NPIs does not greatly affect the results.

Appendix Figure G1: Bootstrap Inference, Main Specification



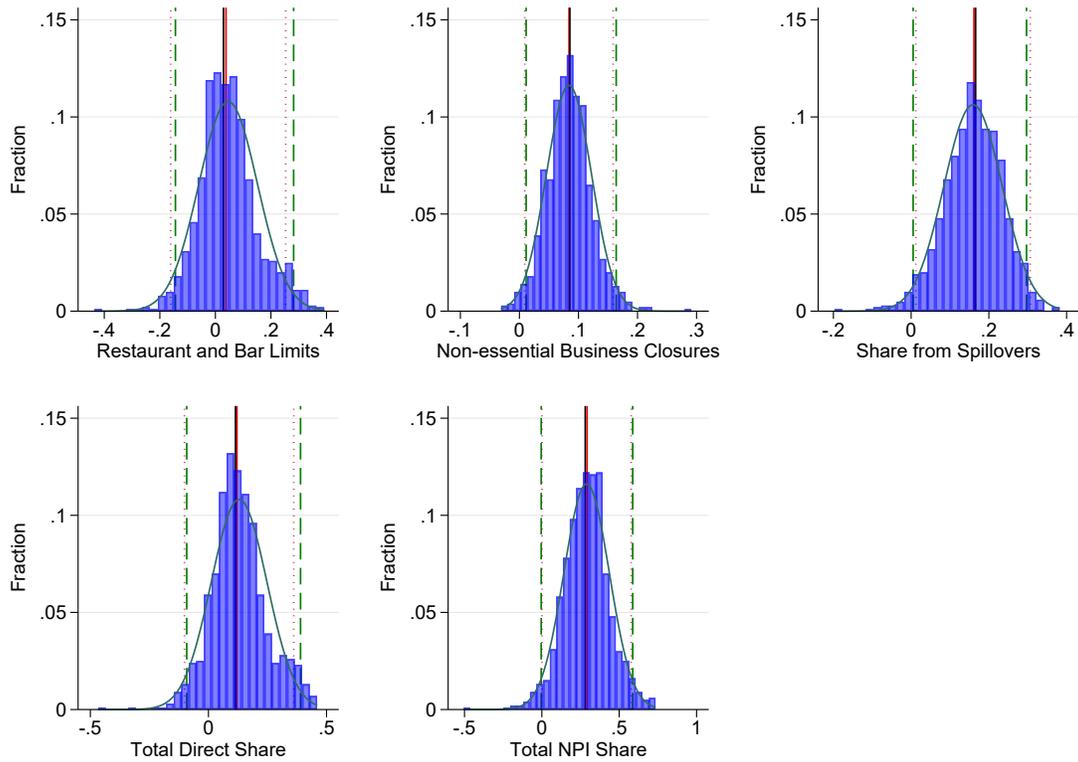
Note: Figure shows the marginal distributions of various share estimates from 1,000 cluster bootstrap iterations with fitted normal density functions. The solid red and black vertical lines denote the median of the bootstrap distribution and original point estimate respectively. The dotted red vertical lines denote the mean of the bootstrap distribution \pm times the standard deviation of the bootstrap distribution. The green dashed lines denote the 2.5 and 97.5 percentiles of the distribution of bootstrap estimates.

Appendix Figure G2: Joint Distributions of Estimates, Main Specification



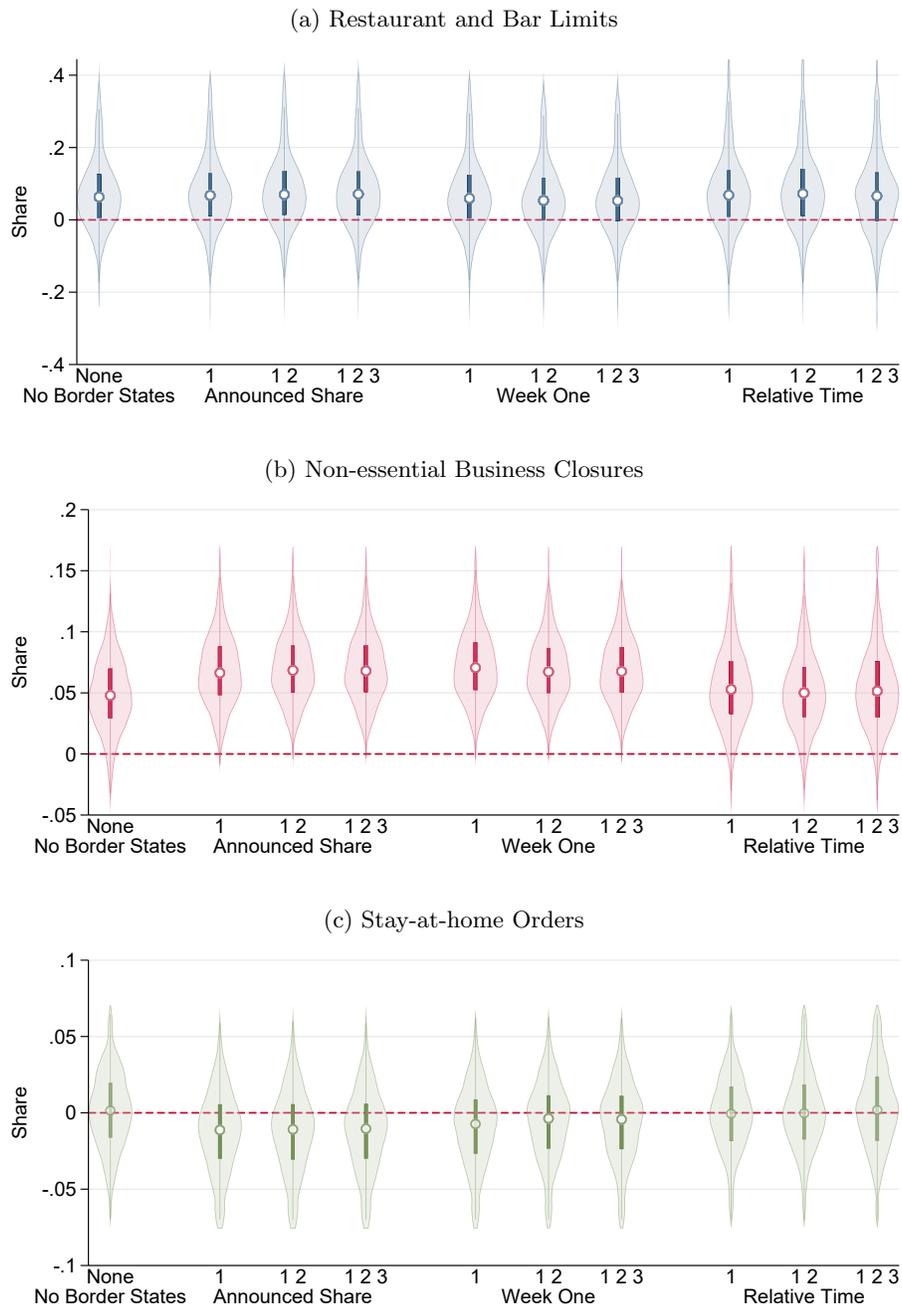
Note: Figure shows the joint distributions of pairs of share estimates from 1,000 cluster bootstrap iterations.

Appendix Figure G3: Bootstrap Inference, Main Specification Including Spillovers



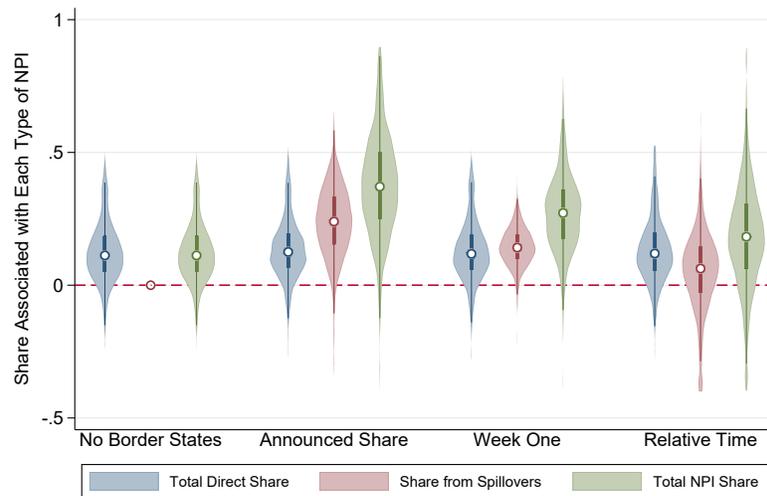
Note: Figure shows the marginal distributions of various share estimates from 1,000 cluster bootstrap iterations with fitted normal density functions. The solid red and black vertical lines denote the median of the bootstrap distribution and original point estimate respectively. The dotted red vertical lines denote the mean of the bootstrap distribution \pm times the standard deviation of the bootstrap distribution. The green dashed lines denote the 2.5 and 97.5 percentiles of the distribution of bootstrap estimates.

Appendix Figure G4: Impact of Spillover Functional Form on Direct Effects



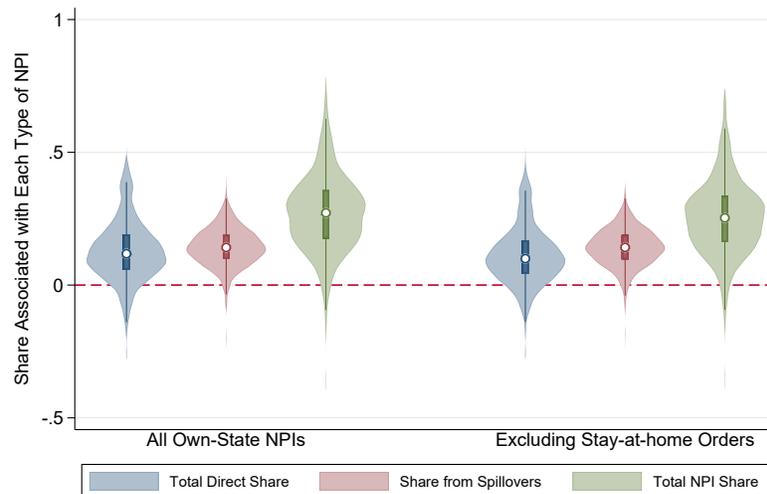
Note: Figure shows violin plots of the bootstrap distributions of each direct policy effect (i.e., own-state NPI announcements) for various ways of specifying spillover effects. Panel (a) shows shares due to own-state announcements of restaurant and bar limits; panel (b) shows shares due to own-state announcements of non-essential business closures; and panel (c) shows shares due to own-state announcements of stay-at-home orders. The first row of X-axis labels denotes the set of policies included for *border states*, where policies 1, 2, and 3 refer to restaurant and bar limits, non-essential business closures, and stay-at-home orders respectively. The second row of X-axis labels denotes the functional form assumption for the spillover effects.

Appendix Figure G5: Impact of Spillover Functional Form on Spillover Share



Note: Figure shows violin plots of the bootstrap distributions of the share due to all direct policy effects (i.e., own-state NPI announcements of all three policies), the share due to spillovers (averaging over specifications with one, two, and three border-state NPIs), and the total share due to NPIs (through direct effects and spillovers). These distributions are plotted for different functional forms for spillovers on the X-axis.

Appendix Figure G6: Impact of Changing the Set of Own-State NPIs



Note: Figure shows violin plots of the bootstrap distributions of the share due to all direct policy effects (i.e., own-state NPI announcements of all three policies), the share due to spillovers (averaging over specifications with one, two, and three border-state NPIs), and the total share due to NPIs (through direct effects and spillovers). These distributions are plotted separately depending on whether stay-at-home orders are included.