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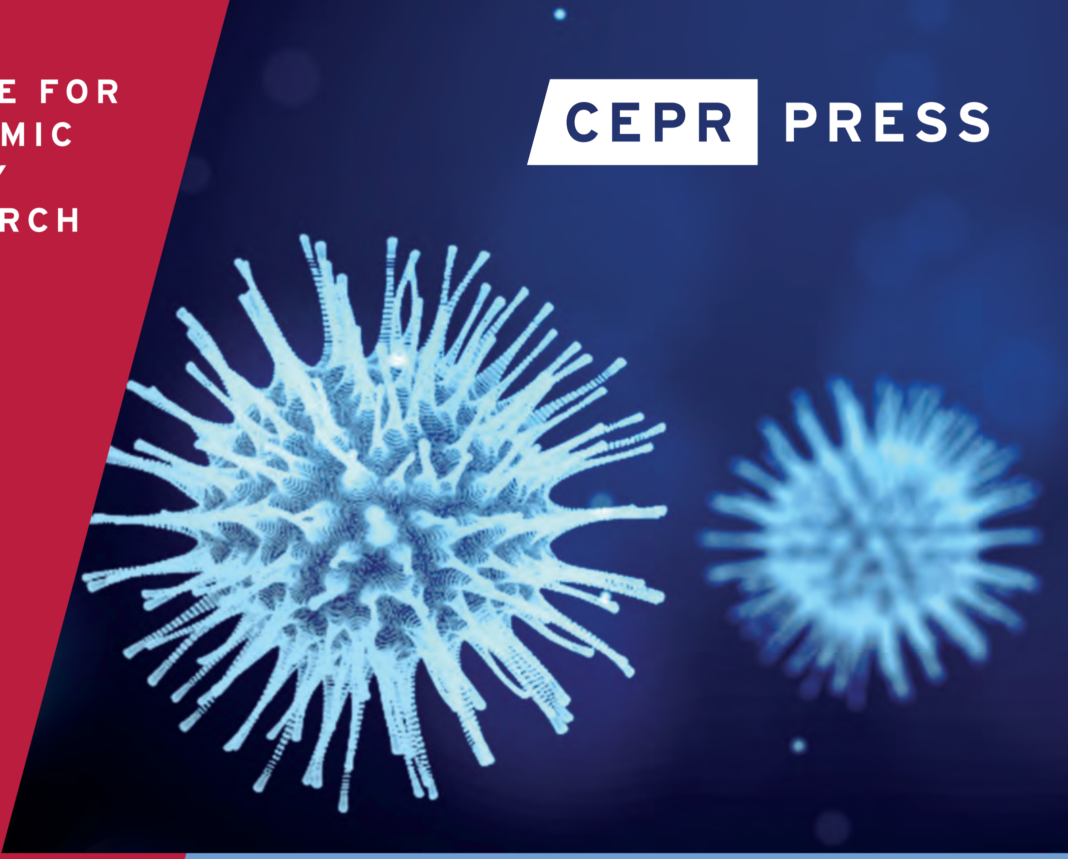

COVID ECONOMICS

VETTED AND REAL-TIME PAPERS

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Covid Economics
Vetted and Real-Time Papers

Covid Economics, Vetted and Real-Time Papers, from CEPR, brings together formal investigations on the economic issues emanating from the Covid outbreak, based on explicit theory and/or empirical evidence, to improve the knowledge base.

Contact: Submissions should be made at https://portal.cepr.org/call-papers-covid-economics. Other queries should be sent to covidecon@cepr.org.

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Ethics

Covid Economics will feature high quality analyses of economic aspects of the health crisis. However, the pandemic also raises a number of complex ethical issues. Economists tend to think about trade-offs, in this case lives vs. costs, patient selection at a time of scarcity, and more. In the spirit of academic freedom, neither the Editors of *Covid Economics* nor CEPR take a stand on these issues and therefore do not bear any responsibility for views expressed in the articles.

Submission to professional journals

The following journals have indicated that they will accept submissions of papers featured in *Covid Economics* because they are working papers. Most expect revised versions. This list will be updated regularly.

American Economic Review

American Economic Review, Applied Economics

American Economic Review, Insights

American Economic Review, Economic Policy

American Economic Review, Macroeconomics

American Economic Review, Microeconomics

American Journal of Health Economics

Canadian Journal of Economics

Economic Journal

Economics of Disasters and Climate Change

International Economic Review

Journal of Development Economics

*Journal of Econometrics**

Journal of Economic Growth

Journal of Economic Theory

*Journal of the European Economic Association**

Journal of Finance

Journal of Financial Economics

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*Journal of Labor Economics**

Journal of Monetary Economics

Journal of Public Economics

Journal of Political Economy

Journal of Population Economics

*Quarterly Journal of Economics**

Review of Economics and Statistics

*Review of Economic Studies**

Review of Financial Studies

(*) Must be a significantly revised and extended version of the paper featured in *Covid Economics*.

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Vetted and Real-Time Papers

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Coronavirus infections and deaths by poverty status: Time trends and patterns[1]

Juergen Jung,[2] James Manley[3] and Vinish Shrestha[4]

Date submitted: 9 June 2020; Date accepted: 11 June 2020

We study the spread of SARS-CoV-2 infections and COVID-19 deaths by county poverty level in the US. We first document a U-shaped relationship between county groupings by poverty level and the intensity of coronavirus events defined as either coronavirus infections or COVID-19 related deaths. The U-shaped relationship prevails for counties with high population density while in counties with low population density, poorer counties exhibit much higher numbers in coronavirus cases, both in infections and deaths. Second, we investigate the patterns of coronavirus events following the announcements of state level stay-at-home mandates. We distinguish between four groups of states: First, Second, Third and Late Movers. Among First Movers—also the states with the largest share of infections—we observe a decrease in the average number of weekly new cases in rich and poor counties two weeks following the mandate announcement. The average numbers of cases per week in richer counties then quickly converges to the number reported in middle income counties, while the poorer counties show a much slower decrease in coronavirus cases. This pattern is accompanied by a dramatic reduction in mobility in all county groupings. Third, comparing counties in Second and Third Mover states, we show that a few days of delay in non-pharmaceutical interventions (NPIs) results in significantly larger numbers of coronavirus cases compared to states that introduce a mandate quicker. Finally, we use weather shocks as instruments to address endogeneity of the announcement date of stay-at-home mandates and establish causality.

1 We would like to thank Victoria from Weather Underground for helping us with data. All remaining errors are ours.

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1 Introduction

Since the first case of a SARS-CoV-2 infection being identified in Washington on January 21, 2020, both cases of SARS-CoV-2 infections and deaths due to COVID-19 have surged exponentially in the US. On March 13, there were just over 2,000 identified cases, but the number of cases increased to more than 161,000 by March 30^{th} and over 600,000 by April 4^{th}.[1] The US now has the highest number of infections and mortality in the world as Figure 1 clearly indicates. Due to growing public health concerns almost all US states have declared a state of emergency and numerous states have enacted "stay at home" orders. The increase in the number of cases (infections as well as deaths) has been concentrated in the East and West coastal regions. Although the geographic spread of the disease is tracked (to some extent since testing is still considered insufficient) by spatial data, less is known about the relationship between the growth of SARS-CoV-2 infections and socioeconomic indicators.

The socioeconomic disparity in health outcomes is well-established in the field of Health Economics. Health outcomes tend to improve with socioeconomic status. Three major channels that affect these differences are the disparity in (i) knowledge regarding health behavior (education-health gradient), (ii) access to health care (income-gradient), and (iii) environmental exposure (Santerre and Neun (2010)). However, social interaction is another critical determinant for infectious diseases such as COVID-19. In contrast to the positive spillover effect generally originating from social interactions, if an infectious person engages in economic or social activities, healthy individuals are at higher risk of being infected. It is not clear a priori whether social interactions correlate in the same way with socioeconomic status as the three channels mentioned above.

Although richer individuals have more resources to self isolate, they are initially more likely to participate in economic and social activities compared to poorer individuals as many forms of social interaction are normal goods.[2] However, once infected, poor individuals may not be able to efficiently self isolate due to resource constraints. Additionally, relatively poor individuals may be more involved in "frontline" essential work which is less likely to be performed from ones home so that NPIs may be less binding for this group (Blau et al. (2020)). Hence, differences in initial transmission pathways and differences in resource constraints between the poor and rich warrants an investigation of the trend in the spread of SARS-CoV-2 infections and COVID-19 related deaths across poverty levels.

Focusing on county-level effects, this paper provides descriptions of emerging trends of both SARS-CoV-2 infection cases and COVID-19 related deaths as of April 28, 2020 based

[1] https://coronavirus.jhu.edu/us-map

[2] Using a household production model Saffer (2008) shows that social interaction is a function of its price, prices of complements or substitute goods, and income. The author further shows that many forms of social interactions increase with income level.

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on socioeconomic status. We rank counties by their poverty rate measured in 2018 and form 20 bins such that each bin represents about the same number of counties. We show that the relation between poverty and the cumulative number of identified infections is U-shaped, with infections concentrated among the richest and the poorest. When dividing the sample into low and high population density counties, we find that in low density counties the rates increase as expected (i.e. increasing most among the poorest), but a U-shaped relationship prevails for counties with high population density. This may be indicative of an increased ability to self-isolate in rich low density areas compared to densely populated areas that are equally rich.

As a form of non-pharmaceutical interventions (NPIs), by the 15^{th} of March, 2020, almost all states in the US had declared a state of emergency and 15 states had implemented a stay at home order.[3] However, it is difficult to directly assess the adequacy of stay at home orders for two reasons. First, we are unsure of how effectively these laws were put into action. For instance, most of the mandates permit performance of essential tasks such as grocery shopping, dog-walking, visiting the pharmacy, etc. Someone out for any reason could make a variety of plausible excuses. Furthermore, enforcement of these laws is determined at the local level and varies from state to state. Finally, the timing of the mandates might have been determined by the projection of future spread in infections, which creates a methodological challenge (Gupta et al. (2020)). We proceed as follows.

Having described the relationship between poverty and infection, we next provide a descriptive analysis of the effects of locally enforced stay-at-home mandates and the weekly number of new coronavirus cases (infections and deaths) evaluated at different poverty levels for groups that announce the mandate at different times. More specifically, we create four groups: (*i*) first movers, (*ii*) second movers, (*iii*) third movers, and (*iv*) late movers, all based on the percentile of distribution in the timing of the announcements of the mandates (i.e., 25^{th}, 50^{th}, and 75^{th} percentiles).[4] Our analysis suggests that the decision to implement the mandate early on (the First Movers) is based on expectations of future cases. We note an increasing trend in infections before the mandate and a substantial rise in cases in the weeks following implementation. This pattern is especially pronounced in richer counties. Among the First Movers, who also had the highest share of infections, the peak in the number of new cases was reached in the third week after issuing a mandate for all counties irrespective of where they rank on the poverty scale. After the three week period we notice a pattern of convergence of the weekly

[3] See USA Today: https://www.usatoday.com/story/news/nation/2020/03/30

[4] We use announcement date rather than enforcement date as individuals and firms are likely to voluntarily adjust their behavior following the announcement of a lockdown. The announcement induces awareness among people regarding the seriousness of the spread in infections. As a result individuals may voluntarily internalize the cost they might impose on others. Krueger, Uhlig and Xi (2020) document the possibility of rational shift towards lower risk activities, which can decrease infections on their own. Similarly, Born, Dietrich and Müller (2020) provide empirical evidence that although Sweden did not have a lockdown policy, Swedes adjusted their behavior similar to other comparable European countries with a lockdown.

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numbers of new infections for rich and poor county groups down towards the lower level of the mid-level poverty county-groups. This result can be explained by the finding that mobility, as measured by distance traveled away from home, declined by up to 40 percent in rich and mid-level poverty counties but only dropped by 30 percent in poor counties. Such observed patterns are consistent with the systematic allocation of the frontline essential workers, whose jobs are infeasible to be performed at home.

As the grouping of the Second versus Third Movers only differs by a few days,[5] we only use the Second and Third Movers to evaluate the potential consequences of delaying the announcement of a mandate by a few days during the coronavirus pandemic. This analysis rests on the assumption that the difference in the timing of a mandate announcement between these two groups is affected by other factors not related to the projection of growth in coronavirus infections. Our findings suggest that even a few days of delay in issuing the mandate can push the trajectory of the weekly numbers of new cases significantly upwards and highlights the urgency of these type of NPI policies. However, we urge caution in interpreting these results as they are based on counties belonging only to states in the second and third movers category which affects the overall external validity.

Finally, we address concerns regarding the endogeneity of the announcement time of the stay-at-home mandates. We use weather shocks as an instrument to complement the announcement of a mandate. We collect county level weather data in addition to mobility data based on cell phone usage from SafeGraph. We use weekly aggregates and argue that severe weather patterns further increase the marginal cost of mobility among counties with a mandate. Assuming that weather patterns do not biologically affect the spread in infections, weather shocks will affect infections only through social interactions, measured with mobility data. Using an instrumental variables estimator, we show that an increase in average travel distance from one's home by 1 km causes about 55 additional infection cases per week on average. Our back-of-the-envelope calculation suggests that there may have been additional 3.1 million cases in absence of stay-at-home mandates. However, we caution that this calculation is based on an IV estimator, which measures the Local Average Treatment Effects (LATE).

Literature Review. COVID-19 studies are emerging rapidly, with researchers investigating the short-term effects of the stay-at home-mandate and various forms of economic disruptions created by the disease. The program evaluation literature investigates the effects of the stay-at-home orders on coronavirus cases and associated labor market effects. Friedson et al. (2020) find that in California the adoption of a statewide Shelter-In-Place Order (SIPO) on March 19, 2020 reduced COVID-19 cases by 125.5–219.7 per 100,000 individuals by April 20, 2020. In a follow up study Dave et al. (2020) expand their identification strategy and utilize

[5]The median announcement dates are March 24^{th} and March 28^{th} for second and third movers, respectively.

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across state variation in the timing of stay-at-home mandates. They find that the mandate decreased the number of cumulative COVID-19 cases by 44 percent. However, Gupta et al. (2020) express some methodological concerns in conducting a program evaluation of stay at home mandates. Specifically, the authors highlight that the state government's decision to adopt the law often predates increases in cases and deaths. This suggests that the projection of future growth in the number of caseloads might be an important determinant for the timing of the law itself, which creates some non-trivial methodological challenges in using these laws as valid natural experiments. Fowler et al. (2020) use a difference-in-differences design and estimate that stay-at-home orders are associated with a 30 percent reduction in weekly cases in the first week after a lockdown which then increases to almost 50 percent in week three. Similarly, Courtemanche et al. (2020) use an event study approach and show that shelter-in-place orders decrease the spread of coronavirus infections tenfold. Born, Dietrich and Müller (2020) directly question whether lockdowns are optimal in cases of pandemics such as coronavirus. Using Sweden as a case study, the authors find that a mandated lockdown would not have had a large effect as Swedes voluntarily changed their behaviors in similar ways to comparable European countries with lockdowns in place.

Another strand of literature focuses on documenting the labor market disruptions associated with COVID-19. Atkinson et al. (2020) show how social distancing was practiced in the US based on cell phone data from SafeGraph. Andersen et al. (2020) investigates how the federal paid sick leave mandate has decreased full-time work and increased staying at home using a difference-in-differences framework and data from SafeGraph.

Coibion, Gorodnichenko and Weber (2020) provide a preliminary analysis of labor market effects of the COVID-19 pandemic in the US. Using Nielsen Homescan panel data, they show that the magnitude of job losses is significantly larger than estimated by using the new unemployment claims. Overall, the authors estimate 20 million jobs were lost between the start of the crisis in January and April 6, 2020. Using Google search data, Kong and Prinz (2020) find that the restaurant and bar limitations and non-essential business closures could explain 4.4–8.5 percent of the increase in unemployment insurance claims, respectively. Mongey, Pilossoph and Weinberg (2020) indicate that workers in "low-ability-to-work-from-home" sectors experienced greater losses in short-term employment and are also more likely to be economically vulnerable. In more related work to our study, Blau et al. (2020) argue that the effects of NPIs such as the "Great Lockdown" can vary across populace and is likely to be less-binding among frontline essential workers, who find it difficult to substitute their work with work done from their homes. Therefore frontline workers are exposed to greater risk of infection. In contrary to this argument, if voluntary adjustments in labor market favor the rich, an effective lockdown may in fact benefit the poor, at least solely from a health standpoint. Kahn, Lange and Wiczer (2020) use novel job vacancy data from an employment analytics and labor market information firm in addition to unemployment insurance claims data and show that job vacancies collapsed by 30 percent in the second half of March with related increases in unemployment insurance claims. Only essential retail and nursing sectors did not experience such a decrease.

Our study contributes to the existing literature in several ways. This is the first study to establish a U-shaped pattern between the poverty level in a county and the occurrence of coronavirus cases in the US, especially in the early days of the pandemic. We also document a pattern of infection spread from higher to lower income counties over time. In the initial phase of the pandemic, infections were more concentrated in counties with less poverty. Using mobility data from SafeGraph we show that the decrease in mobility was lower in poorer counties. This is similar to predictions in Blau et al. (2020). This mechanism can explain the transmission of infections in poorer neighborhoods over time, resulting in U-shaped curve depicting the relationship between COVID-19 events and poverty levels. This highlights heterogeneity in COVID-19 events across the socioeconomic spectrum.

Next, we show that the timing of stay-at-home mandates is often immediately followed by a substantial rise in the number of cases, specifically among the early adopters similar to findings in Gupta et al. (2020). To overcome the likely endogeneity of the implemented stay-at-home mandates, we compare "second" versus "third" mover states (who are differentiated by only a few days of delay in the announcement of stay-at-home mandates), and show that even a few days of delay can noticeably push the COVID-19 trajectory upward. Additionally, using weather shocks as an instrument to complement the effectiveness of the stay-at-home mandate, we highlight that an increase in distance traveled from one's home by 1 km leads to an increase of 55 new weekly infections. The only other papers we are aware of that also use weather data to construct instrumental variables in connection with the coronavirus pandemic are Qiu, Chen and Shi (2020), Brzezinski et al. (2020) and Kapoor et al. (2020). Qiu, Chen and Shi (2020) focuses in China during months of January and February, 2020 to conclude that reduction of over 1.4 million infections and 56,000 deaths may be attributed to both national and provincial public health measures imposed at the end of January. Brzezinski et al. (2020) only focus on mobility—a measure of social distancing—as their dependent variable, and using an IV approach they show that time spent at home can increase by as much as 39 percent in certain states due to a combination of government lockdowns and community responses. While Kapoor et al. (2020) use one-time rainfall during the weekend before a statewide lockdown as an instrument to evaluate the marginal effects of earlier social distancing on the number of coronavirus cases, we use a weekly panel of counties starting from January 22–April 28. We construct our instrument by interacting the stay-at-home mandate with weather shocks (defined as an observed minimum county temperature below the 25^{th} percentile of the temperature distribution of the State in a given month), which enables us to make direct assessments regarding the effectiveness of the policy. In light of the relatively small but fast growing literature on COVID-19, we view our

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and Kapoor et al. (2020)'s methodology as complementary.

The study is organized as follows. Section 2 documents the data used in the study, Section 3 describes the estimation, while Section 4 discusses the results. Section 5 concludes the study.

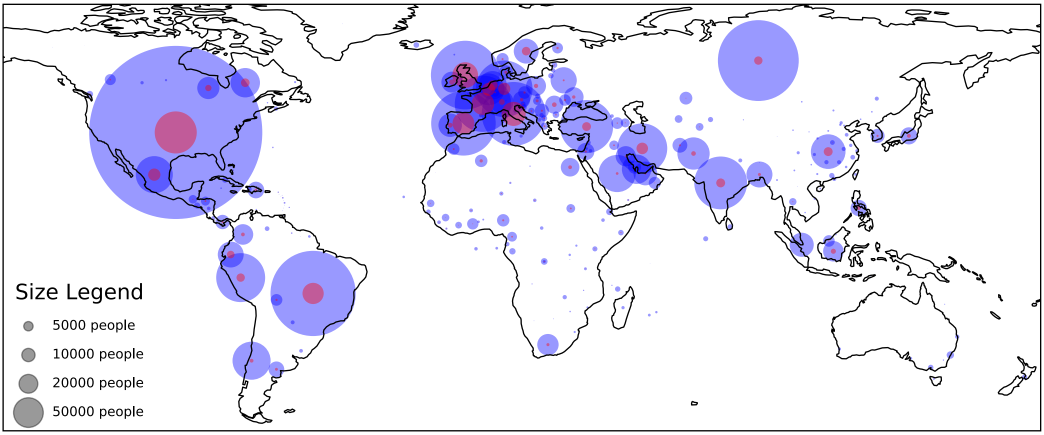

Figure 1: **SARS-CoV-2 Confirmed Infections and COVID-19 Related Deaths**

Notes: Blue circles indicate confirmed SARS-COV-2 infections and red circles indicate COVID-19 related deaths. The data is provided by the Johns Hopkins, Whiting School of Engineering, Center for Systems Science and Engineering as of May 28, 2020. It is accessible at: https://github.com/CSSEGISandData/

2 Data

2.1 County Level SARS-CoV-2/COVID-19 Data

The county level data for cases of SARS-CoV-2 infections and COVID-19 related deaths are extracted from the USAFacts website. This database provides cumulative numbers of infection cases and deaths for each county since January 22^{nd}, 2020 and the numbers at the county-levels are updated by referencing the state and local agencies directly. We first compare these data to another widely used data source from the Johns Hopkins University.[6] Figure B.3 in Appendix B shows that the two data sources have released identical data.

[6] More specifically the Hopkins data is from the Center for Systems Science and Engineering of the Whiting School of Engineering. It is freely accessible at: https://github.com/CSSEGISandData/

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Table 1: **Summary Statistics**

	Sample 1		*Sample 2*	
Variable	Mean	S.D.	Mean	S.D.
Total Cases on March 11, 2020 (per county)	0.42	5.55	0.11	0.62
Total Cases March 23, 2020 (per county)	13.83	137.27	3.91	15.11
Total Cases April 7, 2020 (per county)	126.07	948.82	37.09	124.83
Total Cases April 20, 2020 (per county)	249.30	1734.36	82.94	297.17
Total Cases April 28, 2020 (per county)	323.16	2128.89	120.57	434.73
Total Deaths on March 11, 2020 (per county)	0.01	0.48	0.00	0.00
Total Deaths March 23, 2020 (per county)	0.17	2.03	0.04	0.27
Total Deaths April 7, 2020 (per county)	4.02	39.31	0.91	3.60
Total Deaths April 20, 2020 (per county)	11.98	109.37	2.77	10.53
Total Deaths April 28, 2020 (per county)	17.92	168.51	4.57	18.16
Log of Population Density	3.74	1.75	3.83	1.83
Unemployment Rate	4.44	1.81	4.45	1.96
Percent Less than High School	13.77	6.42	13.05	6.15
Percent over 55	0.33	0.06	0.32	0.07
Percent Black over 55	0.02	0.04	0.02	0.04
Percent on Poverty	16.23	6.44	15.38	6.41
Total Number of Coronavirus Tests	161,343	432,220	91,502	168,953
Stay-at-Home Mandate (Announcement)	0.31	0.46	0.33	0.47
Emergency Declaration	0.50	0.50	0.51	0.50
Restaurant and Bar Restriction	0.39	0.49	0.37	0.48
Non-Essential Business Restriction	0.38	0.49	0.37	0.48
Restriction on Large Gathering	0.18	0.39	0.20	0.40
Average Temperature (in Fahrenheit)	46.42	12.88	NA	NA
Precipitation (in inches)	0.10	0.13	NA	NA
Weather Shock (Proportion)	0.13	0.34	NA	NA

Notes: Sample 1 is a balanced panel of weekly observations from 3,092 US counties starting from January 22–April 28 (14 weeks), 2020 with a total of N=43,288 county/week observations. Sample 2 only contains weekly county level observations from Second (N=5,642) and Third (N=7,602) Mover states. The total number of testing conducted is available at the state level.
Sample 1 is used for estimating the basic specification of Section 4.2, and estimating the IV specification of Section 4.4. Sample 2 is used to estimate the effects in 2^{nd} and 3^{rd} mover states as described in Section 4.3.

Using the cumulative number of events (infection cases and deaths), we focus on five cross sections at the county level: *i*) March 11^{th}, *ii*) March 23^{rd}, *iii*) April 7^{th}, *iv*) April 20^{th}, and *v*) April 28^{th}. The spatial dispersion of cases and deaths are presented in Figures B.1 and B.2 for April 28^{th}, respectively. We also construct the weekly number of new cases starting from the week following January 22^{nd}–April 28^{th}, 2020 which gives us 14 weeks of panel data. Table 1 shows the summary statistics of the samples used for (*i*) calculating the non-parametric results of Section 4.1, (*ii*) estimating the basic specification of Section 4.2, (*iii*) estimating the effects in 2^{nd} and 3^{rd} mover states based on a refined sample in Section 4.3, and (*iv*) estimating an IV

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specification in Section 4.4.

2.2 Other Data Sources

Poverty Data. We use the Small Area Income and Poverty Estimates (SAIPE) county estimates for 2018. The data show the percentage of the population in a county living below the poverty level.[7] The spatial distribution of poverty across counties is shown in Figure 2. The county level population data by age and race is extracted from the Survey of Epidemiology and End Results (SEER) through the NBER website.[8] The land area and unemployment rate data are obtained from the US Census Bureau and Bureau of Labor Statistics, respectively.

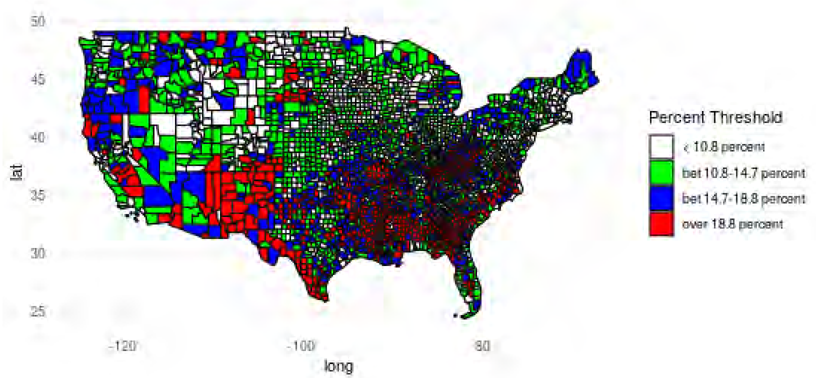

Figure 2: **Poverty Rates in 2018**

Notes: The data is based on Small Area Income and Poverty Estimates (SAIPE) county estimates.

Social Distancing Data. We use social distancing data from SafeGraph[9]. SafeGraph is offering a temporary social distancing metric that provides daily views of median movements from one's home (in meters) based on cell phone data aggregated at the census block level. Daily data is available going back to January 1, 2020. The data is generated using a panel of GPS pings from anonymous mobile devices. A common nighttime location of each mobile device over a 6 week period is generated at the Geohash-7 granularity which is approximately a 153×153 meters area. This location is referred to as the device's "home". Devices are then

[7]The link to the SAIPE website where poverty estimates can be found is https://www.census.gov/data/datasets/2018/demo/saipe/2018-state-and-county.html

[8]The link to the website is https://data.nber.org/data/seer_u.s._county_population_data.html.

[9]See https://www.safegraph.com/

aggregated by home census block group. Variables are provided for each census block group. We use two variables: Device-count and Distance-traveled-from-home. The first is simply the total number of active devices in the census block group. Census block groups with device counts less than five are excluded and the distance traveled variable is the median distance (in meters) traveled from the home location by the devices included in the device count during the time period (excluding any distances of 0). First the median for each device is calculated and then the median (distance traveled from home) over all devices is reported in the SafeGraph data. We then aggregate these census block group medians to the county level using the number of devices in each census block as weights.

Air Pollution Data. In order to evaluate the slowdown of economic and social activities, as an alternative to mobility data, we use data on ground level pollutants. In particular we use data on the daily level of pollutants from AirNow.[10] We extract daily level data from their archived section from January 1^{st}, 2020–April 18^{th}, 2020 for four major pollutants—ground level ozone gas (O_3), PM_{10} (particulate matter of 10 micrometers or less in diameter), $\mathrm{PM}_{2.5}$(particulate matter of 2.5 micrometers or less in diameter), and carbon dioxide (CO_2).

We focus on variation in O_3 levels primarily because of the widespread coverage of monitoring sites to track the levels of O_3. For instance, about 1035 of the monitoring sites in the US actively tracked the levels of O_3 on March 11, whereas only 808, 272, and 160 sites reported the levels of $\mathrm{PM}_{2.5}$, PM_{10}, and CO_2, respectively. Using the daily level measurements of O_3 and the geographic information of the monitoring sites, we are able to construct county level data of O_3 measurements for nearly 700 counties. We then calculate the measure of average change in county level O_3 over two time frames: March 1–March 9, 2020 and March 15–March 23, 2020. The geospatial measurement of variation in O_3 levels is shown in Figure **??**.

Weather data. We gather daily weather data from Weather Underground[11] for each county and aggregate the data at weekly level. We searched Wikipedia to determine the largest (most populous) community in each county and put the result into WeatherUnderground to obtain historical weather data for each county using the weather station assigned to that jurisdiction by Weather Underground.[12] The weather data is then aggregated at the weekly level to suit the analysis.

[10]This agency is operated as a partnership venture between the U.S. Environmental Protection Agency, National Oceanic and Atmospheric Administration (NOAA), National Park Services, NASA, Centers for Disease Control, and tribal, state, and local air quality agencies. Figure B.7 in Appendix B depicts the location of the air monitoring stations where the data has been recorded. See https://www.airnow.gov/about-airnow/

[11]https://www.wunderground.com

[12]More specifically we first searched a county in Google. We then looked through the results to see if the first page mentioned the largest community in the county. If it wasn't mentioned on the first page, we used Wikipedia which was linked on the first page of every set of Google results. Once we had the name of the largest community, we searched for this community on the Weather Underground website. We then navigated to History which automatically updates to the nearest weather station that retains historical weather data. We then saved the name of the weather station and extracted the weather data from Jan–April 2020 for this county.

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Testing data. We gather daily cumulative number of tests administered at the state level from The COVID Tracking Project.[13] The number of coronavirus tests conducted is extracted from the local or state public health authorities. The cumulative number of testing is aggregated at the weekly level.

3 Methods

Our goal is to analyze the relationship between poverty levels at the county level and coronavirus infections/deaths. We first proceed with a non-parametric approach and rank each county according to its poverty level—defined as the percentage of the county's population with income below the national poverty level—in 2018. We next form 20 county-groups so that the overall population size of each county-group is approximately the same. The first so constructed county-group contains the counties with the lowest poverty levels and the last county-group contains counties with the highest poverty levels.[14] We then produce scatter plots with the county-grouping poverty levels on the horizontal axis and the county-group coronavirus infections/deaths on the vertical axis.

Although the approach defined in the preceding paragraph adjusts for population size, it does not account for population density of an area, which is arguably an important determinant of coronavirus infections. We next account for population density within a county-group and divide counties into low and high density counties according to the median density value of all counties. We then rank all low density counties and all high density counties separately and repeat the county-group procedure from the previous paragraph for low and high density counties separately to produce scatter plots of the two different county-group categories.

Following California's implementation of a stay-at-home mandate on March 19, 2020, almost all of the states implemented similar mandates within the next two weeks. We provide a descriptive analysis relating the weekly number of new cases and the poverty levels across areas according to the timing of the mandate. We use the following model specification:

$$W_{c,t} = \alpha + \sum_{j=2}^{j=14} \beta_j \times R_c \times I(t=j) + \sum_{j=2}^{j=14} \tau_j \times M_c \times I(t=j) + \sum_{j=2}^{j=14} \omega_j \times P_c \times I(t=j) \qquad (1)$$
$$+ \delta \times D_c + \gamma \times X_c + \eta_s + \varepsilon_{c,t},$$

where the dependent variable $W_{c,t}$ is either (i) the number of weekly infections, (ii) deaths attributed to COVID-19, or (iii) the average distance traveled from one's home as a measure of social activity. All variables are measured for county c in week t.

[13] The data is available on this website. https://covidtracking.com/api

[14] This approach is similar to the method in Currie and Schwandt (2016) who analyze county level trends in mortality by poverty levels.

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Variable R_c is an indicator for the rich group of counties defined as those below or equal to the 30^{th} percentile of county-group poverty ranking. This ranking is based on the county-group ranking method described in the preceding paragraph. Variable M_c pertains to counties between the 30^{th}–70^{th} percentile, and P_c represents counties above or equal to the 70^{th} percentile. We interact the three county-groups with weekly dummies, $I(t = j)$, where j indicates how many weeks past January 22, 2020 the observation is from. We track up to 14 weeks so that $j \in \{2, 3, ..., 14\}$, where the omitted category used is the week of January 22. Variable D_c controls for the population density (in logs) of a county, and X_c includes other county level control variables such as the percentage of the population that is 55 and older, the percentage of African Americans who are 55 years and older, the unemployment rate, and the percentage of residents without a high school degree. Additionally, we account for state fixed effects η_s to capture time invariant heterogeneity across states. We estimate specification 1 separately for four groups that we distinguish by the timing of the announcement of the mandate. These groups, representing quartiles, include: *i*) First Movers, counties in states that announced the mandate before or on March 22^{nd}, *ii*) Second Movers implemented the mandate after March 22^{th} but before or on March 24^{rd}, *iii*) Third Movers implemented after March 24^{th} but before or on March 29^{th}, and *iv*) Late Movers which implemented after March 29^{th}. Coefficients β_j, τ_j, and ω_j show the weekly trends in new cases in rich, mid-level, and poor counties, respectively.

Estimation of equation 1 provides a descriptive analysis of coronavirus cases based on the timing of the announcements of stay-at-home mandates in rich, mid-level, and poor county groupings. It is possible that the timing of the mandate is itself governed by future projections of infections which causes some non-trivial methodological issues for examining the effectiveness of stay-at-home mandates. We address these issues in the results sections 4.3 and 4.4 where we use event study specifications and IV-specifications respectively.

4 Results

4.1 Emerging Patterns of Coronavirus Cases by Poverty Level

4.1.1 County Poverty Rates and Coronavirus Cases

Figures 3 and 4 plot the number of coronavirus infections and COVID-19 related deaths for each percentile of the county-group poverty distribution. We split the sample into five time periods that are indicated by markers that correspond to the total number of cases by March 11^{th}, March 23^{rd}, April 7^{th}, April 20^{th}, and April 28^{th}, respectively. We use local linear regressions and smoothing parameters based on the leave-one-out cross-validation method to fit a curve for each period sample.[15]

[15] We provide a detailed description of the curve fitting procedure in Appendix A.

Both Figures 3 and 4 show emerging trends in number of infections and deaths according to county-groupings, sorted by the county poverty level. The fitted curve pertaining to March 11 is flat, indicating that the number of identified cases was very low at this time so that no discernible difference between rich and poor counties is detectable. By March 23 a pattern begins to emerge with relatively higher case counts at the very low and very high poverty percentiles. This pattern becomes more pronounced by April 7 where a U-shaped curve begins to show. This indicates that the number of infections are higher at the lowest and the highest poverty groupings with a relatively low number of cases at the mid-level poverty levels. By April 20 the U-shaped curve is well pronounced and consistent as of April 28. The pattern of COVID-19 related deaths, shown in Figure 4, is similar except that deaths are disproportionately concentrated in county-groupings with higher poverty levels.

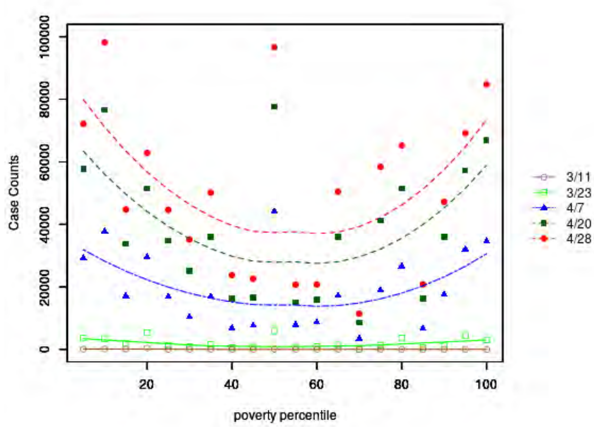

Figure 3: **SARS-CoV-2 Confirmed Infections by Poverty Percentile**

Notes: The source of data is USAFacts, as of April 28, 2020. The curves are fitted using a smoothing method based on local linear regressions as described in Appendix A.

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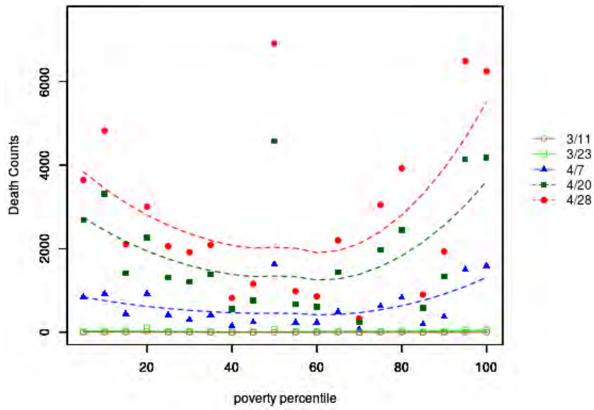

Figure 4: **COVID-19 Related Deaths by Poverty Percentile**

Notes: The source of data is USAFacts, as of April 28, 2020. The curves are fitted using a smoothing method based on local linear regressions as described in Appendix A.

4.1.2 Controlling for Population Density

In order to account for population density in counties, we use the median population density of all counties and split the sample into a low density counties sample and a high density counties sample. We then repeat the grouping procedure above for each sample separately. Figures 5(a) and 5(b) show the patterns of the number of infections by poverty percentile in the high and low population density sample, respectively. As expected, the number of cases in low density counties is substantially lower than in high density counties. For instance, at the 5^{th} percentile of the county poverty grouping, the total number of cases on April 20, 2020 is over 50,000 in the high density group but only 1,500 in the low density group.

The U-shaped relationship between the county-groupings and cases prevails in high density counties as shown in Figure 5(a). In contrast, the relationship between the county-groupings and coronavirus infections in low density counties does not follow a U-shape. It is relatively flat for counties with low poverty rates and strongly increases in counties with high poverty rates as depicted in Figure 5(b). The patterns of COVID-19 related deaths in high and low density counties, shown in Figures 6(a) and 6(b) respectively, mirror the trends for infections as shown in the earlier figures except that COVID-19 related deaths seem to be even more associated with high poverty levels, especially in areas with low population density.

Figures 3–6 raise an interesting question. Why is the overall relationship between coronavirus events and local area socioeconomic status,—as defined by the poverty levels— following a U-shaped pattern in high density areas but not in low density areas? The U-shaped pattern contradicts the well-established positive relationship between income and health outcomes.[16] One possible explanation for the observed difference in the high and low density samples could be due to disproportionately more testing for infections in rich counties which could result in more identified cases in richer county-groupings. This could explain why we observe seemingly higher infection rates as well as COVID-19 death rates in richer counties contrary to what the income gradient literature would predict for other health outcomes. This possibility has been highlighted by the media, suggesting that testing for coronavirus infections is a function of income inequality and as such mirrors the overall trend in health disparity by income. However, in a recent study focused on New York City, Schmitt-Grohé, Teoh and Uribe (2020) find that the spread in the number of tests administered as of April 2, 2020 is evenly distributed across income levels. Another explanation for the difference in the pattern between high and low density counties could be that high income individuals can only self-isolate more effectively than low income individuals when they live in thinly populated areas. In high density counties richer individuals may not be able to benefit as strongly from this logistical advantage due to necessary day-to-day interactions.

[16] For a review of this literature see Wolfe, Evans and Seaman (2012). In a more recent study, Currie and Schwandt (2016) show that the income gradient is well defined at local levels using life expectancy across counties. Shrestha (2019) shows a similar pattern when analyzing the relationship between infant birth weight and the prevalence of low birth weight across counties grouped by poverty levels.

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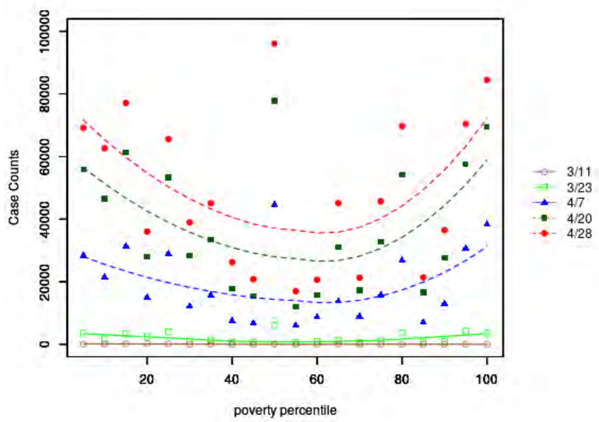
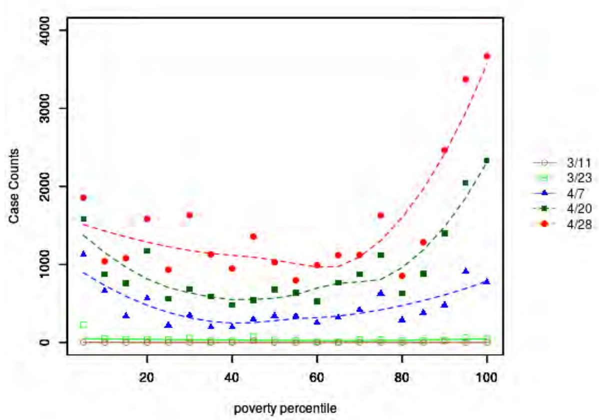


(a) High Population Density



(b) Low Population Density

Figure 5: **SARS-CoV-2 Confirmed Infections by Low and High Population Density Counties**

Notes: The sources of data are USAFacts and the Census. High and low density regions are divided by using the median value of the density. The curves are fitted using a smoothing method based on local linear regressions as described in Appendix A.

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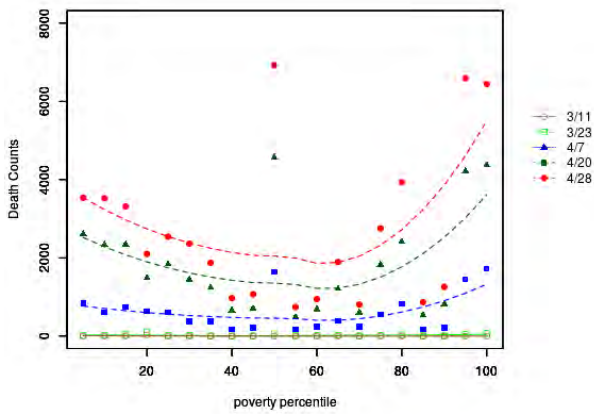
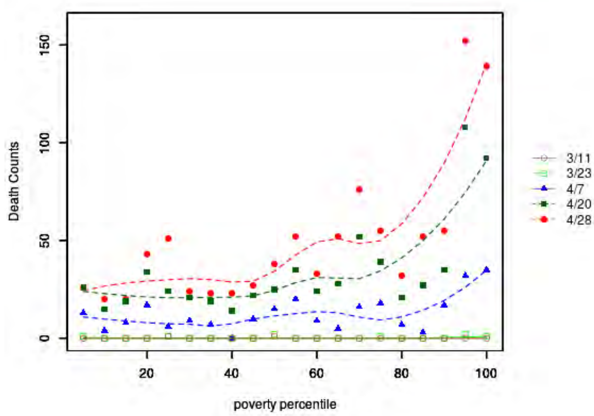


(a) High Population Density



(b) Low Population Density

Figure 6: **COVID-19 Related Deaths by Low and High Population Density Counties**

Notes: The sources of data are USAFacts and the Census. Low density regions are those counties below the median value of county level population density. The curves are fitted using a smoothing method based on local linear regressions as described in Appendix A.

4.1.3 Rich to Poor Propagation Pattern

Coronavirus cases entered the US through international travel (airways and ships)—activities that are likely to be undertaken by richer individuals. To investigate this hypothesis, we first track the initial patterns in COVID-19 cases starting from the last week of February until

March 11, 2020. The early relationship between poverty levels and coronavirus cases is shown in Figure B.5 in Appendix B. In these figures we track the number of cumulative (infection) cases on February 26, 2020, March 4, 2020, and March 11, 2020. The downward sloping best-fit line on the rightmost figure suggests that in the initial phase of the pandemic infections in the US were more concentrated in richer counties.

Two channels can potentially explain the dramatic propagation of infections in richer areas over a short period of time. First, at the very early stages, people may not have fully realized the seriousness of the virus. Since infections were initially concentrated in higher income groups who are also more likely to be involved in social activities (through both employment and social activities), it may have been easier for the virus to spread in richer neighborhoods. Second, although a higher income level allows for more effective self-isolation, self-isolating is more difficult to accomplish in densely populated areas. A comparison between Figures 5(a) and 5(b) shows that the ratio between the number of cases in rich to poor counties is higher in densely populated counties compared to low density areas.[17] In other words, being rich and residing in low-density localities enables one to more effectively self-isolate, compared to a person with similar income living in a densely populated area. However, once the disease has entered a poor neighborhood, it becomes very difficult to control the further spread of the disease as poor households do not have the resources to effectively self-isolate. It is therefore not surprising that the number of cases started to increase dramatically in poor neighborhoods with the passage of time. This can explain the change in the shapes over time of the curves in Figures 5(a) and 5(b).

We next provide a descriptive analysis of whether the probability of death conditional upon infection varies by poverty rate. Figure 7 shows the relationship between poverty and death probability. The latter is defined as the ratio between county-group COVID-19 related deaths and the number of confirmed coronavirus infections in the same county-group. Furthermore, we provide this analysis separately for counties with high and low population densities. We see that the best-fit line pertaining to the high density area is very flat; the markers lie very close to the line, except for those at the bottom two county-groupings where death probability exceeds 0.04. Generally, if we were to consider the pattern of mortality rates from other diseases, the best-fit line would have shown a clear upward sloping trend to indicate that the rate of mortality increases with poverty. We do not find such a clear cut relationship. The fact that the mortality rate is quite similar across the county-groupings along the poverty spectrum in the early days is indicative of (i) the absence of an effective way of treating the virus and (ii) hospitals in high density areas may have been inundated by COVID-19 cases so that even higher income individuals who usually enjoy better access to healthcare face a supply-constraint. The latter

[17] The ratio of infections between poor and rich counties in counties with high population density is close to 1 but only around 0.4 (infection in rich areas) to 1 infection (in poor areas) in counties with low population density.

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point is supported by Figure 7 which shows that in low density areas where the number of cases is much lower, the difference in mortality cases between low and high poverty areas is much more pronounced.

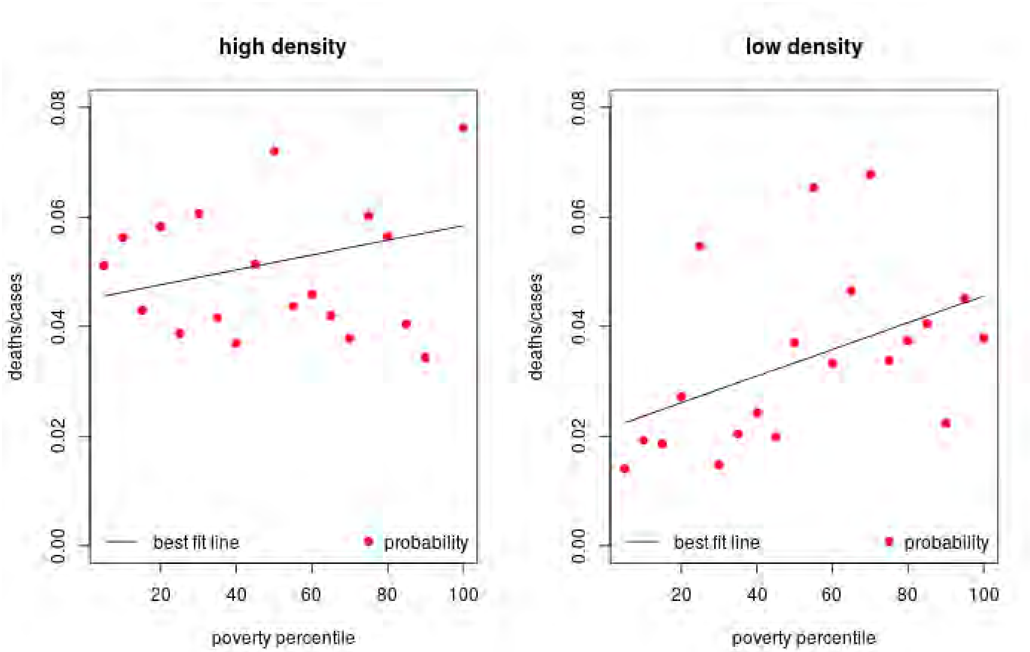

Figure 7: **COVID-19 Probability of Death by Poverty Percentile**

Notes: Data is from USAFacts and the Census. The statistics are calculated from events on April 28, 2020.

4.2 Poverty and the Timing of Stay-at-Home Mandates

We next describe how the pattern between the poverty percentiles of county-groups and coronavirus cases is affected by the timing of announcing stay-at-home mandates. We proceed by grouping US states into four categories: *i*) First Movers, *ii*) Second Movers, *iii*) Third Movers, and *iv*) Late Movers. We then estimate specification 1 using OLS for each group of states separately. In addition, we control for (the log of) population density, the percent of the population older than 55, the percent of African Americans over 55, the county unemployment rate, and state fixed effects. We plot the estimated coefficients of interest for the First, Second, Third, and Late Movers in Panel A of Figure 8.[18] The weekly numbers of new cases are substantially

[18] A detailed regression table is presented in Appendix C, Table C.3.

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higher among the First Mover group compared to any other group, indicating that the timing of the mandate itself was driven by expectations about the future spread of the virus.[19] Although this poses a methodological challenge in identifying the effects of non-pharmaceutical interventions (NPIs) from a program evaluation standpoint, several insights can be gathered from Figure 8.

First, the number of cases increases substantially in the week of announcement of the mandate among the First Movers and keeps increasing in the following weeks. Such increases are concentrated in the very rich and very poor counties, with weekly cases being relatively moderate in middle income counties. This result is consistent with our interpretation of the U-shaped curve in Section 4.1. The weekly number of new cases peaks in the two weeks following the mandate (April 1–April 8) for groups of all income levels. Thereafter the weekly number of cases declines in both very rich and very poor counties so that they converge towards the number of cases in middle income counties. It may not be surprising that the peak is reached after two weeks following the implementation of the mandate as the typical incubation period of the coronavirus infection is about two weeks.[20]

It is interesting to compare the number of new infections between Second and Third Mover states in Figure 8. These two groups differ only by a few days with respect to the announcement of the stay-at-home mandate. Until week 9, after which all of the Second Mover states announced the mandate, the weekly number of new infections are quite comparable between the Second and Third Movers. However, in weeks 10—14 the number of infections in Third Mover states starts to increase relative to Second Mover states. As the announcement dates of the mandate between these two groups are less likely to be affected by differences in future expectation of new cases, these descriptive findings highlight the possible consequences of waiting a few extra days to implement the NPI mandates.

Next, we utilize mobility data from anonymized cell phones to evaluate the effects of the stay-at-home mandates on social-distancing across poverty groups in First, Second, Third, and Last Mover states. We again estimate a specification similar to equation 1 but use the log of distance traveled away from home (in meters) as the dependent variable. The interaction term coefficients are plotted in Figure 8 (Panel B). The figure shows a drop in the distance traveled away from home in the weeks following the announcement of a mandate. The mobility pattern among the First Movers group is consistent with the pattern in earlier cases. Although we see a dramatic drop in distance traveled the reduction is more pronounced in rich and middle income counties. For instance, the distance traveled in rich counties fell by 18 percent and 40 percent respectively in the first and second week following the implementation of the mandate

[19] This point has been highlighted by Gupta et al. (2020), who suggest that the timing of the mandate predates large increases in cases. This is expected as the timing of the mandates are likely to be based on the modeling aspect of the possibility of spread in infections.

[20] Among the people showing symptoms, 97.5 percent do so within 11.5 days of infection (Lauer et al. (2020)).

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when compared to the week of January 22. On the other hand in poor counties the reduction in distance traveled was only 5 and 30 percent in the two weeks following January 22. This can explain the dramatic drop in the weekly number of new cases for First Movers in rich counties as shown in Panel A.

The overall reduction of distance traveled from home is higher in richer counties except in Second Mover counties as we can see in Panel B of Figure 8. This is consistent with the systematic dispersion of "frontline" essential workers, who must provide their labor in person, across counties defined by their poverty levels. Blau et al. (2020) distinguish frontline workers by occupation (e.g., health care workers, protective service workers (police and EMTs), cashiers in grocery and general merchandise stores, production and food processing workers, janitors and maintenance workers, agricultural workers, and truck drivers) and conclude that over 70 percent of these workers cannot work from home.[21] Based on a report using data from the American Community Survey they suggest that on average frontline workers are less educated, earn lower wages, and are more likely to have a minority background compared to the overall workforce. These results suggest that although COVID-19 infections, at the initial stages, were disproportionately occurring in rich counties, the inability to substitute in-person work from home led to the spread of infections in poorer counties over time.

We finally analyze the general trend of air pollution between January 1–April 18 and depict measures of ozone levels, carbon dioxide, and particle measures in Figure B.8. The green dotted vertical line marks March 11, 2020—the date of the state-of-emergency declaration of the median state and the date of the cancellation of activities of the NBA, the first major discontinuation of activities of a major organization. This is eight days before the first shelter-in-place order, which was issued in California on March 19. Panel (a) and (b) of Figure B.8 show a discontinuous drop in both ozone and particle measure PM_{10} following March 11 by more than 7 ppb (for ozone) on some days. Eventually the levels of both of these pollutants rise again. Although the CO_2 level falls, the trend seems to be dropping even before March 11. However, there is no visually evident drop in the level of $PM_{2.5}$. Moreover, to see whether the pattern shown in Figure B.8 is systematically correlated with the time of year, we repeat a similar exercise using air data for 2019. We find no indication that the fall in pollutant levels in March has anything to do with the season as we do not observe such drops in the prior year.[22]

[21] Blau et al. (2020) can be downloaded at https://econofact.org/essential-and-frontline-workers-in-the-covid-19-crisis

[22] The pollutant trend data for 2019 is presented in Figure B.9 in Appendix B.

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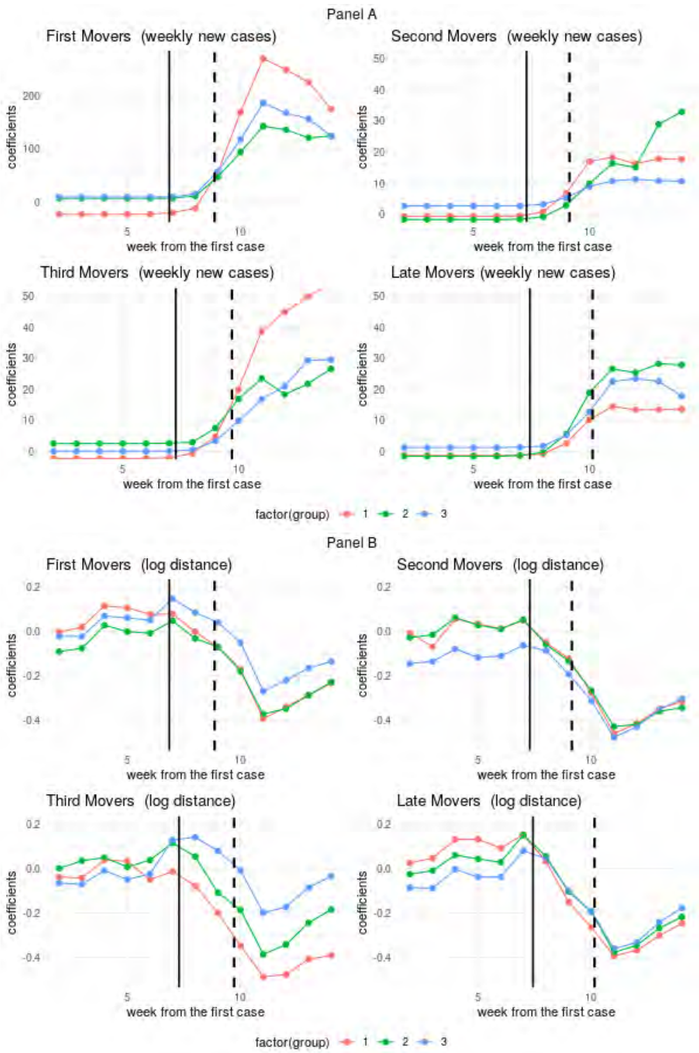

Figure 8: **Weekly New Cases and Mobility By Timing of the Mandate and Poverty**

Notes: **Panel A** plots the interaction coefficients between the week-fixed-effects and poverty-groups of specification 1 where group 1 are poor counties below or at the 30^{th} percentile of the county-groupings shown in Figure 2. Similarly, groups 2 and 3 pertain to middle income and rich counties between the 30^{th}–70^{th} percentile and above or at the 70^{th} percentile, respectively. *i*) First Movers, counties in states that announced the mandate before or on March 22^{th}, *ii*) Second Movers announced the mandate after March 22^{nd} but before or on March 24^{rd}, *iii*) Third Movers implemented after March 24^{th} but before or on March 29^{th}, and *iv*) Late Movers which implemented after March 29^{th}. **Panel B** plots the coefficients of estimating specification 1 with the log of distance from home (in meters) as dependent variable. The vertical solid and dotted lines represent the timing of emergency declaration and mandate announcement, respectively. Detailed estimation results of these coefficients are available in Tables C.3 and C.4 in Appendix C.

4.3 The Effects of Short Delays in Stay-at-Home Mandates

4.3.1 Estimates Based on Event Study Design

Using First Movers for this analysis is problematic as the initial timing of the mandate is by construction endogenous with respect to the number of new cases.[23] However, given that the announcement of the mandate for Second and Third Movers only differs at most by a week—as portrayed in Figure 8—we argue that the declaration of the mandate in these states is governed by other factors such as differences in population density, political ideology and legislative process that are not systematically related to current numbers of infections and COVID-19 related deaths or the expectation about future cases.[24] Although the announcement of the mandate only differs by a week, just a few days of delay can contribute to a large increase in coronavirus cases given the exponential growth potential that is attributed to this kind of virus. To evaluate the effects of a short delay in implementing the mandate, we estimate the following event-study specification:

$$W_{c,t} = \alpha + \sum_{j=2}^{j=14} \lambda_j \times I(t=j) + \sum_{j=2}^{j=14} \kappa_j \times S_c \times I(t=j) + \delta \times D_c + \omega \times NPI_{c,t} + \gamma \times X_c + \eta_s + \varepsilon_{c,t}, \quad (2)$$

where the dependent variable measures weekly new infections in county c in week t and the weekly indicators $I(t=j)$ are interacted with indicator S_c that takes a value of one if a county belongs to a Second Mover state and zero otherwise. The λ_j coefficients will capture the average number of new cases in Third Mover states, whereas the κ_j coefficients indicate whether the average weekly number of cases in Second Mover counties in week j are different from Third Mover counties. The sum of λ_j and κ_j indicates the weekly trend of new cases in Second Mover counties. The specification in equation 2 also includes variable D_c which controls for the population density (in logs) of a county, X_c which includes other county level control variables and state fixed effects η_s in order to capture time invariant heterogeneity across states. As for preferred specifications, we present results with county fixed effects that additionally control for several other NPIs.[25] The estimation is conducted using OLS and standard errors are clustered at the county level.

The estimated coefficients λ_j and $\lambda_j + \kappa_j$ are plotted in Figure 9, where the circle and triangle

[23] This point has also been highlighted in Gupta et al. (2020).

[24] The idea of a national lockdown was resisted by the federal government with aspirations to keep the economy open, and the responsibility of implementing NPIs was explicitly delegated to state governors and local leaders. However, states vary in their political ideology, which creates differences in the timing of mandate implementations that is exogenous with respect to coronavirus infection numbers when focusing on Second and Third Movers.

[25] The results using state fixed effects and controlling for other county level (time invariant) characteristics are similar. They are not presented in the paper but are available upon request.

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markers correspond to the Second and Third Movers, respectively.[26] The figure shows that there are no preexisting differences in the weekly number of new cases prior to the announcement of the mandate (shown by the dotted vertical line) in counties belonging to the Second or Third Mover states. However, in the weeks following the implementation of mandates we see a clear dispersion in the weekly number of new cases between Second and Third Movers. In week 14, Second Mover counties report on average 16 fewer cases per week compared to Third Movers.

After validating the similarity of the trend in new weekly infections prior to the announcement of stay-at-home mandates in Second and Third mover county-groups, we estimate the effects of stay-at-home mandates using panel data and a difference-in-differences framework. We estimate the following event study model:

$$\log\left(W_{c,t}\right) = \alpha + \sum_{j=-5}^{j=-2} \beta_j \times I(t=j) + \sum_{j=0}^{j=5} \beta_j \times I(t=j) + \delta \times NPI_{c,t} + \gamma_c + \sigma_t + \varepsilon_{c,t}, \qquad (3)$$

where the dependent variable is the log of weekly new infection cases in county c in week t and $I(t = j)$ is an indicator variable denoting weeks away from the week the stay-at-home mandate was implemented.[27] Variable $j = -5$ and $j = 5$ denote five weeks before and five weeks after the mandate implementation week, respectively. The week before implementation ($j = -1$) is used as the omitted category and the β coefficients measure how much infection cases differ on average with respect to the number of cases in the omitted category. NPI include a vector of indicators representing other types of non-pharmaceutical interventions such as state of emergency declarations, non-essential business restrictions, restaurant restrictions, and limits on large gatherings. Expression γ_c includes county fixed effects in order to account for time invariant unobserved heterogeneity across counties and variable σ_t represents the week fixed effects that capture common weekly trends in the spread of infections. Additionally, all specifications control for the log of total number of tests conducted, aggregated at weekly level.

The identification in equation 3 is based on within state variation in the timing of the stay-at-home mandates. Usually the validity of the identification rests on the assumption of a parallel trend between treated and control group prior to the intervention. However, the assumption of a parallel trend will not be enough to properly identify the effects of the stay-at-home mandates if the basic reproduction number, R_0, differs with the proportion of infected individuals in a county-group.[28] In the case of a contagious disease such as COVID-19, the

[26] Detailed estimation results from the event study are available in Appendix C, Table C.5.

[27] Before taking log, we add 1 to the actual number of cases. We test the robustness of this method by adding smaller than one. The results do not change. These alternative results are available upon request.

[28] The basic reproduction number (or basic reproductive ratio) is the expected number of cases directly generated by one case in a population where all individuals are susceptible to infection. An outbreak is expected to continue if R_0 is greater than one and to end if it is less than one. It is easy to conceive a situation where the parallel trends assumption in number of cases or deaths may hold even if R_0 is different. The estimation of R_0 is affected by complex interactions of several characteristics including but not limited to population density,

identification relies on a stricter assumption which states that in the absence of a stay-at-home mandate, the basic reproduction number R_0 between county-groups belonging to states that announce the mandate early versus those that announce the mandate late is similar. After controlling for important factors that promote the spread of the contagious disease—such as population density—we argue that similar numbers of caseloads before the implementation of the mandate can signal similarity in the basic reproductive ratio R_0. A descriptive validation of this assumption is provided in Figure 9, which shows very similar caseloads between the Second and Third Mover county-groups prior to the mandate.

The estimation of equation 3 is provided in Table 2. Each column controls for the timing of a specific type of NPI such as a statewide emergency declaration, non-essential business restrictions, restaurant restrictions, and limits to large gatherings. Column (6) controls for all reported NPIs simultaneously and column (7) adds the interaction between a high population density indicator and week dummies to allow for differences in the infection growth rates between low and high density counties over time. It is our preferred specification. We plot these β coefficient estimates along with the 95 percent confidence intervals in Figure 9 (Panel B). The figure clearly shows a reduction in the weekly number of new cases in counties following the week of the stay-at-home mandate implementation. Moreover, the coefficients pertaining to weeks prior to the mandate are very close to zero, which is suggestive of no systematic pre-existing differences in the trajectory of infection numbers across counties based on the timing of the mandate.

4.3.2 External Validity and Sensitivity Analysis

The results presented in the event study—Table 2 and Figure 9—are conditional upon county observations being from the sub-sample of Second and Third Mover states. This negatively affects the external validity of these findings. We designed these groups based on the distribution of the timing of the announcement of stay-at-home mandates. Second Movers comprise states within the first quartile and the median of the announcement time distribution, whereas Third Movers are states from the median to the third quartile.

In order to assess the sensitivity of our analysis with respect to this somewhat arbitrary grouping, we next expand the sample to include county observations from additional states. We begin by including observations from states within the $23^{rd} - 77^{th}$ percentile of the mandate announcement distribution and systematically widen the bracket by 2 percentage points in both directions until we capture county observations from states between the $7^{th} - 93^{rd}$ percentile of the mandate announcement distribution. The results from this exercise are presented in Appendix B in Figure B.6.

human interaction patterns, proportion of susceptible people at the start of an infection, and people's behavior (Delamater et al., 2019). Estimating R_0 is beyond the scope of this study.

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Table 2: **SARS-CoV-2 Confirmed Infections with NPI Controls**

	Dependent variable:						
	log of Weekly New Cases						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
5 weeks before announcement	0.007	0.004	0.008	−0.003	0.004	−0.009	−0.045
	(0.071)	(0.071)	(0.071)	(0.070)	(0.071)	(0.068)	(0.066)
4 weeks before announcement	0.008	0.005	0.008	−0.002	0.005	−0.009	−0.044
	(0.071)	(0.071)	(0.071)	(0.070)	(0.071)	(0.068)	(0.066)
3 weeks before announcement	0.009	0.006	0.010	−0.0001	0.006	−0.007	−0.042
	(0.071)	(0.070)	(0.070)	(0.069)	(0.070)	(0.067)	(0.065)
2 weeks before announcement	−0.049	−0.049	−0.048	−0.037	−0.045	−0.043	−0.046
	(0.054)	(0.054)	(0.054)	(0.054)	(0.054)	(0.053)	(0.051)
week of announcement	−0.013	−0.013	−0.013	−0.400***	−0.010	−0.385***	−0.101
	(0.088)	(0.088)	(0.089)	(0.109)	(0.088)	(0.109)	(0.100)
1 weeks after announcement	−0.240	−0.240	−0.234	−0.872***	−0.456**	−1.096***	−0.356*
	(0.186)	(0.186)	(0.190)	(0.211)	(0.192)	(0.228)	(0.195)
2 weeks after announcement	−0.570*	−0.570*	−0.560*	−1.448***	−1.012***	−1.920***	−0.704**
	(0.291)	(0.292)	(0.298)	(0.320)	(0.306)	(0.359)	(0.292)
3 weeks after announcement	−0.830**	−0.830**	−0.815**	−1.956***	−1.499***	−2.675***	−0.986**
	(0.394)	(0.394)	(0.403)	(0.427)	(0.417)	(0.489)	(0.386)
4 weeks after announcement	−1.231**	−1.231**	−1.211**	−2.608***	−2.128***	−3.574***	−1.417***
	(0.500)	(0.500)	(0.512)	(0.537)	(0.533)	(0.623)	(0.485)
5 weeks after announcement	−1.685***	−1.685***	−1.660***	−3.314***	−2.810***	−4.528***	−1.905***
	(0.607)	(0.607)	(0.622)	(0.649)	(0.649)	(0.758)	(0.586)
Emergency Declaration		X				X	
N.E. Business			X			X	
Restaurants				X		X	X
Gathering					X	X	
High Density × Week							X
Observations	13,244	13,244	13,244	13,244	13,244	13,244	13,244
R^2	0.470	0.470	0.470	0.485	0.475	0.489	0.572

Note: *p<0.1; **p<0.05; ***p<0.01

All specifications control for state specific log of total number of testing (aggregated at weekly level). We control for the timing of a statewide emergency declaration (Column(2)), non-essential business restrictions (Column (3)), restaurant restrictions (Column (4)), and limits on large gatherings (Column (5)). Column (6) controls for the timing of all NPIs simultaneously. The sample only contains county level observations from Second (N=5,642) and Third (N=7,602) Mover states. The standard errors are clustered at the county level to account for within county correlation.

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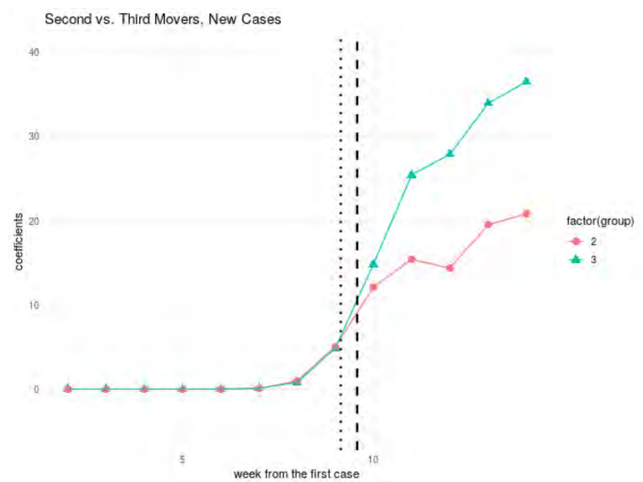
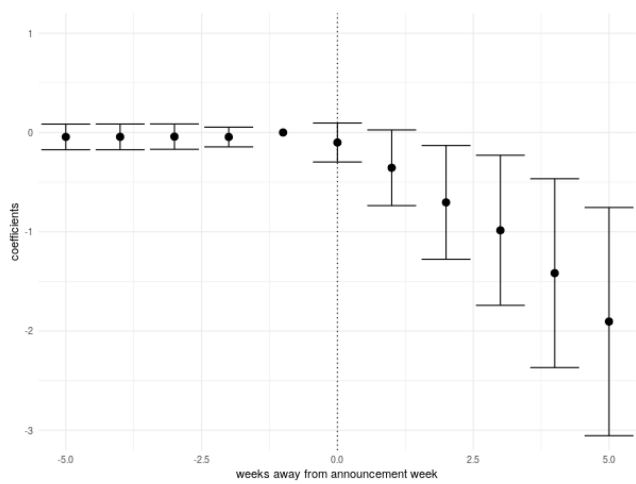


(a) Weekly New Cases in 2nd and 3rd Mover States



(b) Log Weekly Cases in 2nd and 3rd Mover States

Figure 9: **The Consequences of Delays in NPI Announcements**

Notes: **Panel** (a) plots the coefficients λ_j and κ_j after estimating specification 2. Groups 2 and 3 correspond to the Second and Third Movers, respectively. The dotted and dashed vertical lines correspond to the timing of announcement and implementation date of stay at home mandate pertaining to the last state to announce and implement in the Second Movers group. Detailed estimation results of these coefficients are available in Table C.5 in Appendix C (Column 6). **Panel** (b) plots the β coefficients after estimating the event study specification 3. Detailed estimation results are presented in Table 2. Figure 9(a) use the weekly number of new cases and Figure 9(b) uses the log of the weekly number of new cases as the dependent variable, respectively. Both figures are based on a sample that only contains county level observations from Second (N=5,642) and Third (N=7,602) Mover states.

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The first six sub-figures show that the pattern in coefficients from the event study specifications are similar to Figure 9 (the sixth sub-figure covers states in $13^{th} - 87^{th}$ percentiles of mandate announcement distribution). However, once we start to include more of the early movers in the sample, the slope coefficients pertaining to weeks after the mandate level off to zero (sub-figures 8 and 9).

Expanding the pool further turns the slope coefficient after the announcement week positive (sub-figures 8–9). Although we emphasize that event study exercises are not absolute tests for our identification assumption, the observed patterns are indicative of the timing of stay-at-home mandates being endogenous in early mover states as suggested in Gupta et al. (2020). Including observations from early mover states into our analysis on the effectiveness of stay-at-home mandates would therefore invalidate our identification strategy.

In summary, we argue that (i) by eliminating early mover states for whom the timing of the mandate is likely to be endogenous, it is possible to only retain observations from states without preexisting differences in the number of caseloads (i.e., Second versus Third Movers) prior to the announcement of the mandate. As the grouping of second versus third movers are differentiated only by a few days, the timing of announcement date of mandate between these two groups are less likely to be systematically correlated with future projections. Still (ii) we advise caution in extrapolating any external validity from these estimates as they are based on a subset of states.[29]

4.3.3 Heterogeneous Effects by Poverty Levels

In order to evaluate the differential effects of the stay-at-home mandates across the poverty spectrum, we again use the sub-sample of Second and Third Movers and estimate a specification similar to equation 3 but include interaction terms with poverty level indicators:

$$W_{c,t} = \alpha + \sum_{p=1}^{p=3}\sum_{j=-5}^{j=-2} \beta_{j,p} \times I(t=j) \times I(c=p) + \sum_{p=1}^{p=3}\sum_{j=0}^{j=5} \beta_{j,p} \times I(t=j) \times I(c=p) + \tag{4}$$
$$+ \delta \times NPI_{c,t} + \gamma_c + \sum_{l=1}^{l=3} \sigma_t \times I(c=p) + \varepsilon_{c,t},$$

More specifically, we interact indicators representing the weeks away from the announcement of the stay-at-home mandate with indicators representing three distinct income groups using the poverty share of the observed counties as classifier. The poverty groups $p \in \{1,2,3\}$ identify rich areas below or at the 30^{th} percentile of the county-groupings poverty scale, mid-level areas between the 30^{th}–70^{th}, and poor counties above or at the 70^{th} percentile, respectively. For

[29] The second mover states include CO, DE, ID, KY, MN, VT, and WI. The third mover states comprise AK, AZ, KS, MD, NC, NH, RI, TN, UT, and VA.

instance, $I(c = p)$ takes the value 1 if a county c belongs to the poverty group p. Next, the week fixed effects are additionally interacted with the indicators defining poverty groupings to allow for differential effects in spread of infections across different poverty groupings. The coefficients of interest—$\beta_{j,1}$, $\beta_{j,2}$, and $\beta_{j,3}$— trace the effects of the stay-at-home mandate for rich, mid-level, and poor counties, respectively.

The estimates for the β coefficients of specification 4 are summarized in the three panels of Figure 10.[30] Panels A, B and C show that the coefficient estimates for rich, mid-level, and poor counties hover around zero in weeks prior to the announcement week, and they start to fall. The drop in coefficients is concentrated in the very rich and the very poor counties. Although we see a drop in coefficients for counties in mid-level poverty group in sub-figure B, these coefficients are statistically insignificant at the conventional levels and we are unable to reject the hypothesis that the stay-at-home mandate has no effect on caseloads in mid-level counties. These patterns show that the findings in Figure 10 are primarily driven by the effects in richer and poorer counties.

[30] A detailed regression results table is available upon request.

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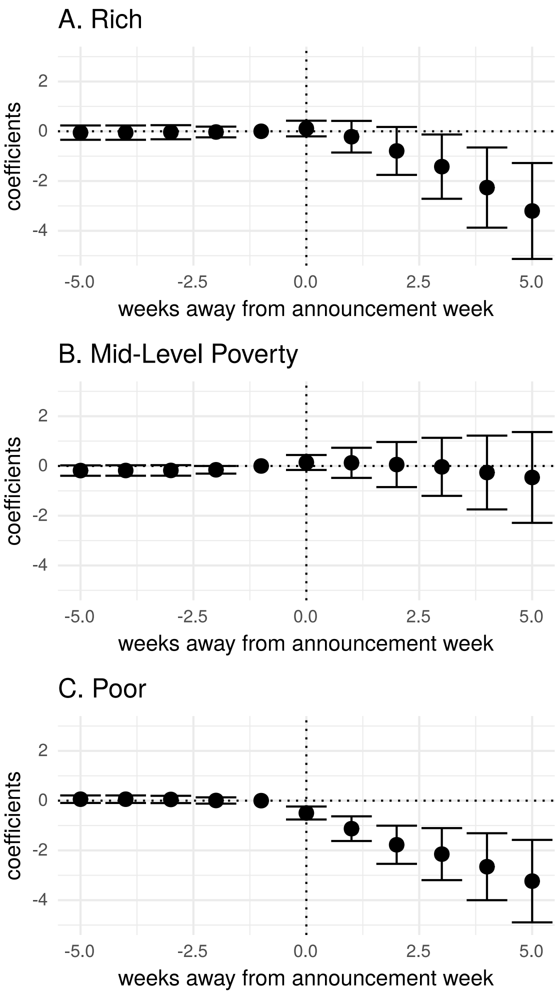

Figure 10: **Heterogeneous Effects by Poverty Levels**

Notes: The estimated coefficients pertaining to equation 4 are plotted for rich, mid-level, and poor areas. Rich areas are defined as counties below or at the 30^{th} percentile of the county-groupings poverty scale. Similarly, groups mid-level and poor counties are between the $30^{th}-70^{th}$ percentile and above or at the 70^{th} percentile, respectively. This specification controls for the log of state-specific total number of testing (at the weekly level), the timing of the restaurant restrictions, restrictions on large gatherings, weekly dummies interacted with high density county indicators, interaction between week fixed effects and poverty groupings (as defined above), and county fixed effects. All figures are based on a sample that only contains county level observations from Second (N=5,642) and Third (N=7,602) Mover states. The standard errors are clustered at the county level and the vertical bars represent the 95 percent confidence lines.

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4.4 How Effective are Stay-at-Home Mandates? Evidence from IV Estimates

For 3,092 counties, we collect daily weather data at the county level, which are used to calculate weekly averages. This gives us a balanced panel for 3,092 counties over the span of 14 weeks, starting from January 22 until April 28.[31] To rectify the endogenous nature of stay-at-home mandates, we use the announcement dates of these mandates along with arguably exogenous weather patterns to evaluate the effect of social distancing on increases in coronavirus caseloads. We use mobility data from SafeGraph as a proxy measure for social distancing.

Severe weather patterns can reduce mobility. However, weather shocks can be more binding while a stay-at-home mandate is issued as the marginal cost of mobility is already high during the shutdown and extreme weather can increase it even more. Hence, detrimental weather patterns can increase the effectiveness of a mandate. To test this, we estimate a two-stage model. The first stage is formalized as:

$$\begin{aligned} \log(mobility)_{c,t} &= \alpha + \kappa \times Precip_{c,t} + \eta \times AvgTemp_{c,t} + \delta \times M_{c,t} \\ &\quad + \beta \times M_{c,t} \times I(mintemp_{c,t} < Q_{25,s,m}) + \lambda \times NPI_{c,t} + \gamma_c + \sigma_t + \varepsilon_{c,t}, \end{aligned} \tag{5}$$

where $Precip_{c,t}$ and $AvgTemp_{c,t}$ are precipitation and average temperature in county c in week t. Indicator variable $M_{c,t}$ equals one if a stay-at-home mandate is in place in the county at time t. We interact the mandate indicator variable with an indicator for very low temperature in a county. More specifically the low temperature indicator variable $I(mintemp_{c,t} < Q_{25,s,m})$ equals one if the minimum temperature in county c in week t is below the 25^{th} percentile of recorded temperatures in a specific month m and state s which we denote $Q_{25,s,m}$.[32] In other words, $I(mintemp_{c,t} < Q_{25,s,m})$ captures weather shocks in county c at time t that are defined as extreme cold weather relative to weather patterns of the county's state in a given month.

Similar to our previous specifications, $NPI_{c,t}$ is a vector of other non-pharmaceutical interventions and γ_c and σ_t represent county and week fixed effects, respectively. Coefficient β describes the mobility pattern in counties with a stay-at-home mandate and an extreme weather shock compared to counties with a mandate but without a weather shock. The identification of β is obtained from the exogenous weather shock. From a program evaluation perspective, after controlling for the mandate status as well as county and week fixed effects, a county A with a mandate can serve both as affected or unaffected group, depending on the weather pattern

[31] There are 3,143 counties or county-equivalents including DC in the US. We dropped 51 counties due to missing weather or mobility data.

[32] Using other state temperature percentiles as cutoffs, such as the median, does not change the estimates but weakens the F-statistic. Also, to account for the rise in temperature over the months in the sample, we use absolute cut-off, given by the 25^{th} percentile of recorded temperatures in all months. The findings from these analysis yield similar results. These results are not presented but are available upon request.

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of the county. Moreover, even within a state, one county might be affected by the weather shock, whereas the other might not be. The county fixed effects will control for the unobserved time invariant relationship between area specific weather patterns and mobility outcomes. If weather shocks exacerbate mobility patterns following the mandate, the estimate on β will be negative. Next, we expand equation 5 and set up an event study that allows tracking the effects of weather shocks on mobility before and after the mandate:

$$\log(mobility)_{c,t} = \alpha + \sum_{j=-5}^{j=-2} \beta_j \times I(mintemp_{c,t} < Q_{25,s,m}) \times I(t=j) + \sum_{j=0}^{j=5} \beta_j \times I(mintemp_{c,t} < Q_{25,s,m}) \times I(t=j) + \kappa \times Precip_{c,t} + \eta \times AvgTemp_{c,t} + \delta \times M_{c,t} + \lambda \times NPI_{c,t} + \gamma_c + \sigma_t + \varepsilon_{c,t} \quad (6)$$

The specification is similar to equation 5 except that the weather shock indicator ($I(mintemp_{c,t} < Q_{25,s,m})$) is interacted with the weeks before and following the announcement of stay at-home-mandates. As omitted category we use again the week prior to the announcement of a mandate. The β_j coefficients provide a test for our claim that weather shocks constitute an additional cost during a lockdown period and decrease mobility more than in periods without a lockdown. If our assumption is valid then the estimates of β_j for $j \in \{-5, -4, -3, -2\}$ should be close to zero, whereas the estimates of β_j for $j > -1$ should be less than zero. This pattern would indicate a binding relationship between weather shocks and mobility following stay-at-home mandate announcements.

We present the results from estimating specification 5 in Table 3. The stay-at-home mandate coefficients are positive and statistically significant at the 1 percent level. This is not surprising. As discussed above, enforcement was generally lax, and as discussed in Section 4.3, the announcement of the mandate is likely to be endogenous as counties with higher mobility are more at risk of infections, which can then trigger a more immediate announcement of a mandate. The coefficients on the interaction term of the mandate and the weather shock indicator are negative, suggesting that among the counties with a mandate, a weather shock reduces mobility comparatively to counties with no weather shocks. Using estimates from Column 1, a cold weather shock reduces mobility on average by 7 percent $[(exp(-0.077)-1) \times 100]$ during the time of the mandate compared to counties with a mandate but without a weather shock. This magnitude is consistent across all the different specifications with respect to NPIs. F-statistics are used to gauge the importance of the interaction term. It is based on a Wald test that compares the restricted (without the interaction term) to the unrestricted (with the interaction term) specification and uses White's standard errors to account for heteroskedasticity. F-statistics are presented for all model specifications. In all 7 specifications, the F-statistic is greater than 10, which is a widely used cutoff to determine the strength of the instrument (Staiger and Stock (1994)). The exclusion restriction for a valid instrument states that weather

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shocks should affect coronavirus infections only through a reduction in human-to-human interactions (proxied with a mobility measure based on cellphone data) and should not directly or biologically affect coronavirus infections.[33]

Additionally, we present estimates of the interaction coefficients β_j from equation 6 in Figure 11. The coefficients fluctuate around zero in the weeks prior to the announcement date. Following the announcement, the coefficient estimates decrease below zero and the drop is statistically significant at the five percent level. However, an interesting pattern emerges. Starting from the third week after the announcement coefficients steadily increase in magnitude, and by the fifth week the coefficient estimates level off, suggesting that people eventually revert to their initial pattern. This is consistent with the trends in mobility shown in Panel B of Figure 8. In most cases the decrease in mobility is accompanied by a rise after a few weeks following the mandate announcement. This result provides evidence that weather shocks, as defined in this study, cause a short term reduction in mobility in the immediate weeks after mandates are declared which allows us to use weather shocks along with mandate announcements as instruments for estimating the effects of mobility on coronavirus outcomes.

Next, we predict (the log of) mobility based on estimating the first-stage specification 5 and use the results of this prediction, reported in Column (1) in Table 3, in a second stage regression of the form:

$$\begin{aligned} \log(cases)_{c,t} = \alpha + \kappa \times Precip_{c,t} + \eta \times AvgTemp_{c,t} + \delta \times M_{c,t} \\ + \beta \times \log(\widehat{mobility}_{c,t}) + \lambda \times NPI_{c,t} + \gamma_c + \sigma_t + \varepsilon_{c,t}, \end{aligned} \tag{7}$$

where all of the variables are similar to those in equation 3, except the dependent variable is the log of weekly new cases, and the independent variable includes the predicted values of log mobility predictions from the first stage. Also, we control for the log of state-specific total number of tests conducted, aggregated at the weekly level.

Table 4 presents the results from instrumenting (log of) mobility with the interaction between the timing of a stay-at-home mandate and weather shocks. The IV estimates suggest that a one percent increase in mobility leads to a 8–9 percent increase in the number of cases per week. Using the mean as the baseline, this would imply that an increase in mobility by 1 km (average travel from home), causes about 55 additional infection cases per week.[34] We caution that the IV estimates represent a Local Average Treatment Effect (LATE) and are conditional to those individuals or areas where weather patterns disrupt mobility. Next, we

[33]There is no evidence presented as of now whether warm weather is detrimental to the coronavirus and can slow down infections.

[34]The average mobility in the sample of counties that experience at least one weather shock after the announcement of the mandate is 14.7 km. An increase in 1 percent of mobility (0.147 km) leads to approximately 8 additional infection cases per week.

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shorten the time window of the analysis and only focus on the months of March and April to test whether the pattern of increasing temperature over the months of January–April is driving our results. The estimates are similar to the main IV findings above which suggests that our results are not driven by differences in temperature from January–April.[35]

In light of the findings highlighted in Table 11 that weather shocks were only binding during the period of the mandate, we provide suggestive evidence regarding the validity of the instrument by estimating the following model specification:

$$\begin{aligned}\log(cases)_{c,t} = {} & \alpha + \kappa \times Precip_{c,t} + \eta \times AvgTemp_{c,t} + \delta \times M_{c,t} \\ & + \sum_{j=-5}^{j=-2} \psi_j I(t=j) \times \log(\widehat{mobility_{c,t}}) + \sum_{j=0}^{j=5} \psi_j I(t=j) \times \log(\widehat{mobility_{c,t}}) \\ & + \lambda \times NPI_{c,t} + \gamma_c + \sigma_t + \varepsilon_{c,t}.\end{aligned} \tag{8}$$

This specification is similar to specification 7, except that the predicted values of (log) mobility from the first stage is interacted with indicators representing the weeks following the announcement of the mandate. Since the instrument is only binding during the period following the announcement of the mandate (compare Figure 11), the predicted values of (log) mobility using weather shocks as the instrument should only affect the number of coronavirus infections after the announcement and should not be systematically related with the prior numbers of infections. Magnitudes of ψ_j close to zero for $j < -1$ in equation 8 would support this assumption.

[35] The detailed estimation results from this exercise are presented in Table C.6 (first stage) and C.7 (IV estimates) in Appendix C.

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Table 3: **Mobility, Mandate and Weather Shocks (First Stage)**

	Dependent variable:						
	log of Mobility						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Precipitation (inch)	0.006	0.006	0.006	0.007	0.006	0.007	0.008
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Average Temperature (degree Fahrenheit)	-0.002^{***}	-0.002^{***}	-0.002^{***}	-0.001^{**}	-0.002^{**}	-0.001^{**}	-0.002^{**}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Stay Home Mandate	0.069^{***}	0.067^{***}	0.070^{***}	0.057^{***}	0.058^{***}	0.051^{***}	0.055^{***}
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
High Precipitation × Stay Home Mandate	-0.080^{***}	-0.081^{***}	-0.080^{***}	-0.082^{***}	-0.083^{***}	-0.085^{***}	-0.083^{***}
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
F-Statistic	44.53	45.4	44.65	46.71	47.73	40.53	47.52
Emergency Declaration		X				X	X
N.E. Business			X			X	
Restaurants				X		X	X
Gathering					X	X	
Observations	43,288	43,288	43,288	43,288	43,288	43,288	43,288

Note: *p<0.1; **p<0.05; ***p<0.01

We control for the timing of (*i*) a statewide emergency declaration (Column(2)), (*ii*) non-essential business restrictions (Column (3)), (*iii*) restaurant restrictions (Column (4)), and limits on large gatherings (Column (5)). Column (6) controls for the timing of all NPIs simultaneously. The standard errors are clustered at the county level to account for within county correlation. The F-statistic is obtained from a Wald test based on restricted and unrestricted regression models. White standard errors robust to heteroskedasticity are used to obtain the F-statistic.

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Table 4: **IV Estimates Using Weather Shocks**

	Dependent variable:						
	log of Weekly New Cases						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log of mobility (IV)	8.841***	8.825***	8.810***	9.058***	9.410***	9.504***	9.033***
	(1.043)	(1.042)	(1.043)	(1.038)	(1.033)	(1.030)	(1.038)
Emergency Declaration		X				X	X
N.E. Business			X			X	
Restaurants				X		X	X
Gathering					X	X	
Observations	43,288	43,288	43,288	43,288	43,288	43,288	43,288

Note: *p<0.1; **p<0.05; ***p<0.01

All specifications control for the log of state-specific total number of testing (at the weekly level). We control for the timing of (*i*) a statewide emergency declaration (Column(2)), (*ii*) non-essential business restrictions (Column (3)), (*iii*) restaurant restrictions (Column (4)), and limits on large gatherings (Column (5)). Column (6) controls for the timing of all NPIs simultaneously. The standard errors are clustered at the county level to account for within county correlation.

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The findings from estimating equation 8 are shown in Figure 12. Estimates of ψ_j for $j < -1$ are close to zero and statistically insignificant at the conventional levels. This result suggests no systematic relationship between predicted mobility and coronavirus cases prior to the announcement of a mandate. However, there is a discontinuous spike in the estimate following the first week of the announcement of a mandate, and consistent with the coefficient estimates shown in Figure 11, the magnitude decreases as time progresses.

In order to put the IV estimates into perspective, we conduct a back-of-the-envelope calculation which helps evaluate the effectiveness of social distancing. The average drop in mobility within counties after a mandate is announced is 3 km, which suggests that social distancing helped reduce the number of infections by 3 million cases across 3,142 counties [3 (km)$\times$55(cases/km)$\times 3,142$(counties)$\times$5.7(weeks from March 19-April 28)]. We caution that our back-of-the envelope calculation is based on estimates that relate to LATE and is driven by individuals affected by weather shocks during the period following the announcement of the mandate. As a comparison, Hsiang et al. (2020) find that shutdown orders prevented about 4.8 million novel coronavirus infections in the United States. Similarly, Flaxman et al. (2020) estimate that the shutdowns saved about 3.1 million lives in 11 European countries, including 500,000 in the United Kingdom, and dropped infection rates by an average of 82 percent.

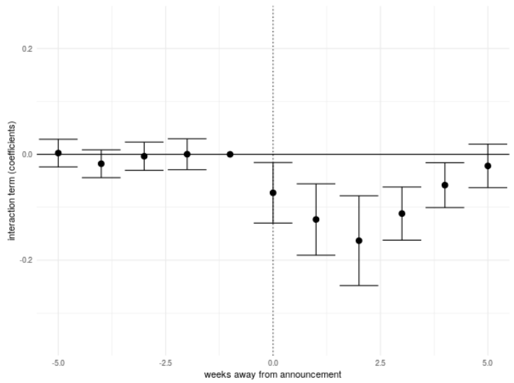

Figure 11: **Weather Shock and Mobility – An Event Study Approach**

Notes: To test our assumption that weather shocks impose additional cost to mobility following the announcement of stay-at-home mandates, we estimate the specification outlined in equation 6 and plot the estimates of β_j. The week prior to the announcement week is used as omitted category. The specification additionally controls for average temperature (in Fahrenheit), precipitation (in inches), NPIs (such as restrictions on restaurants, restrictions on bars, and state-of-emergency declarations), and county and week fixed effects. Standard errors are clustered at the county level and the vertical bars represent the 95 percent confidence intervals.

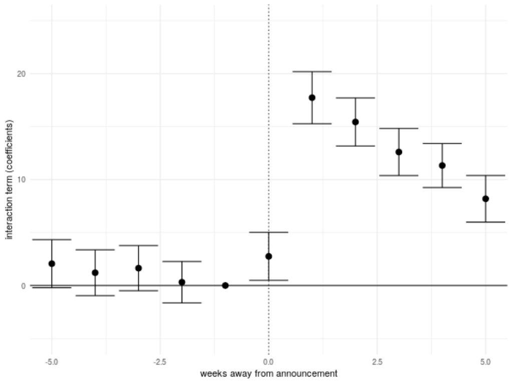

Figure 12: **Predicted Log of Mobility and Coronavirus Infections**

Notes: Along the lines of findings highligted in Figure 11, we test the assumption that the prediced values of the log of mobility using the First Stage only affect coronavirus infections following the announcement of the mandate and have no effect in prior infections. The estimates of ψ_j from equation 8 are plotted along the weeks away from announcement of the mandate. The week prior to the announcement week is used as an omitted category. Additionally, the specification controls for average temperature (in Fahrenheit), precipitation (in inches), NPIs (such as restrictions on restaurants, restrictions on bars, and state-of-emergency declarations), and county and week fixed effects. Standard errors are clustered at the county level and the vertical bars represent the 95 percent confidence intervals.

5 Conclusion

Reviewing evidence of the 2020 outbreak of COVID-19 in the United States, we see a strong poverty gradient in both infections and deaths but also an important interaction between poverty and population density. Overall infections increase at both end of the income distribution, creating a U-shaped curve shown in 3. A similar pattern is observed for confirmed deaths although the rate is higher among the poor (Figure 4). Further breaking down the distribution by population density (Figures 5(a) and 5(b)), we see that while the U-shaped curve prevails in high density counties, in low density counties the increase in infections among the poor dwarfs the rate among the rest of the population essentially creating an exponential curve. Along the same lines we see that while death rates are higher across the board in high density counties, the impact of poverty is more strongly felt in the lower density areas 7.

Our second main finding is the impact of social distancing. By forming groups of states based on the timing of the announcement of stay at home mandates, we compare Second versus Third Mover states—groups that differ only by a few days of delay in announcing the mandate. We show that even a few days of delay in NPIs can increase the number of Coronavirus infections

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significantly. One methodological concern is that forming of mandates are determined by the future projection in caseloads. To overcome this issue, we use weather shocks (denoted by low temperature shocks) along with the mandates as an instrument. The IV results suggest that an increase in average mobility of 1 km results in 55 additional infection cases per week.

Although we account for testing at the state level by using data from The Covid Tracking Project in many specifications, we caution that lack of quality data on testing at the county level may underestimate the number of infections (Manski and Molinari (2020)). This poses a limitation to the findings of this study, particularly if testing is correlated with socioeconomic characteristics. Presuming that the cause of death is accurately classified, a U-shaped curve portraying COVID-19 deaths is consistent and supports the U-shaped curve for infections. To an extent, this lowers the intensity of concerns due to differences in testing across localities. Nevertheless, we emphasize the need for adequate testing data at local levels, which can further benefit studies in this sector.

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Compliance with Ethical Standards

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Conflict of Interest: The authors declare that they have no conflict of interest.

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A Fitting Curves Using the Leave-One-Out Methods

This section describes the curve fitting method we use to produce the smooth curves in Figures 3–6. We start with 20 points that are calculated using the non-parametric method described in Section 3.[36] We then employ a local linear estimation that results in a non-parametric fit that incorporates these 20 points. However, the fit depends on a smoothing parameter. If the smoothing parameter is very high, the curve becomes the the best fit line of an OLS estimate. If the smoothing parameter is low, noise increases and the lines starts to move through every point. Fitting a smooth curve through the 20 points becomes a trade-off between bias (using high value and producing a very smooth curve) and noise (using a low value). We use a procedure that minimizes the residual mean squared error (RMSE) from a prediction resulting from leaving one of the 20 points out when estimating a local regression.

This leave-one-out cross-validation method minimizes the RMSE but is robust to the possibility of in-sample over fitting. This method works as follows. In the case of 20 points, we first start with the starting value of the smoothing parameter α_1. We use the last 19 points (excluding the first point) and estimate the local linear model. Then we use this estimation to predict the value of the first point we left out. The difference between the first (actual point) and the first (predicted point) contributes to the MSE. We perform similar estimations by excluding each point of the 20 points and using the resulting 19 to perform local linear regression. We then perform similar out-of-sample predictions and use the excluded point to calculate the RMSE. The RMSE for the first value of the starting smoothing parameter α_1 is $\text{RMSE}(\alpha_1) = \sum_{i=1}^{20} \frac{(x_i - \hat{x}_{\alpha_1,i})}{20}$ where x_i is the actual point observation and $\hat{x}_{\alpha_1,i}$ is the prediction of point i based on local regressions using smoothing parameter α_1. We next repeat this for tightly packed values of the smoothing parameter $\alpha \in [\underline{\alpha}, \bar{\alpha}]$, which gives a series of $\text{RMSE}(\alpha)_\alpha$. We then choose the minimum RMSE and its associated smoothing parameter $\hat{\alpha}$.

[36] This method ranks all US counties according to percentage of individuals living under the poverty level and then forms 20 county-groups of roughly equal population size. Each group is an observation in Figures 3–6.

B Additional Figures

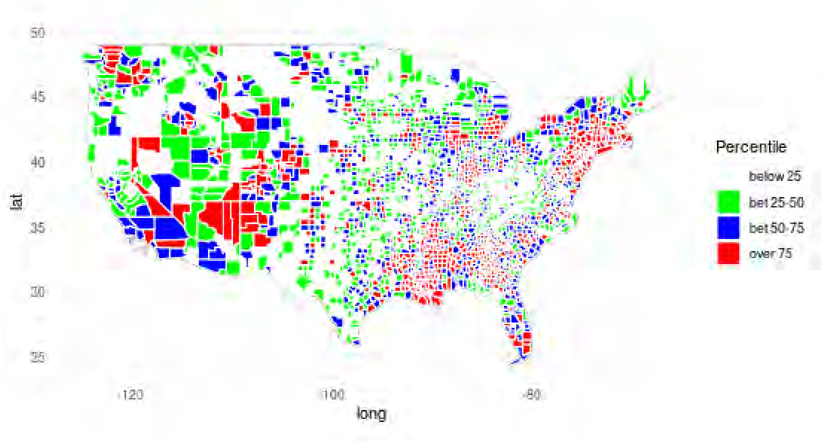

Figure B.1: **SARS-CoV-2 Confirmed Infections by County**

Notes: The data is from USAFacts as of April 28, 2020.

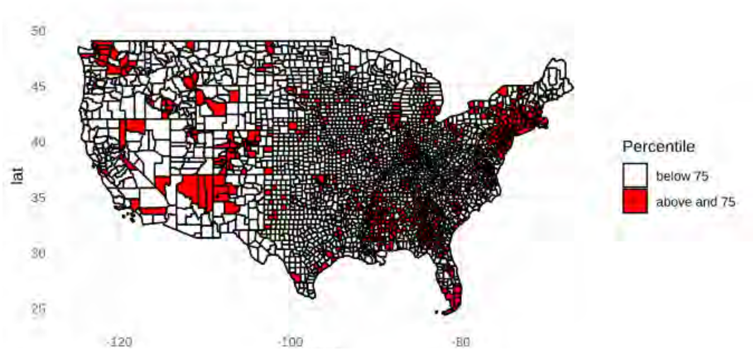

Figure B.2: **COVID-19 Related Deaths by County**

Notes: The data is from USAFacts and shows counties with deaths above the 75^{th} percentile as of April 28, 2020.

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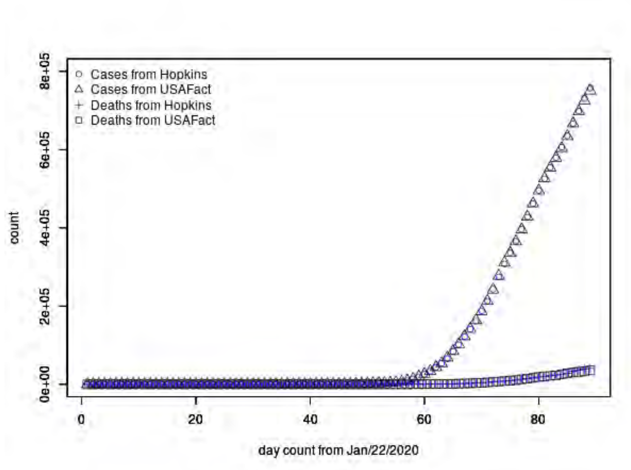

Figure B.3: **Data Comparison: Johns Hopkins vs. USAFacts**

Notes: Data are from USAFacts and the Center for Systems Science and Engineering of the Whiting School of Engineering at the Johns Hopkins University. The two data sources are essentially identical for infection cases as well as for COVID-19 related deaths.

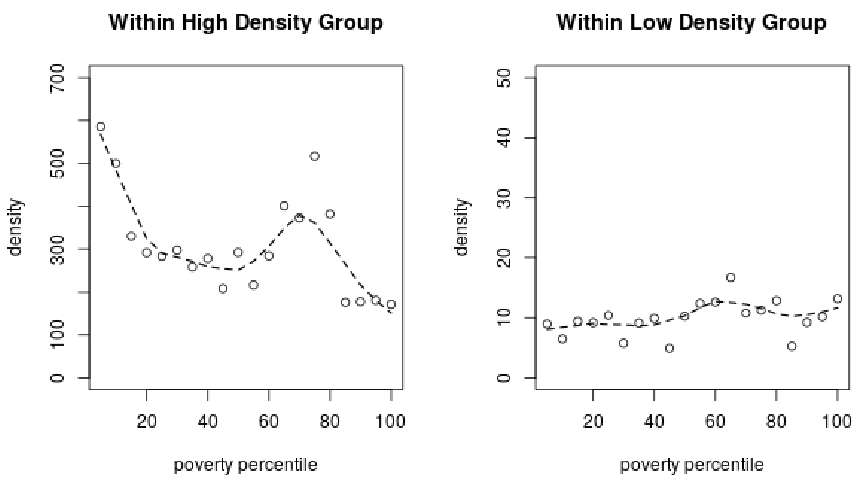

Figure B.4: **Density**

Notes: Data are from the US Census.

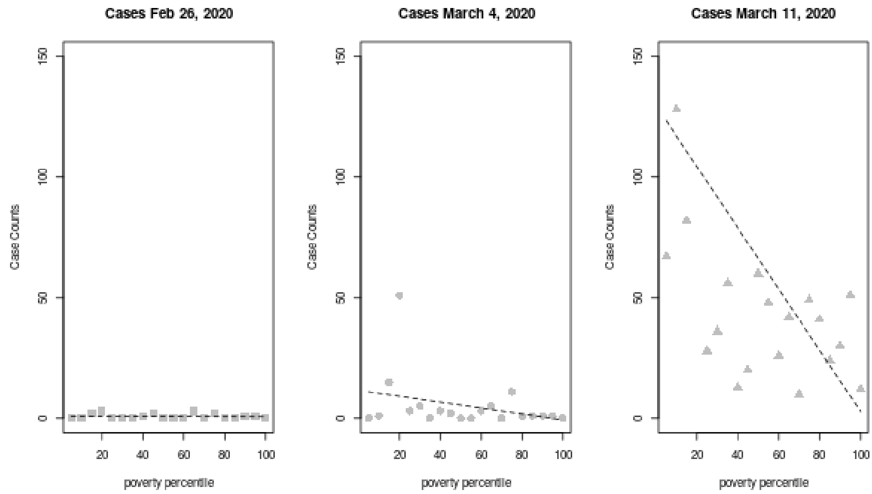

Figure B.5: **Coronavirus Infections and Poverty in the Early Months**

Notes: Data are from USAFacts. We report the number of cumulative (infection) cases on February 26, March 4, and March 11, 2020. The downward sloping best-fit line on the rightmost figure suggests that in the initial phase of the pandemic infections in the US were more prevalent in richer counties.

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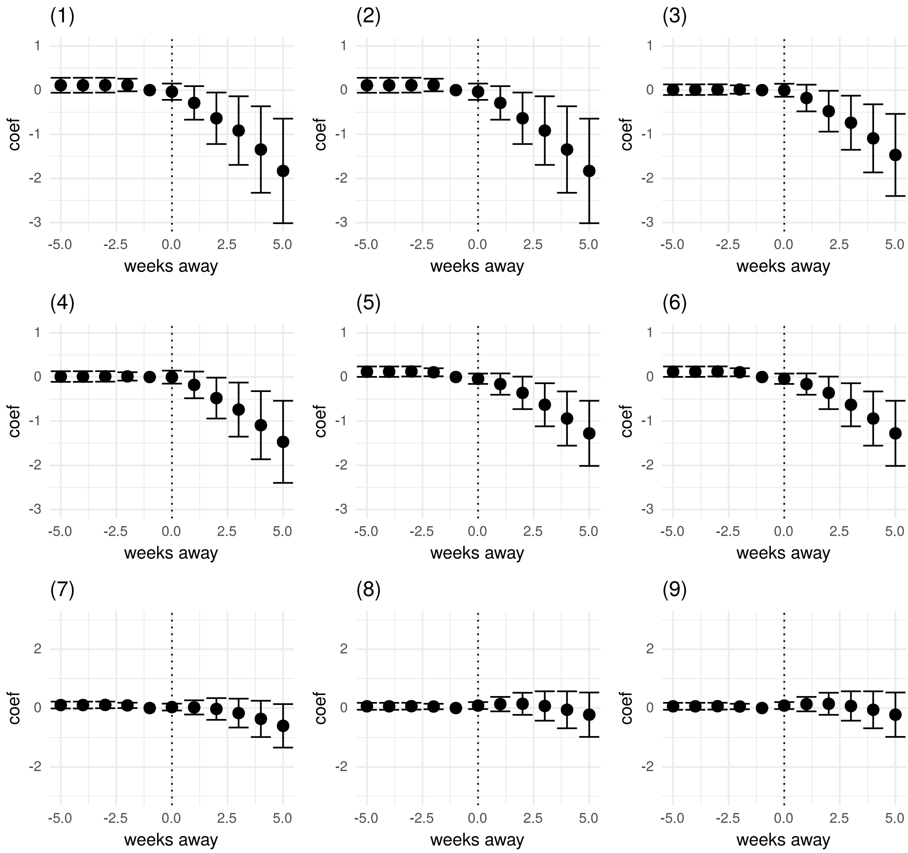

Figure B.6: **Robustness Exercise by Expanding the Pool of States**

Notes: The log of weekly number of new cases is the dependent variable in all panels. The panels plot the β coefficients after estimating the event study specification 3 for samples that only contain county level observations from Second and Third Mover states. In the panel (1) Second and Third Mover states are defined as states between the $25^{rd} - 75^{th}$ percentile of the mandate announcement distribution. We then systematically widen the bracket by 2 percentile in each direction until we capture county observations from states between the $7^{th} - 93^{rd}$ percentile of the timing distribution in the right bottom panel (panel 9).

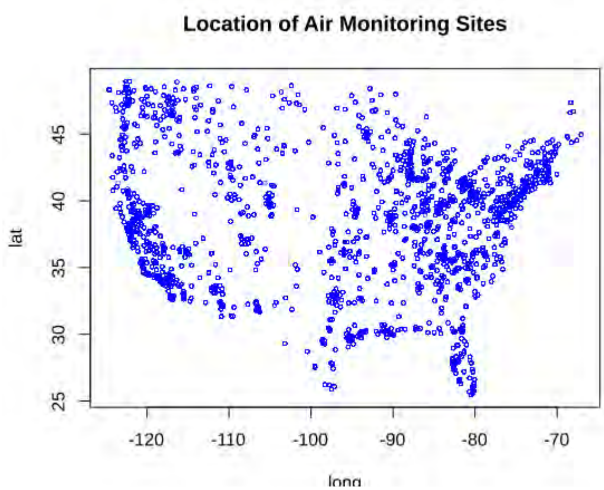

Figure B.7: **Air Monitoring Stations**

Notes: Data are from AirNow daily measurements.

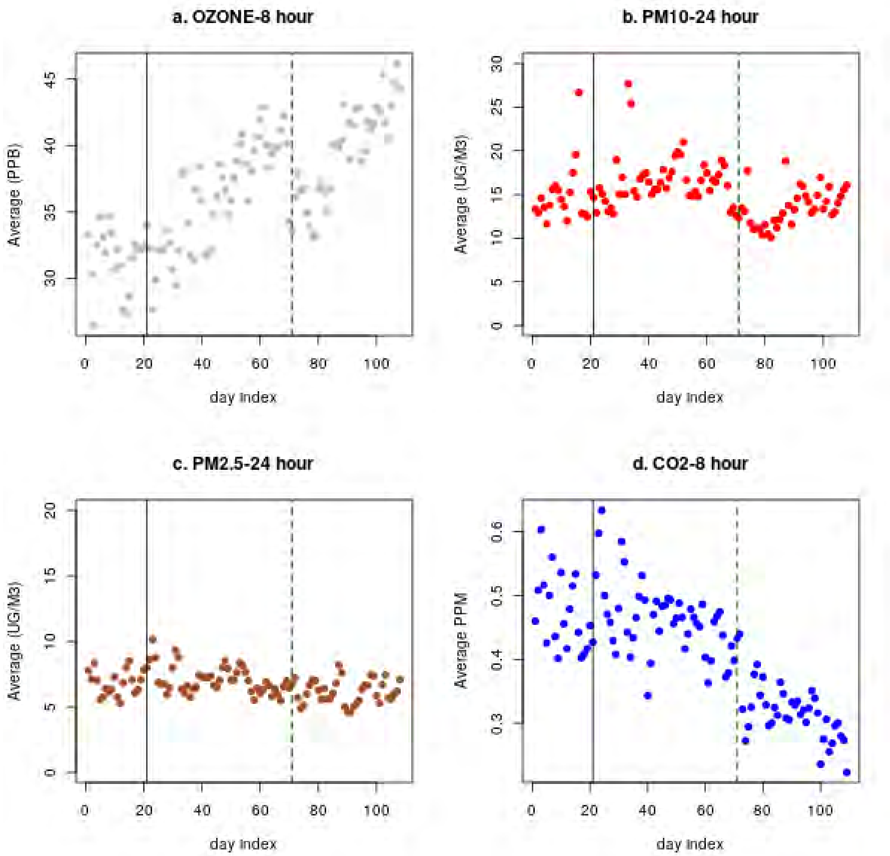

Figure B.8: **Air Quality**

Notes: The source of data are AirNow daily measurements. The solid line represents the first identified case in US (January 21) and the dotted line refers to the date of federal emergency declaration (March 11).

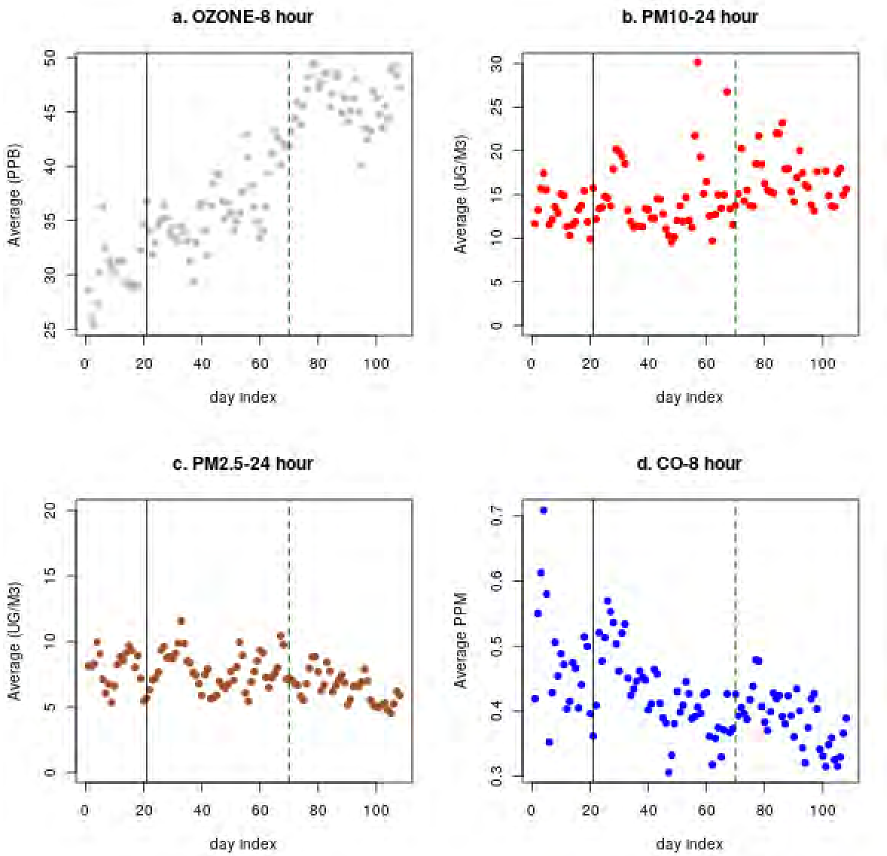

Figure B.9: **Air Quality (2019)**

Notes: Data are from AirNow daily measurements. The solid line represents the first identified case in US (January 21, 2019) and the dotted line refers to the date of federal emergency declaration (March 11, 2019).

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C Additional Tables

Table C.1: **NPI Roll Out by States**

	state	SaHAnnounce	SaHImplement	Emergency	School	Restaurant	NE.Business	Gathering
1	Alaska	3/27/20	3/28/20	03/11/20	03/16/20	03/17/20	03/24/20	03/28/20
2	Alabama	4/3/20	4/4/20	03/13/20	03/19/20	03/20/20	03/20/20	
3	Arkansas			03/11/20	03/17/20	03/19/20		
4	Arizona	3/30/20	3/31/20	03/11/20	03/16/20	03/20/20		
5	California	3/19/20	3/19/20	03/04/20	03/19/20	03/15/20	03/11/20	03/19/20
6	Colorado	3/25/20	3/26/20	03/10/20	03/23/20	03/17/20	03/19/20	03/26/20
7	Connecticut	3/22/20	3/23/20	03/10/20	03/17/20	03/16/20	03/12/20	03/23/20
8	Delaware	3/24/20	3/24/20	03/13/20	03/16/20	03/16/20	03/16/20	03/24/20
9	Florida	4/1/20	4/3/20	03/09/20	03/16/20	03/17/20	04/03/20	03/30/20
10	Georgia	4/1/20	4/3/20	03/14/20	03/18/20	03/24/20	03/24/20	
11	Hawaii	3/23/20	3/25/20	03/04/20	03/23/20	03/17/20	03/16/20	03/25/20
12	Iowa			03/09/20	04/03/20	03/17/20	03/17/20	
13	Idaho	3/25/20	3/25/20	03/13/20	03/23/20	03/25/20	03/25/20	03/25/20
14	Illinois	3/20/20	3/21/20	03/09/20	03/17/20	03/16/20	03/13/20	03/21/20
15	Indiana	3/23/20	3/25/20	03/06/20	03/19/20	03/16/20	03/12/20	03/24/20
16	Kansas	3/28/20	3/30/20	03/12/20	03/18/20		03/17/20	
17	Kentucky	3/25/20	3/26/20	03/06/20	03/16/20	03/16/20	03/19/20	03/26/20
18	Louisiana	3/22/20	3/23/20	03/11/20	03/16/20	03/17/20	03/13/20	03/23/20
19	Massachusetts	3/23/20	3/24/20	03/10/20	03/17/20	03/17/20	03/13/20	03/24/20
20	Maryland	3/30/20	3/30/20	03/05/20	03/16/20	03/16/20	03/16/20	03/23/20
21	Maine	3/31/20	4/2/20	03/15/20	03/16/20	03/18/20	03/18/20	03/25/20
22	Michigan	3/23/20	3/24/20	03/10/20	03/16/20	03/16/20	03/13/20	03/23/20
23	Minnesota	3/25/20	3/28/20	03/13/20	03/18/20	03/17/20		
24	Missouri	4/6/20	4/6/20	03/13/20	03/23/20	03/17/20	03/23/20	
25	Mississippi	4/1/20	4/3/20	03/14/20	03/20/20	03/24/20	03/24/20	03/31/20
26	Montana	3/23/20	3/28/20	03/12/20	03/16/20	03/20/20	03/24/20	03/28/20
27	North Carolina	3/27/20	3/30/20	03/10/20	03/16/20	03/17/20	03/14/20	03/30/20
28	North Dakota			03/13/20	03/16/20	03/20/20		
29	Nebraska			03/13/20	04/03/20	03/19/20	03/16/20	
30	New Hampshire	3/27/20	3/28/20	03/13/20	03/16/20	03/16/20	03/16/20	03/28/20
31	New Jersey	3/21/20	3/21/20	03/09/20	03/18/20	03/16/20	03/16/20	03/21/20
32	New Mexico	3/23/20	3/24/20	03/11/20	03/16/20	03/16/20	03/16/20	03/24/20
33	Nevada	4/1/20	4/1/20	03/12/20	03/16/20	03/17/20	03/19/20	03/21/20
34	New York	3/20/20	3/22/20	03/07/20	03/18/20	03/16/20	03/13/20	03/20/20
35	Ohio	3/23/20	3/24/20	03/09/20	03/17/20	03/15/20	03/12/20	03/24/20
36	Oklahoma			03/15/20	03/17/20	03/25/20	03/24/20	03/26/20
37	Oregon	3/20/20	3/23/20	03/08/20	03/16/20	03/17/20	03/16/20	
38	Pennsylvania	3/23/20	3/23/20	03/06/20	03/16/20	03/17/20	03/16/20	03/23/20
39	Rhode Island	3/28/20	3/28/20	03/09/20	03/16/20	03/16/20	03/17/20	
40	South Carolina	4/6/20	4/7/20	03/13/20	03/16/20	03/18/20	03/18/20	
41	South Dakota			03/13/20	03/16/20		04/06/20	
42	Tennessee	3/30/20	4/1/20	03/12/20	03/20/20	03/23/20	03/23/20	04/01/20
43	Texas	3/31/20	4/2/20	03/13/20	03/23/20	03/20/20	03/20/20	
44	Utah	3/29/20	4/1/20	03/06/20	03/16/20	03/18/20	03/16/20	
45	Virginia	3/30/20	3/30/20	03/12/20	03/16/20	03/17/20	03/15/20	
46	Vermont	3/25/20	3/25/20	03/13/20	03/18/20	03/17/20	03/13/20	03/25/20
47	Washington	3/23/20	3/23/20	02/29/20	03/17/20	03/16/20	03/11/20	03/25/20
48	Wisconsin	3/24/20	3/25/20	03/12/20	03/18/20	03/17/20	03/17/20	03/25/20
49	West Virginia	3/23/20	3/24/20	03/16/20	03/16/20	03/17/20		03/24/20
50	Wyoming			03/13/20	03/16/20	03/19/20	03/20/20	

Notes: Author's search and Ortiz and Hauck (2020) for stay-at-home announcement date, Mervosh, Lu and Swales (2020) for stay-at-home implementation and Gupta et al. (2020) for the other categories. SaH refers to stay-at-home mandate.

Table C.2: **Groupings of States**

	First Movers	Second Movers	Third Movers	Late Movers
1	CA	CO	AK	AL
2	CT	DE	AZ	AR
3	HI	ID	KS	DC
4	IL	KY	MD	FL
5	IN	MN	NC	GA
6	LA	VT	NH	IA
7	MA	WI	OK	ME
8	MI		RI	MO
9	MT		TN	MS
10	NJ		UT	ND
11	NM			NE
12	NY			NV
13	OH			OK
14	OR			SC
15	PA			SD
16	WA			TX
17	WV			WY

Notes: We distinguish by the timing of the announcement of the mandate. These groups include: *i*) First Movers, counties in states that announced the mandate before or on March 22^{nd}, *ii*) Second Movers implemented the mandate after March 22^{nd} but before or on March 24^{rd}, *iii*) Third Movers implemented after March 24^{th} but before or on March 29^{th}, and *iv*) Late Movers which implemented after March 29^{th}.

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Table C.3: **Weekly New Infection Cases by Poverty and Timing of Mandate**

	Dependent variable:			
	Weekly Cases			
	First Movers	Second Movers	Third Movers	Late Movers
Rich County × Week 2	−23.807	−0.641	−2.197	−1.267
	(26.367)	(2.158)	(3.373)	(1.586)
Rich County × Week 3	−23.817	−0.641	−2.197	−1.267
	(26.369)	(2.158)	(3.373)	(1.586)
Rich County × Week 4	−23.812	−0.641	−2.197	−1.267
	(26.368)	(2.158)	(3.373)	(1.586)
Rich County × Week 5	−23.798	−0.641	−2.197	−1.267
	(26.365)	(2.158)	(3.373)	(1.586)
Rich County × Week 6	−23.469	−0.641	−2.179	−1.267
	(26.312)	(2.158)	(3.369)	(1.586)
Rich County × Week 7	−20.724	−0.510	−1.983	−1.158
	(26.038)	(2.153)	(3.303)	(1.544)
Rich County × Week 8	−12.567	0.821	−0.590	−0.706
	(25.677)	(2.206)	(3.060)	(1.472)
Rich County × Week 9	55.952	6.779*	4.774*	2.617***
	(56.377)	(3.737)	(2.575)	(0.903)
Rich County × Week 10	169.993*	16.972*	19.930***	10.217***
	(99.773)	(9.163)	(7.110)	(2.810)
Rich County × Week 11	271.308*	18.331**	38.734**	14.473***
	(139.020)	(9.135)	(17.357)	(4.391)
Rich County × Week 12	250.438**	16.255**	44.768**	13.449***
	(126.242)	(7.688)	(21.702)	(3.932)
Rich County × Week 13	226.470**	17.841*	49.803**	13.452***
	(102.670)	(9.246)	(22.922)	(3.431)
Rich County × Week 14	175.535**	17.690*	53.826**	13.600***
	(77.701)	(9.552)	(26.397)	(3.078)
Mid-Level County × Week 2	6.546	−1.685	2.601	−1.484
	(8.828)	(1.902)	(1.940)	(1.776)
Mid-Level County × Week 3	6.546	−1.685	2.601	−1.484
	(8.828)	(1.902)	(1.940)	(1.776)
Mid-Level County × Week 4	6.546	−1.685	2.601	−1.484
	(8.828)	(1.902)	(1.940)	(1.776)
Mid-Level County × Week 5	6.543	−1.685	2.601	−1.484
	(8.828)	(1.902)	(1.940)	(1.776)
Mid-Level County × Week 6	6.555	−1.685	2.601	−1.475
	(8.828)	(1.902)	(1.940)	(1.773)
Mid-Level County × Week 7	6.930	−1.554	2.704	−1.273
	(8.805)	(1.894)	(1.929)	(1.721)
Mid-Level County × Week 8	10.795	−0.830	3.005	−0.261
	(8.184)	(1.816)	(1.881)	(1.368)
Mid-Level County × Week 9	47.390**	2.902	7.564***	5.612***
	(24.084)	(2.963)	(2.856)	(1.851)
Mid-Level County × Week 10	94.292**	9.859*	16.880**	18.907**
	(39.622)	(5.130)	(6.732)	(8.434)
Mid-Level County × Week 11	143.098**	16.402**	23.483***	26.507**
	(55.932)	(6.951)	(8.654)	(10.324)
Mid-Level County × Week 12	136.451***	15.047**	18.351***	25.351***
	(51.244)	(6.816)	(6.739)	(8.396)
Mid-Level County × Week 13	121.215***	28.924*	21.755***	28.194***
	(34.563)	(14.791)	(7.514)	(8.401)
Mid-Level County × Week 14	124.574***	32.931**	26.579***	27.890***
	(39.666)	(15.582)	(8.087)	(5.466)
Poor County × Week 2	8.845	2.713	0.108	1.292
	(16.081)	(2.227)	(2.148)	(1.184)
Poor County × Week 3	8.842	2.713	0.108	1.292
	(16.080)	(2.227)	(2.148)	(1.184)
Poor County × Week 4	8.842	2.713	0.108	1.292
	(16.080)	(2.227)	(2.148)	(1.184)
Poor County × Week 5	8.848	2.713	0.108	1.292
	(16.081)	(2.227)	(2.148)	(1.184)
Poor County × Week 6	8.869	2.713	0.117	1.292
	(16.081)	(2.227)	(2.152)	(1.184)
Poor County × Week 7	9.233	2.713	0.160	1.376
	(16.078)	(2.227)	(2.150)	(1.189)
Poor County × Week 8	14.975	3.221	0.561	1.759
	(15.608)	(2.197)	(2.224)	(1.277)
Poor County × Week 9	57.239*	5.338*	3.365	5.329***
	(31.753)	(3.140)	(2.714)	(1.768)
Poor County × Week 10	118.245**	8.921*	9.950**	12.735***
	(48.053)	(5.390)	(4.250)	(3.872)
Poor County × Week 11	187.175**	10.746	16.814***	22.497***
	(73.321)	(6.890)	(5.337)	(6.577)
Poor County × Week 12	168.389**	11.271**	20.976**	23.414***
	(73.998)	(4.918)	(8.635)	(5.623)
Poor County × Week 13	157.119***	10.788**	29.314**	22.568***
	(57.825)	(4.752)	(12.201)	(4.914)
Poor County × Week 14	124.689***	10.663**	29.502***	17.832***
	(47.078)	(4.627)	(9.501)	(3.769)
Observations	12,348	5,642	7,602	18,284
R^2	0.160	0.195	0.140	0.104
Note:	*p<0.1; **p<0.05; ***p<0.01			

The weekly number of new cases is the dependent variable. Additionally, the specifications control for the log of density, percent of blacks over 55, unemployment rate, and state fixed effects. The standard errors are clustered at the state level to account for within state correlation. This specification is represented by expression 1.The coefficients are plotted in Panel A of Figure 8 for brevity.

Table C.4: **Mobility Patterns by Poverty and Timing of Mandate**

	Dependent variable:			
	log(distance away from home, meters)			
	First Movers	Second Movers	Third Movers	Late Movers
Rich County×Week 2	−0.003	−0.006	−0.038	0.025
	(0.035)	(0.029)	(0.084)	(0.016)
Rich County×Week 3	0.020	−0.069	−0.043	0.047**
	(0.038)	(0.043)	(0.084)	(0.020)
Rich County×Week 4	0.115***	0.057	0.039	0.131***
	(0.040)	(0.036)	(0.062)	(0.018)
Rich County×Week 5	0.106***	0.034	0.033	0.132***
	(0.034)	(0.044)	(0.053)	(0.021)
Rich County×Week 6	0.077**	0.013	−0.050	0.091***
	(0.034)	(0.030)	(0.098)	(0.018)
Rich County×Week 7	0.079*	0.050**	−0.013	0.153***
	(0.045)	(0.023)	(0.083)	(0.032)
Rich County×Week 8	−0.001	−0.051**	−0.078	0.032
	(0.046)	(0.025)	(0.081)	(0.035)
Rich County×Week 9	−0.070	−0.122***	−0.200*	−0.150***
	(0.055)	(0.045)	(0.109)	(0.039)
Rich County×Week 10	−0.172***	−0.275***	−0.348**	−0.264***
	(0.054)	(0.054)	(0.148)	(0.042)
Rich County×Week 11	−0.393***	−0.459***	−0.486***	−0.393***
	(0.034)	(0.065)	(0.139)	(0.038)
Rich County×Week 12	−0.340***	−0.414***	−0.477***	−0.369***
	(0.038)	(0.058)	(0.123)	(0.038)
Rich County×Week 13	−0.287***	−0.348***	−0.408***	−0.302***
	(0.035)	(0.055)	(0.121)	(0.040)
Rich County×Week 14	−0.232***	−0.315***	−0.391***	−0.247***
	(0.038)	(0.067)	(0.149)	(0.047)
Mid-Level County×Week 2	−0.090***	−0.027	0.001	−0.025
	(0.018)	(0.040)	(0.040)	(0.019)
Mid-Level County×Week 3	−0.075***	−0.015	0.035	−0.009
	(0.021)	(0.054)	(0.051)	(0.020)
Mid-Level County×Week 4	0.028	0.062	0.049	0.060***
	(0.020)	(0.058)	(0.033)	(0.023)
Mid-Level County×Week 5	−0.001	0.029	0.007	0.043*
	(0.017)	(0.045)	(0.032)	(0.022)
Mid-Level County×Week 6	−0.008	0.011	0.038	0.029
	(0.022)	(0.041)	(0.038)	(0.023)
Mid-Level County×Week 7	0.049	0.053	0.115**	0.150***
	(0.036)	(0.062)	(0.057)	(0.047)
Mid-Level County×Week 8	−0.032	−0.058	0.053	0.055
	(0.043)	(0.045)	(0.047)	(0.046)
Mid-Level County*Week 9	−0.070	−0.133***	−0.109***	−0.104**
	(0.053)	(0.049)	(0.033)	(0.042)
Mid-Level County×Week 10	−0.178***	−0.266***	−0.187***	−0.194***
	(0.058)	(0.065)	(0.039)	(0.050)
Mid-Level County×Week 11	−0.372***	−0.428***	−0.386***	−0.378***
	(0.053)	(0.084)	(0.066)	(0.051)
Mid-Level County×Week 12	−0.347***	−0.419***	−0.342***	−0.345***
	(0.060)	(0.088)	(0.056)	(0.055)
Mid-Level County×Week 13	−0.287***	−0.359***	−0.244***	−0.268***
	(0.052)	(0.087)	(0.052)	(0.055)
Mid-Level County×Week 14	−0.227***	−0.343***	−0.185***	−0.217***
	(0.045)	(0.103)	(0.069)	(0.055)
Poor County×Week 2	−0.022	−0.143*	−0.065	−0.086***
	(0.023)	(0.085)	(0.055)	(0.015)
Poor County×Week 3	−0.023	−0.136	−0.071	−0.089***
	(0.022)	(0.098)	(0.055)	(0.013)
Poor County×Week 4	0.070***	−0.078	−0.009	−0.004
	(0.024)	(0.063)	(0.057)	(0.014)
Poor County×Week 5	0.062**	−0.117	−0.049	−0.038***
	(0.028)	(0.073)	(0.039)	(0.014)
Poor County×Week 6	0.051*	−0.110	−0.025	−0.038**
	(0.028)	(0.077)	(0.041)	(0.015)
Poor County×Week 7	0.148***	−0.062	0.128***	0.080**
	(0.040)	(0.067)	(0.031)	(0.032)
Poor County×Week 8	0.085	−0.086	0.141***	0.045*
	(0.052)	(0.064)	(0.053)	(0.025)
Poor County×Week 9	0.041	−0.192**	0.079	−0.101***
	(0.068)	(0.079)	(0.068)	(0.023)
Poor County×Week 10	−0.050	−0.310***	−0.009	−0.192***
	(0.073)	(0.104)	(0.079)	(0.031)
Poor County×Week 11	−0.268***	−0.476***	−0.199***	−0.361***
	(0.072)	(0.126)	(0.065)	(0.032)
Poor County×Week 12	−0.219***	−0.429***	−0.173***	−0.333***
	(0.073)	(0.115)	(0.058)	(0.030)
Poor County×Week 13	−0.165**	−0.351***	−0.086	−0.241***
	(0.069)	(0.094)	(0.056)	(0.029)
Poor County×Week 14	−0.135*	−0.302***	−0.034	−0.177***
	(0.077)	(0.113)	(0.046)	(0.030)
Observations	12,348	5,642	7,602	18,284
R^2	0.308	0.254	0.284	0.229
Note:	*p<0.1; **p<0.05; ***p<0.01			

The dependent variable used is the log of distance traveled (in meter). Additionally, the specifications control for the log of density, percent of blacks over 55, unemployment rate, and state fixed effects. The standard errors are clustered at the state level to account for within state correlation. This specification is represented by expression 1 where we use the log of distance traveled away from home (in meters) as the dependent variable. The coefficients from Column (1) are plotted in Panel B of Figure 8 for brevity.

Table C.5: **Effects of a Few Days of Delay in Weekly New Cases**

	Dependent variable:					
	Weekly New Cases					
	(1)	(2)	(3)	(4)	(5)	(6)
Week 2	−0.002	−0.002	−0.002	−0.002	−0.002	−0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Week 3	−0.002	−0.002	−0.002	−0.002	−0.002	−0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Week 4	−0.002	−0.002	−0.002	−0.002	−0.002	−0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Week 5	−0.002	−0.002	−0.002	−0.002	−0.002	−0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Week 6	0.007	0.007	0.007	0.007	0.007	0.007
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Week 7	0.114***	0.660	0.114***	0.114***	0.114***	0.629
	(0.026)	(0.440)	(0.026)	(0.026)	(0.026)	(0.430)
Week 8	0.807***	6.367	0.807***	0.807***	0.807***	6.048
	(0.155)	(4.231)	(0.155)	(0.155)	(0.155)	(4.152)
Week 9	4.866***	10.457**	6.546	−5.453***	4.866***	8.753
	(0.759)	(4.451)	(4.554)	(1.807)	(0.759)	(7.881)
Week 10	14.866***	20.457***	17.051**	1.573	14.420***	17.973*
	(2.147)	(5.411)	(6.628)	(1.751)	(2.129)	(9.620)
Week 11	25.468***	31.059***	27.653***	12.175***	21.243***	24.504***
	(3.529)	(6.909)	(7.272)	(2.024)	(3.819)	(8.740)
Week 12	27.899***	33.490***	30.084***	14.606***	23.674***	26.935***
	(4.586)	(7.794)	(7.720)	(2.557)	(5.056)	(8.970)
Week 13	33.948***	39.540***	36.133***	20.656***	29.724***	32.985***
	(5.921)	(8.701)	(8.678)	(3.862)	(6.556)	(9.488)
Week 14	36.519***	42.111***	38.704***	23.227***	32.295***	35.556***
	(5.821)	(8.882)	(8.619)	(3.881)	(6.164)	(9.857)
Week 2 × Second Mover	0.002	0.002	0.002	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Week 3 × Second Mover	0.002	0.002	0.002	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Week 4 × Second Mover	0.002	0.002	0.002	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Week 5 × Second Mover	0.002	0.002	0.002	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Week 6 × Second Mover	−0.007	−0.007	−0.007	−0.007	−0.007	−0.007
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Week 7 X Second Mover	−0.022	1.097	−0.022	−0.022	−0.022	1.033
	(0.038)	(0.818)	(0.038)	(0.038)	(0.038)	(0.804)
Week 8 X Second Mover	0.164	0.194	0.242	0.164	0.164	0.570*
	(0.258)	(0.251)	(0.298)	(0.258)	(0.258)	(0.329)
Week 9 X Second Mover	0.157	0.157	−0.007	−4.205**	0.157	−4.648**
	(1.235)	(1.235)	(1.471)	(1.860)	(1.235)	(2.300)
Week 10 × Second Mover	−2.727	−2.727	−3.150	−5.913*	−9.307*	−14.792**
	(3.139)	(3.139)	(3.769)	(3.540)	(5.143)	(7.400)
Week 11 × Second Mover	−10.056**	−10.056**	−10.479**	−13.242***	−12.857**	−18.051***
	(4.509)	(4.510)	(5.026)	(4.953)	(5.044)	(6.712)
Week 12 × Second Mover	−13.541***	−13.541***	−13.964**	−16.728***	−16.343***	−21.536***
	(5.227)	(5.227)	(5.597)	(5.739)	(5.614)	(7.058)
Week 13 × Second Mover	−14.412*	−14.412*	−14.835*	−17.599**	−17.214**	−22.407**
	(7.368)	(7.368)	(7.739)	(7.825)	(7.403)	(8.702)
Week 14 × Second Mover	−15.703**	−15.703**	−16.126**	−18.890**	−18.504**	−23.698***
	(7.123)	(7.124)	(7.506)	(7.578)	(7.335)	(8.710)
Emergency Declaration		X				X
N.E. Business			X			X
Restaurants				X		X
Gathering					X	X
Observations	13,244	13,244	13,244	13,244	13,244	13,244
R^2	0.060	0.060	0.060	0.064	0.062	0.066

Note: *p<0.1; **p<0.05; ***p<0.01

We control for the timing of a statewide emergency declaration (Column(2)), non-essential business restrictions (Column (3)), restaurant restrictions (Column (4)), and limits on large gatherings (Column (5)). Column (6) controls for the timing of all NPIs simultaneously. The sample only contains county level observations from Second (N=5,642) and Third (N=7,602) Mover states. The standard errors are clustered at the state level to account for within county correlation. The coefficients are plotted in Figure 9 for brevity.

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Table C.6: **Mobility, Mandate and Weather Shocks Using Shorter Window (First Stage)**

	Dependent variable:						
	log of Mobility						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Precipitation (inch)	0.065***	0.063***	0.062***	0.067***	0.066***	0.067***	0.067***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Average Temperature (degree F)	−0.0004	−0.0005	−0.001	0.00003	−0.0003	−0.0002	−0.00004
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Stay Home Mandate	0.059***	0.057***	0.061***	0.048***	0.052***	0.045***	0.046***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
High Precipitation X Stay Home Mandate	−0.060***	−0.061***	−0.061***	−0.061***	−0.062***	−0.064***	−0.061***
	(0.015)	(0.015)	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)
F-Stat	35.79	37.07	36.55	36.09	38.51	35.11	37.14
Emergency Declaration		X				X	X
N.E. Business			X			X	
Restaurants				X		X	X
Gathering					X	X	
Observations	27,828	27,828	27,828	27,828	27,828	27,828	27,828

Note: *p<0.1; **p<0.05; ***p<0.01

The sample is limited to months of March and April. We control for the timing of (*i*) a statewide emergency declaration (Column(2)), (*ii*) non-essential business restrictions (Column (3)), (*iii*) restaurant restrictions (Column (4)), and limits on large gatherings (Column (5)). Column (6) controls for the timing of all NPIs simultaneously. The standard errors are clustered at the state level to account for within county correlation.

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Table C.7: **IV Estimates Using Weather Shocks Using Shorter Window**

	Dependent variable:						
	log of Weekly New Cases						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log of mobility (IV)	4.827***	4.786***	4.834***	4.956***	5.285***	5.335***	4.904***
	(0.817)	(0.817)	(0.817)	(0.814)	(0.815)	(0.812)	(0.814)
Emergency Declaration		X				X	X
N.E. Business			X			X	
Restaurants				X		X	X
Gathering					X	X	
Observations	27,828	27,828	27,828	27,828	27,828	27,828	27,828

Note: *p<0.1; **p<0.05; ***p<0.01

All specifications control for the log of state-specific total number of testing (at the weekly level). The sample is limited to months of March and April. We control for the timing of (*i*) a statewide emergency declaration (Column(2)), (*ii*) non-essential business restrictions (Column (3)), (*iii*) restaurant restrictions (Column (4)), and limits on large gatherings (Column (5)). Column (6) controls for the timing of all NPIs simultaneously. The standard errors are clustered at the state level to account for within county correlation.

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Demand or supply? Price adjustment during the Covid-19 pandemic

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We study planned price changes in German firm-level survey data to infer the relative importance of supply and demand during the Covid-19 pandemic. Supply and demand forces coexist, but demand deficiencies dominate in the short run. Quarter-on-quarter producer price inflation is predicted to decline by as much as 1.5 percentage points through August 2020. These results imply a role for demand stimulus policy to buffer the Covid-19 economic crisis.

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1 Introduction

As the Covid-19 pandemic is disrupting economies across the globe, policymakers are in search for suitable stabilization policy measures. The scope and design of effective policy hinges on the channels through which the pandemic affects economic activity. On the one hand, policymakers need to consider measures that shield productive capacity going forward to weather disruptions for non-economic reasons in the supply of goods and services. Keynesian stimulus policy, on the other hand, addresses demand deficiencies arising, for example, from actual and expected income risk and higher economic uncertainty. The relative strength of the forces working on supply and demand during the Covid-19 crisis therefore is a key input to effective policy.

Prices reflect shifts in demand and supply. Given demand, a reduction in the supply of goods and services generates inflation. Holding production constant, deficient demand leads to disinflation. We build on this basic economic prediction and study planned price changes of German firms to forecast aggregate sectoral inflation in the short run. A challenge to this approach, and price measurement in general during the Covid-19 crisis, is that certain goods and services are temporarily not available or transferable. The focus of this paper is on *planned* price changes of continuing firms which, in our view, alleviates this concern. Moreover, using transactions data to adjust for changes in current expenditures patterns, Cavallo (2020) shows that official inflation figures are biased upward by only 0.09 percentage points relative to actual inflation in the German economy.

The main result of this paper is that forces working on supply and demand coexist, but demand deficiencies dominate in the short run. We predict aggregate sectoral inflation to decline up by as much as 1.5 percentage points through August 2020, reflecting a substantial drop in aggregate demand. This forecast does not incorporate the temporary reduction in the German value-added tax rate effective July 2020, which very likely will reduce inflation even further.

We reach this conclusion through the analysis of monthly producer price micro data for the German economy. Early into the crisis, the German producer price index decreased by 0.8% in March and by 1.9% in April year-on-year, suggesting a dominant role for demand-side forces. However, this naive conclusion rests on only two data points and is possibly confounded by the decline in economic activity that started even before the Covid-19 recession or the substantial drop in oil prices. We therefore turn to unique firm-level micro level survey data from the ifo-Business Climate Survey (ifo-BCS) and study planned price adjustments through August 2020 while controlling for other determinants of price-setting behavior. The ifo-BCS is a large survey of German firms in all relevant sectors of the economy that provides monthly information on the extensive margin of realized and three-month ahead planned price-setting decisions of firms. Planned price changes of firms in this survey are a strong predictor of quarter-on-quarter producer price inflation in the manufacturing and retail/wholesale industries. This feature makes the ifo-BCS highly suitable to predict inflation in the short run if historical correlations continue to hold.

In the April and May 2020 ifo-BCS survey questionnaires, firms assess their current business exposure to the Covid-19 pandemic. We first show that firms differentially exposed display very similar dynamics in planned price changes up to March 2020. In March 2020, most of the public health measures to fight the Covid-19 pandemic in Germany were implemented, e.g. nation-wide school closures on March 13 and a nation-wide curfew on March 22. Relative to firms with no or only weak exposure to Covid-19, we estimate a substantial rise of up to ten percentage points in the probability of planned price decreases for firms with strong negative exposure, and a concurrent decline in the probability of planned price increases. Conversely, positively exposed firms display an approximately seven percentage point higher chance of planned price increases and are less likely to plan price decreases. Since more than 70% of firms report negative effects due to Covid-19, the frequency of planned price decreases is predicted to increase up to about five percentage points. The frequency of planned price increases is predicted to decline, if anything. These findings suggest a dominant role for demand in price-setting behavior early into the crisis.

We investigate heterogeneity in planned price changes with respect to demand and supply forces underlying the estimated average effect using additional information available from the survey responses. We use the change in order books over time as a proxy variable for positive and negative shifts in demand. We also explore a number of specific regular and newly asked survey questions that ask firms about negative supply-side shifts, such as the lack of intermediate inputs. As cell sizes become small in this case, heterogeneity analysis lacks sufficient statistical power to draw strong conclusions. However, whenever we find significant effects, they suggest that positive demand shifts increase the probability of planned price increases and negative demand shifts increase the probability of planned price decreases. Disruptions in supply, in turn, dampen the estimated average effect and increase the chance of planned price increases. Our evidence is therefore consistent with the coexistence of forces working on both supply and demand in the Covid-19 recession. On average, however, demand deficiencies dominate such that the estimated average effect suggests a demand-driven recession.

In a final step, we assess the implications of the Covid-19 crisis on the aggregate sectoral price level in the short run. To this end, we project current and future producer price inflation on the frequency of planned price decreases and price increases. The estimated coefficients from this regression provide the effects of a unit increase in planned price adjustments today. We multiply these coefficients by the aggregated firm-level effects.

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Under the assumption that the correlation between planned price changes and inflation continues to hold, we then predict producer price inflation to decrease over time, falling as much as 1.5 percentage points through August 2020.

Our results provide support for a number of studies that highlight the importance of weak demand in the crisis. This includes the traditional channels of a reduction in overall demand due to higher actual and expected income (unemployment) risk and the corresponding increase in precautionary savings (compare, e.g. (Eichenbaum et al., 2020)). Other studies have suggested that demand and supply distortions of some sectors spill over to others. Guerrieri et al. (2020) argue that the supply reduction in some sectors will lead to an overall reduction in demand, also in not primarily affected sectors. This is the case especially if goods and services are no perfect substitutes in consumption. Caballero and Simsek (2020) also highlight potential demand deficiencies to originate in asset price spirals. Barrot et al. (2020) argue that distance to demand of different sectors will lead to differential effects of output and prices across goods, upstream sectors (further away from demand) being most adversely affected. Farhi and Baqaae (2020) support the basic economic intuition about inflationary supply shocks and deflationary demand shocks in a disaggregated New Keynesian economy.

We are not aware of another empirical study about producer price setting in the Covid-19 crisis. Cabral and Xu (2020) focus on price development of very few special goods such as face masks. Brinca et al. (2020) estimate sectoral labor supply and demand shocks for the US economy and find that the former outsize the latter. Investigating U.S. household expectations early in the Covid-19 crisis, Dietrich et al. (2020) document an increase in consumer price inflation expectations in March 2020. There also exist early contributions that empirically investigate spending during the crisis. As documented by Cavallo (2020), but also Baker et al. (2020) or Carvalho et al. (2020), the applied measures heavily distort the composition of spending. This mirrors the substantial heterogeneity in price setting documented in our study. Finally, our study is related to a growing number of contributions that study firm-level exposure to the Covid-19 crisis. Bartik et al. (2020), Hassan et al. (2020), and Buchheim et al. (2020a) are examples.

The remainder of this paper is organized as follows. Section 2 documents the data and provides summary statistics of the survey variables. Section 3 presents estimation results on the relationship between Covid-19 exposure and price setting expectations, also conditional on demand and supply shifts. Section 4 aggregates the estimated effects to aggregate sectoral producer price inflation. Section 5 concludes.

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2 Data

2.1 Planned Price Changes from the ifo Business Climate Survey

We use the ifo Business Climate Survey (ifo-BCS) which is a monthly firm survey among a large sample of German firms in all relevant sectors of the economy.[1] The survey contains mostly qualitative questions, including information about the extensive margin of planned and realized price changes. Specifically, we use a question on whether firms plan to increase, decrease, or leave unchanged their prices over the following three months as well as a similar question on price realizations in the preceding month.[2]

Overall, we build our analysis on a sample of on average 6,081 firms (2,175 in manufacturing, 2,101 in services, and 1,805 in retail/wholesale).[3] Figure 1 exhibits aggregate time series of realized and planned pricing decisions since 2012. Following the patterns of the overall PPI, realized and planned price increases have become less frequent during the first months of 2020 while price decreases were reported more frequently. This pattern strongly amplified after March when the German government implemented strong measures to prevent the spread of Covid-19. This is in line with Buchheim et al. (2020b) who—using the same survey as this paper—show that German firms were unexpectedly hit by the Covid-19 crisis when it reached their domestic market in March 2020.[4]

Planned price adjustments in the ifo-BCS very closely co-move with quantitative producer price changes observed in administrative data. Figure 2 documents this co-movement since 2007 for manufacturing, wholesale, and retail industries separately. Here, planned price changes of firms in the ifo-BCS are aggregated using their representative weights [5] in

[1] The ifo-BCS micro data provides the basis for the most recognized leading indicator of the German business cycle. See Sauer and Wohlrabe (2020) for details. According to a meta-study by Sauer and Wohlrabe (2019), questions are usually answered by senior management such as firm owners, members of the executive board, or department heads.

[2] The price realizations data of the ifo-BCS have been used in other recent studies: Bachmann et al. (2019) study the relation between uncertainty and price setting, Balleer et al. (2017) investigate the relationship between financial constraints and price setting, Link (2019) examines the effect of the 2015 introduction of a nation-wide minimum wage on price setting of firms and Balleer and Zorn (2019) study the response of producer prices to monetary policy shocks.

[3] We harmonize the data following Link (2020) which primarily involves the cleaning and assignment of industry codes of the official German WZ08 industry classification system. Moreover, and in contrast to the service and retail/wholesale sectors, the manufacturing survey is run at the product type level. During the time period used, the survey only covers the main product of each firm and the special questions related to the Covid-19 pandemic described below always refer to the firm as a whole.

[4] In the beginning of March, only a few German counties were strongly affected by Covid-19. In the subsequent weeks, infection rates increased exponentially resulting in nation-wide school closures on March 13 as well as the implementation of a nation-wide curfew on March 22. Buchheim et al. (2020b) document that firms' business outlook showed their strongest decrease only after March 13. As roughly three out of four respondents participated in the March wave of the ifo-BCS before this date, the April wave constitutes the first in which all respondents face the Covid-19 crisis.

[5] Weighting follows a two-step procedure (Sauer and Wohlrabe, 2020). In the first step, we aggregate at the industry level weighting by size based on employment and turnover in the manufacturing industry and

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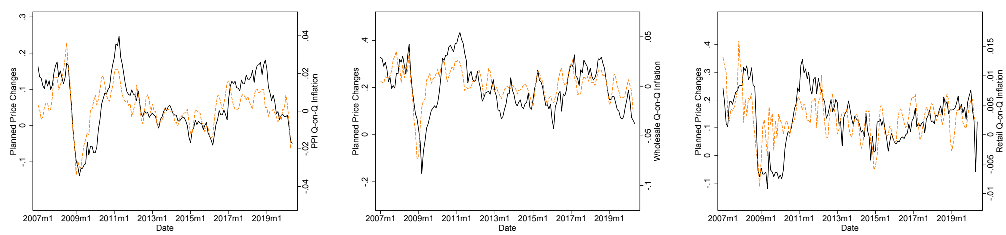
Figure 1 – Frequency of Realized and Planned Price Decreases and Increases over Time

Notes: The figures depict the frequency of realized and planned price decreases and increases as reported to the ifo-BCS. The time series cover the sample used in this study, i.e., manufacturing, retail/wholesale, and services industries, and are seasonally adjusted.

Figure 2 – Co-Movement of Planned Price Changes and Producer Price Inflation

(a) Manufacturing **(b)** Wholesale **(c)** Retail



Notes: The figure plots times series of the realized change in producer price indices from the German Federal Statistical Office (Destatis) relative to three months before (dashed orange line; right axis) against mean reported 3-months ahead planned price changes from the ifo-BCS weighted by firms' representative weights (solid black line; left axis) for the samples of (a) manufacturing, (b) wholesale, (c) retail (incl. car sellers) industries. Destatis does not provide a monthly producer price index covering the entire service sector which is hence not displayed here. All series are seasonally adjusted.

the survey and compared to the official producer price inflation from the German Federal Statistical Office (Destatis).[6] In manufacturing the contemporaneous correlation between these two series is 0.75. In wholesale, the corresponding correlation is 0.64 and in retail about 0.53.

2.2 Descriptive Evidence on the Covid-19 Crisis

In April and May 2020, the ifo-BCS asked firms to assess how strongly their business situation is affected by the Covid-19 crisis on a scale ranging from -3 to 3. Table 1 shows

all other sectors, respectively. In the second step, industries are aggregated using gross value added shares as weights.

[6]A respective comparison for the service sector is missing due to lack of an official monthly producer price index by Destatis.

Table 1 – Summary Statistics: Prices and Business Conditions

	Covid-19 Exposure							
	-3	-2	-1	0	1	2	3	Total
Planned Price Increase	.088	.076	.095	.107	.183	.21	.312	.102
	.284	.266	.293	.31	.387	.408	.464	.303
Planned Price Decrease	.22	.142	.092	.044	.042	.044	.05	.136
	.414	.349	.288	.205	.201	.206	.219	.342
Planned Price Change	.308	.219	.186	.151	.225	.254	.362	.238
	.462	.414	.39	.358	.418	.436	.482	.426
Price Increase	.041	.051	.057	.079	.169	.19	.321	.068
	.198	.219	.232	.27	.375	.393	.468	.252
Price Decrease	.154	.099	.057	.041	.045	.041	.062	.096
	.361	.298	.232	.199	.208	.2	.242	.295
Price Change	.195	.149	.114	.121	.214	.231	.383	.164
	.396	.356	.318	.326	.41	.422	.487	.37
Positive Business Conditions	.008	.039	.209	.524	.594	.792	.865	.188
	.091	.194	.406	.5	.492	.406	.343	.391
Negative Business Conditions	.908	.545	.138	.038	.029	.029	.059	.467
	.29	.498	.345	.19	.168	.167	.235	.499
Positive Business Expectations	.121	.114	.097	.09	.167	.285	.336	.122
	.326	.318	.296	.287	.373	.452	.474	.327
Negative Business Expectations	.743	.674	.542	.3	.252	.218	.229	.582
	.437	.469	.498	.459	.435	.414	.421	.493
Expected Revenue Change in %	-37.156	-22.922	-12.658	-6.232	-.248	3.956	13.365	-20.988
	21.79	13.522	9.88	9.046	9.56	15.255	40.19	21.385
Observations	3620	2592	2397	1252	552	316	223	10952
Percent	33.05	23.67	21.89	11.43	5.04	2.89	2.04	100

Notes: Numbers depict means and standard deviations (small) of variables by column group. Observations are grouped by the Covid-19 Exposure variable on a scale ranging from -3 ("strong negatively affected") to 3 ("strongly positively affected"). Observations in 2020:M04/M05 are used. Expected Revenue Change in % was asked in 2020:M04 and refers to overall revenue in 2020.

summary statistics for this measure of Covid-19 exposure in relation to price setting information and other survey responses explained below. The results show substantial heterogeneity in whether and how the crisis affects firms. In April and May 2020, 33% of firms exhibit a strong negative exposure to the Covid-19 crisis (-3), 24% assess their situation as negatively affected (-2), while 22% state to be only weakly negatively affected (-1). 11% of firms assess no particular exposure to Covid-19. 5%, 3%, and 2% state that they are positively affected with increasing exposure of 1, 2, and 3, respectively.

In every month, the ifo-BCS elicits firms' current business situation and expectations for the next six months on a trichotomous [-1/0/1]-scale. As Table 1 shows, firms that state that they are positively affected by the Covid-19 crisis mostly assess their business situation and outlook positively and vice versa. In addition, the April survey asked firms about the expected impact of the Covid-19 crisis on their revenues in 2020 measured as a percentage increase or decrease. Clearly, firms expect a higher loss in revenue when more

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adversely affected by Covid-19. However, it is important to note that the overlap between Covid-19 exposure and current business situation is not perfect. Quite a few firms with no or positive Covid-19 exposure contemporaneously report a negative business situation and expectation, and also a drop in expected revenues. In contrast, some firms with no or negative Covid-19 exposure state a positive business situation and expectation. Hence, Covid-19 exposure captures information over and above the normally assessed business situation that is specific to the situation during the pandemic.

Tables A.3 through A.2 in the Appendix document the exposure to the Covid-19 crisis across industries, separately by manufacturing, retail/wholesale, and services. Overall, firms in services are more adversely affected than retail/wholesale firms. Services depict close to no positive exposure with some sectors in which (close to) the entity of firms are strongly negatively affected. Not surprisingly, these include business related to travel, the hospitality sector, and entertainment industries. Retail firms are on average more adversely affected than firms in wholesale, but exhibit a 20% share that has positive exposure which mostly corresponds to supermarkets. Moreover, manufacturing firms are less severely affected on average. Among these, firms that manufacture leather goods, drinks, and that offer repair and installation of machines are most adversely affected. Most positively affected are firms that manufacture food, rubber and plastic goods, pharmaceutical as well as paper and cardboard goods.

Table 1 documents the relationship between Covid-19 exposure and pricing decisions reported in the April and May waves of the ifo-BCS. About 16.4% of firms changed their prices in March and April, 9.6% of which decreased and 6.8% of which increased their prices. Firms strongly affected by Covid-19 change their prices more often than mildly affected firms. The frequency of price increases rises in Covid-19 exposure, while the frequency of price decreases falls. Looking forward, about 23.8% of firms planned to change their prices in the subsequent three months, 10.2% of which planned to increase and 13.6% of which planned to decrease their prices. Hence, as already shown in Figure 1, firms overall tend to decrease prices more often in the first months of the Covid-19 crisis and also plan prices to decrease more strongly moving forward. Underneath these general trends, positively affected firms tend to increase their prices, while negatively affected firms tend to decrease their prices. Planned price changes are similarly distributed as realized price changes. Tables A.4 to A.6 shows the equivalent of Table 1 separately for manufacturing, wholesale/retail and services. While firms in manufacturing and services change and expect to change prices less often and firms in retail/wholesale change and expect to change prices more often than the overall average, the overall price setting patterns are reflected across sectors.

In the following, we group firms by Covid-19 exposure in April 2020 into four categories: Very strongly and strongly negatively affected firms with a Covid-19 exposure of

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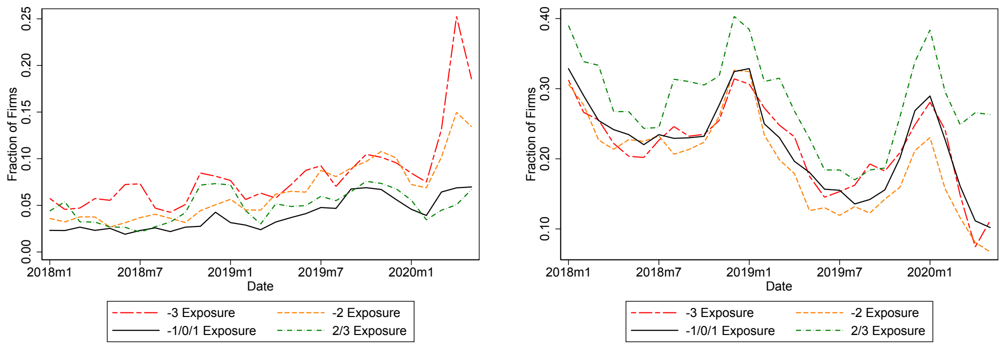
Figure 3 – Planned Price Decreases and Increases over Time by Covid-19 Exposure

(a) Planned Price Decreases **(b)** Planned Price Increases



Notes: Frequency of planned price decreases (left) and price increases (right) reported to the ifo-BCS. Firms are grouped by the degree to which their businesses are affected by the Covid-19 crisis reported to the survey in 2020:M04 on a scale ranging from -3 ("strongly negatively affected") to 3 ("strongly positively affected").

-3 and -2, respectively, strongly positively affected firms (2 or 3) and a baseline category of no or only weakly affected firms with exposure between -1 and 1. Figure 3 plots the fraction of firms with planned price decreases and increases from January 2018 to May 2020 for these groups. Generally, price decreases have become more frequent since 2018, while price increases have fallen. Over time, pricing decisions across Covid-19 exposure categories followed similar trends albeit slight differences in the levels of the variables. Once the Covid-19 crisis hit Germany in March 2020, a clear increase in the dispersion of price setting decisions is visible across categories leading to the differences in planned price changes outlined in Table 1.

Importantly, there is a large degree of heterogeneity in price setting behavior of firms underneath the average developments: A substantial share of positively affected firms decreases prices and plans to decrease prices further, and vice versa. This speaks in favor of the existence of differential changes in demand and supply that are heterogeneous across firms. The ifo-BCS allows to take a detailed look into these underlying mechanisms of price setting by exploring questions related to supply and demand which are mostly specific to the subsets of manufacturing, retail/wholesale, and services firms.

The differential impact of the Covid-19 crisis on firms businesses is related to several measures capturing the supply side of firms as documented in Table 2. Details about the underlying survey question and availability of these variables can be found in the notes to the Table. First, strongly negatively affected firms in manufacturing use only about 54% of their production potential in April and capacity utilization has strongly decreased

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relative to twelve months before for all negatively affected firms.[7] This reduction in potential output is likely to be caused by supply-side restrictions. As depicted in Appendix Tables A.7 through A.9, this pattern is also prevalent in additional measures of adverse supply developments such as distorted supply chains and plant closures.[8] Second, firms in manufacturing and retail/wholesale were asked in April 2020 whether they were experiencing delivery problems of intermediate products and goods. While a substantial share of firms is affected by supply bottlenecks across all groups of Covid-19 exposure, these restrictions are more frequent the stronger the exposure and even more frequent to strongly positively (63%) compared to strongly negatively affected firms (45%).

Reduced productive activity is also strongly associated with measures indicating a fall in demand. To investigate this, we use information provided by firms about whether their orders have increased, decreased, or remained the same compared to the previous month. A reduction in orders may affect output in different ways in manufacturing and retail/wholesale. In the first case, it may severely constrain the production of final goods. In the second case, it may reduce output less if not all goods on offer in a retail/wholesale firm are affected. We therefore report orders separately for the different subsets of firms in Table 2. The general patterns are very similar, however. Between 84 and 93% of the firms strongly negatively exposed to Covid-19 experience a reduction in orders, while only between 2 and 3% of those experience an increase in orders. While only a small share of firms that are strongly positively affected by the Covid-19 crisis report a decrease in orders, more than half of those report an increase in orders. These patterns are supported by negatively affected firms that report a reduction in demand as one reason for their adverse exposure, see Tables A.7 through A.9 in the Appendix. Hence, while negatively affected firms on average report a reduction in demand and positively affected firms on average report an increase, the summary statistics convey substantial heterogeneity in order developments within each group of Covid-19 exposure.

[7] Manufacturing firms regularly report their quantitative capacity utilization in the first month of each quarter. Hence, data are only available for April 2020.

[8] Unsurprisingly, negative exposure to the Covid-19 crisis is associated with a higher frequency of plant closures. In addition, firms were asked for reasons of their exposure to the Covid-19 crisis in March 2020. On average, 34% of firms reported disruptions in their supply chain with respect to intermediate products and goods which was increasing in the degree of negative exposure to Covid-19 reported in the subsequent month. Disruptions in the delivery or sales of final goods were also more relevant for negatively exposed firms. In turn, we do not find a clear relationship between the Covid-19 exposure and firms general dependency on imported intermediates elicited in April 2020 as well as the probability that their production was constrained by a lack of material. Details about the underlying survey question and availability of these variables can again be found in the notes to the Tables.

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Table 2 – Summary Statistics: Suppy and Demand Indicators by Industry

	Covid-19 Exposure							
	-3	-2	-1	0	1	2	3	Total
				Manufacturing				
Capacity Utilization (in %)	54.432	70.4	79.662	85	87.901	81.333	84.5	71.439
	20.279	18.1	15.404	13.676	12.244	21.674	19.527	20.864
Cap. Util. (Y-Y in PP)	-27.414	-13.911	-4.776	-.222	4.538	.132	11.176	-11.954
	21.945	16.839	14.354	13.885	10.335	18.177	11.254	20.222
Supply Bottleneck	.451	.456	.418	.297	.365	.485	.625	.426
	.498	.499	.494	.458	.485	.508	.5	.495
Less Orders	.844	.668	.318	.09	.051	.123	.049	.535
	.363	.471	.466	.287	.221	.331	.218	.499
More Orders	.027	.035	.102	.244	.325	.494	.683	.099
	.163	.183	.303	.43	.47	.503	.471	.299
				Retail/Wholesale				
Supply Bottleneck	.497	.548	.533	.368	.566	.61	.661	.523
	.5	.498	.5	.485	.498	.491	.477	.5
Less Orders	.851	.626	.386	.174	.134	.093	.141	.533
	.356	.484	.487	.379	.341	.292	.349	.499
More Orders	.024	.045	.105	.151	.267	.396	.592	.119
	.154	.208	.307	.359	.443	.49	.493	.323
				Services				
Less Orders	.929	.624	.179	.054	.021	.039	.314	.503
	.257	.485	.383	.227	.144	.196	.471	.5
More Orders	.015	.042	.179	.365	.448	.647	.543	.142
	.122	.201	.383	.482	.499	.483	.505	.349

Notes: Numbers depict means and standard deviations (small) of variables by column group. Observations are grouped by the Covid-19 Exposure variable on a scale ranging from -3 ("strong negatively affected") to 3 ("strongly positively affected"). Observations in 2020:M04/M05 are used. Capacity Utilization (in %), Cap. Util. (Y-Y in PP), and Supply Bottleneck are only available in 2020:M04. The variables are only asked in the respective subsamples as displayed in the table. The order variables (Less/More Orders) refers to the question whether orders of firms are currently relatively large, normal, or small (in the Retail/Wholesale survey this question refers to the order situation vs. last year). The Supply Bottleneck variable refers to the question whether firms have supply problems regarding important raw materials/goods from Germany and abroad. For manufacturing firms, this question is only asked in the online survey (more than 75% of firms used the online survey).

3 Empirical Analysis

3.1 Average Effects of Covid-19 Exposure on Planned Price Adjustment

In a first step, we explore differences in planned price changes across Covid-19 exposure categories, separately for each month-year t between 2018:M01 and 2020:M05 based on the following regression:

$$\begin{aligned} Y_{i,t} = \; & \delta_{-3}\mathbb{1}\left(Covid_{i,04/20} = -3\right) + \delta_{-2}\mathbb{1}\left(Covid_{i,04/20} = -2\right) + \delta_{\{2,3\}}\mathbb{1}\left(Covid_{i,04/20} = 2 \vee 3\right) \\ & + c + \alpha_s + X'_{i,t-3}\beta + Taylor_1 + \ldots + Taylor_{12} + u_{i,t} \end{aligned} \tag{1}$$

Here, $Y_{i,t}$ refers to an indicator for planned price increases or decreases over the following three months for firm i. In addition to dummy variables for each Covid-19 exposure category as of 2020:M04, we include two-digit WZ08 industry fixed effects (α_s), separate indicators for positive and negative responses to the questions about business situation, business expectations, and orders, lagged by three months, to control for past economic activity of firms (collected in $X_{i,t-3}$), a constant (c), and dummy variables to control for Taylor pricing, i.e., price changes that occur in fixed time intervals (e.g. every six months, see Lein (2010) and Bachmann et al. (2019)).

Figure 4 shows the time series of the frequency of planned price increases and decreases for each Covid-19 exposure category, net of controls.[9] In every month, the difference between each line relative to firms with weak or no exposure corresponds to the estimated coefficient δ_i, with $i = -3, -2, \{2,3\}$, from Equation (1).[10] The frequency-weighted average of all lines in a given month equals the month's sample average.

Overall, the patterns in Figure 3 remain to hold after controlling for other determinants of firms' planned price changes. The left panel shows that the frequency of planned price decreases displays essentially identical dynamics across exposure categories prior to 2020:M03, indicated by the vertical red line, when measures to prevent the spread of Covid-19 were installed (see Footnote 4). Figure 4 suggests that these patterns would have continued if it were not for the Covid-19 pandemic. However, we observe a marked change in the frequency of planned price decreases in 2020:M04. The frequency of planned price decreases skyrockets for very strongly negatively exposed firms, rapidly rises for firms with strong negative exposure, and remains at similar levels for positively exposed firms, suggesting a strong deflationary effect of Covid-19 exposure. Similarly, the right panel of Figure 4 shows that the frequency of planned price increases displays similar

[9] See Yagan (2015) for a similar approach in a different context.

[10] Figure A.1 in the Appendix shows the estimated coefficients together with corresponding 95%-confidence intervals based on standard errors clustered at the industry level. Level differences between Covid-19 exposure categories are not statistically different relative to the baseline category in most months.

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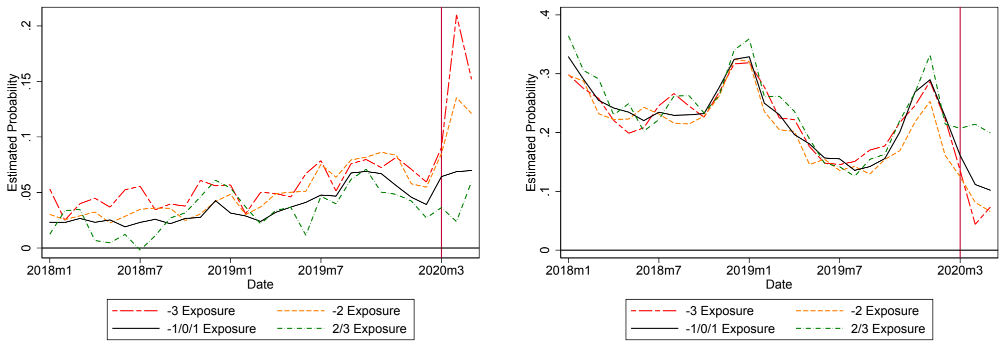
Figure 4 – Effects of Covid-19 Exposure on Planned Price Adjustment

(a) Planned Price Decreases **(b)** Planned Price Increases



Notes: This figure shows the time series of the frequency of planned price decreases (left) and price increases (right) for each Covid-19 exposure category as of 2020:M04, net of controls. In every month, the difference between each line relative to firms with weak or no exposure corresponds to the estimated coefficient δ_i, $i = -3, -2, \{2,3\}$ from Equation (1). The frequency-weighted average of all lines in a given month equals the month's sample average.

dynamics across exposure categories prior to 2020:M03. Interestingly, there is no spike comparable to the frequency of planned price decreases that would suggest upward price pressure during the Covid-19 pandemic. The frequency of planned price increase remains at similar levels for firms with positive exposure and falls for those with (very) strong negative exposure.

Next, we exploit the panel dimension of the ifo-BCS and the timing of events to account for level differences, seasonality (the frequency of price increases is highest at the beginning of each year), and business cycle movements (slight upward and downward trends in planned price decreases and increases, respectively, consistent with German economy cooling during this period) observed in Figure 4. We estimate the following regression on the sample 2018:M01 to 2020:M05:

$$\begin{aligned} Y_{i,t} =& \delta_{-3}\mathbb{1}\left(Covid_{i,t} = -3\right) + \delta_{-2}\mathbb{1}\left(Covid_{i,t} = -2\right) + \delta_{\{2,3\}}\mathbb{1}\left(Covid_{i,t} = 2 \vee 3\right) \\ &+ c_i + \alpha_s + X'_{i,t-3}\beta + Taylor_1 + \ldots + Taylor_{12} + \gamma_t + u_{i,t} \end{aligned} \tag{2}$$

We estimate Equation (2) for the each outcome $Y_{i,t}$. We set the Covid-19 exposure category $Covid_{i,t}$ to zero for all observations prior to 2020:M04. Building on Equation (1), we add firm fixed effects c_i, which also absorb level differences in the probability to plan price adjustments across exposure categories, and month-year fixed effects γ_t. All other variables are as before.

Table 3 shows results. Columns 1, 4 and 7 contain estimation results when only the Covid-19 exposure category indicators are included in the regression. Columns 2, 5 and

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8 show results based on Equation (2) without firm fixed effects. Columns 3, 6 and 9 add firm fixed effects to the specification.[11]

Table 3 – Effects of the Covid-19 Pandemic on Planned Price Adjustment

	Planned Price Decrease			Planned Price Increase			Planned Price Change		
Covid-19 Exposure	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Neg.$^{very\ strong}$	0.15***	0.11***	0.099***	-0.11***	-0.026***	-0.018**	0.044***	0.084***	0.081***
	(0.0075)	(0.0090)	(0.0084)	(0.0056)	(0.0083)	(0.0088)	(0.0086)	(0.011)	(0.011)
Neg.^{strong}	0.075***	0.049***	0.042***	-0.12***	-0.018**	-0.0031	-0.045***	0.031***	0.039***
	(0.0073)	(0.0087)	(0.0081)	(0.0058)	(0.0082)	(0.0085)	(0.0089)	(0.011)	(0.011)
Positive	-0.020**	-0.038***	-0.024**	0.055***	0.082***	0.066***	0.034	0.044**	0.042*
	(0.0093)	(0.011)	(0.011)	(0.020)	(0.021)	(0.022)	(0.021)	(0.021)	(0.022)
Observations	89018	75315	75118	89018	75315	75118	89018	75315	75118
Time + Ind. FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Firm FE	No	No	Yes	No	No	Yes	No	No	Yes

Notes: Linear Probability Model estimates, standard errors (clustered at firm level) in parentheses. Neg.$^{very\ strong}$, Neg.^{strong} and Positive are binary indicators refering to values -3, -2, {2,3} relative to {-1,0,1} of Covid-19 Exposure variable (scale: -3 to 3). Ind. FE refers to two-digit WZ08 industry fixed effects. Controls include separate indicators for positive and negative responses to questions about business situation, business expectations, orders (all lagged by three months) and Taylor dummies. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Column 2 shows that the probability of planned price decrease spikes by eleven percentage points for firms very strongly negatively affected by Covid-19, relative to the baseline category of weak or no exposure. For firms strongly negatively affected, the probability of planned price decreases rises about five percentage points relative to the only weakly affected firms. By contrast, firms with positive Covid-19 exposure experience a decline in the chance of planned price decrease by about four percentage points. These estimates are all significant at least at the 5% significance level. They are also economically large compared to the unconditional frequency of planned price decreases of 5.2 percent with standard deviation equal 22.3 percentage points in the period 2018:M01–2019:M12. Column 3 shows that their magnitude decreases slightly after controlling for firm fixed effects.

Columns 5 and 6 tabulate the corresponding effects on the probability of planned price increases. Firms very strongly negatively affected by Covid-19 display an approximately three percentage points lower chance of planned price increases. In the case of strongly negatively exposed firms there are no significant effects. By contrast, firms that report a positive effect of Covid-19 on their business situation show a significant eight percentage points increase in the probability of planned price increase, relative to a pre-2020 frequency of 22.6 percent with standard deviation equal 41.8 percentage points.

Columns 8 and 9 of Table 3 show the net effect of planned price increases and de-

[11] The number of observations drops slightly in these specifications due to firms only observed for a single month.

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creases on planned price changes. For firms with very strong negative exposure, planned price change increases by about eight percentage points, reflecting the fact that the rise in the probability of planned price decreases outweighs the decline in the probability of planned price increases. The same is true for firms strongly negatively affected by Covid-19, which increase the chance of planned price change by about four percentage points. The probability of planned price change for firms with positive exposure rises by about four percentage points, although in this case reflecting a stronger increase in the probability of planned price increase relative to the probability of planned price decrease. Again, these estimates are economically sizable compared to a pre-2020 probability to change prices of 27.9 percent with standard deviation equal 44.8 percentage points.

Table A.10 in the Appendix reports estimates from estimating Equation (2) separately for the manufacturing, retail/wholesale, and services industries. The results show that negative Covid-19 exposure leads to higher probability of planned price decrease across all sectors, with somewhat weaker effects in manufacturing, possibly reflecting the presence of long-term contracts between buyers and suppliers. Positive Covid-19 exposure does not display any significant differences in planned price adjustment in services, presumably because there only very few observations as Section 2 discussed. Positively affected firms plan fewer price decreases in the manufacturing industry while they plan to increase their prices in the retail/wholesale sector.

The sharp decline in oil prices during the Covid-19 crisis might lead to significantly lower producer prices. The results of Table 3 are potentially driven by this channel. We address this concern by including time fixed effects for each two-digit WZ08 industry. Table A.11 in the Appendix presents results. Columns 3, 6, and 9 show that the regression coefficients are not significantly different from our baseline specifications. Hence, our results are robust to the drop in oil prices.

We interpret our results as demand effects dominating the supply shock, consistent with the arguments in Guerrieri et al. (2020), Caballero and Simsek (2020) and others. The shift in price-setting behavior towards more planned price decreases by firms negatively affected by Covid-19 is consistent with the notion of negative demand effects. Conversely, the shift towards more planned price increases in firms with positive exposure suggest positive demand effects. These results suggest that the Covid-19 recession is a demand-driven recession. Importantly, this conclusion does not contradict the narrative that supply-side forces, e.g., a drop in labor supply or supply chain disruptions, forced the economy into recession. It is worth to recall that these estimates reflect average behavior, and thus do not preclude the presence of supply-side effects. On the one hand, the economic shield policies might have dampened the negative effect of supply-side disruptions. On the other hand, shocks to the supply side can propagate to demand deficiencies that potentially exceed the effects of the original shock in magnitude. This coexistence of

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supply and demand forces pulls the price level in opposite directions. Disentangling the effects of demand and supply to show their coexistence and price-setting implications is a challenge that we take up in the next.

3.2 Heterogeneous Effects: Disentangling Demand and Supply

The analysis in Section 2 shows that the Covid-19 pandemic and its propagation through the economy has sharply differential impacts on firms. While some firms display signs of demand deficiencies witnessed by weaker order books, another smaller set of firms experiences an increase in demand reflected by higher orders. Others seem to suffer from supply-side constraints indicated by the lack of intermediate inputs due to supply chain disruptions, capacity or production constraints and other specific questions in the survey. In this section, we use this additional survey information as proxy variables for supply and demand forces to investigate their implications for price-setting behavior.

Let $SDshift_{i,t}$ denote an indicator for a proxy variable for a given force. We extend Equation (2) as follows:

$$
\begin{aligned}
Y_{i,t} = & \eta_{-3,0}\mathbb{1}\left(Covid_{i,t} = -3\right) + \eta_{-3,1}\mathbb{1}\left(Covid_{i,t} = -3 \wedge SDshift_{i,t} = 1\right) \\
& + \eta_{-2,0}\mathbb{1}\left(Covid_{i,t} = -2\right) + \eta_{-2,1}\mathbb{1}\left(Covid_{i,t} = -2 \wedge SDshift_{i,t} = 1\right) \\
& + \eta_{\{2,3\},0}\mathbb{1}\left(Covid_{i,t} = 2 \vee 3\right) + \eta_{\{2,3\},1}\mathbb{1}\left(\left(Covid_{i,t} = 2 \vee 3\right) \wedge SDshift_{i,t} = 1\right) \\
& + c_i + \alpha_s + X'_{i,t-3}\beta + Taylor_1 + \ldots + Taylor_{12} + \gamma_t + u_{i,t}
\end{aligned} \tag{3}
$$

Here, all coefficients refer to the group of weakly exposed firms as the reference group and can, hence, be compared to the results of Equation (2). We estimate Equation (3) using additional information from the ifo-BCS to capture supply and demand-side forces, one at a time. Since the specific questions we use slightly differ across survey questionnaires for each sector, and to allow (coarsely) for heterogeneity across industries, we provide separate results for the manufacturing, retail/wholesale, and services industries in the following. Regarding positive and negative demand shifts, we use information about order books as discussed in Section 2 and summarized in Table 2. This variable is observed in every month and all survey questionnaires for each sector. Regarding negative supply shifts, we explore a number of different questions that are measured in either March, April or May 2020 and that we employ as time-constant indicators. Table 4 employs information about lacking intermediate inputs caused by supply chain disruptions which was measured in April 2020 and which is available in both manufacturing and wholesale/retail industries.

Table 4 reports the estimates. Since our interactions consider narrowly defined groups and we look at within firm-variation in these groups, our results lack sufficient statistical

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power in many of these subgroups. Our results should therefore not be understood as reflecting a decomposition of the estimated effects in Table 3. Instead, they are indicative of the presence and direction of demand and/or supply effects in planned price changes. In the following, we will therefore discuss those subgroups that exhibit significant results.

Let us start with negative shifts in supply due to lack of intermediate inputs. In general, negative supply shifts dampen to some extent the pricing patterns documented in Table 3. Overall, the estimated effects are in line with basic economic theory in which a reduction in supply asserts inflationary pressure all other things equal. The supply effects are in no case large enough to offset the overall patterns, however. Column 4 in Table 4 shows that firms with negative Covid-19 exposure which do not experience negative supply shifts are about 2 percentage points less likely to increase prices compared to weakly exposed firms. For firms with the same exposure that do experience negative supply shifts this probability increases by about 3 percentage points. A similar pattern is visible in wholesale/retail industries. Here, very strongly negatively affected firms are approximately six percentage points more likely to schedule price increases than comparable firms without negative supply shifts. As column 1 shows, these firms are also about eight percentage points less likely to plan price decreases.

These results are confirmed based on the various different measures described in Section 2. Table A.12 in the Appendix uses a similar but separately measured question about supply side disruptions in March 2020. Here, negative supply shifts also offset the overall pricing patterns of positively affected firms in wholesale/retail industries. Significant effects in Table A.13 in the Appendix reports the same direction when firms assess their production possibilities to be currently constrained in all sectors. Table A.14 interacts Covid-19 exposure with capacity utilization in manufacturing and exhibits that strongly negatively exposed firms with lower capacity utilization are expected to decrease prices less often. We further consider whether imports play a particular role for pricing (see Table A.15). They generally do not interact significantly with how Covid-19 exposure affects prices with one exception: Strongly negatively affected firms in retail/wholesale which depend on imported intermediate goods from China expect to decrease prices significantly more often.

Next, let us consider the effect of demand shifts. A reduction in orders suggests a negative shift in demand, while an increase in orders suggests a positive shift in demand. Generally, a reduction in orders is expected to positively affect planned price decreases and negatively affect planned price increases and vice versa. Our results show that this is the case, primarily in the retail/wholesale sector. Here, very strongly and strongly negatively exposed firms are 19 and about 8 percentage points more likely to plan price decreases and very strongly negatively affected firms are 12 percentage points less likely to plan price increases. This is confirmed using an alternative question about negative demand shifts in

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Table 4 – Effects of the Covid-19 Pandemic on Planned Price Adjustment: Supply and Demand

	Planned Price Decrease			Planned Price Increase			Planned Price Change		
Covid-19 Exposure	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Manufacturing					
Neg.^{vs}	0.081***	0.048*	0.087***	-0.0018	-0.0080	-0.013	0.079***	0.040	0.073***
Neg.^{s}	0.016	0.060***	0.056***	-0.024*	-0.011	-0.011	-0.0072	0.049**	0.045***
Pos.	-0.082**	-0.055**	-0.044	0.070	0.055	0.048	-0.012	-0.00014	0.0040
Neg.^{vs} $\times$ Supply^{-}	-0.018			-0.017			-0.035		
Neg.^{s} $\times$ Supply^{-}	0.021			0.030*			0.051*		
Pos. $\times$ Supply^{-}	0.030			-0.024			0.0064		
Neg.^{vs} $\times$ Orders^{-}		0.045			-0.0047			0.041	
Neg.^{s} $\times$ Orders^{-}		-0.0098			0.00072			-0.0091	
Pos. $\times$ Orders^{-}		0.0026			0.24*			0.24*	
Neg.^{vs} $\times$ Orders^{+}			-0.033			0.060			0.027
Neg.^{s} $\times$ Orders^{+}			-0.056*			0.0099			-0.046
Pos. $\times$ Orders^{+}			-0.020			0.053			0.032
Observations	19186	26591	26591	19186	26591	26591	19186	26591	26591
				Retail/Wholesale					
Neg.^{vs}	0.19***	-0.016	0.15***	-0.086***	0.032	-0.067***	0.10***	0.016	0.085***
Neg.^{s}	0.070***	0.0038	0.058***	-0.037	0.010	-0.022	0.032	0.014	0.036
Pos.	-0.026	-0.048***	-0.023	0.039	0.088***	0.0059	0.013	0.040	-0.018
Neg.^{vs} $\times$ Supply^{-}	-0.084**			0.063**			-0.021		
Neg.^{s} $\times$ Supply^{-}	-0.027			0.034			0.0071		
Pos. $\times$ Supply^{-}	-0.018			0.078			0.059		
Neg.^{vs} $\times$ Orders^{-}		0.19***			-0.12***			0.080*	
Neg.^{s} $\times$ Orders^{-}		0.084***			-0.048			0.036	
Pos. $\times$ Orders^{-}		0.12*			-0.028			0.095	
Neg.^{vs} $\times$ Orders^{+}			-0.22***			0.13			-0.091
Neg.^{s} $\times$ Orders^{+}			-0.049			0.053			0.0041
Pos. $\times$ Orders^{+}			-0.022			0.18***			0.16***
Observations	20585	22077	22077	20585	22077	22077	20585	22077	22077
				Services					
Neg.^{vs}		0.093**	0.11***		-0.023	-0.017		0.070	0.090***
Neg.^{s}		0.043*	0.066***		-0.030*	-0.032**		0.013	0.034*
Pos.		-0.030*	0.0078		-0.014	0.071		-0.044	0.079
Neg.^{vs} $\times$ Orders^{-}		0.014			0.0073			0.022	
Neg.^{s} $\times$ Orders^{-}		0.030			-0.0037			0.026	
Pos. $\times$ Orders^{-}		0.13			0.18			0.30***	
Neg.^{vs} $\times$ Orders^{+}			-0.048			0.040			-0.0080
Neg.^{s} $\times$ Orders^{+}			-0.11***			-0.0033			-0.11**
Pos. $\times$ Orders^{+}			-0.033			-0.100			-0.13*
Observations		25952	25952		25952	25952		25952	25952

Notes: Linear Probability Model estimates, standard errors (clustered at firm level) not shown, available upon request from authors. Neg.$^{very\ strong}$, Neg.^{strong} and Positive are binary indicators refering to values -3, -2, {2,3} relative to {-1,0,1} of Covid-19 Exposure variable (scale: -3 to 3). All specifications include time and two-digit WZ08 industry fixed effects as well as controls. Controls include separate indicators for positive and negative responses to questions about business situation, business expectations, orders (all lagged by three months) and Taylor dummies. Supply^{-} (Supply Bottleneck) is only observed in 2020:M04 and imputed to 2020:M05. For manufacturing firms, this question is only asked in the online survey (more than 75% of firms used the online survey). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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April 2020, in this case also significant for services (compare Table A.16 in the Appendix). While a reduction in orders intensifies the overall patterns in Table 3 for negatively affected firms, it offsets the patterns for positively affected firms. While positively affected firms are less likely to decrease prices in general, they are more likely to do so when experiencing reductions in demand (compare columns 2 for wholesale/retail industries). A single counterexample to the basic economic intuition outlined above are positively affected firms which experience negative demand shifts in manufacturing. These firms are more likely to plan price increases. Behind their positive exposure might be demand effects other than orders or optimistic demand expectations that drive these results.

An increase in orders offsets the overall pricing patterns. Negatively exposed firms are more likely to plan price increases and less likely to plan price decreases compared to weakly affected firms. When negatively exposed firms experience a positive demand shift, the probability to plan price decreases falls and the probability to plan price increases rises. These coefficients are significant for strongly negatively affected firms in manufacturing, very strongly negatively and positively affected firms in wholesale/retail industries and strongly negatively affected firms in services. An increase in orders intensifies the response of positively exposed firms. In wholesale/retail, these firms are 18 percentage points more likely to plan price increases than comparable firms without an increase in orders.

It has been argued that the unavailability of goods biases measures of inflation during the Covid-19 pandemic. We observe planned price changes for firms that partly or fully close. Table A.17 shows that the overall pricing patterns from Table 3 intensify when firms experience closures. Strongly negatively affected firms in manufacturing that experience closures are eleven percentage points more likely to decrease prices than comparable firms that do not experience closures. Similar and significant effects are exhibited for very strongly negatively affected firms in wholesale/retail. Positively exposed firms that experience closures in services are, in turn, less likely to increase prices. Closures therefore induce deflationary rather than inflationary pressure in line with the arguments and evidence in Cavallo (2020).

Our results generally support the basic economic mechanism of the effect of relative supply and demand on price setting. Our results also document substantial heterogeneity in these relative effects across sectors in our sample. Overall, our results support the initial interpretation that the price developments in response to the Covid-19 crisis can be understood as primarily demand-driven. One the one hand, our estimates document deflationary pressure generated by the strong increase in price decreases in response to the Covid-19 pandemic. We show that this is mainly driven by strongly negatively affected firms which experience a decline in demand. This suggests that the main reason behind the negative exposure in fact is a decline in demand, especially in the retail/wholesale

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sector. On the other hand, those firms that increase prices more are firms that are positively exposed and experience a positive demand shift. Hence, this suggests that the main reason behind the positive exposure is an increase in demand, again especially in the retail/wholesale sector. Inflationary pressure is also exerted by firms experiencing negative supply shifts. The next section addresses the relative importance of these different groups of firms for overall inflation dynamics.

4 Implications for Aggregate Sectoral Inflation

What are the implications of the Covid-19 crisis for inflation in the short and medium run? Up to first order, inflation (π_t) is given by the contributions of price increases and decreases:

$$\pi_t = fr_t^+ \tilde{\pi}_t^+ + fr_t^- \tilde{\pi}_t^- \tag{4}$$

where $fr_{i,t}^+$ denotes the frequency of price increase (the extensive pricing margin), $\tilde{\pi}_{i,t}^+$ the average size of price increase (the intensive pricing margin), and corresponding notation for price decreases applies.

We proceed in two steps. We first aggregate at the sector level our estimated effects on planned price adjustment to predict extensive pricing margin behavior, taking into account the observed heterogeneity in planned price changes. Since the survey question on which we build our analysis covers planned price changes for the following three months, this exercise corresponds to a projection one quarter out, through August 2020.

In a second step, we project current and future PPI inflation on the current extensive pricing margin. We think of this forecasting exercise as a reduced-form approach to determine the effects of the frequency of planned price changes on inflation, encompassing any associated changes in the intensive pricing margin, which we do not observe. The validity of this approach rests on the assumption that historical correlations continue to hold during the Covid-19 crisis.

4.1 Aggregating Frequency of Planned Price Changes

Table 5 aggregates at the sector level the estimated firm-level effects on planned price adjustment along the extensive margin, separately for price increases and decreases. We report estimates from the baseline specification with the full set of controls and fixed effects, estimated by sector and documented in Columns (3) and (6) of Table A.10 in the Appendix. Since our estimated effects are identified off the response of each exposure category relative to the base category of no or weak exposure, the level effect on inflation is not identifiable. Thus, to aggregate our estimates we implicitly assume that the base exposure category is unaffected by Covid-19. Following Nakamura and Steinsson (2018), this approach is common in the macroeconomic literature using cross-sectional variation for identification.

Table 5 – Aggregate Impact of Planned Price-Setting Behavior

	Price Increases			Price Decreases		
	coeff.	weights	agg.	coeff.	weights	agg.
			Manufacturing			
Negative^{vs}	.0016591	.3247141	.0005387	.0602028	.3247141	.0195487
Negative^{s}	.0015589	.2979125	.0004644	.036958	.2979125	.0110103
Positive	.0519614	.0296705	.0015417	-.0611839	.0296705	-.0018154
			$\sum$ .0025449			$\sum$ .0287436
			Wholesale/Retail			
Negative^{vs}	-.0478655	.3156459	-.0151085	.1306029	.3156459	.0412243
Negative^{s}	.0002179	.2255568	.0000491	.0360999	.2255568	.0081426
Positive	.0718418	.1114699	.0080082	-.0110947	.1114699	-.0012367
			$\sum$ -.0070512			$\sum$ .0481301
			Services			
Negative^{vs}	-.0184733	.3575888	-.0066058	.1058252	.3575888	.0378419
Negative^{s}	-.0223124	.1908276	-.0042578	.0640678	.1908276	.0122259
Positive	.0021453	.0227419	.0000488	-.0104401	.0227419	-.0002374
			$\sum$ -.0108149			$\sum$ .0498304

Notes: Coefficients refer to columns (6) and (9) in Table A.10. Weights refer to representative weights in the ifo-BCS for firms in 2020:M04/M05. Weights do not sum up to one as we omit the base category of no/weak exposure.

We pool all observations in 2020:M04 and 2020:M05 and use the representative weights provided in the ifo-BCS to aggregate estimates for each exposure category.[12] Table 5 shows that the frequency of planned price increases is predicted to increase by approximately 0.3 percentage points in manufacturing and predicted to decrease by 0.7 and 1 percentage points in wholesale/retail and services, respectively. These changes are economically small. For example, in manufacturing the increase in the frequency of planned price increases corresponds to less than three percent of the sample average.Positively exposed firms plan to increase prices by up to 7 percentage points, but their weight is small, ranging from about 2 percent in services to 11 percent in retail/wholesale. Hence, firms with negative exposure explain the bulk of changes in the frequency of planned price increases, accounting for more than 50 percent of all observations.

The frequency of price decreases is predicted to increase by 2.8, 4.8 and 4.9 percentage points in manufacturing, retail/wholesale and services, respectively. These figures are economically large. In manufacturing, for example, this change equals as much as one third of the sample average in the frequency of planned price decreases.Again, firms with

[12]Using the relative frequency of each Covid-19 exposure category yields very similar results.

negative exposure account for the bulk of these changes, offsetting the fall in planned price decreases of positively exposed firms.

4.2 Forecasting Aggregate Sectoral Inflation

We estimate the following regression to predict quarter-on-quarter inflation rates in every month:

$$\pi_{t+h} = \alpha_h + \beta_{fr^+} fr_t^+ + \beta_{fr^-} fr_t^- + \gamma_\pi \pi_{t,\dots,t-k} + \gamma_{fr^+} fr^+_{t-1,\dots,t-k} + \gamma_{fr^-} fr^-_{t-1,\dots,t-k} + \varepsilon_{t+h} \quad (5)$$

This forecasting model predicts current and future inflation by current changes in the frequency of planned price decreases and price increases, controlling for their lagged values, lagged inflation, and month-fixed effects (not shown in Equation (5)). Here, h denotes the forecast horizon in months and k denotes the lag order of lagged control variables which we set according to the Bayesian information criterion at horizon $h = 0$.[13] We estimate this equation separately for the manufacturing, wholesale, and retail industries, using data on manufacturing producer price inflation excluding energy, wholesale inflation, and retail inflation including cars and value-added tax rate changes, respectively.[14] The frequency of planned price adjustment obtains from aggregating the firm-level responses at the 2-digit and 3-digit industry levels in manufacturing and wholesale/retail, respectively, using representative weights included in the ifo-BCS, and then aggregating at the aggregate manufacturing and aggregate wholesale/retail level using value-added shares from the Federal Statistical Office. We start the sample in 2006:M05 and end in 2020:M03.[15] For statistical inference, we compute Newey and West (1987) standard errors. At each forecasting step h, we include the forecast errors from the previous step $h-1$ to improve forecast efficiency.

Figure 5 plots the marginal effects of Covid-19 on inflation, i.e., the combined effect of the coefficient estimates β_{fr^+} and β_{fr^-} scaled by the change in the frequency of price adjustment from Table 5.[16] The figure shows the contributions of planned price increases and decreases to inflation in the left and right columns, respectively, for manufacturing,

[13]We allow the lag order to differ across control variables and sectors but omit additional indices for ease of exposition. Model selection according to the Bayesian information criterion results in four lags of past inflation, one lag of planned price increases, and four lags of planned price decreases in manufacturing; the same specification in wholesale except for three lags of planned price decreases; and four lags of inflation but no lags of planned price adjustment in retail.

[14]We split the retail and wholesale industries here because there does not appear to exist a common price index or corresponding weights for these two sectors.

[15]Aggregation weights for the retail and wholesale industries are missing in 2009:M11. We use a linear interpolation to impute the values for planned price increases and decreases in this month.

[16]The confidence intervals shown represent sampling variation of β_{fr^+} and β_{fr^-} and we take as given the points estimates of the changes in the frequency of planned price increases and decreases.

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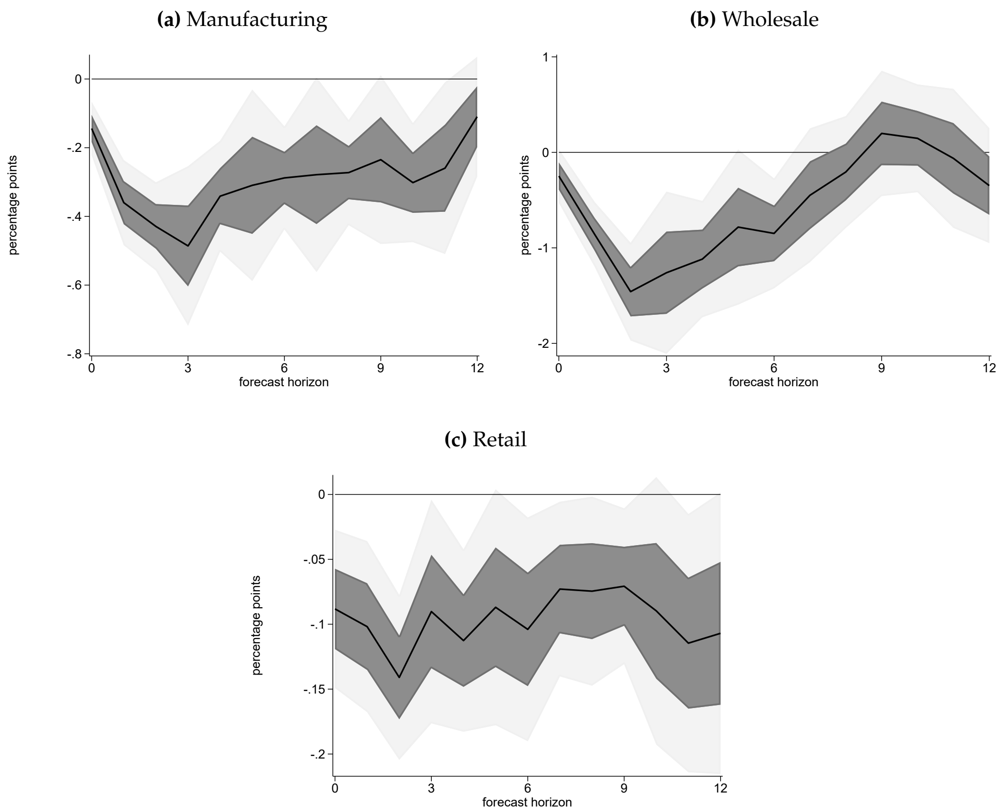
wholesale, retail, from top to bottom, in that order. The total effect on inflation in each sector equals the sum across columns. The exercise followed in this figure is to study the consequences of a one-off change in the monthly frequency of planned price adjustment of the magnitude calculated in Table 5 for average exposure in 2020:M04 and 2020:M05. In that sense, the estimate is conservative in that it does not account for movements in the frequency of planned price changes due to Covid-19 over several months.

Figure 5 – Contributions of Planned Price Changes to Predicted Inflation



Notes: Change in inflation in percentage points due to one-time shift in planned price increases and decreases in response to Covid-19. Forecast horizon in months.

In manufacturing, we estimate that inflation declines by more than 0.4 percentage points over the next three months and remains subdued thereafter. Upward price pressures due to planned price increases are dwarfed by a substantial and persistent decline in inflation due to planned price decreases.[17] Quarter-on-quarter producer price inflation in manufacturing averaged 0.27 percent with standard deviation of 0.5 percentage points

[17]Figure A.2 in the Appendix shows separately the contributions of planned price increases and decreases.

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over the period 2006:M01 to 2020:M03, so this decline is sizable. In retail and wholesale, the frequency of planned price increases actually declines pushing inflation in the same direction as planned price decreases. The total effect is a whopping 1.5 percentage points decline in wholesale over the next three months, relative to a 0.32 percent sample average with standard deviation of 1.5 percentage points. Wholesale inflation returns to normal after about eight months. In the retail sector, inflation is predicted to fall by about 0.15 percentage points through 2020:M08, relative to a 0.28 percent sample average with standard deviation of 0.33 percentage points. Notice that all inflation forecasts peak three to four months out, consistent with the fact that the ifo-BCS asks about planned price adjustment in the following three months.

5 Conclusion

This paper provides evidence that supply and demand forces coexist, but demand deficiencies dominate early into the Covid-19 economic crisis. We base this conclusion on a predicted decline of inflation through August 2020 by as much as 1.5 percentage points, not including the temporary reduction in the German value-added tax rate effective July 2020.

Our paper does not address the likely path of the price level in the longer run. Many different forces can push or pull inflation in either direction. These include: extraordinary fiscal stimulus, cost-push factors such as broken supply chains and deglobalization among to the former, sustained weak demand and high uncertainty among the latter.

Our findings suggest a role for policy to stabilize aggregate demand. Monetary policy, constrained by the effective lower bound, seems an unlikely candidate. Moreover, even if there was policy room, a higher frequency of price change implies more aggregate price flexibility such that monetary stimulus becomes less effective. Fiscal policy appears a more promising candidate. The stimulus package announced on June 6, including a temporary reduction in value-added taxes, transfers to families with children, and investment subsidies, is a step in that direction.

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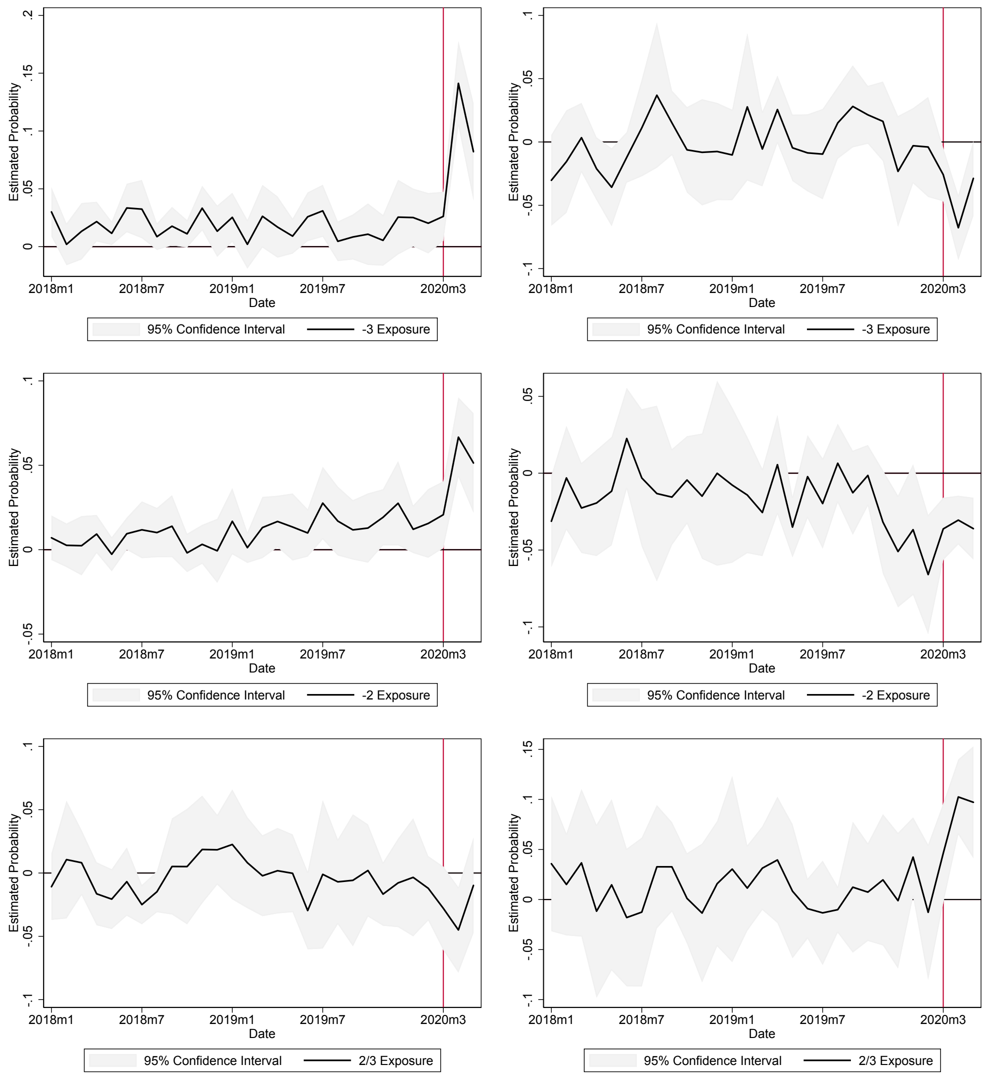
A Additional Graphs and Tables

Figure A.1 – Effects of Covid-19 Exposure on Planned Price Decreases and Increases



Notes: Figures shows estimates of δ_{-3}, δ_{-2} and $\delta_{2\vee 2}$ from equation (1) in time t for price decreases (left column) and price increases (right column). Each figure refers to a Covid-19 exposure category as of 2020:M04. Standard errors are clustered at two-digit WZ08 industry level.

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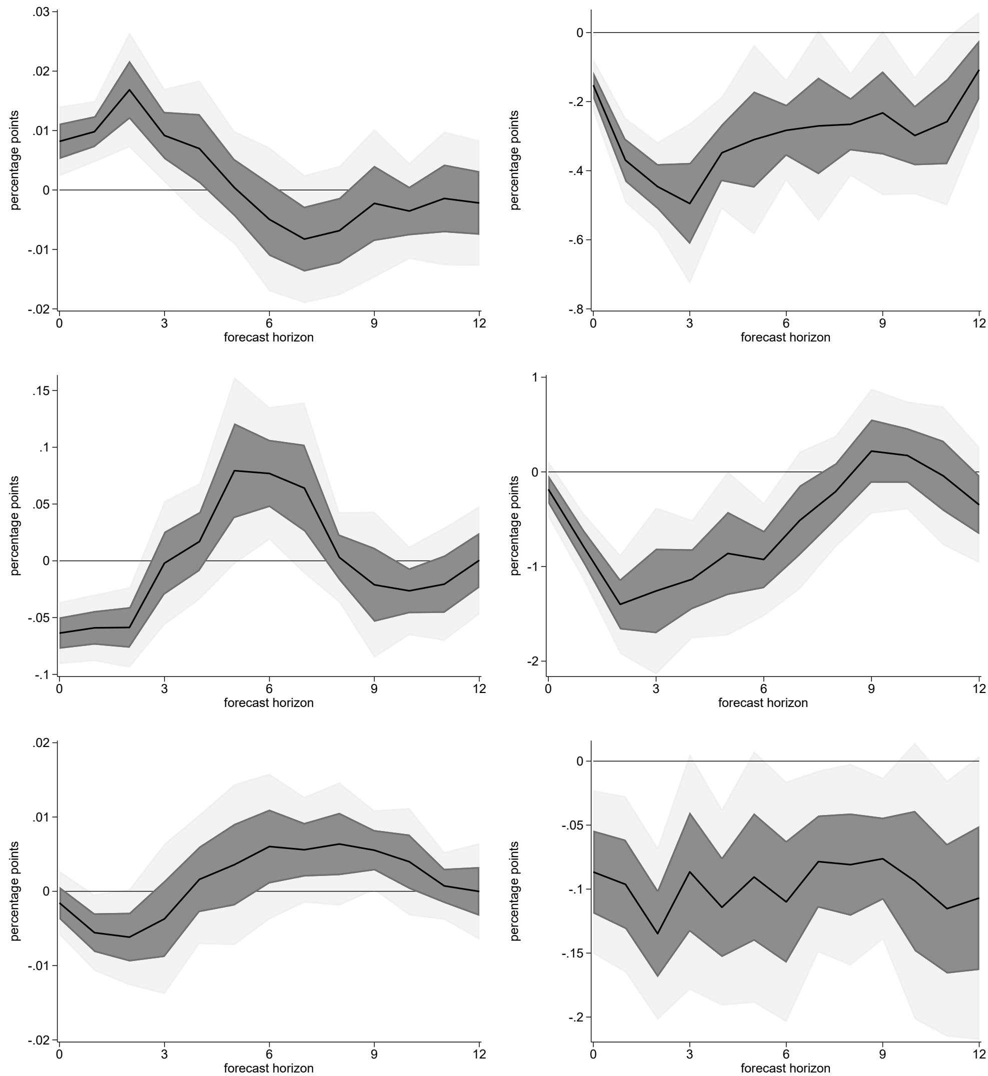
Figure A.2 – Contributions of Planned Price Increases and Decreases to Predicted Inflation



Notes: Change in inflation in percentage points due to one-time shift in planned price increases and decreases in response to Covid-19. Forecast horizon in months. Left column: planned price increases. Right: planned price decreases. Top: manufacturing. Middle: wholesale. Bottom: retail.

Table A.1 – Covid-19 Exposure by Industry: Retail/Wholesale

	Covid-19 Exposure				
	-3	-2	-1/0/1	2/3	Total
Retail trade, except of motor vehicles and motorcycles	43.44	21.79	18.08	16.69	100.00
	(596)	(299)	(248)	(229)	(1372)
Wholesale and retail trade and repair of motor vehicles and motorcycles	45.18	32.49	20.30	2.03	100.00
	(89)	(64)	(40)	(4)	(197)
Wholesale trade, except of motor vehicles and motorcycles	26.79	24.92	35.07	13.23	100.00
	(401)	(373)	(525)	(198)	(1497)
Total	35.42	24.01	26.52	14.06	100.00
	(1086)	(736)	(813)	(431)	(3066)

Notes: Values show percentage of firms by subgroup (row). Observations are grouped by the Covid-19 Exposure variable on a scale ranging from -3 ("strong negatively affected") to 3 ("strongly positively affected"). In addition, groups are clustered into four categories. Observations in 2020:M04/M05 are used. Number of observations in parentheses.

Table A.2 – Covid-19 Exposure by Industry: Services

	Covid-19 Exposure				
	-3	-2	-1/0/1	2/3	Total
Accommodation	97.79	1.58	0.63	0.00	100.00
	(310)	(5)	(2)	(0)	(317)
Activities auxiliary to financial services and insurance activities	17.78	33.33	46.67	2.22	100.00
	(8)	(15)	(21)	(1)	(45)
Activities of head offices; management consultancy activities	24.08	24.61	43.46	7.85	100.00
	(46)	(47)	(83)	(15)	(191)
Activities of membership organisations	100.00	0.00	0.00	0.00	100.00
	(2)	(0)	(0)	(0)	(2)
Advertising and market research	46.30	22.22	25.00	6.48	100.00
	(50)	(24)	(27)	(7)	(108)
Air transport	33.33	33.33	33.33	0.00	100.00
	(1)	(1)	(1)	(0)	(3)
Architectural and engineering activities; technical testing and analysis	8.75	18.79	68.72	3.73	100.00
	(61)	(131)	(479)	(26)	(697)
Computer programming, consultancy and related activities	12.63	20.36	58.51	8.51	100.00
	(49)	(79)	(227)	(33)	(388)
Construction of buildings	0.00	50.00	0.00	50.00	100.00
	(0)	(1)	(0)	(1)	(2)
Creative, arts and entertainment activities	80.00	0.00	15.00	5.00	100.00

	(16)	(0)	(3)	(1)	(20)
Education	56.16	21.92	19.18	2.74	100.00
	(41)	(16)	(14)	(2)	(73)
Electricity, gas, steam and air conditioning supply	0.00	0.00	100.00	0.00	100.00
	(0)	(0)	(7)	(0)	(7)
Employment activities	58.21	26.87	13.43	1.49	100.00
	(78)	(36)	(18)	(2)	(134)
Financial service activities, except insurance and pension funding	33.33	28.99	37.68	0.00	100.00
	(23)	(20)	(26)	(0)	(69)
Food and beverage service activities	93.97	4.31	1.72	0.00	100.00
	(109)	(5)	(2)	(0)	(116)
Gambling and betting activities	76.47	0.00	23.53	0.00	100.00
	(13)	(0)	(4)	(0)	(17)
Human health activities	16.67	16.67	50.00	16.67	100.00
	(1)	(1)	(3)	(1)	(6)
Information service activities	23.21	17.86	32.14	26.79	100.00
	(13)	(10)	(18)	(15)	(56)
Insurance, reinsurance and pension funding, except compulsory social security	0.00	0.00	100.00	0.00	100.00
	(0)	(0)	(3)	(0)	(3)
Land transport and transport via pipelines	33.94	32.12	29.70	4.24	100.00
	(56)	(53)	(49)	(7)	(165)
Legal and accounting activities	3.57	15.18	66.96	14.29	100.00
	(4)	(17)	(75)	(16)	(112)
Libraries, archives, museums and other cultural activities	66.67	33.33	0.00	0.00	100.00
	(2)	(1)	0	0	(3)
Motion picture, video and television programme production, sound recording and music publishing activities	65.96	14.89	12.77	6.38	100.00
	(31)	(7)	(6)	(3)	(47)
Office administrative, office support and other business support activities	57.35	13.97	25.00	3.68	100.00
	(78)	(19)	(34)	(5)	(136)
Other personal service activities	37.50	62.50	0.00	0.00	100.00
	(3)	(5)	(0)	(0)	(8)
Other professional, scientific and technical activities	26.76	19.72	45.07	8.45	100.00
	(19)	(14)	(32)	(6)	(71)
Postal and courier activities	45.45	36.36	13.64	4.55	100.00
	(10)	(8)	(3)	(1)	(22)
Programming and broadcasting activities	76.19	9.52	9.52	4.76	100.00
	(16)	(2)	(2)	(1)	(21)
Public administration and defence; compulsory social security	0.00	0.00	100.00	0.00	100.00
	(0)	(0)	(2)	(0)	(2)
Publishing activities	38.57	28.57	21.43	11.43	100.00

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	(27)	(20)	(15)	(8)	(70)
Real estate activities	12.77	18.44	67.38	1.42	100.00
	(18)	(26)	(95)	(2)	141
Remediation activities and other waste management services	0.00	16.67	83.33	0.00	100.00
	(0)	(1)	(5)	(0)	(6)
Rental and leasing activities	40.00	20.00	40.00	0.00	100.00
	(22)	(11)	(22)	(0)	(55)
Repair of computers and personal and household goods	9.09	45.45	27.27	18.18	100.00
	(1)	(5)	(3)	(2)	(11)
Residential care activities	100.00	0.00	0.00	0.00	100.00
	(2)	(0)	(0)	(0)	(2)
Scientific research and development	10.00	25.45	58.18	6.36	100.00
	(11)	(28)	(64)	(7)	(110)
Security and investigation activities	16.13	22.58	48.39	12.90	100.00
	(5)	(7)	(15)	(4)	(31)
Services to buildings and landscape activities	16.42	28.36	46.27	8.96	100.00
	(11)	(19)	(31)	(6)	(67)
Sewerage	15.38	0.00	84.62	0.00	100.00
	(2)	(0)	(11)	(0)	(13)
Social work activities without accommodation	0.00	100.00	0.00	0.00	100.00
	(0)	(1)	(0)	(0)	(1)
Specialised construction activities	18.18	9.09	72.73	0.00	100.00
	(4)	(2)	(16)	(0)	(22)
Sports activities and amusement and recreation activities	78.13	15.63	6.25	0.00	100.00
	(25)	(5)	(2)	(0)	(32)
Telecommunications	19.05	19.05	23.81	38.10	100.00
	(4)	(4)	(5)	(8)	(21)
Travel agency, tour operator and other reservation service and related activities	99.12	0.88	0.00	0.00	100.00
	(112)	(1)	(0)	(0)	(113)
Warehousing and support activities for transportation	35.35	31.16	28.37	5.12	100.00
	(76)	(67)	(61)	(11)	(215)
Waste collection, treatment and disposal activities; materials recovery	7.69	18.46	64.62	9.23	100.00
	(5)	(12)	(42)	(6)	(65)
Water transport	64.71	23.53	11.76	0.00	100.00
	(11)	(4)	(2)	(0)	(17)
Total	35.90	19.05	39.92	5.14	100.00
	(1376)	(730)	(1530)	(197)	(3833)

Notes: Values show percentage of firms by subgroup (row). Observations are grouped by the Covid-19 Exposure variable on a scale ranging from -3 ("strong negatively affected") to 3 ("strongly positively affected"). In addition, groups are clustered into four categories. Observations in 2020:M04/M05 are used. Number of observations in parentheses.

Table A.3 – Covid-19 Exposure by Industry: Manufacturing

	Covid-19 Exposure				
	-3	-2	-1/0/1	2/3	Total
Manufacture of basic metals	41.94	29.57	26.88	1.61	100.00
	(78)	(55)	(50)	(3)	(186)
Manufacture of basic pharmaceutical products and pharmaceutical preparations	0.00	13.64	45.45	40.91	100.00
	(0)	(3)	(10)	(9)	(22)
Manufacture of beverages	65.45	21.82	9.09	3.64	100.00
	(36)	(12)	(5)	(2)	(55)
Manufacture of chemicals and chemical products	22.28	21.78	42.08	13.86	100.00
	(45)	(44)	(85)	(28)	(202)
Manufacture of coke and refined petroleum products	36.36	27.27	27.27	9.09	100.00
	(4)	(3)	(3)	(1)	(11)
Manufacture of computer, electronic and optical products	21.43	29.46	43.30	5.80	100.00
	(48)	(66)	(97)	(13)	(224)
Manufacture of electrical equipment	22.53	34.26	38.27	4.94	100.00
	(73)	(111)	(124)	(16)	(324)
Manufacture of fabricated metal products, except machinery and equipment	32.23	31.29	33.33	3.14	100.00
	(205)	(199)	(212)	(20)	(636)
Manufacture of food products	24.82	16.79	33.58	24.82	100.00
	(34)	(23)	(46)	(34)	(137)
Manufacture of furniture	61.36	30.68	7.95	0.00	100.00
	(54)	(27)	(7)	(0)	(88)
Manufacture of leather and related products	63.16	31.58	5.26	0.00	100.00
	(12)	(6)	(1)	(0)	(19)
Manufacture of machinery and equipment n.e.c.	28.81	35.25	34.29	1.65	100.00
	(210)	(257)	(250)	(12)	(729)
Manufacture of motor vehicles, trailers and semi-trailers	59.20	25.60	14.40	0.80	100.00
	(74)	(32)	(18)	(1)	(125)
Manufacture of other non-metallic mineral products	16.28	23.26	55.81	4.65	100.00
	(35)	(50)	(120)	(10)	(215)
Manufacture of other transport equipment	50.00	37.50	12.50	0.00	100.00
	(4)	(3)	(1)	(0)	(8)
Manufacture of paper and paper products	19.57	22.46	31.16	26.81	100.00
	(27)	(31)	(43)	(37)	(138)
Manufacture of rubber and plastic products	26.42	25.28	39.25	9.06	100.00
	(70)	(67)	(104)	(24)	(265)
Manufacture of textiles	47.06	25.00	20.59	7.35	100.00
	(32)	(17)	(14)	(5)	(68)
Manufacture of tobacco products	0.00	0.00	100.00	0.00	100.00
	(0)	(0)	(1)	(0)	(1)
Manufacture of wearing apparel	50.00	32.14	14.29	3.57	100.00
	(14)	(9)	(4)	(1)	(28)

Manufacture of wood and of products of wood and cork, except furniture; manufacture of articles of straw and plaiting materials	17.57	32.43	42.57	7.43	100.00
	(26)	(48)	(63)	(11)	(148)
Other manufacturing	31.15	36.07	27.87	4.92	100.00
	(19)	(22)	(17)	(3)	(61)
Printing and reproduction of recorded media	43.08	26.92	22.31	7.69	100.00
	(56)	(35)	(29)	(10)	(130)
Repair and installation of machinery and equipment	20.00	60.00	20.00	0.00	100.00
	(2)	(6)	(2)	(0)	(10)
Total	30.23	29.40	34.10	6.27	100.00
	(1158)	(1126)	(1306)	(240)	(3830)

Notes: Values show percentage of firms by subgroup (row). Observations are grouped by the Covid-19 Exposure variable on a scale ranging from -3 ("strong negatively affected") to 3 ("strongly positively affected"). In addition, groups are clustered into four categories. Observations in 2020:M04/M05 are used. Number of observations in parentheses.

Table A.4 – Summary Statistics by Industry: Manufacturing

	Covid-19 Exposure							
	-3	-2	-1	0	1	2	3	Total
Planned Price Increase	.039	.039	.066	.075	.133	.159	.22	.058
	.195	.193	.249	.263	.341	.367	.419	.233
Planned Price Decrease	.197	.147	.101	.06	.025	.024	.049	.133
	.398	.354	.302	.238	.158	.155	.218	.34
Planned Price Change	.236	.186	.167	.135	.158	.183	.268	.191
	.425	.389	.374	.342	.366	.389	.449	.393
Price Increase	.027	.037	.045	.066	.108	.117	.2	.045
	.161	.188	.208	.249	.312	.324	.407	.208
Price Decrease	.102	.095	.056	.038	.025	0	.033	.076
	.303	.293	.229	.192	.157	0	.183	.266
Price Change	.129	.132	.101	.104	.133	.117	.233	.122
	.335	.338	.301	.306	.341	.324	.43	.327
Positive Business Conditions	.012	.035	.189	.51	.577	.78	.854	.161
	.11	.183	.392	.501	.496	.416	.358	.368
Negative Business Conditions	.878	.555	.166	.048	.026	.073	.049	.471
	.328	.497	.373	.214	.159	.262	.218	.499
Positive Business Expectations	.147	.122	.087	.097	.121	.284	.31	.124
	.354	.327	.282	.296	.327	.454	.468	.33
Negative Business Expectations	.677	.656	.586	.357	.318	.247	.19	.587
	.468	.475	.493	.48	.467	.434	.397	.493
Expected Revenue Change in %	-30.892	-21.628	-13.565	-7.284	-.412	.818	29.095	-18.493
	16.748	12.586	9.711	8.582	9.371	16.779	71.065	18.19
Observations	1158	1126	888	418	158	82	42	3872
Percent	29.91	29.08	22.93	10.8	4.08	2.12	1.08	100

Notes: Numbers depict means and standard deviations (small) of variables by column group. Observations are grouped by the Covid-19 Exposure variable on a scale ranging from -3 ("strong negatively affected") to 3 ("strongly positively affected"). Observations in 2020:M04/M05 are used. Expected Revenue Change in % was asked in 2020:M04 and refers to overall revenue in 2020.

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Table A.5 – Summary Statistics by Industry: Retail/Wholesale

	Covid-19 Exposure							
	-3	-2	-1	0	1	2	3	Total
Planned Price Increase	.126	.163	.177	.175	.25	.264	.357	.175
	.332	.37	.382	.381	.434	.442	.481	.38
Planned Price Decrease	.282	.151	.12	.061	.066	.055	.056	.166
	.45	.358	.325	.239	.248	.229	.231	.372
Planned Price Change	.408	.315	.297	.236	.316	.319	.413	.341
	.492	.465	.457	.425	.466	.467	.494	.474
Price Increase	.062	.087	.112	.17	.222	.238	.389	.122
	.242	.282	.316	.376	.416	.427	.489	.328
Price Decrease	.185	.121	.099	.102	.065	.055	.069	.127
	.388	.327	.3	.303	.246	.229	.255	.333
Price Change	.247	.209	.212	.272	.286	.293	.458	.249
	.431	.407	.409	.446	.453	.456	.5	.433
Positive Business Conditions	.011	.034	.129	.404	.563	.779	.917	.196
	.105	.181	.336	.492	.497	.416	.277	.397
Negative Business Conditions	.906	.57	.151	.045	.036	.011	.007	.47
	.292	.495	.359	.208	.188	.105	.083	.499
Positive Business Expectations	.07	.079	.085	.08	.113	.282	.326	.102
	.256	.27	.279	.271	.317	.451	.471	.303
Negative Business Expectations	.82	.703	.61	.379	.286	.243	.222	.62
	.385	.457	.488	.486	.453	.43	.417	.485
Expected Revenue Change in %	-32.271	-24.107	-13.115	-7.945	-.496	4.36	11.09	-19.601
	19.298	14.262	9.809	9.154	10.95	15.064	20.604	20.377
Observations	1086	736	547	266	248	183	144	3210
Percent	33.83	22.93	17.04	8.29	7.73	5.7	4.49	100

Notes: Numbers depict means and standard deviations (small) of variables by column group. Observations are grouped by the Covid-19 Exposure variable on a scale ranging from -3 ("strong negatively affected") to 3 ("strongly positively affected"). Observations in 2020:M04/M05 are used. Expected Revenue Change in % was asked in 2020:M04 and refers to overall revenue in 2020.

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Table A.6 – Summary Statistics by Industry: Services

	Covid-19 Exposure							
	-3	-2	-1	0	1	2	3	Total
Planned Price Increase	.099	.046	.075	.1	.124	.098	.235	.085
	.299	.21	.263	.3	.331	.3	.431	.279
Planned Price Decrease	.189	.126	.066	.024	.021	.039	.029	.112
	.392	.332	.248	.152	.143	.196	.171	.316
Planned Price Change	.288	.173	.141	.123	.145	.137	.265	.198
	.453	.378	.348	.329	.353	.348	.448	.398
Price Increase	.032	.029	.034	.043	.127	.102	.143	.039
	.177	.169	.181	.203	.334	.306	.355	.194
Price Decrease	.161	.08	.034	.014	.028	.041	.057	.085
	.368	.271	.181	.119	.166	.2	.236	.279
Price Change	.193	.109	.068	.057	.155	.143	.2	.124
	.395	.312	.252	.233	.363	.354	.406	.33
Positive Business Conditions	.003	.051	.271	.591	.664	.86	.676	.208
	.054	.22	.445	.492	.474	.351	.475	.406
Negative Business Conditions	.934	.505	.104	.026	.021	.02	.27	.461
	.249	.5	.306	.161	.142	.141	.45	.499
Positive Business Expectations	.139	.137	.113	.09	.308	.3	.405	.136
	.346	.344	.316	.287	.463	.463	.498	.343
Negative Business Expectations	.737	.673	.464	.222	.123	.08	.297	.545
	.44	.469	.499	.416	.33	.274	.463	.498
Expected Revenue Change in %	-46.179	-23.712	-11.571	-4.462	.386	10.688	2.722	-24.649
	24.094	14.004	9.988	9.13	6.895	8.623	39.016	24.46
Observations	1376	730	962	568	146	51	37	3870
Percent	35.56	18.86	24.86	14.68	3.77	1.32	.96	100

Notes: Numbers depict means and standard deviations (small) of variables by column group. Observations are grouped by the Covid-19 Exposure variable on a scale ranging from -3 ("strong negatively affected") to 3 ("strongly positively affected"). Observations in 2020:M04/M05 are used. Expected Revenue Change in % was asked in 2020:M04 and refers to overall revenue in 2020.

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Table A.7 – Summary Statistics: Additional Supply and Demand Indicators (Manufacturing)

	Covid-19 Exposure							
	-3	-2	-1	0	1	2	3	Total
Production Constr. Lack of Material	.247	.248	.201	.131	.202	.239	.2	.22
	.432	.432	.401	.338	.404	.431	.41	.414
Distorted Supply Chain of Interm. Prod.	.41	.328	.274	.185	.253	.227	.056	.31
	.492	.47	.447	.389	.438	.424	.236	.463
Distorted Supply Chain of Final Prod.	.201	.182	.144	.079	.067	.114	.056	.158
	.402	.387	.352	.271	.251	.321	.236	.364
Cost Increase of Interm. Prod./Raw Material	.124	.1	.079	.074	.107	.114	.056	.098
	.33	.301	.271	.263	.311	.321	.236	.298
Dependance on Imports	.549	.532	.541	.455	.516	.576	.688	.532
	.498	.5	.499	.5	.504	.502	.479	.499
Dep. on Imports China	.368	.33	.309	.208	.242	.364	.375	.318
	.483	.471	.463	.407	.432	.489	.5	.466
Dep. on Imports Italy	.297	.311	.287	.24	.242	.303	.188	.289
	.457	.464	.453	.429	.432	.467	.403	.453
Demand Reduction	.482	.391	.24	.132	.173	.136	.056	.327
	.5	.489	.427	.34	.381	.347	.236	.469
Production Constr. Lack of Orders	.803	.663	.339	.089	.071	.152	.15	.51
	.398	.473	.474	.285	.259	.363	.366	.5
Closure	.255	.126	.046	.007	.025	.037	0	.126
	.436	.332	.21	.085	.158	.189	0	.332

Notes: Numbers depict means and standard deviations (small) of variables by column group. Observations are grouped by the Covid-19 Exposure variable on a scale ranging from -3 ("strong negatively affected") to 3 ("strongly positively affected"). The variables Distorted Supply Chain of Intermediate Products, Distorted Supply Chain of Final Products, Cost Increase of Intermediate Prod./Raw Material, and Demand Reduction are only available in 2020:M03 and are imputed to Covid-19 Exposure in 2020:M04. The variables Production Constr. Lack of Material (firms that are constrained in production due to lack of material), Dependance on Imports, Dep. on Imports China, Dep. on Imports Italy, and Production Constr. Lack of Orders (firms that are constrained in production due to lack of orders) are only available in 2020:M04. For the Closure variable (firms that stopped production/ closed plants as a response to the Covid-19 pandemic), observations in 2020:M04/M05 are used.

Table A.8 – Summary Statistics: Additional Supply and Demand Indicators (Retail/Wholesale)

	Covid-19 Exposure							
	-3	-2	-1	0	1	2	3	Total
Distorted Supply Chain of Interm. Prod.	.419	.403	.427	.202	.317	.29	.255	.381
	.494	.491	.496	.404	.468	.458	.44	.486
Distorted Supply Chain of Final Prod.	.211	.172	.147	.03	.059	.081	.078	.156
	.408	.378	.355	.172	.238	.275	.272	.363
Cost Increase of Interm. Prod./Raw Material	.147	.122	.164	.081	.178	.097	.078	.137
	.355	.328	.372	.274	.385	.298	.272	.344
Dependance on Imports	.595	.586	.62	.609	.587	.649	.556	.598
	.491	.493	.486	.49	.494	.48	.501	.49
Dep. on Imports China	.321	.32	.358	.345	.254	.26	.333	.321
	.467	.467	.48	.478	.437	.441	.475	.467
Dep. on Imports Italy	.314	.315	.347	.282	.369	.416	.397	.33
	.465	.465	.477	.452	.484	.496	.493	.47
Demand Reduction	.614	.386	.271	.152	.158	.081	.039	.391
	.487	.488	.446	.36	.367	.275	.196	.488
Production Constr. Lack of Orders	.623	.691	.478	.2	.104	.07	0	.495
	.485	.463	.501	.404	.308	.258	0	.5
Closure	.443	.238	.174	.135	.125	.115	.139	.268
	.497	.426	.379	.343	.331	.32	.347	.443

Notes: Numbers depict means and standard deviations (small) of variables by column group. Observations are grouped by the Covid-19 Exposure variable on a scale ranging from -3 ("strong negatively affected") to 3 ("strongly positively affected"). The variables Distorted Supply Chain of Intermediate Products, Distorted Supply Chain of Final Products, Cost Increase of Intermediate Prod./Raw Material, and Demand Reduction are only available in 2020:M03 and are imputed to Covid-19 Exposure in 2020:M04. The variables Dependance on Imports, Dep. on Imports China, Dep. on Imports Italy, and Production Constr. Lack of Orders (firms that are constrained in production due to lack of orders) are only available in 2020:M04. For the Closure variable (closure of stores/officies as a response to the Covid-19 pandemic), observations in 2020:M04/M05 are used.

Table A.9 – Summary Statistics: Additional Supply and Demand Indicators (Services)

	Covid-19 Exposure							
	-3	-2	-1	0	1	2	3	Total
Demand Reduction	.685	.414	.222	.078	.098	.25	.313	.4
	.465	.493	.416	.27	.3	.452	.479	.49
Production Constr. Lack of Orders	.673	.623	.296	.091	.03	0	.167	.457
	.469	.485	.457	.288	.173	0	.383	.498
Closure	.244	.047	.036	.028	.027	.02	0	.11
	.43	.211	.187	.166	.164	.14	0	.313

Notes: Numbers depict means and standard deviations (small) of variables by column group. Observations are grouped by the Covid-19 Exposure variable on a scale ranging from -3 ("strong negatively affected") to 3 ("strongly positively affected"). The variable Demand Reduction is only available in 2020:M03 and is imputed to Covid-19 Exposure in 2020:M04. The variable Production Constr. Lack of Orders (firms that are constrained in production due to lack of orders) is only available in 2020:M04. For the Closure variable (closure of stores/officies as a response to the Covid-19 pandemic), observations in 2020:M04/M05 are used.

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Table A.10 – Effects of the Covid-19 Pandemic on Planned Price Adjustment by Sector

	Planned Price Decrease			Planned Price Increase			Planned Price Change		
Covid-19 Exposure	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Manufacturing				
Neg.$^{very\ strong}$	0.11***	0.082***	0.060***	-0.10***	-0.012	0.0016	0.010	0.070***	0.062***
	(0.013)	(0.016)	(0.014)	(0.0069)	(0.011)	(0.013)	(0.014)	(0.018)	(0.018)
Neg.^{strong}	0.064***	0.048***	0.037***	-0.10***	-0.010	0.0016	-0.040***	0.037**	0.038**
	(0.011)	(0.014)	(0.013)	(0.0066)	(0.011)	(0.012)	(0.013)	(0.017)	(0.017)
Positive	-0.050***	-0.059**	-0.061***	0.035	0.076**	0.052	-0.015	0.017	-0.0092
	(0.016)	(0.024)	(0.024)	(0.034)	(0.037)	(0.042)	(0.036)	(0.040)	(0.042)
Observations	31927	26975	26915	31927	26975	26915	31927	26975	26915
					Retail/Wholesale				
Neg.$^{very\ strong}$	0.19***	0.15***	0.13***	-0.13***	-0.065***	-0.048***	0.059***	0.082***	0.083***
	(0.015)	(0.019)	(0.017)	(0.011)	(0.018)	(0.018)	(0.017)	(0.022)	(0.024)
Neg.^{strong}	0.062***	0.056***	0.036**	-0.096***	-0.020	0.00022	-0.034*	0.035	0.036
	(0.014)	(0.017)	(0.016)	(0.015)	(0.021)	(0.019)	(0.018)	(0.024)	(0.024)
Positive	-0.034***	-0.033**	-0.011	0.045	0.084***	0.072**	0.011	0.051	0.061*
	(0.013)	(0.016)	(0.016)	(0.028)	(0.032)	(0.031)	(0.029)	(0.032)	(0.032)
Observations	26123	22185	22127	26123	22185	22127	26123	22185	22127
					Services				
Neg.$^{very\ strong}$	0.16***	0.10***	0.11***	-0.10***	-0.018	-0.018	0.056***	0.086***	0.087***
	(0.011)	(0.013)	(0.013)	(0.0100)	(0.015)	(0.015)	(0.014)	(0.017)	(0.019)
Neg.^{strong}	0.093***	0.061***	0.064***	-0.15***	-0.032**	-0.022	-0.060***	0.029	0.042**
	(0.013)	(0.016)	(0.015)	(0.0093)	(0.012)	(0.014)	(0.016)	(0.018)	(0.020)
Positive	0.0022	-0.012	-0.010	-0.047	0.012	0.0021	-0.044	0.00024	-0.0083
	(0.020)	(0.023)	(0.027)	(0.041)	(0.036)	(0.046)	(0.044)	(0.038)	(0.049)
Observations	30968	26155	26076	30968	26155	26076	30968	26155	26076
Time + Ind. FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Firm FE	No	No	Yes	No	No	Yes	No	No	Yes

Notes: Linear Probability Model estimates, standard errors (clustered at firm level) in parentheses. Neg.$^{very\ strong}$, Neg.^{strong} and Positive are binary indicators refering to values -3, -2, {2,3} relative to {-1,0,1} of Covid-19 Exposure variable (scale: -3 to 3). Ind. FE refers to two-digit WZ08 industry fixed effects. Controls include separate indicators for positive and negative responses to questions about business situation, business expectations, orders (all lagged by three months) and Taylor dummies. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

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Table A.11 – Effects of the Covid-19 Pandemic on Planned Price Adjustment: Robustness Check

	Planned Price Decrease			Planned Price Increase			Planned Price Change		
Covid-19 Exposure	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Neg.$^{very\ strong}$	0.11***	0.099***	0.096***	-0.027***	-0.018**	-0.023**	0.085***	0.081***	0.073***
	(0.0090)	(0.0084)	(0.0094)	(0.0084)	(0.0088)	(0.0094)	(0.011)	(0.011)	(0.012)
Neg.^{strong}	0.050***	0.042***	0.046***	-0.018**	-0.0031	-0.0058	0.031***	0.039***	0.040***
	(0.0087)	(0.0081)	(0.0085)	(0.0082)	(0.0085)	(0.0086)	(0.011)	(0.011)	(0.012)
Positive	-0.043***	-0.024**	-0.025**	0.085***	0.066***	0.058***	0.043**	0.042*	0.032
	(0.011)	(0.011)	(0.012)	(0.021)	(0.022)	(0.022)	(0.021)	(0.022)	(0.023)
Observations	75315	75118	75035	75315	75118	75035	75315	75118	75035
Time + Ind. FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Time X Ind. FE	No	No	Yes	No	No	Yes	No	No	Yes
Firm FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes

Notes: Linear Probability Model estimates, standard errors (clustered at firm level) in parentheses. Neg.$^{very\ strong}$, Neg.^{strong} and Positive are binary indicators refering to values -3, -2, {2,3} relative to {-1,0,1} of Covid-19 Exposure variable (scale: -3 to 3). Ind. FE refers to two-digit WZ08 industry fixed effects. All specifications include controls, namely separate indicators for positive and negative responses to questions about business situation, business expectations, orders (all lagged by three months) and Taylor dummies. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12 – Effects of the Covid-19 Exposure and Supply Shifts

	Planned Price Decrease			Planned Price Increase			Planned Price Change		
Covid-19 Exposure	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Manufacturing					
Neg.^{vs}	0.072***	0.069***	0.069***	-0.0010	-0.0093	-0.0070	0.070***	0.059***	0.062***
Neg.^{s}	0.037**	0.057***	0.047***	-0.0048	-0.0054	-0.0091	0.033*	0.051***	0.038**
Pos.	-0.050*	-0.059**	-0.058**	0.046	0.039	0.040	-0.0035	-0.019	-0.018
Neg.^{vs} × Dist. Supply Chain of Interm. Prod.	0.010			-0.015			-0.0051		
Neg.^{s} × Dist. Supply Chain of Interm. Prod.	0.041			-0.011			0.030		
Pos. × Dist. Supply Chain of Interm. Prod.	-0.066*			-0.016			-0.082		
Neg.^{vs} × Dist. Supply Chain of Final Prod.		0.036			0.013			0.050	
Neg.^{s} × Dist. Supply Chain of Final Prod.		-0.031			-0.017			-0.048*	
Pos. × Dist. Supply Chain of Final Prod.		-0.046			0.059			0.013	
Neg.^{vs} × Cost Incr. of Interm. Prod./Raw Mat.			0.049			0.0012			0.051
Neg.^{s} × Cost Incr. of Interm. Prod./Raw Mat.			0.035			0.0071			0.042
Pos. × Cost Incr. of Interm. Prod./Raw Mat.			-0.037			0.030			-0.0071
Observations	24458	24458	24458	24458	24458	24458	24458	24458	24458
				Retail/Wholesale					
Neg.^{vs}	0.13***	0.15***	0.15***	-0.071***	-0.072***	-0.066***	0.057**	0.078***	0.083***
Neg.^{s}	0.079***	0.066***	0.051***	-0.054**	-0.035	-0.032	0.025	0.031	0.019
Pos.	-0.049***	-0.040**	-0.047***	0.067*	0.073**	0.060*	0.019	0.032	0.014
Neg.^{vs} × Dist. Supply Chain of Interm. Prod.	0.045			0.013			0.058		
Neg.^{s} × Dist. Supply Chain of Interm. Prod.	-0.056*			0.061*			0.0057		
Pos. × Dist. Supply Chain of Interm. Prod.	0.011			0.031			0.042		
Neg.^{vs} × Dist. Supply Chain of Final Prod.		-0.014			0.028			0.015	
Neg.^{s} × Dist. Supply Chain of Final Prod.		-0.046			0.026			-0.020	
Pos. × Dist. Supply Chain of Final Prod.		-0.060***			0.030			-0.030	
Neg.^{vs} × Cost Incr. of Interm. Prod./Raw Mat.			-0.015			0.0030			-0.012
Neg.^{s} × Cost Incr. of Interm. Prod./Raw Mat.			0.050			0.015			0.065
Pos. × Cost Incr. of Interm. Prod./Raw Mat.			0.0063			0.17			0.18
Observations	19910	19910	19910	19910	19910	19910	19910	19910	19910

Notes: Linear Probability Model estimates, standard errors (clustered at firm level) not shown, available upon request from authors. Neg.$^{very\ strong}$, Neg.^{strong} and Positive are binary indicators refering to values -3, -2, {2,3} relative to {-1,0,1} of Covid-19 Exposure variable (scale: -3 to 3). All specifications include time and two-digit WZ08 industry fixed effects as well as controls. Controls include separate indicators for positive and negative responses to questions about business situation, business expectations, orders (all lagged by three months) and Taylor dummies. The variables Distorted Supply Chain of Intermediate Products, Distorted Supply Chain of Final Products, and Cost Increase of Intermediate Prod./Raw Material are only available in 2020:M03 and are imputed to Covid-19 Exposure in 2020:M04/M05. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table A.13 – Effects of the Covid-19 Exposure and Production Constraints

	Planned Price Decrease	Planned Price Increase	Planned Price Change
Covid-19 Exposure	(1)	(2)	(3)
		Manufacturing	
Neg.^{vs}	0.15**	-0.080***	0.065
Neg.^{s}	0.023	-0.020	0.0036
Pos.	-0.031	0.16**	0.13*
Neg.^{vs} × Prod. Constr.	-0.066	0.077***	0.010
Neg.^{s} × Prod. Constr.	0.029	0.016	0.045
Pos. × Prod. Constr.	-0.056	-0.13	-0.19**
Observations	24911	24911	24911
		Retail/Wholesale	
Neg.^{vs}	0.43	0.022	0.45**
Neg.^{s}	0.079	-0.022	0.057
Pos.	-0.056***	0.13**	0.079
Neg.^{vs} × Prod. Constr.	-0.29	-0.080	-0.37*
Neg.^{s} × Prod. Constr.	-0.033	-0.021	-0.053
Pos. × Prod. Constr.	0.029	-0.025	0.0035
Observations	12826	12826	12826
		Services	
Neg.^{vs}	-0.082***	0.076	-0.0064
Neg.^{s}	0.082	-0.062***	0.020
Pos.	-0.038***	-0.0046	-0.042
Neg.^{vs} × Prod. Constr.	0.19***	-0.094	0.096
Neg.^{s} × Prod. Constr.	-0.021	0.033	0.012
Pos. × Prod. Constr.	0.055	0.032	0.087
Observations	24520	24520	24520

Notes: Linear Probability Model estimates, standard errors (clustered at firm level) not shown, available upon request from authors. Neg.$^{very\ strong}$, Neg.^{strong} and Positive are binary indicators refering to values -3, -2, {2,3} relative to {-1,0,1} of Covid-19 Exposure variable (scale: -3 to 3). All specifications include time and two-digit WZ08 industry fixed effects as well as controls. Controls include separate indicators for positive and negative responses to questions about business situation, business expectations, orders (all lagged by three months) and Taylor dummies. Prod. Constr. (Production Constraints) is only observed in 2020:M04 and imputed to 2020:M05. For retail/wholesale firms, this question is only asked in the online survey. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table A.14 – Effects of the Covid-19 Exposure and Supply Shifts in Manufacturing

	Planned Price Decrease	Planned Price Increase	Planned Price Change
	(1)	(2)	(3)
Neg.^{vs}	0.030	0.0039	0.034
Neg.^{s}	-0.030	-0.0058	-0.036
Pos.	-0.094	0.18	0.084
Neg.^{vs} $\times$ Cap. Ut. in %	0.00096	-0.00024	0.00072
Neg.^{s} $\times$ Cap. Ut. in %	0.0011*	0.0000018	0.0011*
Pos. $\times$ Cap. Ut. in %	0.00033	-0.0012	-0.00085
Observations	24543	24543	24543

Notes: Linear Probability Model estimates, standard errors (clustered at firm level) not shown, available upon request from authors. Neg.$^{very\ strong}$, Neg.^{strong} and Positive are binary indicators refering to values -3, -2, {2,3} relative to {-1,0,1} of Covid-19 Exposure variable (scale: -3 to 3). All specifications include time and two-digit WZ08 industry fixed effects as well as controls. Controls include separate indicators for positive and negative responses to questions about business situation, business expectations, orders (all lagged by three months) and Taylor dummies. The variable Capacity Utilization (in %) is only available in 2020:M04 and is imputed to Covid-19 Exposure in 2020:M04/M05. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table A.15 – Effects of the Covid-19 Exposure and Dependence on Imports

	Planned Price Decrease			Planned Price Increase			Planned Price Change		
Covid-19 Exposure	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Manufacturing				
Neg.^{vs}	0.055**	0.054***	0.075***	0.0025	-0.0021	-0.010	0.057**	0.052**	0.065***
Neg.^{s}	0.032	0.021	0.033*	-0.0090	-0.0093	-0.0068	0.023	0.012	0.026
Pos.	-0.041	-0.065*	-0.070*	0.078	0.087*	0.047	0.037	0.022	-0.023
Neg.^{vs} × General	0.029			-0.021			0.0072		
Neg.^{s} × General	-0.014			-0.0039			-0.018		
Pos. × General	-0.046			-0.034			-0.079		
Neg.^{vs} × from Italy		0.045			-0.020			0.025	
Neg.^{s} × from Italy		0.0095			-0.0051			0.0044	
Pos. × from Italy		-0.0059			-0.087			-0.092	
Neg.^{vs} × from China			-0.013			0.0023			-0.011
Neg.^{s} × from China			-0.028			-0.014			-0.042
Pos. × from China			0.012			0.042			0.055
Observations	19241	19241	19241	19241	19241	19241	19241	19241	19241
					Retail/Wholesale				
Neg.^{vs}	0.15***	0.14***	0.11***	-0.077***	-0.061***	-0.052**	0.068**	0.078***	0.058**
Neg.^{s}	0.083***	0.065***	0.066***	-0.0053	-0.023	-0.022	0.078**	0.042	0.045
Pos.	-0.054***	-0.032*	-0.044**	0.090*	0.096**	0.090**	0.037	0.064*	0.046
Neg.^{vs} × General	0.0053			0.029			0.034		
Neg.^{s} × General	-0.045			-0.025			-0.070*		
Pos. × General	0.026			0.0017			0.028		
Neg.^{vs} × from Italy		0.028			0.0074			0.035	
Neg.^{s} × from Italy		-0.027			0.010			-0.017	
Pos. × from Italy		-0.022			-0.016			-0.038	
Neg.^{vs} × from China			0.12***			-0.022			0.095**
Neg.^{s} × from China			-0.033			0.0054			-0.028
Pos. × from China			0.016			0.0027			0.018
Observations	20620	20628	20628	20620	20628	20628	20620	20628	20628

Notes: Linear Probability Model estimates, standard errors (clustered at firm level) not shown, available upon request from authors. Neg.$^{very\ strong}$, Neg.^{strong} and Positive are binary indicators refering to values -3, -2, {2,3} relative to {-1,0,1} of Covid-19 Exposure variable (scale: -3 to 3). All specifications include time and two-digit WZ08 industry fixed effects as well as controls. Controls include separate indicators for positive and negative responses to questions about business situation, business expectations, orders (all lagged by three months) and Taylor dummies. The variables Dependance on Imports, Dep. on Imports China, and Dep. on Imports Italy are only available in 2020:M04 and are imputed to Covid-19 Exposure in 2020:M05. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table A.16 – Effects of the Covid-19 Exposure and Demand Shifts

Covid-19 Exposure	Planned Price Decrease (1)	Planned Price Increase (2)	Planned Price Change (3)
		Manufacturing	
Neg.^{vs}	0.060***	0.0041	0.064***
Neg.^{s}	0.042**	-0.0096	0.033
Pos.	-0.059**	0.047	-0.012
Neg.^{vs} × Demand Red.	0.033	-0.024	0.0095
Neg.^{s} × Demand Red.	0.023	0.0031	0.026
Pos. × Demand Red.	-0.025	-0.045	-0.070
Observations	24458	24458	24458
		Retail/Wholesale	
Neg.^{vs}	0.094***	-0.061**	0.033
Neg.^{s}	0.046**	-0.0049	0.041
Pos.	-0.036**	0.084**	0.048
Neg.^{vs} × Demand Red.	0.078**	-0.0031	0.075**
Neg.^{s} × Demand Red.	0.020	-0.054	-0.034
Pos. × Demand Red.	-0.071***	-0.11	-0.18**
Observations	19910	19910	19910
		Services	
Neg.^{vs}	0.052**	-0.0063	0.046*
Neg.^{s}	0.050***	-0.023	0.027
Pos.	-0.018	0.011	-0.0064
Neg.^{vs} × Demand Red.	0.069***	-0.015	0.054*
Neg.^{s} × Demand Red.	0.030	-0.020	0.011
Pos. × Demand Red.	0.013	0.017	0.030
Observations	24180	24180	24180

Notes: Linear Probability Model estimates, standard errors (clustered at firm level) not shown, available upon request from authors. Neg.$^{very\ strong}$, Neg.^{strong} and Positive are binary indicators refering to values -3, -2, {2,3} relative to {-1,0,1} of Covid-19 Exposure variable (scale: -3 to 3). All specifications include time and two-digit WZ08 industry fixed effects as well as controls. Controls include separate indicators for positive and negative responses to questions about business situation, business expectations, orders (all lagged by three months) and Taylor dummies. The variable Demand Reduction is only available in 2020:M03 and is imputed to Covid-19 Exposure in 2020:M04/M05. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.17 – Alternative Disruptive Shock Indicators

	Planned Price Decrease		Planned Price Increase		Planned Price Change	
Covid-19 Exposure	(1)	(2)	(3)	(4)	(5)	(6)
			Manufacturing			
Neg.^{vs}	0.11***	0.090***	-0.029*	-0.014	0.082**	0.075***
Neg.^{s}	0.064***	0.034**	-0.0035	-0.0084	0.061**	0.026
Pos.	-0.070***	-0.059**	0.088**	0.077**	0.018	0.018
Neg.^{vs} × Lack of Orders	-0.034		0.025		-0.0088	
Neg.^{s} × Lack of Orders	-0.025		-0.0041		-0.030	
Pos. × Lack of Orders	0.030		-0.088**		-0.058	
Neg.^{vs} × Closure		-0.032		0.0097		-0.022
Neg.^{s} × Closure		0.11***		-0.016		0.092**
Pos. × Closure		-0.000031		-0.051		-0.051
Observations	24911	26975	24911	26975	24911	26975
			Retail/Wholesale			
Neg.^{vs}	0.17***	0.11***	-0.13***	-0.079***	0.044	0.035
Neg.^{s}	0.037	0.043**	-0.053	-0.020	-0.016	0.023
Pos.	-0.051***	-0.034**	0.13***	0.084**	0.080*	0.050
Neg.^{vs} × Lack of Orders	-0.036		0.100***		0.064	
Neg.^{s} × Lack of Orders	0.019		0.012		0.031	
Pos. × Lack of Orders	0.13		-0.10		0.028	
Neg.^{vs} × Closure		0.087***		0.028		0.12***
Neg.^{s} × Closure		0.069*		-0.011		0.059
Pos. × Closure		-0.015		0.025		0.010
Observations	12826	22185	12826	22185	12826	22185
			Services			
Neg.^{vs}	0.079***	0.095***	0.025	-0.025	0.10***	0.070***
Neg.^{s}	0.053**	0.062***	-0.043***	-0.036***	0.0097	0.026
Pos.	-0.033**	-0.010	0.026	0.018	-0.0063	0.0073
Neg.^{vs} × Lack of Orders	0.039		-0.061**		-0.022	
Neg.^{s} × Lack of Orders	0.017		0.020		0.036	
Pos. × Lack of Orders	0.13		-0.097		0.031	
Neg.^{vs} × Closure		0.041		0.028		0.069**
Neg.^{s} × Closure		-0.019		0.082		0.062
Pos. × Closure		-0.098***		-0.38***		-0.48***
Observations	24520	26155	24520	26155	24520	26155

Notes: Linear Probability Model estimates, standard errors (clustered at firm level) not shown, available upon request from authors. Neg.$^{very\ strong}$, Neg.^{strong} and Positive are binary indicators refering to values -3, -2, {2,3} relative to {-1,0,1} of Covid-19 Exposure variable (scale: -3 to 3). All specifications include time and two-digit WZ08 industry fixed effects as well as controls. Controls include separate indicators for positive and negative responses to questions about business situation, business expectations, orders (all lagged by three months) and Taylor dummies. The variable Lack of Orders (firms that are constrained in production due to lack of orders) is only available in 2020:M04 and is imputed to Covid-19 Exposure in 2020:M05. The Closure variable is observed in 2020:M04/M05. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Lockdown accounting[1]

Charles Gottlieb,[2] Jan Grobovšek,[3] Markus Poschke[4] and Fernando Saltiel[5]

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We measure the effect of lockdown policies on employment and GDP across countries using individual- and sector-level data. Employment effects depend on the ability to work from home, which ranges from about half of total employment in rich countries to around 35% in poor countries. This gap reflects differences in occupational composition, self-employment levels, and individual characteristics across countries. GDP effects of lockdown policies also depend on countries' sectoral structure. Losses in poor countries are attenuated by their higher value-added share in essential sectors, notably agriculture. Overall, a realistic lockdown policy implies GDP losses of 20-25% on an annualized basis.

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1 Introduction

Many countries are implementing social distancing and lockdown policies to tame the spread of Covid-19. These measures involve the closure of workplaces to limit interpersonal contact. They may remain in place in some form for a significant amount of time (Kissler et al., 2020). So far, 114 countries have implemented policies that require closing or work from home for all but essential workplaces (Hale et al., 2020). We measure the effect of such lockdowns on labor input and GDP for a large set of countries, with a focus on the determinants of their variation with country income per capita.

In sectors required to shutter workplaces, work can only be conducted from workers' homes. The ability to work from home (WFH) therefore is a key factor determining the economic consequences of social distancing policies. It has been measured for the United States (Dingel and Neiman, 2020) and for a set of European countries.[1] These studies have found that around 40% of jobs could potentially be carried out from home.[2] Evidence on the ability to work from home in poorer countries is more scant.[3] Yet, it is sorely needed, as poor countries are also implementing social distancing measures, often drastic ones.[4]

Lockdown policies by design affect different sectors differently: While some sectors are deemed essential and are permitted to remain open, non-essential sectors are closed down, in particular if their nature makes social distancing hard. The effects of sectoral lockdown policies have been evaluated for a few specific countries (in particular Fadinger et al. (2020) for Germany and Barrot et al. (2020) for France and a set of European countries), but little is known about their effect elsewhere, in particular in poorer countries.

Our paper addresses these issues, and makes two contributions. Our first contribution is to build a measure of WFH ability using individual-level data on job task content from countries across the income distribution, validate it, and analyze its variation with country income. To start, we show that individual WFH ability varies systematically by occupation, education, gender, and self-employment status, in similar ways across ten countries. We then show that a measure of WFH ability built using these individual characteristics strongly predicts the likelihood of remaining employed during the pandemic in the most recent survey data from the US and Peru.

Next, we compute a measure of WFH ability for 57 countries using harmonized individual-level data. We find that WFH ability is significantly lower in poor countries, both at the aggregate level and for many population subgroups. In a decomposition, we show that this is driven by differences in employment composition and demographics across countries: Workers in poor countries are more likely to be in occupations with low ability to WFH, they are more often self-employed, and they have lower levels of education, all of which are associated with lower WFH ability. Cross-country differences in WFH ability are thus closely associated with the systematic changes in the employment structure that occur with development (Gollin, 2008; Duernecker and Herrendorf, 2016).

[1] See e.g. Adams-Prassl et al. (2020), Barrot et al. (2020), Boeri et al. (2020), del Rio-Chanona et al. (2020), Fadinger et al. (2020), Koren and Pető (2020) and Mongey and Weinberg (2020).

[2] Bick et al. (2020) find that almost three quarters of workers in these jobs did in fact exclusively work from home in May 2020, when many US states were implementing lockdowns. This compares to only 8% of employees working from home full time in February 2020, with some more working from home part of the time. In line with this, Hensvik et al. (2020) find that in the US, the share of working hours performed from home in 2011 to 2018 is around 15%. Mas and Pallais (forthcoming) report a similar number. These numbers exhibit substantial variation across occupations.

[3] We build on Saltiel (2020), who first documented WFH ability for countries at various levels of development. More recently, Hatayama et al. (2020) consider two additional data sources.

[4] Twenty-two low- and lower-middle income countries have implemented lockdowns with a stringency index above 80 (corresponding to the 75^{th} percentile of the world distribution) (Hale et al., 2020).

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Our second contribution consists in measuring the potential effects of four different lockdown policies on employment and GDP for 85 countries using a multi-sector model, the WFH ability measure, and disaggregated data from each country. In a decomposition, we show that while low WFH ability in poor countries implies a larger effect of lockdowns on their output and employment, their sectoral structure favors them: high employment and value added shares in sectors considered to be essential and therefore only marginally affected by lockdowns (in particular agriculture) cushion the effect of lockdowns. High income countries also have a favorable sectoral structure, with value added concentrated in high-end service sectors conducive to WFH. Middle-income countries, meanwhile, are disproportionately negatively affected by lockdowns, as their economic activity is centred on non-essential activities with low WFH ability. In other words, the effects of lockdowns are closely associated with structural change – the systematic changes in the sectoral structure of economies that occur with development (Kuznets, 1973; Gollin et al., 2002; Restuccia et al., 2008; Herrendorf et al., 2014; Duarte and Restuccia, 2019).

Beyond these contributions, our WFH measure has two advantages. First, it is built using data from countries of widely varying levels of income per capita, and not only one specific rich country. Second, the fact that it reflects variation of WFH ability across 72 detailed demographic groups implies that it can be used for the analysis of very fine-grained policies. It can thus serve as a valuable input in evaluating the costs of potential lockdown policies, in the quantitative analysis of lockdown and reopening policies, and in efforts to project the recovery.[5] Detailed data from the paper are available at `https://work-in-data.shinyapps.io/work_in_data/`. The website presents our measures of country-level WFH ability by detailed employment subgroups, and allows for downloads. It also contains a "lockdown simulator" that illustrates the effects of arbitrary sectoral lockdown policies (set by the user) by country.

The paper is structured as follows. In Section 2, we measure the ability to work from home at the individual level. In Section 3, we study how the share of work from home employment varies with country income per capita. In Section 4, we quantify the costs of lockdown policies on aggregate employment and GDP through the lens of a multi-sector model.

2 Measurement of the ability to work from home

2.1 Data sources

To measure the feasibility of working from home, we use data from the first two rounds of the STEP household survey, covering workers in urban areas across ten countries in 2012-2013, including Armenia, Bolivia, China (Yunnan Province), Colombia, Georgia, Ghana, Kenya, Laos, Macedonia and Vietnam. STEP surveys are representative of the working age (15-64 year old) population in urban areas across these countries. We use data on the main respondents. We observe their age, gender and educational attainment, along with information on their labor market outcomes, including their current employment status and whether they have worked in the past twelve months. Furthermore, we observe whether they work as wage employees, in self-employment or in unpaid family work. We also observe workers' occupations under the harmonized ISCO-08 classification, along with measures of tasks they perform at work.[6,7]

[5] See e.g. Alon, Kim, Lagakos and VanVuren (2020); Alvarez et al. (2020); Farhi and Baqaee (2020); Brotherhood et al. (2020); Eichenbaum et al. (2020); Glover et al. (2020); Hall et al. (2020); Jones et al. (2020); Petrosky-Nadeau and Valletta (2020).

[6] STEP includes information on workers' sector of employment in four categories: agriculture, fishery and mining; manufacturing and construction; commerce; and other services.

[7] We restrict the analysis to respondents who have been employed in the past twelve months. We further drop individuals in unpaid family work or in the armed forces.

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2.2 Work from home definition

Our approach to measuring the feasibility of working from home follows Dingel and Neiman (2020) in aiming to capture whether workers could potentially work from home, and not whether they have done so in the past. STEP data allow us to construct a WFH measure across a wide range of countries by leveraging comparable worker-level data on job task content. Since STEP covers countries whose GDP per capita ranges from $4,300 to upwards of $15,000, our measure represents an important input for cross-country comparisons.

Our preferred definition rules out working from home if a worker performs any of the following tasks at work: lifting anything heavier than 50 pounds, repairing/maintaining electronic equipment, operating heavy machinery or industrial equipment, or reporting they have a physically demanding job. Our definition also rules out work from home for those indicating that contact with customers is very important, unless they also report using e-mail for their job.

2.3 Determinants of the ability to work from home

In the first column of Table 1, we present average WFH feasibility across occupations. Overall, 45% of urban employment could be done remotely in the ten STEP countries.[8] The feasibility of WFH varies strongly across broad occupation groups. While the majority of jobs in managerial and professional occupations and in clerical support (groups 1-4) can be carried out from home, few jobs in elementary occupations, crafts, or occupations involving plant or machine operation (groups 6-9) can be done remotely.[9]

The ability to WFH varies not only with an individual's occupation, but also across other personal and job characteristics. In the second and third columns of Table 1, we show that educational attainment is a strong predictor of the ability to work from home, as the estimated share for high school completers surpasses that of dropouts by 20 percentage points. The estimated WFH shares are statistically different in all but two broad occupation groups (craft workers and elementary occupations). Similarly, the ability to WFH for wage employees (50%) is far higher than that for self-employed workers (35.3%). The difference is statistically significant for managers, technicians, services/sales workers and plant/machine operators. Lastly, women have a far higher ability to WFH (51.5%) than men (37.4%). These differences are significant in six of the nine broad occupation groups.

Table 1: Feasibility of working from home by definition and one-digit occupation

		Educational Attainment		Self-Employment		Gender	
	Full Sample	HS Graduate	HS Dropout	Wage Employee	Self-Employed	Female	Male
One-Digit Occupation	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Managers	0.655	0.682	0.450	0.731	0.561	0.690	0.634
Professionals	0.622	0.633	0.416	0.625	0.591	0.628	0.612
Technicians and Associate Professionals	0.585	0.620	0.398	0.601	0.476	0.634	0.542
Clerical Support Workers	0.691	0.716	0.574	0.694	0.634	0.739	0.608
Services and Sales Workers	0.385	0.425	0.350	0.427	0.346	0.385	0.383
Skilled Agricultural, Forestry and Fishery Workers	0.227	0.368	0.206	0.246	0.226	0.317	0.134
Craft and Related Trades Workers	0.304	0.294	0.311	0.277	0.331	0.518	0.172
Plant and Machine Operators, and Assemblers	0.250	0.286	0.210	0.271	0.188	0.444	0.204
Elementary Occupations	0.379	0.416	0.362	0.391	0.322	0.518	0.213
Sample Average	0.450	0.532	0.334	0.500	0.353	0.515	0.374
Observations	17598	10093	7505	11099	6499	9355	8243

Source: Skills Toward Employability and Productivity (STEP) Survey. Note: Table 1 documents the share of workers who can work from home by one-digit occupation, educational attainment, self-employment and gender. Results are weighted using sample weights to represent the working-age population of 15-64 year olds in each STEP country.

[8] Given our interest in developing a cross-country WFH measure, we rely on STEP data instead of O*NET. We present a direct comparison of task measures in the O*NET and STEP in Table A1. In Table A2, we report the estimated WFH employment share applying our preferred definition to the O*NET classification.

[9] We alternatively consider a measure in which we entirely rule out working from home unless workers report using e-mail at work. This reduces the overall WFH share to 8.8%, in line with Saltiel (2020).

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The evidence presented so far shows that both occupations and workers' characteristics are important determinants of WFH ability. To further understand the contribution of different factors to the ability to WFH, we estimate the following regression:

$$WFH_{iock} = \beta \boldsymbol{X}_i + \gamma_o + \lambda_c + \theta_k + \varepsilon_{iock} \tag{1}$$

where $\boldsymbol{X}_i$ represents a vector of worker i's observed characteristics, including educational attainment, age, gender and self-employment status; γ_o captures occupation fixed effects; λ_c denotes country fixed effects; and θ_k captures fixed effects for the four industries reported in STEP. Coefficient estimates shown in Table A3 indicate that higher-educated workers, women and wage employees are far more likely to work from home even within narrowly defined occupations, echoing the bivariate patterns reported in Table 1.[10]

To assess the relative importance of different determinants of the ability to work from home, we perform a variance decomposition. Workers' characteristics on their own account for 3-4% of the variance in the WFH measure, along with an additional 3-4% through the covariance with occupational categories. While one-digit occupational groups additionally account for 3.8% of the variance of the WFH measure, industries account for a negligible share (0.4%) of the variance (see Table A4 for full results).[11] The contribution of country fixed effects to the WFH variance is minor (1.6%), supporting our approach in Section 3, where we extend the STEP-based WFH measure to a much larger set of countries.

2.4 Validation

To assess the validity of our WFH measure as a predictor of employment, we take advantage of the April survey rounds of the *Encuesta Permanente de Empleo* (EPE) in Peru and the Current Population Survey in the US. Both surveys follow a rotating panel design, thus allowing us to observe workers' employment outcomes in their latest pre-Covid survey round.[12] We focus on individuals who were employed in the corresponding pre-Covid survey wave and examine whether they had a job in April 2020. We observe their initial occupation, industry and self-employment status, along with observed characteristics, such as gender, age and educational attainment.

For each worker in the Peru and US samples, we impute a predicted WFH score using the estimated coefficients from equation (1) for the STEP sample. We then examine the relationship between workers' predicted WFH feasibility and the likelihood they were employed in April, 2020. Our approach thus resembles Adams-Prassl et al. (2020) and Bick et al. (2020), yet provides novel evidence in a developing country. We control for workers' gender to account for disparities in labor market outcomes during Covid (Alon, Doepke, Olmstead-Rumsey and Tertilt, 2020), and for the essential nature of the sector, as WFH should be a stronger predictor of employment in non-essential industries.[13] Conditional on these two factors, an increase in the WFH score from 0 to 100 is associated with a 91 percentage point increased likelihood of remaining employed through April in the US, and 71pp in Peru. In the US, we further find that WFH ability is a stronger predictor of employment outcomes for workers in non-essential

[10] We do not find significant differences in WFH ability by age and thus ignore it in the rest of the analysis.

[11] Three-digit occupations explain a larger share of the variance of workers' WFH ability. Comparability across data sources requires us to focus on one-digit occupations in the rest of the paper.

[12] The structure of the EPE survey implies that April 2020 respondents were previously surveyed in April 2019. The 4-8-4 design in the CPS implies that we initially observe workers in different months in 2019 and early 2020. We do not use information from March 2020, as employment outcomes may have already been affected by the health shock.

[13] See Section 4.3 for information on essential sectors.

sectors, who could only work form home during lockdowns.[14] The strong association of our WFH measure with employment outcomes in countries as different as Peru and the US provides evidence of its predictive power, even beyond the STEP survey countries.

3 The ability to work from home across countries

In this section, we combine the measures of workers' ability to work from home from Section 2 with data on employment shares for detailed population subgroups for a wide range of countries to study how the ability to work from home varies across countries. In doing so, we narrow the analysis to urban employment, since urban areas are the focus of distancing policies as their higher population density provides favourable ground for the spread of contagious diseases (Alirol et al., 2011; Yang et al., 2015; Diop et al., 2020).

3.1 Data and measurement

We built a micro-dataset that contains information on labor market outcomes for 18 million working-age individuals across 57 countries ranging from Ethiopia to Luxembourg. To do so, we harmonized 617 country-year household and labor force surveys.[15] Beyond information on individual demographics, the dataset contains detailed information on workers' education, employment status, occupation, and sector of activity. In contrast to other sources of data on employment by sector or occupation, such as the International Labor Organization (ILO), our dataset contains individual level information and thus allows us to study employment at a highly disaggregated level.[16]

Section 2 shows that the ability to WFH varies significantly both across occupations, and with worker characteristics such as education, gender, and employment status. To account for these differences, we partition workers into 72 groups indexed by j, resulting from the full interaction of one-digit occupation (nine levels), education (high school graduates vs. dropout), gender, and employment status (self vs. wage employment). Denoting the share of type j workers that can work from home in the STEP data by η_j,[17] and the employment share of worker type j in country c by μ_{cj}, we measure the share of employment that can work from home in country c, h_c, as

$$h_c = \sum_j \mu_{cj}\eta_j. \tag{2}$$

We measure h_c for 57 countries and analyze how and why WFH ability of urban workers varies with country income.

3.2 Cross-country differences

Figure 1a provides a visual representation of the share of urban employment that can work from home by country income level. It reveals a large gap in WFH ability across country income groups: while in high-income countries, about half of urban employment can work from home,

[14] We fail to find significant interactions of the essential nature of an industry and WFH scores in Peru. This may be explained by the strict lockdown put in place (Hale et al., 2020). See Table A5 for full results.

[15] Table A6 in Appendix A.2 provides an overview of data sources.

[16] At its highest level of detail, the ILO data on employment patterns in urban areas provides information along up to two dimensions: either occupation and sex, or economic activity and sex.

[17] Table A7 reports the values.

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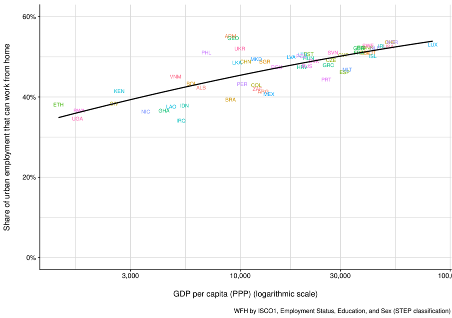
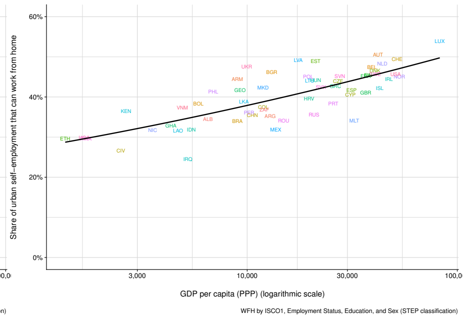
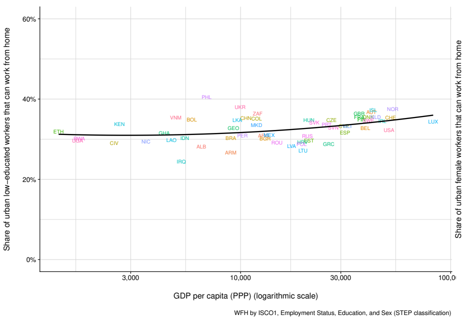
Figure 1: Ability to work from home of urban employment



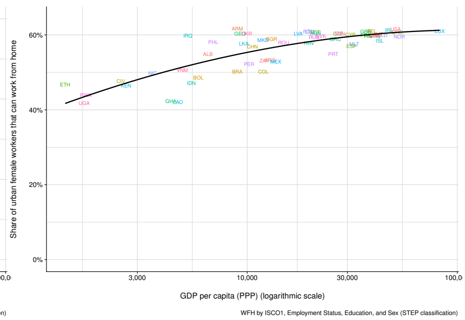
(a) Urban employment, total



(b) Urban self-employed workers



(c) Urban low-skilled workers



(d) Urban female workers

Figures 1a, 1c, 1b and 1d use WFH ability measures that are occupation, employment status, sex and education specific. Their values are reported in Table A2.

only a third can do so in low-income countries. The income elasticity of the share of urban employment that can work from home amounts to 0.046.[18]

Our individual-level analysis of the ability to work from home in Section 2 highlights that three subgroups are less able to work from home: the self-employed, the low-skilled, and male workers. How does WFH ability of these subgroups differ across country income levels? Figure 1 shows that the WFH ability of female and self-employed workers is substantially lower in low-income countries compared to high-income countries, with a gap of 20 percentage points between WFH ability of these groups in the richest and the poorest countries. In contrast, WFH ability of low-skill workers does not vary systematically with country income per capita.

3.3 Sources of differences

Cross-country differences in the ability to work from home by construction reflect differences in employment composition, which differs systematically with development, given the well-known changes in the sectoral and occupational structure of economies with development (Gollin et al., 2002; Gollin, 2008; Herrendorf et al., 2014; Duernecker and Herrendorf, 2016; Duarte and Restuccia, 2019). In particular, a large share of urban workers in low-income countries are self-employed and pursue elementary occupations or work as service or sales workers. In fact, these two occupation groups account for over half of employment in low-income countries, while they amount to only one fifth of employment in high-income countries (Gottlieb et al., 2020).

[18] See Table A8 for full regression results and income elasticities for urban subgroups and aggregate employment.

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Table 2: Decomposition of work from home employment by worker characteristics

Panel A: Work from home urban employment

	data WFH	counterfactual WFH				
quintile	(h_c)	occupation ($\hat{h}_c^o$)	employment type ($\hat{h}_c^s$)	gender ($\hat{h}_c^g$)	education ($\hat{h}_c^e$)	N
Q1	0.38	0.44	0.41	0.39	0.45	12
Q2	0.48	0.47	0.49	0.48	0.47	11
Q3	0.47	0.47	0.46	0.47	0.46	11
Q4	0.49	0.47	0.48	0.49	0.48	11
Q5	0.53	0.48	0.52	0.53	0.50	12

Panel B: Work from home employment

	data WFH	counterfactual WFH				
quintile	(h_c)	occupation ($\hat{h}_c^o$)	employment type ($\hat{h}_c^s$)	gender ($\hat{h}_c^g$)	education ($\hat{h}_c^e$)	N
Q1	0.35	0.42	0.39	0.35	0.43	12
Q2	0.47	0.45	0.47	0.46	0.45	11
Q3	0.45	0.45	0.44	0.45	0.43	11
Q4	0.47	0.45	0.46	0.47	0.46	11
Q5	0.51	0.46	0.50	0.51	0.48	12

Note: This Table reports the average work from home employment share h_c by quintiles of the income distribution. Columns (2)-(5) report the counterfactual work from home employment. For instance $\hat{h}_c^s$ is the counterfactual employment when the distribution of wage and self-employment is held constant across countries. The last column reports the sample size for each income group (N). Panel A reports the decomposition for urban employment, panel B reports the decomposition for total employment.

In contrast, the majority of urban employment in high-income countries is concentrated in managerial and professional occupations, which are more amenable to work from home. These jobs also are more likely to be carried out by workers with higher education and are also more likely to be done by females.

We next provide a quantitative assessment of the importance of these differences in the composition of urban employment. To do so, we compute, for each country, a counterfactual share of WFH employment that would prevail if its distribution of one characteristic is set to the cross-country average. We illustrate the calculation for the occupation distribution. We index the 8 combinations of attributes other than occupations by g. Let the employment distribution over occupations in country c be μ_{co}, and its average across all countries $\bar{\mu}_o$. To measure the importance of variation in the occupation distribution across countries, we compute a counterfactual WFH measure ($\hat{h}_c^o$) using the average occupation distribution:

$$\hat{h}_c^o = \sum_o \sum_g \mu_{cog} \frac{\bar{\mu}_o}{\mu_{co}} \eta_{og}, \qquad \bar{\mu}_o = \frac{1}{C} \sum_c \sum_g \mu_{cog} \tag{3}$$

where η_{og} is the share of WFH employment of workers in occupation o with characteristics g. We proceed analogously for employment status, education, and gender.

In Table 2, we report the counterfactual WFH employment levels by quintiles of the income per capita distribution for each of the worker characteristic we use to predict the WFH ability, namely occupation, employment status, education and gender.

Our decomposition shows that cross-country differences in the urban ability to WFH are largely driven by differences in occupation and educational attainment (see Panel A). Differences in employment status matter somewhat, while differences in gender composition play second fiddle. For instance, if the distribution of occupations were identical for all countries, the share of WFH employment would be on average 6 p.p. (=0.44-0.38) higher in the poorest quintile, and 5 p.p. lower (=0.53-0.48) in the richest quintile. By this metric, the variation in the occupation distribution on its own accounts for 73% (=1-(0.48-0.44)/(0.53-0.38)) of the

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interquintile difference in urban WFH ability.

The distribution of education also plays a major role. If it were common across countries, the WFH ability in countries of the lowest quintile would be 7 p.p. higher, while it would be 3 p.p. lower in countries in the highest quintile. On its own, the distribution of education thus explains 67% of the interquintile difference.

Hence, differences in WFH ability across country-income levels are largely driven by differences in the distribution of occupations and individual characteristics, in particular educational attainment. These findings are similar at the country level (see Panel B).

4 Sectoral lockdown policies

In this section, we evaluate the impact of various sectoral lockdown policies on aggregate effective employment and GDP for 85 countries across the income per capita distribution. These effects will depend on a country's WFH ability, but also on its industrial structure, since lockdown policies are typically specified on a sectoral level.

4.1 Model description

We use a multi-sector model to simulate the effects of lockdowns. We assume that following a lockdown, the ratio of effective employment relative to trend in country c equals

$$n^c = \sum_{i=1}^{I} n_i^c \mu_i^c = \sum_{i=1}^{I} \left[1 - \lambda_i \left(1 - h_i^c\right)\right] \mu_i^c = \underbrace{\sum_{i=1}^{I} \left(1 - \lambda_i\right) \mu_i^c}_{\equiv n_w^c} + \underbrace{\sum_{i=1}^{I} \lambda_i h_i^c \mu_i^c}_{\equiv n_h^c} \tag{4}$$

where n_i^c is the level of employment in sector i relative to trend and μ_i^c the pre-shock employment share of sector i. We posit that the lockdown policy shuts down a fraction $\lambda_i \in [0, 1]$ of workplaces in sector i. Locked down employment can be substituted at the rate $h_i^c \in [0, 1]$, the share of WFH employment in sector i. Implicitly, we assume that work from home is as efficient as regular work. The last equality of (4) separates effective labor into regular work, n_w^c, and aggregate work from home, n_h^c. In the absence of lockdowns, $n^c = n_w^c = 1$ and $n_h^c = 0$.

GDP relative to trend is given by

$$y^c = \prod_{i=1}^{I} \left(n_i^c\right)^{\nu_i^c} = \prod_{i=1}^{I} \left[1 - \lambda_i \left(1 - h_i^c\right)\right]^{\nu_i^c} \tag{5}$$

where $\nu_i^c \in [0, 1]$ is the nominal value added share of sector i, $\sum_i \nu_i^c = 1$. In Appendix A.3, we show how to derive equation (5) from a model featuring intersectoral trade in intermediate inputs. The central assumption is that labor and capital, post-shock, cannot move across sectors and that the sectoral drop in capital utilization is proportional to that of labor.[19,20] Simulation results describe employment and GDP changes relative to trend while a lockdown policy is in

[19] Our analysis abstracts from factors other than the lockdown that affect employment and output. Such factors could be, among others, reductions in labor supply (voluntary or for health reasons), financial frictions, or frictions in final or intermediate goods markets. The model does, however, capture adjustments in the demand and supply of final and intermediate goods under the conditions spelled out in Appendix A.3.

[20] Fadinger et al. (2020) use a similar approach, with the difference that capital utilization does not change. The model in Barrot et al. (2020) features non-unitary elasticities of substitution both between intermediate inputs and between final goods, while capital utilization is implicitly proportional to labor.

place. Hence, ignoring dynamic adjustments, the change in annual GDP with a two-month lockdown would be one-sixth of the reported change.

4.2 Data and measurement

We define sectors according to the one-digit ISIC classification. Country-specific sectoral value added shares ν_i^c are obtained from the United Nations Statistics Division and the World Input Output Database.[21]

We construct the country-sector-specific WFH rate h_i^c by combining the WFH ability computed in the STEP data and employment shares by one-digit ISIC sector and one-digit ISCO occupation from ILO.[22] We use the ILO data rather than the harmonized micro dataset employed in Section 3 in order to maximize country coverage. The downside is that the ILO data do not allow to use the full heterogeneity of WFH ability presented in Table A7, but only variation across occupations and sectors. Thus, we compute the WFH ability by occupation o and broad sector b, $\eta_{o,b}$.[23] Then, $h_i^c = \sum_o \mu_{oi}^c \eta_{ob}$, $\forall i \in b$, where μ_{oi}^c is the employment share of occupation o in sector i. In total, we can measure ν_i^c and h_i^c for 85 countries.

4.3 Lockdown policies

We study four lockdown policies. In the first "complete" lockdown policy, workplaces in all sectors are shut down: $\lambda_i = 1, \forall i$. In this case, the fraction of aggregate effective labor coincides with the aggregate WFH rate, namely $n^c = \sum_{i=1}^I h_i^c \mu_i^c$. In the second "non-agricultural" lockdown policy, all sectors are shut down with the exception of agriculture. This policy fleshes out the importance of a sector considered to be essential in most countries and therefore allowed to operate normally. The third and fourth lockdown scenarios replicate policies actually in place. The "hard" lockdown leaves open a limited number of essential sectors, $\lambda_i = 0$, closes down non-essential sectors, $\lambda_i = 1$, and partially shuts down workplaces in other sectors, $\lambda_i \in (0, 1)$. We construct λ_i using the index of essential sectors assembled by Fana et al. (2020), who document activities exempt from the strict March 2020 lockdown decrees in Germany, Italy, and Spain.[24] Finally, we consider a "soft" lockdown policy, designed to capture the situation as shutdowns are eased. We define it as lifting three-quarters of the employment restrictions in agriculture and industry, and half the restrictions in services. The latter are being lifted more slowly as they involve more interpersonal interaction, which fosters the risk of virus transmission. This leaves substantial restrictions in sectors such as accommodation and food services, education, and arts, entertainment and recreation. Appendix A.4 summarizes our approach and presents the values for λ_i under each policy.

4.4 Results

Figure 2 plots aggregate effective employment and GDP relative to trend against countries' income level. The top panes portray the complete lockdown. Here, the aggregate employment

[21] For each country, we consider the most recent observation over the period 2010-2019. Whenever possible, we follow the ISIC Revision 4 classification. For a few countries, we use the ISIC Revision 3.1 classification and impute the missing data.

[22] For each country, we consider the most recent observation over the period 2010-2019. In most countries, including poor ones, the data are from 2017.

[23] The broad sectors are agriculture, fishing and mining; manufacturing, construction and transportation; commerce; and other services. Figures shown in Table A9.

[24] Lockdown policies were similar in North American jurisdictions, like the State of New York in the US and Ontario and Québec in Canada.

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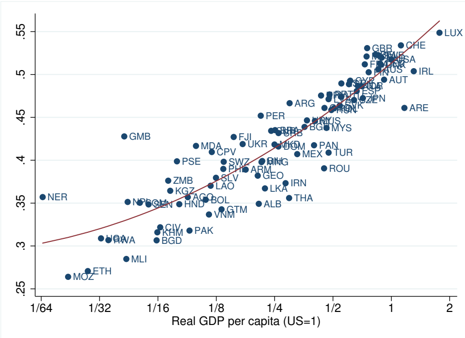
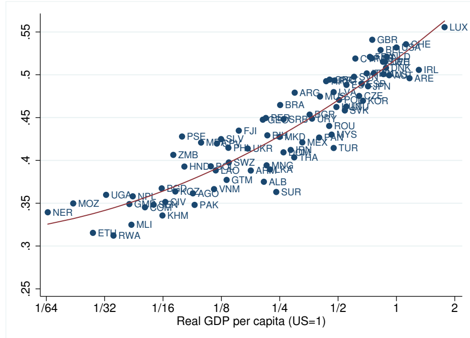
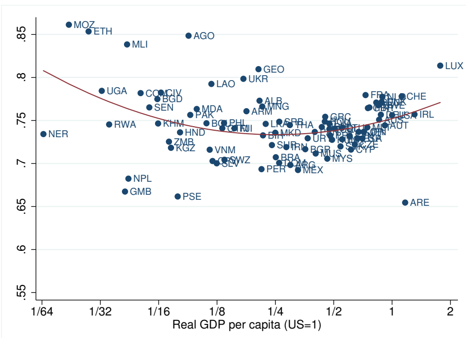
Figure 2: Effective employment and GDP relative to trend under complete and hard lockdowns



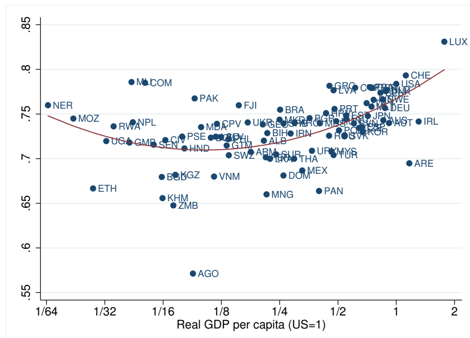
(a) Employment, complete lockdown



(b) GDP, complete lockdown



(c) Employment, hard lockdown



(d) GDP, hard lockdown

Real GDP per capita of each country corresponds to the 2017 PPP-adjusted series from Feenstra et al. (2015), normalized to the U.S. The trend line is a quadratic fit of the logarithm of real GDP per capita.

decline corresponds to the fraction of aggregate employment that cannot work from home. Employment declines most in poorer countries, which have a lower aggregate WFH capacity. The change in GDP mirrors this pattern. The bottom panes portray the hard lockdown. Strikingly, for this realistic policy, effective employment and GDP no longer increase in income. Rather, they exhibit a U-shape. Figure A1 shows that results are similar for the non-agricultural and soft lockdowns.

Table 3 presents the effect of each lockdown policy by quintile of the distribution of country income per capita. The first two columns show, for each quintile, average GDP and employment. Columns (3) and (4) disaggregate employment into the regular and work from home components. Under the complete lockdown, employment drops to 33.6% of its pre-shock level in the poorest quintile, compared to 50.8% in the richest quintile. Put differently, the aggregate WFH capacity is roughly one-third in poor countries and one-half in rich countries. This is in line with the results presented in Section 3.

The pattern of employment by country income level is completely reversed under the non-agricultural lockdown policy: in this scenario, employment declines least in the poorest countries, due to their high employment shares in agriculture (shown in column 3). GDP declines more than employment, in particular in the poorest countries. This is because the agricul-

Table 3: Average impact of lockdown policies on country income groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Complete lockdown						
	y	n	n_w	n_h	G	H	V
Q1	0.356	0.335	0.000	0.335	-0.192	-0.077	-0.114
Q2	0.401	0.382	0.000	0.382	-0.070	-0.032	-0.039
Q3	0.432	0.419	0.000	0.419	0.002	-0.009	0.011
Q4	0.473	0.465	0.000	0.465	0.096	0.040	0.056
Q5	0.517	0.511	0.000	0.511	0.184	0.083	0.101
	Non-agricultural lockdown						
	y	n	n_w	n_h	G	H	V
Q1	0.508	0.631	0.377	0.254	0.018	-0.066	0.084
Q2	0.488	0.589	0.265	0.324	-0.019	-0.026	0.007
Q3	0.478	0.522	0.132	0.390	-0.039	-0.009	-0.030
Q4	0.499	0.520	0.073	0.448	0.004	0.038	-0.034
Q5	0.526	0.527	0.021	0.506	0.058	0.079	-0.021
	Hard lockdown						
	y	n	n_w	n_h	G	H	V
Q1	0.717	0.756	0.597	0.159	-0.016	-0.025	0.009
Q2	0.716	0.753	0.572	0.181	-0.018	-0.011	-0.007
Q3	0.716	0.726	0.518	0.209	-0.017	-0.003	-0.014
Q4	0.743	0.732	0.497	0.235	0.020	0.014	0.006
Q5	0.765	0.758	0.516	0.242	0.049	0.033	0.016
	Soft lockdown						
	y	n	n_w	n_h	G	H	V
Q1	0.909	0.908	0.841	0.067	-0.002	-0.005	0.003
Q2	0.910	0.905	0.828	0.078	-0.001	-0.002	0.001
Q3	0.908	0.895	0.807	0.088	-0.003	-0.001	-0.002
Q4	0.912	0.897	0.799	0.098	0.001	0.003	-0.001
Q5	0.917	0.902	0.798	0.104	0.007	0.006	0.000

Note: The results indicate averages over quintiles of countries ordered by real GDP per capita in 2017. Each bin consists of 17 countries. Average empirical GDP per capita expressed relative to the U.S. equals 0.05 in Q1, 0.14 in Q2, 0.30 in Q3, 0.55 in Q4, and 0.95 in Q5. The terms G, H and V are as defined in equation (6).

tural employment share generally exceeds the sector's value added share (Restuccia et al., 2008; Gollin et al., 2014; Herrendorf and Schoellman, 2015). The largest declines in employment and and value added now occur in middle-income countries.

This pattern is preserved under the realistic hard lockdown scenario, as also seen in Figure 2. Here, employment declines by 20 to 30%, with the largest declines in middle-income countries.[25] Although poorer countries have the lowest ability to WFH, their employment share in essential sectors is highest. As a result, they maintain larger regular employment n_w, and their total employment n is comparable to the richest countries, despite lower WFH ability.[26] This pattern is entirely driven by the large agricultural employment share in poor countries. Since the value added share of agriculture in poor countries falls short of its employment share, output losses significantly exceed employment in the poorest countries, and are similar to those in middle-income countries. Results are qualitatively similar for the soft lockdown scenario.

[25] Model-implied employment reductions in the hard lockdown policy are close to those observed during the pandemic. For instance, US employment declined by 20% from February to April 2020 according to CPS data (Bick and Blandin, 2020).

[26] On average, WFH ability does not differ significantly between essential and non-essential sectors (Figure A2).

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4.5 Sources of output changes: a decomposition

The results in Table 3 show that the effects of a lockdown policy in a particular country reflect not only the ability to work from home, but also the sectoral structure of its economy. We now assess the quantitative importance of these two components.

Let $\overline{y}$ be the GDP ratio for a reference economy with average value added shares and work from home rates across the sample of countries, $\overline{\nu}_i = \frac{1}{C}\sum_{c=1}^{C}\nu_i^c$, $\overline{h}_i = \frac{1}{C}\sum_{c=1}^{C}h_i^c$. Then, the log of the GDP ratio y^c for any country c relative to the reference economy can be decomposed as

$$\underbrace{\ln y^c - \ln \overline{y}}_{\equiv G^c} = \underbrace{\sum_{i=1}^{I}\left(\frac{\nu_i^c + \overline{\nu}_i}{2}\right)(\ln n_i^c - \ln \overline{n}_i)}_{\equiv H^c} + \underbrace{\sum_{i=1}^{I}\left(\frac{\ln n_i^c + \ln \overline{n}_i}{2}\right)(\nu_i^c - \overline{\nu}_i)}_{\equiv V^c}, \tag{6}$$

where

$$H^c \approx \sum_{i=1}^{I}\left(\frac{\nu_i^c + \overline{\nu}_i}{2}\right)\lambda_i\left(h_i^c - \overline{h}_i\right), \qquad V^c \approx \sum_{i=1}^{I}\lambda_i\left(\frac{h_i^c + \overline{h}_i}{2} - 1\right)(\nu_i^c - \overline{\nu}_i).$$

The term H^c captures the (value added-weighted) effect of differences in WFH ability, which affect effective sectoral employment. The term V^c captures the effect of differences in the weight of sectors across countries. A country thus experiences a small GDP drop relative to an "average" economy if it exercises relatively more work from home ($H^c > 0$), if it has a relatively high value-added share in sectors maintaining high employment ($V^c > 0$), or both. Our decomposition separates these factors.

We find that under the hard (soft) lockdown, the variance of V^c accounts for 80.6% (91.0%) of the variance of G^c, compared to 18.8% (22.8%) for the variance in H^c. The share explained by the covariance term is small. Hence, in the full cross-section of countries, sectoral structure is the main determinant of the effect of lockdowns on GDP.

The role of H and V differs systematically across country income groups. Columns (5)-(7) of Table 3 present the average G, H, and V by quintile. Under complete lockdown, both H and V contribute to the fact that GDP declines more steeply in poorer countries. GDP in poor countries drops disproportionately both because they have a lower WFH capacity *and* because their value added is concentrated in sectors where employment – due to low average WFH capacity – is affected most severely. Under the non-agricultural lockdown scenario, the two forces work in opposite directions. While their low WFH capacity, captured by H, still contributes negatively to the relative GDP of poor countries, their high value added share in agriculture, captured by V, more than compensates.

This pattern remains under the realistic hard lockdown policy. Here, the larger value added share of the poorest countries in essential sectors, captured by V, eliminates about a third of their lower WFH ability, captured by H. Lower-middle income countries suffer output losses roughly as large as the poorest countries. Although they have higher WFH ability, they are penalized by their sectoral structure, with high value added shares in sectors with low WFH ability. In countries in the third quintile of the income distribution, the sectoral composition accounts for over 80% of GDP losses compared to the reference economy. The richest countries, in contrast, benefit from both the highest WFH ability and a favorable sectoral structure, with relatively large value added shares in sectors with high WFH ability. Their sectoral structure is responsible for about a third of their lower GDP loss compared to the reference economy. This is similar, to a lesser extent, for countries in the fourth quintile of the income distribution.

The same pattern arises for the soft lockdown policy. However, all effects are weaker,

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and countries over the whole income spectrum experience a similar-sized drop in GDP. These patterns are visualized in Figure A3.

5 Conclusion

This paper measures the costs of lockdown policies on employment and GDP for low, middle and high-income countries. The ability to work from home and a country's sectoral composition are two key variables that determine these costs. We provide a novel measure of the ability to WFH and use a multi-sector model to measure these costs. Our results show that lockdown policies affect middle-income countries most, while low-income and high-income countries are less affected. Looking forward, more work is needed to further understand the essential nature of sectors to think about the optimal design of sectoral lockdown policies.

Our study provides valuable numbers for the study of the effects of recent lockdown policies across the world. The measures of WFH ability we compute should be useful to inform others' projections of costs from lockdowns. For ease of access, we provide a "lockdown simulator" that allows simulating the effect of arbitrary sectoral lockdowns policies.[27]

[27] Accessible at https://work-in-data.shinyapps.io/work_in_data/.

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A Appendix

A.1 Appendix tables and figures

Table A1: Work from home measurement

Questionnaire	ONET	STEP
Section	*Work context*	*Skills at work*
1	Performing General Physical Activities is very important (4.0+ of 5)	Do you regularly have to lift or pull anything weighing at least 25 kilos? Binary response.
2	Handling and Moving Objects is very important (4.0+ of 5)	
3	Controlling Machines and Processes [not computers nor vehicles] is very important (4.0+ of 5)	As part of this work, do you (did you) operate or work with any heavy machines or industrial equipment? Binary response.
4	Operating Vehicles, Mechanized Devices, or Equipment is very important (4.0+ of 5)	
5	Performing for or Working Directly with the Public is very important (4.0+ of 5)	Time involved with customers. Ranked on a scale from 1-10 only for workers who answered positively to "Do you contact non-coworkers?" Deemed important if responded with a 9 or 10.
6	Repairing and Maintaining Mechanical Equipment is very important (4.0+ of 5)	As part of this work, do you (did you) repair/maintain electronic equipment? Binary response.
7	Repairing and Maintaining Electronic Equipment is very important (4.0+ of 5)	
8	Inspecting Equipment, Structures, or Materials is very important (4.0+ of 5)	
Section	*Generalized work activities*	
9	"Average respondent says they use email less than once per month"	Does your work require the use of the following [e-mail]? Binary response.
10		As a part of your work do you (did you) use a computer? Binary response.

Note: The questions in the STEP column are taken from this questionnaire. The questions from the O*NET classification used Dingel and Neiman (2020) are taken from their codes, in particular from the file `onet_characteristics.do`.

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Table A2: Feasibility of working from home by definition and one-digit occupation

	Occupation, ISCO One Digit	O*NET - DN(2020)	O*NET	STEP
1	Managers	70.0	82.2	65.5
2	Professionals	69.8	78.2	62.2
3	Technicians and Associate Professionals	37.0	53.4	58.5
4	Clerical Support Workers	53.4	59.2	69.1
5	Services and Sales Workers	15.8	30.6	38.5
6	Skilled Agricultural, Forestry and Fishery Workers	3.3	31.3	22.7
7	Craft and Related Trades Workers	4.9	15.0	30.4
8	Plant and Machine Operators and Assemblers	0.5	3.0	25.0
9	Elementary Occupations	6.8	15.7	37.9
	Average	41.6	53.8	45.0

Note: Column 1 reports the share of employment that can work from home using the work from home classification proposed by Dingel and Neiman (2020) using O*NET data. Column 2 reports the share of employment that can work from home using a work from home classification based on STEP questionnaires, applied to O*NET data. Column 3 reports the share of employment that can work from home using the work from home classification based on STEP questionnaires, using STEP data. The exact questionnaire questions used to construct these classifications are reported in Table A1.

Table A3: Determinants of working from home: observables and occupations

	(1)	(2)	(3)
HS Graduate	0.173	0.077	0.068
	(0.027)	(0.026)	(0.028)
Age < 40	-0.003	-0.005	0.002
	(0.015)	(0.021)	(0.015)
Male	-0.156	-0.134	-0.113
	(0.020)	(0.054)	(0.022)
Wage Employment	0.106	0.065	0.044
	(0.024)	(0.029)	(0.023)
Occupation FE	None	One-Digit	Three-Digit
R^2	0.083	0.125	0.182
Observations		17,598	

Source: Skills Toward Employability and Productivity (STEP) Survey.
Note: Standard errors in parentheses. Table A3 presents the estimated coefficients from equation (1) across different specifications. The first column does not include occupation fixed effects, whereas the second and third columns include one- and three- digit occupation fixed effects, respectively. Results are weighted using sample weights to represent the working-age population of 15-64 year olds in the sample.

Table A4: Variance decomposition of working from home: share explained by different factors

% Explained	(1)	(2)
Variance	1.000	1.000
$Var(X_i)$	0.042	0.030
$Var(\gamma_O)$	0.038	0.100
$Var(\lambda_c)$	0.016	0.014
$Var(\theta_k)$	0.004	0.002
$Cov(X_i, \gamma_O)$	0.034	0.041
$Cov(X_i, \lambda_c)$	-0.006	-0.004
$Cov(X_i, \theta_k)$	0.009	0.004
$Cov(\gamma_O, \lambda_c)$	-0.004	-0.003
$Cov(\gamma_O, \theta_k)$	0.008	0.007
$Cov(\theta_k, \lambda_c)$	-0.002	-0.001
$Var(\varepsilon_{iock})$	0.864	0.813
R^2	0.136	0.187
Occ FE	One-Digit	Three-Digit
Observations	16,299	

Source: Skills Toward Employability and Productivity (STEP) Survey.
Note: Table A4 presents a variance decomposition following equation (1). The 'Variance' row denotes the share of the variance in the WFH measure to be explained and all the rows below denote the share of the variance accounted by each variable. The first and second columns include one- and three- digit occupation fixed effects, respectively. Results are weighted using sample weights to represent the working-age population of 15-64 year olds in the sample.

Table A5: WFH validation exercise: evidence for USA and Peru

	USA			Peru		
	(1)	(2)	(3)	(4)	(5)	(6)
Predicted WFH Score	0.397	0.809	0.918	0.427	0.671	0.712
	(0.017)	(0.021)	(0.031)	(0.079)	(0.094)	(0.137)
Male		0.210	0.206		0.140	0.135
		(0.006)	(0.006)		(0.030)	(0.030)
Industry Emp. (% Carried Out)			0.352			0.171
			(0.027)			(0.093)
Interaction: WFH $\times$ Industry			-0.366			0.004
			(0.046)			(0.194)
Constant	0.527	0.198	0.067	0.032	-0.156	-0.247
	(0.009)	(0.013)	(0.018)	(0.037)	(0.055)	(0.074)
Observations	28442	28442	28442	1060	1060	1060
R^2	0.020	0.055	0.072	0.027	0.046	0.068

Source: Skills Toward Employability and Productivity (STEP) Survey, Current Population Survey (USA) and *Encuesta Permanente de Empleo* (Peru).
Note: Standard errors in parentheses. Table A5 presents evidence on the relationship between predicted WFH scores, gender and the essential nature of sectors on employment outcomes in April, 2020 in USA and Peru. WFH scores are predicted using the estimated coefficients from a modified version of equation (1) (without industry and country fixed effects). Industry employment shares during lockdowns follow from Fana et al. (2020), as described in Section 2.2. Results are weighted using sample weights in each country.

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Table A6: Individual level dataset. Information on data sources, sample size and country years covered.

Name	Years	Sample size (in thds)	GDP per capita (PPP)	Source
Albania	2002–2012	22	4'845–9'918	LSMS
Argentina	2004–2006	114	12'074–13'770	LFS
Armenia	2013–2013	1	8'979–8'979	STEP
Austria	1999–2017	951	34'938–51'524	LFS
Belgium	1999–2017	456	32'357–46'522	LFS
Bolivia	2012–2012	1	5'860–5'860	STEP
Brazil	2002–2006	628	8'358–9'515	LFS
Bulgaria	1995–2017	301	6'390–20'027	LSMS, LFS
China	2012–2012	1	10'596–10'596	STEP
Colombia	2012–2012	1	11'934–11'934	STEP
Cote d'Ivoire	1985–1988	4	2'429–2'734	LSMS
Croatia	2002–2017	151	13'750–24'368	LFS
Cyprus	1999–2017	197	25'255–36'137	LFS
Czech Republic	1999–2017	720	20'059–36'061	LFS
Denmark	1999–2017	383	33'525–49'607	LFS
Estonia	1999–2017	109	10'772–31'013	LFS
Ethiopia	2013–2014	40	1'248–1'357	LFS, UES
Finland	1999–2017	183	31'433–42'902	LFS
France	2003–2017	804	31'567–40'975	LFS
Georgia	2013–2013	1	9'254–9'254	STEP
Ghana	2005–2017	68	3'007–5'154	LSMS, STEP, LFS
Greece	1999–2017	1'093	22'683–31'340	LFS
Hungary	1999–2017	1'206	14'380–27'531	LFS
Iceland	1999–2017	76	37'628–51'970	LFS
Indonesia	1993–2014	52	3'811–9'710	ILFS
Iraq	2006–2006	26	5'223–5'223	LSMS
Ireland	1999–2017	973	33'680–73'297	LFS
Kenya	2013–2013	2	2'652–2'652	STEP
Lao People's Democratic Republic	2012–2012	2	4'693–4'693	STEP
Latvia	1999–2017	157	9'655–26'643	LFS
Lithuania	1999–2017	264	10'373–30'936	LFS
Luxembourg	1999–2017	157	64'436–99'477	LFS
Macedonia, The Former Yugoslav Republic of	2013–2013	1	11'910–11'910	STEP
Malta	2009–2017	71	26'792–41'847	LFS
Mexico	2005–2005	149	13'691–13'691	LFS
Netherlands	1999–2017	692	37'786–50'024	LFS
Nicaragua	2005–2005	10	3'548–3'548	LSMS
Norway	1999–2017	193	37'645–63'768	LFS
Peru	2009–2014	114	8'515–11'086	LFS
Philippines	2015–2015	1	6'896–6'896	STEP
Poland	1999–2017	1'231	13'114–28'420	LFS
Portugal	1999–2017	718	22'413–28'567	LFS
Romania	1999–2017	1'113	7'441–25'262	LFS
Russian Federation	2004–2015	80	12'554–25'777	RLMS-HSE
Rwanda	2013–2016	24	1'551–1'872	LFS
Slovakia	1999–2017	482	14'190–30'433	LFS
Slovenia	1999–2017	304	21'855–33'947	LFS
South Africa	2012–2019	228	11'965–12'201	QLFS
Spain	1999–2017	857	25'102–37'233	LFS
Sri Lanka	2012–2012	1	9'653–9'653	STEP
Sweden	1999–2017	1'312	34'468–47'892	LFS
Switzerland	1999–2017	397	42'028–62'927	LFS
Uganda	2009–2013	17	1'571–1'759	LSMS
Ukraine	2012–2012	1	9'956–9'956	STEP
United Kingdom	1999–2017	654	31'110–42'138	LFS
United States	2002–2016	372	46'828–55'265	CPS
Viet Nam	2012–2012	2	4'917–4'917	STEP
		18'168	1'248–99'477	

Note: Table A6 includes the underlying sources for the dataset used in Section 3.

Table A7: Work from home measures across detailed subgroups.

		Males				Females			
		HS Graduate		HS Dropout		HS Graduate		HS Dropout	
	Full Sample	Wage Employee	Self-Employed	Wage Employee	Self-Employed	Wage Employee	Self-Employed	Wage Employee	Self-Employed
One-Digit Occupation	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Managers	0.655	0.697	0.571	0.636	0.405	0.807	0.649	0.575	0.403
Professionals	0.622	0.628	0.619	0.266	0.575	0.640	0.589	0.442	0.273
Technicians and Associate Professionals	0.585	0.608	0.539	0.401	0.153	0.628	0.828	0.665	0.111
Clerical Support Workers	0.691	0.639	0.496	0.496	0.583	0.760	0.834	0.636	0.479
Services and Sales Workers	0.385	0.456	0.347	0.435	0.222	0.439	0.411	0.370	0.350
Skilled Agricultural, Forestry and Fishery Workers	0.227	0.391	0.261	0.100	0.094	0.411	0.627	0.339	0.298
Craft and Related Trades Workers	0.304	0.196	0.145	0.130	0.209	0.614	0.481	0.586	0.473
Plant and Machine Operators, and Assemblers	0.250	0.245	0.203	0.161	0.190	0.558	0.049	0.430	0.165
Elementary Occupations	0.379	0.262	0.102	0.191	0.260	0.589	0.351	0.507	0.451
Sample Average	0.450	0.495	0.368	0.261	0.206	0.620	0.483	0.476	0.367
Observations	17,598	3,599	1,299	1,923	1,422	3,918	1,277	1,659	2,501

Source: Source: Skills Toward Employability and Productivity (STEP) Survey. Note: Table A7 documents the share of workers who can work from home across the 72 possible combinations of one-digit occupation, educational attainment, self-employment and gender. Results are weighted using sample weights to represent the working-age population of 15-64 year olds in each STEP country.

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Table A8: Work from home employment and GDP per capita

	Share of employment that can Work from Home				
	Total employment	Urban employment	Urban Self-employed	Urban Low skilled	Urban female
	(1)	(2)	(3)	(4)	(5)
GDP per capita (ppp), log	0.055	0.046	0.051	0.007	0.047
	(0.007)	(0.005)	(0.006)	(0.006)	(0.005)
Constant	−0.079	0.026	−0.092	0.247	0.113
	(0.063)	(0.048)	(0.056)	(0.056)	(0.045)
Observations	57	57	57	57	57
R^2	0.559	0.613	0.589	0.024	0.641

Note: Table A8 presents evidence from a country-level regression of log GDP per capita against different WFH measures. The analysis covers the 57 countries included in Table A6.

Table A9: Feasibility of working from home by occupation and industry

	STEP-Based Industry			
	Agriculture, Fishery, Mining	Manufacturing, Constr. & Transp.	Commerce	Other Services
One-Digit Occupation	(1)	(2)	(3)	(4)
Managers	0.539	0.584	0.641	0.708
Professionals	0.611	0.747	0.728	0.611
Technicians and Associate Professionals	0.489	0.588	0.664	0.583
Clerical Support Workers	0.646	0.617	0.655	0.732
Services and Sales Workers	0.522	0.344	0.358	0.436
Skilled Agricultural, Forestry and Fishery Workers	0.215	0.312	0.444	0.542
Craft and Related Trades Workers	0.105	0.314	0.260	0.315
Plant and Machine Operators, and Assemblers	0.272	0.284	0.250	0.234
Elementary Occupations	0.190	0.189	0.312	0.481
Industry-Average	0.266	0.366	0.395	0.536
Observations	929	3,195	4,048	8,127

Source: Skills Toward Employability and Productivity (STEP) Survey. Note: Table A9 documents the share of workers who can work from home by one-digit occupation and industry categories available in STEP data. Results include workers for whom industry information is available. Results are weighted using sample weights to represent the working-age population of 15-64 year olds in each STEP country.

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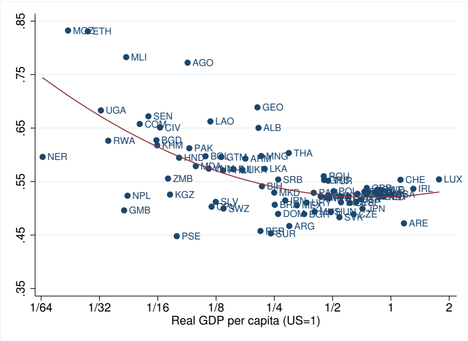
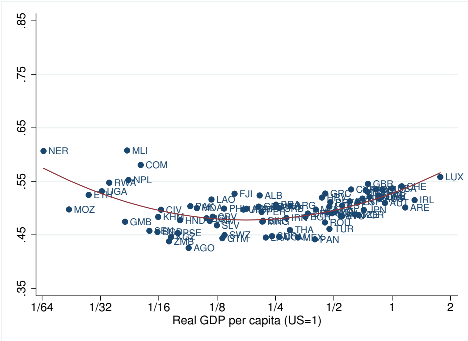
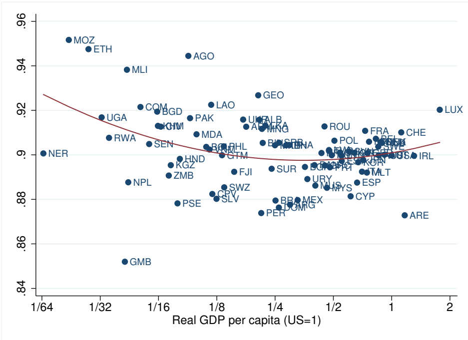
Figure A1: Effective employment and GDP relative to trend under non-agricultural and soft lockdowns



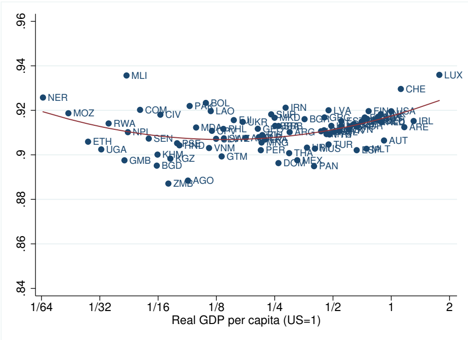
(a) Employment, non-agr. lockdown



(b) GDP, non-agr. lockdown



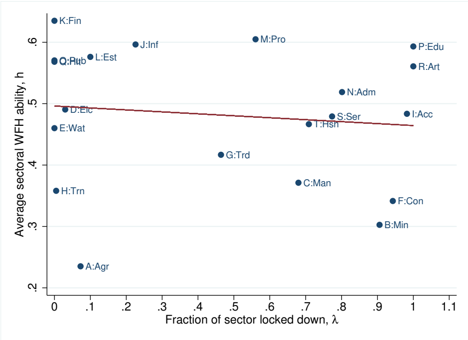
(c) Employment, soft lockdown



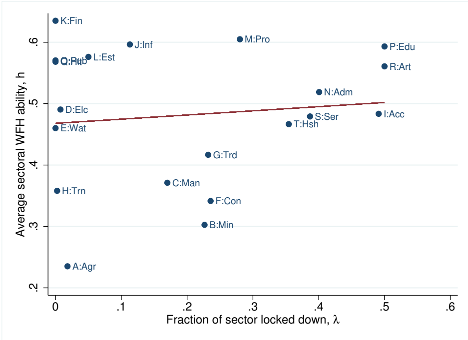
(d) GDP, soft lockdown

Real GDP per capita of each country corresponds to the 2017 PPP-adjusted series from Feenstra et al. (2015), normalized to the U.S. The trend line is a quadratic fit of the logarithm of real GDP per capita.

Figure A2: Average sectoral WFH ability across countries and fraction of sector that shuts down



(a) Hard lockdown



(b) Soft lockdown

Figure A2 presents estimated WFH ability for one-digit ISIC sectors, averaged across countries, along with the share of employment that can be carried out in a hard and soft lockdown in panels A and B, respectively. We present the line of best fit in both panels.

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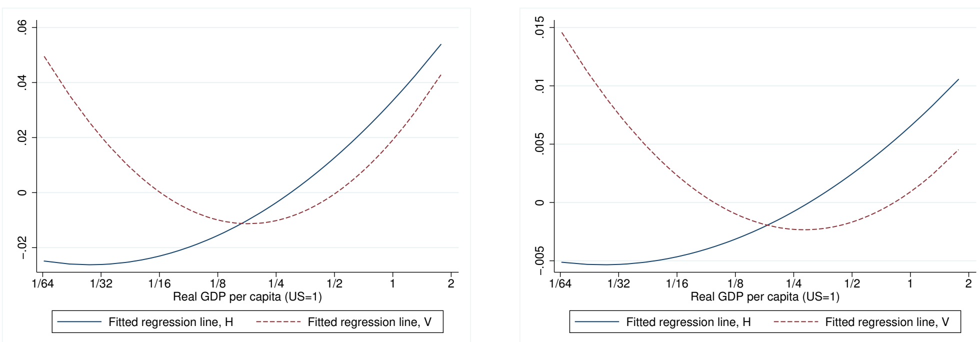
Figure A3: Fitted components H and V under the hard and soft lockdowns



(a) Hard lockdown

(b) Soft lockdown

In each scenario, the plots are the fitted lines $\hat{H}$ and $\hat{V}$ of the respective regressions $H = \beta_0 + \beta_1 \log GDP + \beta_2 (\log GDP)^2 + \varepsilon$ and $V = \gamma_0 + \gamma_1 \log GDP + \gamma_2 (\log GDP)^2 + \varepsilon$. In the hard lockdown scenario, the regression coefficients and t-statistics (in parenthesis) are $\hat{\beta}_1 = 0.033$ (9.13) and $\hat{\beta}_2 = 0.005$ (4.45), with $R^2 = 0.752$ for H, and $\hat{\gamma}_1 = 0.035$ (2.45) and $\hat{\gamma}_2 = 0.010$ (2.50), with $R^2 = 0.072$ for V. In the soft lockdown, we have $\hat{\beta}_1 = 0.007$ (9.20) and $\hat{\beta}_2 = 0.001$ (4.42), with $R^2 = 0.758$ for H, and $\hat{\gamma}_1 = 0.005$ (1.90) and $\hat{\gamma}_2 = 0.002$ (2.65), with $R^2 = 0.112$ for V.

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A.2 Data sources

Our individual level dataset consolidates labor force surveys and the labor force section of household surveys for 57 countries. It contains information on individual characteristics, employment status, job type, occupation and sector of activity. Table A6 lists the data sources, the GDP per capita (ppp) that corresponds to the country year of the dataset taken from Zeileis (2019), as well as the sample size. Note that the sample size here corresponds to the number of working-age individuals (age 15-64) that work.

A.3 Model derivation

Here we derive the model that underpins equation (5) that used to calculate GDP relative to trend. Consider a closed economy where gross output in sector i is

$$g_i = z_i x_i^{\theta_i} \prod_{j=1}^{I} m_{ij}^{\gamma_{ij}},$$

with parameters $\theta_i \in [0,1]$ and $\gamma_{ij} \in [0,1]$ such that $\theta_i + \sum_{j=1}^{I} \gamma_{ij} = 1$. The sector's TFP is z_i and there are two types of production factors: x_i is a bundle of the sector's human and physical capital and m_{ij} is intermediate consumption of goods from sector j. Let p_i denote the price of output of sector i. Assuming perfect competition, profit maximization with respect to intermediate inputs implies $p_j m_{ij} = \gamma_{ij} g_{ij}$, $\forall i, j$. In particular, the sector's value added equals

$$V_i \equiv p_i g_i - \sum_{j=1}^{I} p_j m_{ij} = \theta_i p_i g_i.$$

The representative household chooses final consumption c_i to maximize utility

$$Y = \prod_{s=1}^{I} c_i^{\phi_i}$$

with parameters $\phi_i \in [0,1]$ such that $\sum_{i=1}^{I} \phi_i = 1$. The optimality condition is hence $p_i c_i = \phi_i Y$, $\forall i$. The product market clears according to $c_i + \sum_{j=1}^{I} m_{ji} = g_i$, $\forall i$.

Let Y denote real GDP and $P \equiv 1$ its normalized price so that $PY = Y = \sum_{i=1}^{I} p_i c_i$. In equilibrium, it can be shown that GDP is

$$Y \propto \prod_{i=1}^{I} \left(z_i x_i^{\theta_i} \right)^{d_i}$$

with parameter vector $d = \phi'(I - \Gamma)^{-1}$ where I is the identity matrix and Γ is the matrix with elements γ_{ij}. In particular, d_i equals the Domar weight of sector i, $d_i = \frac{p_i y_i}{Y}$. If z_i is constant and the only exogenous shock occurs through the supply of x_i, then $Y \propto \prod_{i=1}^{I} x_i^{\nu_i}$ where $\nu_i = \theta_i d_i = \frac{V_i}{Y}$ equals the (constant) aggregate value added share of sector i in the economy. GDP relative to trend is then $y \equiv \frac{\tilde{Y}}{Y} = \prod_{i=1}^{I} \left(\frac{\tilde{x}_i}{x_i} \right)^{v_i}$ where $\tilde{x}_i / x_i$ denotes the relative utilization of factor x_i following the shock. Our final assumption is that capital and labor (l) enter homothetically into x and that they change in equal proportion following the shock,

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resulting in

$$y \equiv \frac{\tilde{Y}}{Y} = \prod_{i=1}^{I} \left(\frac{\tilde{l}_i}{l_i} \right)^{v_i}.$$

Economies can differ in their underlying parameters, which implies that v_i is country-specific.

A.4 Lockdown scenarios

Table A10 summarizes the percent of sectoral workplace employment that is shut down under the various lockdown senarios used in section 4. The complete lockdown signifies that all sectors are shut down. The non-agricultural lockdown signifies that all sectors except agriculture are shut down, i.e., $\lambda = 0$ for agriculture and $\lambda = 1$ in all remaining sectors.

Table A10: Lockdown scenarios, percent of sectoral workplace employment that is shut down

	Complete	Non-agr.	Hard	Soft
Agriculture / forestry / fishing (A)	100	0	7	2
Mining and quarrying (B)	100	100	91	23
Manufacturing (C)	100	100	68	17
Electricity / gas / steam / air cond. (D)	100	100	3	1
Water supply / sewerage (E)	100	100	0	0
Construction (F)	100	100	94	24
Wholesale and retail trade (G)	100	100	46	23
Transportation and storage (H)	100	100	0	0
Accommodation and food service (I)	100	100	98	49
Information and communication (J)	100	100	23	11
Finance and insurance (K)	100	100	0	0
Real estate (L)	100	100	10	5
Professional / scientific / technical serv. (M)	100	100	56	28
Administrative and support services (N)	100	100	80	40
Public administration and defence (O)	100	100	0	0
Education (P)	100	100	100	50
Health and social work (Q)	100	100	0	0
Arts / entertainment / recreation (R)	100	100	100	50
Other service activities (S)	100	100	77	39
Private households with empl. persons (T)	100	100	71	35

Note: Table A10 presents the share of sector-level employment which is shutdown under four lockdown scenarios. See Section 4 for details.

The hard lockdown is based on Fana et al. (2020) who encode the March 2020 legislative confinement measures in Germany, Italy and Spain. In particular, they report for each country the degree to which two-digit ISIC sectors are considered essential and therefore the degree to which they are allowed to function normally. Their final index is an average across the three countries, justified by the fact that there is relatively little discrepancy between them. To aggregate up to one-digit sectors, we use employment weights: $\lambda_i = 1 - \sum_{j \in i} \mu_j e_j$, where $e_j \in [0, 1]$ is the essential index and μ_j is the employment share of the two-digit sectors j belonging to one-digit sector i.[28]

We perform two manual changes. Fana et al. (2020) document that the sector *Education* (ISIC code P) is entirely essential in Germany and Italy, while non-essential in Spain, implying $\lambda = 0.33$. Instead, we shut it down completely, $\lambda = 1$. Our choice is guided by the fact that

[28] The employment shares are averaged across all available countries using the ILO data at the two-digit ISCO level.

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both Germany and Italy closed down all educational establishment in March 2020. Second, according to Fana et al. (2020), the sector *Real estate activities* (ISIC code L) is completely non-essential, implying $\lambda = 1$. Instead, we assign it the value $\lambda = 0.1$. We conjecture that restrictions to real estate employment activities such as brokerage have a minimal impact on bulk of the sector's value added, which consists mainly of imputed own-occupied housing as well as established rental arrangements.

Finally, in the soft lockdown scenario, we reduce the value of λ from the hard lockdown by a fraction, namely by 75% for agriculture and industry (ISIC codes A-F) and 50% for services (ISIC codes G-T). This is guided by the notion that service sectors require more interaction with customers and are therefore more likely to suffer restrictions.

Figure A2 plots the average WFH ability of sectors across countries relative to the degree to which sectors are shut down, λ. Neither in the hard nor in soft lockdown there exists a clear relationship between the two variables, meaning that on average the propensity to exercise WFH does not correlate with how essential a sector is.

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Understanding socioeconomic disparities in travel behavior during the COVID-19 pandemic[1]

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We document the magnitudes of and mechanisms behind socioeconomic differences in travel behavior during the COVID-19 pandemic. We focus on King County, Washington, one of the first places in the U.S. where COVID-19 was detected. We leverage novel and rich administrative and survey data on travel volumes, modes, and preferences for different demographic groups. Large average declines in travel, and in public transit use in particular, due to the pandemic and related policy responses mask substantial heterogeneity across socioeconomic groups. Travel intensity declined considerably less among less-educated and lower-income individuals, even after accounting for mode substitution and variation across neighborhoods in the impacts of public transit service reductions. The relative inability of less-educated and lower-income individuals to cease commuting explains at least half of the difference in travel responses across groups.

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1 Introduction

The novel coronavirus (COVID-19) has led to swift and unprecedented changes in people's travel behavior around the world. A combination of voluntary precautions and policy responses by governments, including city lockdowns and stay-at-home orders, have sharply curtailed trips for both work and leisure in many places, potentially helping to contain the spread of the virus. However, the ability of different groups of individuals to adjust their travel behavior in the face of the pandemic and various policy directives has raised important equity concerns.

In this paper, we study changes in travel behavior in response to COVID-19 in King County, Washington. King County is home to Seattle, one of the first cities in the U.S. affected by the pandemic and one with some of the earliest and most sweeping policy responses. Using anonymized geolocated cell phone data from SafeGraph Inc., we first document overall changes in travel in response to COVID-19 as well as how changes in mobility correlate with neighborhood disadvantage. Exploiting passenger boardings information derived from sensors on board King County Metro's fleet of vehicles, we go on to explore changes in public transit use in particular, and how those changes vary with neighborhood demographics. Supplementary administrative data on transit boardings by individuals using full-fare transit cards vs. reduced-fare cards available only to lower-income individuals shed additional light on the disparate responses by higher- and lower-income riders. Finally, we explore the mechanisms behind the differential changes in travel behavior among higher- and lower-income individuals. We take advantage of both administrative data and the results of a novel survey of low-income transit users in King County to uncover the sources of the observed changes in mobility across socioeconomic groups during the pandemic.

We document a steep decline in mobility in King County as the pandemic took hold. Based on cell phone tracking data, the average number of census block groups (CBGs) people visited each day (excluding their home CBG) fell by 57% between February and April 2020. During the same period, public transit use declined by an even sharper 74%.

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These large average declines mask substantial heterogeneity across socioeconomic groups. Mobility responses were particularly swift, pronounced, and persistent among more highly educated and higher-income individuals.

We go on to examine the underlying mechanisms behind these changes in travel behavior across socioeconomic groups. We focus specifically on the roles of transportation mode substitution, public transit service adjustments, and commuting for work.

First, residents of more-educated neighborhoods engaged in a greater degree of mode substitution, but only early on in the crisis. During the initial stages of King County's lockdown, we observe a differentially large decline in public transit use relative to overall travel in highly educated neighborhoods. This implies that high-income residents disproportionately shifted away from public transit and toward cars. However, the role of mode substitution in driving differences in travel behaviors across socioeconomic groups has faded over time, suggesting a convergence in substitution elasticities.

Second, public transit service adjustments in the aftermath of the lockdown can explain only a small fraction of the gap in transit use between higher- and lower-income riders. Between mid-March and mid-April, King County transit authorities limited service in various parts of the local public transportation system. To the extent that these service adjustments differentially affected residents of more- vs. less-educated neighborhoods, it could help to explain the differential response in travel behavior we observe between groups. However, ridership of higher-income users relative to lower-income users declines as much or more within transit routes as between transit routes, indicating that the supply of public transit plays little role in driving differential changes in transit use across socioeconomic groups.

Finally, we find that the relative inability of less-educated and lower-income people to work remotely is likely an important contributor to the smaller mobility response for that group. In the depths of the lockdown, weekly and daily cycles of travel consistent with commuting for work remain conspicuous among residents of less-educated neighborhoods and among individuals using reduced-fare public transit cards. For residents of more-educated

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neighborhoods and individuals using regular-fare public transit cards, commute cycles are clear in the data before the crisis but largely vanish after the pandemic took hold.[1] Under the conservative assumption that weekday trips are for work and weekend trips are for other activities, the magnitude of this change indicates that at least half of the socioeconomic gap in travel reductions is attributable to commuting for work.

We complement these results using newly collected survey data from an ongoing study in King County. The survey highlights how intended uses of transit changed among low-income individuals as the COVID-19 crisis took hold. We find that low-income individuals consistently report intentions to use transit for activities that could be deemed essential, such as for work, school, and health, even as the crisis and its policy response unfolded. Meanwhile, intentions among low-income individuals to use transit to travel for recreational, family, shopping, and other reasons trailed off after the first local viral outbreaks and continued to decline as the policy response materialized.

Our results add to the growing body of evidence suggesting that the burden of the pandemic, not only in terms of its direct health effects but also in terms of the economic costs of avoidance and mitigation efforts, are spread unevenly across demographic groups. Several recent papers have identified COVID-19's disparate effects along other dimensions. For example, Abedi et al. (2020) shows that the virus' incidence varies across demographic groups in the U.S. Borjas (2020) and Schmitt-Grohé et al. (2020) demonstrate that COVID-19 testing rates vary systematically by neighborhood demographics in New York City. Baker et al. (2020) document heterogeneity in household consumption patterns across demographic groups and by income level in response to the pandemic. Adams-Prassl et al. (2020), Cajner et al. (2020), Fairlie et al. (2020), Montenovo et al. (2020), and Kahn et al. (2020) highlight how the economic costs of the crisis have been borne disproportionately by lower-income groups, for whom working remotely is less likely to be feasible and for whom income and job losses overall have been greater in the aftermath of the crisis.

[1] This pattern is particularly striking given that job losses among low-income people in the wake of the pandemic have been substantially larger than those among high-income people (Cajner et al. 2020).

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We contribute to this literature by illustrating the magnitude and sources of heterogeneity in the mobility impacts of the crisis. Brzezinski et al. (2020) document differences in mobility by education across U.S. counties. We build on this work by showing that these differences remain at the neighborhood and individual levels and by providing evidence on why these disparities emerge. Understanding COVID-19's heterogeneous mobility impacts is important because they could represent both causes and consequences of many of the other documented disparate effects of the pandemic. To the extent that less-educated and lower-income groups continue to travel at higher rates than more-educated and higher-income groups (especially in modes frequently necessitating interaction with other people, like public transit), it could contribute to higher viral transmission rates among those groups. The higher mobility among less-educated and lower-income groups is also potentially a consequence of their inability to work remotely and home conditions that are generally less hospitable to sheltering in place due to lack of adequate internet access, space constraints, and limited access to outdoor areas.

Our results have important policy implications. The relatively inelastic mobility response to COVID-19 among less-educated and lower-income groups does not appear to be primarily a result of a failure of messaging or differences in the degree to which they comply with directives regarding non-essential travel. Rather, our findings suggest that their more muted mobility response arises from economic necessity, and in particular the need to travel to jobs that cannot be performed remotely. To the extent that policymakers might desire to reduce mobility among these groups further to address public health concerns, changes in public communication strategies or stricter enforcement of rules regarding non-essential travel may have only modest effects. Changes in what operations are deemed essential, and thus which workers are expected to be physically present at jobs during pandemics, could go further in reducing disparities in travel behavior. Raising the opportunity cost of traveling for work via transfers tied to staying home (for example, through the unemployment insurance system) could also potentially reduce these disparities. However, such measures could come

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at the cost of forgoing certain, possibly critical, goods and services during the crisis, and might translate into even larger job losses among individuals at the lower end of the income distribution.

Our findings also help to foreshadow possible changes in mobility patterns and modes as local economies reopen. The large and persistent drop in public transit use relative to overall mobility points to substitution away from modes that involve close proximity to others and toward those that do not (e.g., single-occupancy driving). To the extent that this persists, it could have important implications for traffic congestion and pollution, especially as lockdown orders are lifted. There also may be a lasting shift to remote working for some types of jobs (Lavelle 2020, Molla 2020), which could generate more persistent disparities in travel behaviors to the extent that people in different socioeconomic groups hold jobs that can be performed remotely at different rates.

2 Mobility Responses to the COVID-19 Pandemic

2.1 The King County Context

We focus on King County, Washington. King County was among the first U.S. locations impacted by the pandemic, and thus has one of the longest post-COVID-19 periods to analyze. The first confirmed COVID-19 case on U.S. soil was identified in the state of Washington on January 21, 2020, and one of the first COVID-19-related deaths occurred in the Seattle area on February 28. The outbreak of COVID-19 at the Life Care Center in Kirkland, a suburban area in King County just east of Seattle, in late February set the wheels in motion for sweeping state and local government policy responses. The Seattle mayor issued a proclamation of civil emergency on March 3. Public schools as well as restaurants, bars, and entertainment facilities statewide were closed March 16. The state issued its official stay-at-home order March 23, two days before all nonessential businesses in the state were forced to close.

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King County also provides data that uniquely speak to travel patterns in general and differences in travel behavior by income in particular. First, we have access to multiple sources of transit ridership information in King County. Through a partnership with King County Metro, we have data on passenger counts derived from sensors on board most King County buses. We also have separate, individual-level boarding data based on "taps" by transit fare cards. Taps by regular-fare and reduced-fare cards allow us to track trips by higher- and lower-income riders separately.

Second, we were in the midst of a randomized controlled trial studying transit fare policy when COVID-19 cases began to emerge in the U.S. and when King County went into lockdown.[2] That study included surveys that provide detail on travel intentions and behavior during the shutdown.

2.2 Changes in Overall Mobility

We use data from SafeGraph Inc. to measure overall changes in travel intensity as the COVID-19 crisis unfolded. SafeGraph tracks the locations of millions of mobile devices on which individuals have agreed to allow applications to access data on their precise locations. These data are anonymized and aggregated to CBGs. SafeGraph determines a device's home CBG based on where it resides most frequently. Using origin and destination information, the SafeGraph data allow us to construct high-frequency measures of mobility among individuals.[3]

In King County, we observe around 100,000 devices in the SafeGraph data, or about 78 devices per CBG. Assuming each device is attached to one individual, this corresponds to a roughly 5% sample of all individuals living in King County. Coverage rates are not strongly correlated with socioeconomic characteristics of a neighborhood.[4] Our primary measure of

[2] See Brough et al. (2020b) for results from a pilot phase of that study.

[3] More information about the SafeGraph data can be found here: https://docs.safegraph.com/docs/social-distancing-metrics.

[4] The number of devices per resident in a CBG is correlated with neither median income ($r = 0.01$) nor fraction white ($r = 0.02$). The fraction with a bachelor's degree has a slightly greater, but still weak, negative

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mobility derived from the SafeGraph data is the average number CBGs visited each day, other than the home CBG, per device. We focus primarily on the relationship between travel behavior and the share of adult residents in a CBG with a bachelor's degree because it is one of the strongest predictors of changes in travel intensity between February and April 2020.[5]

Travel intensity overall fell enormously throughout King County during the lockdown, but fell particularly sharply in neighborhoods with high average levels of education and income. Panel (a) of Figure 1 shows the percent change in the SafeGraph measure of travel intensity between February and April across CBGs in King County. The average percent decline across all CBGs in the county was 57%, but this average masks substantial heterogeneity. The more lightly shaded areas had larger reductions in overall travel intensity as measured by cell phone location tracking. The lighter areas are concentrated in northwestern King County, where more highly educated and higher-income households reside.

Panel (b) of Figure 1 correlates the percent change in travel intensity among residents of a CBG between February and April with the fraction of that CBG's residents that have a bachelor's degree. As the figure shows, the decline in overall mobility among residents of less-educated CBGs is substantially smaller in magnitude than the decline among residents of more-educated CBGs. Column (1) of Table 1 quantifies this relationship. A CBG where 10% of the residents have a bachelor's degree sees on average a 45% decline in overall mobility, whereas a CBG where 90% of residents have a bachelor's degree sees on average a 69% decline. As Appendix Table 1 shows, we find a similar relationship with CBG income levels; every additional $100,000 in median income in a CBG is associated with a 13 percentage point larger decline in mobility. Appendix Table 1 also demonstrates that the correlation between neighborhood education and the decline in travel is robust to controlling for income

correlation with devices per person ($r = 0.19$). See the scatterplot in Appendix Figure 1.

[5] We run a LASSO model for a cross-section of CBGs with the percent change in travel intensity as the outcome and various CBG characteristics as predictors. The full list of CBG characteristics we use as predictors appears in the notes to Appendix Table 1 (in reference to column (4)). Only the share of residents with a college degree and the share of residents working in the technology sector survive the LASSO.

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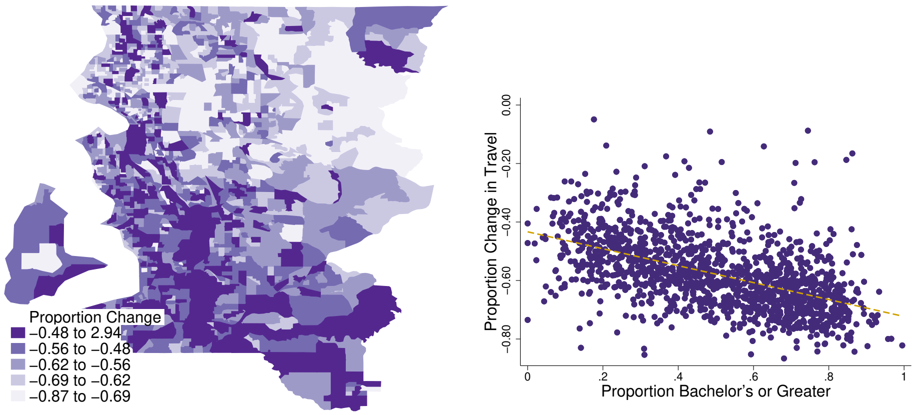
and several other neighborhood demographic and economic characteristics. Moreover, this correlation is new; Appendix Figure 2 shows no relationship between neighborhood education level and the January-February change in travel.



(a) Map of King County, WA (b) Correlation with Neighborhood Education

Figure 1. Percent Change in Travel Intensity between April and February 2020, by Census Block Group
Notes: The unit of observation is census block group (CBG). Travel intensity is the number of other CBGs visited per device that usually resides in the given CBG, as measured by the SafeGraph Social Distancing Metrics dataset. Fraction with a bachelor's degree comes from the 2014-2018 5-year ACS estimates. To aid with presentation, some CBGs in eastern King County are omitted from the map, and a small number of CBGs with positive change in travel are omitted from the scatterplot.

2.3 Changes in Public Transportation Use

2.3.1 Neighborhood-Level Measures of Transit Use

We take advantage of King County Metro automated passenger counter (APC) data to study changes in public transit use across neighborhoods. The APC data are based on information collected from sensors installed on a subset (approximately 70%) of buses that run in King County. These sensors track boardings, which we aggregate to CBGs in a manner similar to how SafeGraph data are aggregated. We examine how boardings in each CBG change over time and by neighborhood characteristics.

Across CBGs, the average percent decline in transit boardings between February and

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Table 1. Correlates of Percent Change in Travel

	(1) Safegraph Travel	(2) Safegraph Travel	(3) APC Boardings	(4) APC Boardings	(5) ORCA Boardings	(6) ORCA Boardings	(7) ORCA Boardings
Fraction Bachelor's	-0.30***	-0.17***	-0.31***	-0.19***			
	(0.018)	(0.017)	(0.024)	(0.021)			
Weekday		0.13***		0.20***			0.40***
		(0.0079)		(0.011)			(0.046)
Frac Bac x Weekday		-0.18***		-0.15***			
		(0.013)		(0.019)			
LIFT					0.19***	0.24***	0.11***
					(0.037)	(0.035)	(0.030)
LIFT x Weekday							0.10**
							(0.052)
Constant	-0.42***	-0.52***	-0.58***	-0.73***	-0.51***	-0.49***	-0.83***
	(0.010)	(0.010)	(0.014)	(0.013)	(0.022)	(0.018)	(0.042)
Route FE	No	No	No	No	No	Yes	No
Mean of Dep. Var.	-0.57	-0.57	-0.74	-0.74	-0.42	-0.42	-0.42
R^2	0.092	0.11	0.039	0.064	0.0072	0.14	0.033
N	38340	38340	35160	35160	6680	6680	6680

Notes: Each column shows results from an OLS regression. Coefficients for all independent variables are shown unless noted in the table. The dependent variable in each column is the percent change in the outcome between the follow-up period and February. Columns (1)-(2) use SafeGraph data on the average number of other census block groups (CBGs) visited per device usually residing in the CBG as the outcome; columns (3)-(4) use transit boardings as measured by automated passenger counters; and columns (5)-(7) use transit boardings as measured by boardings paid for with an ORCA card. The unit of observation is the CBG-day in columns (1)-(4) and the route-day-fare type in columns (5)-(7); fare type is either LIFT or full adult fare. Fraction with a bachelor's degree comes from the 2014-2018 ACS. Each column covers all of King County, WA. The sample time period is April 2020 in columns (1)-(4) and March 10-20, 2020 in columns (5)-(7). Standard errors are clustered by CBG in columns (1)-(4) and by route in columns (5)-(7). Statistical significance at the 10, 5, and 1 percent level is denoted, respectively, by *, **, and ***.

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April was 74%, about a 30% larger decline than that for overall travel during the same period. Again, the large average decline in transit use across CBGs obscures significant heterogeneity. As shown in column (3) of Table 1, a ten percentage point higher share of residents with a bachelor's degree is associated with a 3.1 percentage point larger drop in public transit use. Appendix Figure 3 shows the scatterplot of this relationship and maps the decline in transit use. Replicating the results for overall travel intensity, Appendix Table 3 documents that the drop in boardings also correlates with income. Every additional $100,000 in median income in a CBG is associated with a 17 percentage point larger decline in mobility.

2.3.2 Individual-Level Measures of Transit Use

We obtained administrative data on boardings among King County Metro customers who used transit cards to pay their fares on local public transportation. These cards include regular, adult-fare "ORCA" transit cards (of which there are millions in circulation) as well as reduced-fare "ORCA LIFT" transit cards available only to low-income riders (of which there are over 50,000 in circulation).[6] Taps by ORCA and ORCA LIFT cards allow us to track trips by higher- and lower-income riders separately. However, unlike the APC data, ORCA records omit people who pay by cash or who evade payment.

Notably, King County Metro eliminated fares on all its buses, light rail, and other routes on March 21, 2020 to ensure social distancing protocols among customers and drivers could be maintained. Therefore, we do not have data on taps using ORCA or ORCA LIFT cards after that date.[7]

We see a large overall decline in public transit trips using fare cards as the crisis began to unfold in early March. Again, however, the decline was more marked for higher- relative

[6] ORCA LIFT cards are available to any household with income less than 200% of the federal poverty line. People enroll in LIFT by requesting a card in-person at public and social service agencies, which verify income prior to issuing the card.

[7] For comparison, we show in Appendix Tables 2 and 4 the change in overall travel as measured in the SafeGraph and APC data between February and mid-March. Qualitatively, the results are very similar to those discussed above.

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to lower-income riders. Column (5) of Table 1 shows the results. Comparing boardings in February to mid-March, regular ORCA rides fell by 51%, whereas LIFT rides fell by only 32%. Hence, these individual-level data corroborate the neighborhood-level data in showing a sharply heterogeneous mobility response to COVID-19 by socioeconomic group.

3 Mechanisms

Overall travel intensity fell more sharply for residents of high-education than low-education neighborhoods in the face of the COVID-19 pandemic in King County. Measured changes in public transportation use specifically were particularly large among more highly educated and higher-income individuals. We next turn to a deeper investigation of potential mechanisms underlying the differences in travel responses to COVID-19 across socioeconomic groups.

3.1 Reliance on Different Modes

We first consider the role of transportation mode substitution during the COVID-19 crisis. From a distributional perspective, examining transportation mode is important for at least two reasons. First, low-income people tend to rely more heavily on public transit (Glaeser et al. 2008). Second, COVID-19 itself and policy responses to it may differentially affect travel by transit versus other modes. Transit is more likely to facilitate viral transmission, which may lead people to avoid it during a pandemic. To the extent that high-income individuals have greater access to means of transportation other than public transit, mode substitution as an avoidance strategy may be more feasible for those individuals. In that case, we would expect to see a larger decline in public transit use than in overall travel for high-income individuals than we see for low-income individuals.

A key result from above is that transit use decreases more than travel overall. Thus, in light of the fact that less-educated and lower-income individuals rely disproportionately on public transit, reliance on different modes alone cannot explain the overall mobility gap that

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emerges during the crisis between high- and low-income travelers. Instead, the larger decrease in transit use suggests that many individuals engaged in transportation mode substitution (e.g., away from public transit and toward cars or other modes) in response to the pandemic.

However, while the degree of mode substitution differed across socioeconomic groups early on in the pandemic, our results indicate that differential mode substitution does not drive differences in travel behavior in the shutdown steady state. Between February and April, a neighborhood where 90% of residents hold bachelor's degrees experienced a 69% decrease in overall mobility compared to an 86% percent decrease in transit boardings. Therefore, transit use is about 25% (1 - .86/.69) more responsive than overall travel in high-education neighborhoods. In a neighborhood where 10% of residents hold bachelor's degrees, transit responds by 36% more than travel overall (a 61% vs. a 45% drop). If anything, the relative drop in transit use is about 8% smaller in high-education neighborhoods.[8]

During the transition, however, mode substitution did differ by socioeconomic status. Between February and mid-March, transit fell by 45% more than travel overall in high-education neighborhoods (64% vs. 44%). Transit fell by only 26% more than travel overall in low-education neighborhoods during the same period (24% vs. 19%). Thus, individuals in high-education neighborhoods substituted away from transit to other modes of travel more than individuals in low-education neighborhoods during the early stages of the shutdown. However, this gap disappeared or even reversed as the crisis deepened.

3.2 Supply of Public Transportation

One possible explanation for the differences in the magnitudes of the decline in overall travel intensity, and in public transit use in particular, between higher- and lower-income King County residents are public transit service adjustments. Unlike other forms of transportation, policymakers directly control the volume of transit by mapping routes and setting service frequency. In the face of the pandemic, King County Metro limited service in various parts

[8] This analysis assumes that the average number of other CBGs visited on transit by residents of a CBG is proportional to the number of transit boardings in the CBG.

of its system. King County Metro announced its first reductions in service on March 23, 2020. By mid-April, Metro had made three rounds of service adjustments; at that point, Metro was operating 27% fewer service trips than typical weekday service (Switzer 2020). Policy decisions could drive differences in travel behavior if they leave open bus lines in low-income neighborhoods that rely on transit while closing them elsewhere. Transit also requires a minimum volume to operate efficiently; even a reduction in service responding to lower passenger volume during a lockdown could amplify any differences across neighborhoods that appear for other reasons.

While service changes may have reduced transit ridership, we find no evidence that this widened the gap in travel behavior between higher- and lower-income riders. In the transit data, we can compare how full-fare ORCA and low-income LIFT riders *who ride the same route* respond to COVID-19. If a reduced supply of transit drives lower passenger volume, then higher- and lower-income riders on the same route should respond similarly. Figure 2 displays these data.

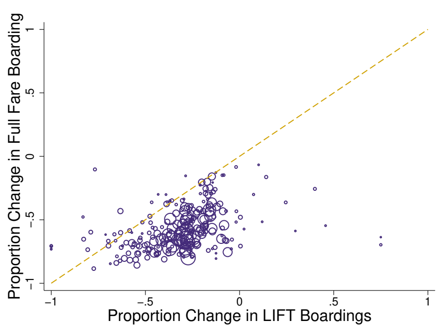

Figure 2. Percent Change in ORCA Boardings by Route, Base vs. Low-Income Fare
Notes: The unit of observation is a King County Metro route (almost always a bus route). The outcome includes only boardings paid for with an ORCA card, excluding cash and non-payment. Percent changes compare March 10-20 to February boardings. Reduced-fare LIFT versus full-fare boardings are detected by the payment type, which depends on the card serial number. The size of the circle is proportional to the sum of LIFT and full-fare boardings in February. We exclude routes that average less than 50 boardings per day in February to aid with presentation.

Each circle in the figure represents a Metro route, with bigger circles corresponding to more popular routes. The figure plots the percent change in LIFT boardings on each route between

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February and mid-March against the percent change in full-fare boardings on the same route during the same period. Nearly all routes are below the 45 degree line, indicating that high-income boardings decrease more than low-income boardings within routes. Formally, as shown in columns (5) and (6) of Table 1, when we control for route fixed effects in a regression of the percent change in boardings on a LIFT dummy, the coefficient of the LIFT dummy does not change statistically and actually increases in magnitude slightly. This implies that variation across neighborhoods in the impacts of public transit service reductions do not drive the disparities in changes in transit use across socioeconomic groups.[9]

3.3 Commuting

Constraints on lower-income individuals' ability to work remotely could limit their decline in travel. According to Dingel and Neiman (2020), approximately 42% of Seattle area jobs can be performed at home. Mongey et al. (2020) find that nationwide, jobs that do not permit working remotely and jobs that involve high physical proximity tend to be held by individuals that are less educated, have lower income, and are more credit constrained. A disproportionate number of less-educated residents could also be commuting to work at businesses deemed essential and permitted to stay open during the lockdown.[10] Kearney and Pardue (2020) find that, due to essential business exceptions, lower-educated and minority workers are disproportionately likely to be traveling to work during city lockdowns relative to working from home.

We find three pieces of evidence suggesting that differential degrees of remote working across socioeconomic groups represent a key mechanism driving differences in travel behavior during the pandemic.

[9]Transit service reductions in King County were almost entirely effected by route eliminations and frequency changes as opposed to route adjustments. While changes in stop locations within routes are not a likely source of differences in transit use reductions across socioeconomic groups, to the extent that higher- and lower-income riders were differentially affected by frequency changes, that could contribute to observed disparities. The most likely concern is that differing service reductions by time of day create the gap. However, we show in Appendix Table 6 that the results remain within any given time of day.

[10]For information on which types of businesses were deemed essential, see https://coronavirus.wa.gov/what-you-need-know/whats-open-and-closed/essential-business.

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3.3.1 Weekly Travel Cycles

First, the weekly cycle of travel attenuates more for high-income people than for low-income people in the aftermath of the crisis. In Panels (a)-(c) of Figure 3, we show daily time series for travel from all three data sources (SafeGraph, APC, and ORCA). For the SafeGraph and APC data, we break out the data based on whether a CBG's share of residents with a bachelor's degree is above vs. below the median share. For the ORCA data, we split results based on whether the card used is a full-fare ORCA card or a reduced-fare LIFT card.

Based on the SafeGraph data (Panel (a) of Figure 3), prior to March, daily travel among residents of more- and less-educated CBGs tracks closely. In early March, however, travel begins to taper, particularly among residents of more-educated CBGs. This tapering began soon after the Life Care Center Outbreak was detected in late February and as local social distancing directives were issued, but well before all schools closed and the state's stay-at-home order went into effect. Travel continued to decline, and the gap between residents of more- and less-educated areas continued to grow, as the policy response materialized.

Most notably, we observe that the pre-COVID-19 weekday versus weekend cycle of trips persists to a greater extent among residents of less-educated CBGs than among residents of more-educated CBGs post-COVID-19. In other words, less-educated and lower-income individuals not only traveled more in general as the COVID-19 crisis unfolded, but they traveled more particularly between Mondays and Fridays each week. This suggests that requirements of their jobs contributed to the more muted travel response for these groups. The fact that the weekend travel declines among residents of less-educated neighborhoods more closely matched those of residents of more-educated neighborhoods suggests that differences in travel for recreational or other non-work related purposes is unlikely to be the primary driver behind the overall observed differential decline between groups.

In Panel (b) of Figure 3, we show average daily public transit boardings across CBGs by education level using the APC data. The transit data show a more pronounced weekly pattern than the overall travel data, reflecting the fact that many individuals in King County

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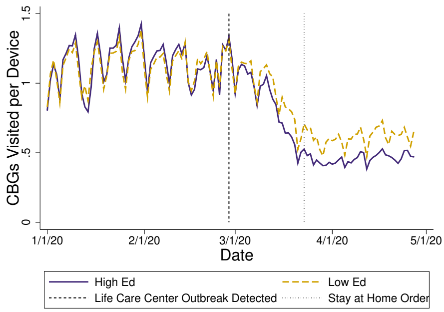
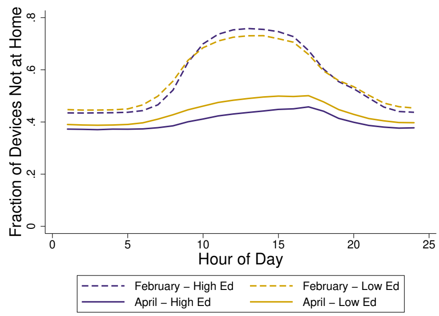
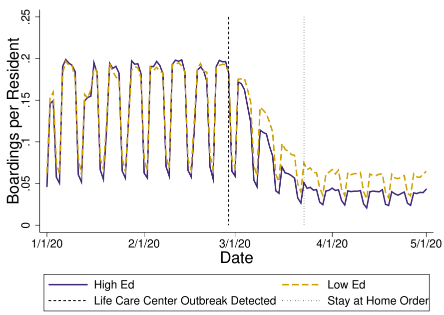


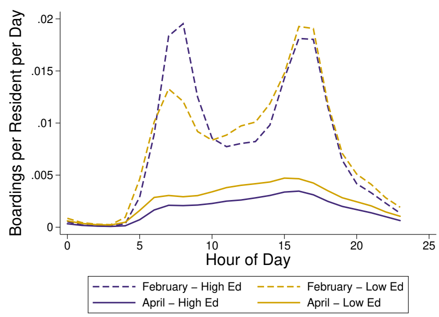
(a) Overall Travel by Nbd Education and Date



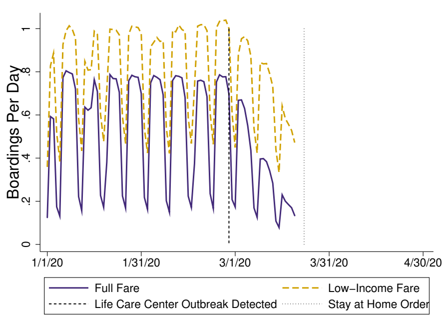
(d) Overall Travel by Nbd Education and Hour



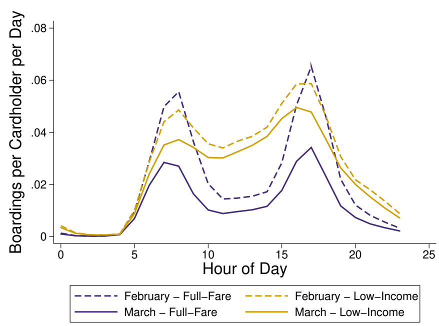
(b) Transit Boardings by Nbd Education and Date



(e) Transit Boardings by Nbd Education and Hour



(c) Transit Boardings by Fare Type and Date



(f) Transit Boardings by Fare Type and Hour

Figure 3. Daily and Hourly Time Series of Travel in King County

Notes: The left column shows daily travel for all days and the right column shows hourly travel averaged over all weekdays in a month. Panels (a), (b), (d), and (e) compare census block groups (CBGs) based on whether they are above or below median for fraction with a bachelor's degree in the 2014-2018 ACS. Panels (a) and (d) use SafeGraph data on cell phone locations to track CBG visits per device and the fraction of devices not observed in their home CBG. Panels (b) and (e) show the same neighborhood comparison for transit boardings measured by automated passenger counters per population from the 2014-2018 ACS. Panels (c) and (f) show ORCA card boardings per card by whether the fare charged is the full adult fare or a reduce LIFT fare; the denominator is the number of cards ever used for that type of fare in January-March 2020.

regularly use transit to get to work but not necessarily for other types of trips. Average boardings per resident are also very similar across high- and low-education neighborhoods prior to March. However, the post-COVID-19 patterns of decline and the growing gap in use between residents of more- vs. less-educated CBGs echo those observed in the SafeGraph data. The weekly cycle of public transit boardings remains more conspicuous for residents of lower-education CBGs than for residents of higher-education CBGs, again implying that much of the differential transit use among those living in low-education areas is for the purposes of traveling to work as opposed to for recreational or other purposes.[11]

We illustrate the time pattern of average boardings per day for ORCA and LIFT cardholders in Panel (c) of Figure 3. The pre-COVID-19 level differences in travel by rider income level are more pronounced in the individual-level data. However, consistent with the SafeGraph and APC data broken out by neighborhood education level, we see an earlier and sharper decline in transit use among regular ORCA cardholders than among LIFT cardholders. Because King County transit authorities eliminated fares on March 21, we do not observe a long post-COVID-19 period in these data. However, the data up to the point of fare elimination corroborate the patterns observed in the two other data sources.

Using the SafeGraph data, at least 54% of the gap between high- and low-income people can be attributed to work travel. Consider column (2) of Table 1 and neighborhoods with 90% and 10% of residents with bachelor's degrees. The coefficients imply that weekday ridership fell by 71% in the high-education neighborhood and 43% in the low-education neighborhood, for a gap of 28%. On the weekend, the difference narrows to 13%. Columns (4) and (7) show similar results for transit boardings as measured by automated passenger counters and ORCA card readers, respectively. Suppose we assume that the weekend effect represents the change in travel that happens every day for non-work reasons. Then, a week that experienced this effect every day would have an income gap of 13%, or 46% of the true gap. The other 54% must be due to work travel. This is almost certainly a lower bound

[11] As Appendix Figure 4 shows, there were no such differential patterns of transit use over time in 2019.

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for the role of work travel. Low-income people are much more likely to work outside regular business hours and on weekends (Shierholz et al. 2012), so the true gap in non-work travel is probably narrower than just what we observe on weekends.

3.3.2 Daily Travel Cycles

A second piece of evidence in favor of differences in remote working as an explanation for disparities in travel behavior are patterns of travel by time of day. These patterns indicate that much of the relatively greater volume of travel among low-income residents of King County is occurring at times at which people are typically travelling to and from work.

Panels (d)-(f) of Figure 3 use the SafeGraph, APC, and ORCA data to illustrate the times of day people travel, broken out by month and by education level of the neighborhood (for the SafeGraph and APC data) or by income of the rider (for the ORCA data).[12] In Panels (d) and (e), there is a clear hourly pattern of travel among residents of both higher- and lower-educated neighborhoods in February; for example, in the SafeGraph data, the fraction of devices not observed in their home CBG rises from around 40% to near 80% within the span of a few hours each morning, then gradually falls back to 40% beginning in the late afternoon. Both lines flatten substantially by April, but more so for residents of higher income neighborhoods. There remains an evident hourly pattern of travel for residents of less-educated neighborhoods, particularly during peak evening travel hours. Similar but even more pronounced changes in travel within days are evident in the APC and ORCA data for public transit use.

3.3.3 Survey Results

As a third and final piece of evidence, we lean on direct survey responses of low-income transit riders. We were in the process of conducting a transit-related survey among low-income residents of King County when the COVID-19 pandemic emerged (Brough et al. 2020a).

[12] For these figures, we average data over only weekdays. Results are very similar if include weekends.

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The survey was given to individuals who agreed to participate in a study in which they had a chance of receiving free transit fares for a limited period of time. The eligible population included those who visited a Washington Department of Social and Health Services office in King County and who qualified for any public assistance program (e.g., SNAP, Medicaid, or TANF). Between December and March, 1318 individuals enrolled in our study and had provided details about their travel intentions in our intake survey.[13]

As the COVID-19 crisis deepened, low-income individuals consistently reported intentions to take trips for essential activities even as intentions to take trips for other activities diminished. All participants in our survey report the activities for which they expect to use the study transit card, allowing them to pick multiple items. We categorize these items into essential (work, school, public benefits, and health), commercial (shopping and errands), and social (recreation, family, religious/community, and other) activities. Figure 4 shows how the average number of activities the person selects in each category varies with the time of the report (which is tied to the timing of their public benefits office visit and enrollment in the study).

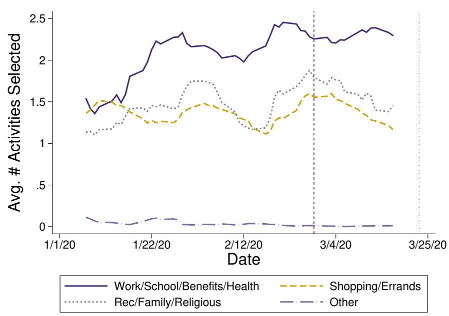

Figure 4. Intended Destinations of Low-Income Transit Users over Time, Survey Responses
Notes: The data come from an intake survey for an ongoing study that provides subsidized transit fares. Respondents state the purposes for which they intend to use the subsidy. The outcome shown in the graph is the average number of items selected in that category for respondents completing the intake survey on a given day.

During the first two months of 2020, all types of trips remain constant. At the onset of COVID-19, intentions to use transit for essential trips remains constant, but intentions to

[13]See Appendix Table 7 for descriptive statistics for this sample.

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use transit for recreational, family, shopping, and other reasons trail off. Following up with a small sample of 119 participants beginning in March 2020 via phone and web travel surveys confirms these intentions.[14] Most trips were for essential (28%) or commercial (40%) purposes. Only 32% are for family, religious, or recreational purposes.

These survey data corroborate our previous results based on weekly and daily travel patterns and point to commuting (as opposed to telecommuting) to work as an important factor driving differences in travel behavior across socioeconomic groups during COVID-19.

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4 Conclusion

In this paper, we examine socioeconomic differences in travel behavior during the COVID-19 pandemic in King County, Washington. Taking advantage of rich administrative data, we document large average declines in travel, and transit use in particular, in response to the pandemic and associated policy responses. However, the average declines mask substantial heterogeneity across socioeconomic groups. Even after accounting for mode substitution and differential public transit service reductions, travel intensity declined less among less-educated and low-income individuals. Using a combination of administrative and survey data, we trace the differences in travel behavior between groups predominantly to the relative inability of less-educated and lower-income individuals to perform their jobs from home.

We add to the growing body of research documenting the disparate, and largely regressive, impacts of the COVID-19 crisis. Our results echo recent findings on, for example, the relatively worse health and labor market impacts of the COVID-19 pandemic for less-educated and lower-income individuals (Abedi et al. 2020, Cajner et al. 2020, Montenovo et al. 2020). The disparities in travel behavior we identify in this paper could be both a cause and consequence of differences across socioeconomic groups in these other outcomes. A higher propensity to travel away from home during the pandemic could contribute to a

[14] Surveying is in progress at the time of writing. Thus far, we have attempted to contact a representative sub-sample of 476 people, with a response rate of 25% among that group. See Appendix Table 7 for descriptive statistics for this sub-sample.

greater prevalence of the virus among certain groups. Meanwhile, differential abilities to work remotely as an avoidance strategy could generate differences in travel behavior along education and income lines.

Notably, our results pertain to King County, which is home to a major city. They may not generalize to other settings, and in particular to rural areas with limited public transit options. Nonetheless, our findings have important immediate and longer-term policy implications. Our results suggest that disparities in travel behavior are less likely the result of a failure of messaging or of the enforcement of orders to refrain from non-essential travel (such as for recreation), and more likely the result of a need among individuals in some groups to travel for work. Our findings also may presage possible shifts in transportation modes and broader changes in mobility patterns as local economies reopen. The movement away from public transit may augur increases in road congestion and pollution as economic activity ramps up. To the extent that remote working becomes a permanent fixture for some companies, it could mitigate traffic congestion, but also increase disparities in travel behavior across socioeconomic groups if those companies tend to have relatively highly educated and high-income workforces.

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Appendix Figures and Tables

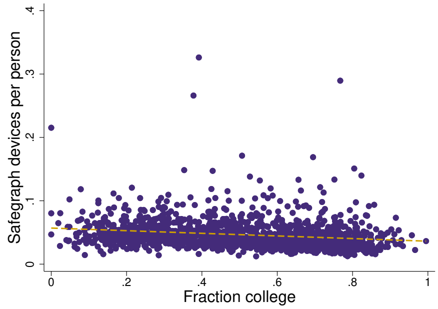

Figure A.1. Correlation of SafeGraph Device Prevalence with Education Levels, by CBG
The unit of observation is census block group (CBG). Number of devices residing in the CBG is the average over January and February from the SafeGraph Social Distancing Metrics dataset. Total population and fraction with a bachelor's degree comes from the 2014-2018 5-year ACS estimates.

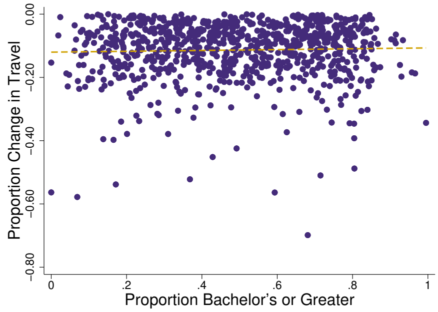

Figure A.2. Percent Change in Travel Intensity between January and February, by Census Block Group
The unit of observation is census block group (CBG). Travel intensity is the number of other CBGs visited per device that usually resides in the given CBG, as measured by the SafeGraph Social Distancing Metrics dataset. Fraction with a bachelor's degree comes from the 2014-2018 5-year ACS estimates. To be consistent with the main text, CBGs with positive change in travel are omitted from the scatterplot.

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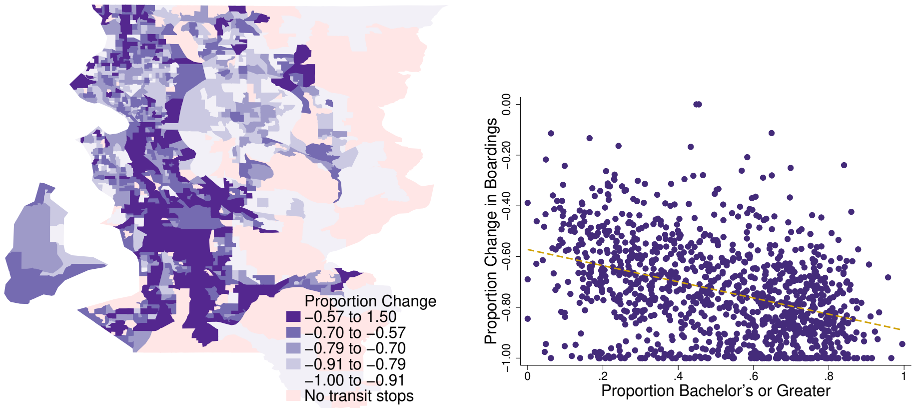

(a) Map of King County, WA (b) Correlation with Neighborhood Education

Figure A.3. Percent Change in Transit Boardings between April and February, by Census Block Group

Notes: The unit of observation is census block group (CBG). Boardings come from King County Metro and are measured by automated passenger counters. Fraction with a bachelor's degree comes from the 2014-2018 5-year ACS estimates. To be consistent with the main text, some CBGs in eastern King County are omitted from the map, and CBGs with positive change in boardings are omitted from the scatterplot.

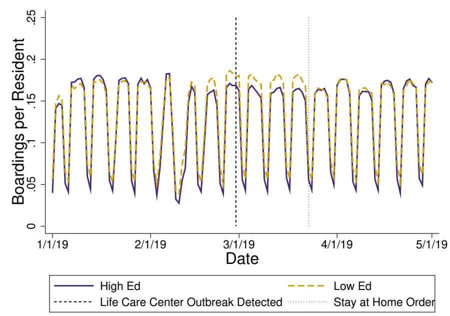

Figure A.4. Transit Boardings by Nbd Education and Date, 2019

Notes: The graph shows daily travel comparing census block groups (CBGs) based on whether they are above or below median for fraction with a bachelor's degree in the 2014-2018 ACS. It shows transit boardings measured by automated passenger counters per population from the 2014-2018 ACS.

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Table A.1. Correlates of Percent Change in Travel from a CBG, April vs Feb, SafeGraph

	(1) % Δ Travel	(2) % Δ Travel	(3) % Δ Travel	(4) % Δ Travel	(5) % Δ Travel
Fraction Bachelor's	-0.30***		-0.24***	-0.15***	-0.17***
	(0.018)		(0.021)	(0.041)	(0.017)
Median Income (100k)		-0.13***	-0.048***	-0.023	
		(0.011)	(0.013)	(0.017)	
Weekday					0.13***
					(0.0079)
Weekday X Frac Bac					-0.18***
					(0.013)
Constant	-0.42***	-0.45***	-0.40***	-0.83***	-0.52***
	(0.010)	(0.014)	(0.013)	(0.23)	(0.010)
Other CBG Characteristics	No	No	No	Yes	No
Mean of Dep. Var.	-0.57	-0.57	-0.57	-0.57	-0.57
R^2	0.092	0.063	0.098	0.12	0.11
N	38340	37854	37854	37854	38340

Notes: Each column shows results from an OLS regression. Coefficients for all independent variables are shown unless noted in the table. The dependent variable in each column is the percent change between February and April of SafeGraph data on the number of other census block groups (CBGs) visited per device usually residing in the CBG as the outcome. Each column covers all of King County, WA. The unit of observation is the CBG-day. Fraction with a bachelor's degree and median income come from the 2014-2018 ACS. Column (4) includes controls for 2014-2018 ACS values for the fraction of the CBG that is male, under 18, over 65, white, black, American Indian/Pacific Islander, Asian-American, Hispanic, moved in the past year, commuting 30+ minutes, commuting by transit, in families, in families with married head, in poverty, English-speaking, receiving public assistance, renting, employed, in the labor force, employed in various occupations, with a vehicle, a smartphone, a computer, and an internet connection. Standard errors are clustered by CBG. Statistical significance at the 10, 5, and 1 percent level is denoted, respectively, by *, **, and ***.

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Table A.2. Correlates of Percent Change in Travel from a CBG, Mid-March vs Feb, SafeGraph

	(1) % Δ Travel	(2) % Δ Travel	(3) % Δ Travel	(4) % Δ Travel	(5) % Δ Travel
Fraction Bachelor's	-0.31***		-0.28***	-0.20***	-0.12***
	(0.017)		(0.023)	(0.043)	(0.020)
Median Income (100k)		-0.11***	-0.016	-0.017	
		(0.0091)	(0.011)	(0.017)	
Weekday					0.14***
					(0.0091)
Weekday X Frac Bac					-0.23***
					(0.016)
Constant	-0.16***	-0.21***	-0.16***	0.094	-0.28***
	(0.0093)	(0.011)	(0.010)	(0.23)	(0.011)
Other CBG Characteristics	No	No	No	Yes	No
Mean of Dep. Var.	-0.32	-0.32	-0.32	-0.32	-0.32
R^2	0.084	0.040	0.083	0.095	0.093
N	14200	14020	14020	14020	14200

Notes: Each column shows results from an OLS regression. Coefficients for all independent variables are shown unless noted in the table. The dependent variable in each column is the percent change between February and mid-March of SafeGraph data on the number of other census block groups (CBGs) visited per device usually residing in the CBG as the outcome. Mid-March refers to March 11-20, 2020. Each column covers all of King County, WA. The unit of observation is the CBG-day. Fraction with a bachelor's degree and median income come from the 2014-2018 ACS. Column (4) includes controls for 2014-2018 ACS values for the fraction of the CBG that is male, under 18, over 65, white, black, American Indian/Pacific Islander, Asian-American, Hispanic, moved in the past year, commuting 30+ minutes, commuting by transit, in families, in families with married head, in poverty, English-speaking, receiving public assistance, renting, employed, in the labor force, employed in various occupations, with a vehicle, a smartphone, a computer, and an internet connection. Standard errors are clustered by CBG. Statistical significance at the 10, 5, and 1 percent level is denoted, respectively, by *, **, and ***.

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Table A.3. Correlates of Percent Change in Transit Boardings from a CBG, April vs Feb

	(1) % Δ Travel	(2) % Δ Travel	(3) % Δ Travel	(4) % Δ Travel	(5) % Δ Travel
Fraction Bachelor's	-0.31***		-0.18***	-0.0046	-0.19***
	(0.024)		(0.030)	(0.061)	(0.021)
Median Income (100k)		-0.17***	-0.11***	-0.022	
		(0.013)	(0.016)	(0.026)	
Weekday					0.20***
					(0.011)
Weekday X Frac Bac					-0.15***
					(0.019)
Other CBG Characteristics	No	No	No	Yes	No
Mean of Dep. Var.	-0.74	-0.74	-0.74	-0.74	-0.74
R^2	0.039	0.041	0.049	0.074	0.064
N	35160	34650	34650	34650	35160

Notes: Each column shows results from an OLS regression. Coefficients for all independent variables are shown unless noted in the table. The dependent variable in each column is the percent change between February and April of King County Metro data on the number of transit boardings measured by automated passenger counters as the outcome. Each column covers all of King County, WA. The unit of observation is the CBG-day. Fraction with a bachelor's degree and median income come from the 2014-2018 ACS. Column (4) includes controls for 2014-2018 ACS values for the fraction of the CBG that is male, under 18, over 65, white, black, American Indian/Pacific Islander, Asian-American, Hispanic, moved in the past year, commuting 30+ minutes, commuting by transit, in families, in families with married head, in poverty, English-speaking, receiving public assistance, renting, employed, in the labor force, employed in various occupations, with a vehicle, a smartphone, a computer, and an internet connection. Standard errors are clustered by CBG. Statistical significance at the 10, 5, and 1 percent level is denoted, respectively, by *, **, and ***.

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Table A.4. Correlates of Percent Change in Transit Boardings from a CBG, Mid-March vs Feb

	(1) % Δ Travel	(2) % Δ Travel	(3) % Δ Travel	(4) % Δ Travel	(5) % Δ Travel
Fraction Bachelor's	-0.50***		-0.47***	-0.21**	-0.31***
	(0.030)		(0.046)	(0.094)	(0.032)
Median Income (100k)		-0.19***	-0.028	0.0053	
		(0.021)	(0.029)	(0.046)	
Weekday					0.43***
					(0.027)
Weekday X Frac Bac					-0.24***
					(0.041)
Other CBG Characteristics	No	No	No	Yes	No
Mean of Dep. Var.	-0.44	-0.44	-0.44	-0.44	-0.44
R^2	0.026	0.013	0.026	0.034	0.058
N	11720	11550	11550	11550	11720

Notes: Each column shows results from an OLS regression. Coefficients for all independent variables are shown unless noted in the table. The dependent variable in each column is the percent change between February and mid-March of King County Metro data on the number of transit boardings measured by automated passenger counters as the outcome. Mid-March refers to March 11-20, 2020. Each column covers all of King County, WA. The unit of observation is the CBG-day. Fraction with a bachelor's degree and median income come from the 2014-2018 ACS. Column (4) includes controls for 2014-2018 ACS values for the fraction of the CBG that is male, under 18, over 65, white, black, American Indian/Pacific Islander, Asian-American, Hispanic, moved in the past year, commuting 30+ minutes, commuting by transit, in families, in families with married head, in poverty, English-speaking, receiving public assistance, renting, employed, in the labor force, employed in various occupations, with a vehicle, a smartphone, a computer, and an internet connection. Standard errors are clustered by CBG. Statistical significance at the 10, 5, and 1 percent level is denoted, respectively, by *, **, and ***.

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Table A.5. Correlates of Percent Change in Transit Boardings, Mid-March vs Feb, ORCA Boardings

	(1) % Change Travel	(2) % Change Travel	(3) % Change Travel
LIFT	0.19***	0.24***	0.11***
	(0.037)	(0.035)	(0.030)
Weekday			0.40***
			(0.046)
LIFT x Weekday			0.10**
			(0.052)
Constant	-0.51***	-0.49***	-0.83***
	(0.022)	(0.018)	(0.042)
Route FE	No	Yes	No
Mean of Dep. Var.	-0.42	-0.42	-0.42
R^2	0.0072	0.14	0.033
N	6680	6680	6680

Notes: Each column shows results from an OLS regression. Coefficients for all independent variables are shown unless noted in the table. The dependent variable in each column is the percent change between February and mid-March of King County Metro data on the number of transit boardings measured by fares paid with an ORCA card as the outcome. Mid-March refers to March 11-20, 2020. Each column covers all of King County, WA. The unit of observation is the route-day-fare type. Fare types are either the low-income LIFT fare or the full adult fare. Standard errors are clustered by route. Statistical significance at the 10, 5, and 1 percent level is denoted, respectively, by *, **, and ***.

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Table A.6. Correlates of Percent Change in Transit Boardings, Mid-March vs Feb, ORCA Boardings

	(1) Peak AM	(2) Peak AM	(3) Peak PM	(4) Peak PM	(5) Off AM	(6) Off AM	(7) Off PM	(8) Off PM
LIFT	0.016	0.043***	0.062***	0.078***	0.10***	0.10***	0.0086	0.014***
	(0.012)	(0.0096)	(0.016)	(0.017)	(0.030)	(0.030)	(0.0083)	(0.0051)
Constant	-0.82***	-0.87***	-0.80***	-0.82***	-0.93***	-0.86***	-0.96***	-0.95***
	(0.0096)	(0.0048)	(0.012)	(0.0084)	(0.0071)	(0.015)	(0.011)	(0.0025)
Route FE	No	Yes	No	Yes	No	Yes	No	Yes
Mean of Dep. Var.	-0.81	-0.81	-0.77	-0.77	-0.88	-0.88	-0.96	-0.96
R^2	0.00041	0.17	0.0024	0.073	0.0056	0.16	0.00012	0.18
N	6680	6680	6680	6680	6680	6680	6680	6680

Notes: Each column shows results from an OLS regression. Coefficients for all independent variables are shown unless noted in the table. The dependent variable in each column is the percent change between February and mid-March of King County Metro data on the number of transit boardings measured by fares paid with an ORCA card as the outcome. Mid-March refers to March 11-20, 2020. Each column covers all of King County, WA but restricts the outcome to boardings occuring during a certain time of the day. The unit of observation is the route-day-fare type. Fare types are either the low-income LIFT fare or the full adult fare. Standard errors are clustered by route. Statistical significance at the 10, 5, and 1 percent level is denoted, respectively, by *, **, and ***.

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Table A.7. Descriptive Statistics for Travel Survey

	Study Population Mean/SE	***Follow-up Survey Responders*** Mean/SE
Intake Survey		
Age	40.4	40.2
	(13.1)	(13.6)
Travel Intention: Number of Essential Destinations	2.00	2.13
	(1.26)	(1.21)
Travel Intention: Number of Commercial Destinations	1.32	1.36
	(0.77)	(0.82)
Travel Intention: Number of Social Destinations	1.39	1.50
	(1.15)	(1.16)
Travel Intention: Number of Other Destinations	0.061	0.050
	(0.24)	(0.22)
Days Used Public Transit in Past Month	15.6	16.2
	(10.9)	(10.9)
Value of a Monthly Transit Pass	14.8	17.1
	(18.9)	(20.6)
Follow-up Survey		
Total Trips Taken		2.09
		(2.27)
Total Trips Taken in Feb		2.96
		(4.15)
Never Left Home		0.37
		(0.48)
Number of Essential Trips		0.58
		(1.07)
Number of Commercial Trips		0.84
		(1.41)
Number of Social/Other Trips		0.67
		(1.50)
Observations	1318	119

Notes: The data come from an ongoing study that provides subsidized transit fares. The top panel reports variables from an intake survey. The bottom panel shows variables measured through a phone and web survey conducted 1-3 months later.

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VETTED AND REAL-TIME PAPERS

Mobility reductions in response to Covid-19 in India: Comparing voluntary, state and central responses[1]

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In response to the surge of Covid-19 cases nations focused on reducing mobility to contain transmission of the virus. This change in mobility patterns can be achieved through two channels; (1) voluntary reductions in mobility due to rising public awareness and (2) explicit social distancing policies imposed by governments. In India, two weeks prior to the national lockdown imposed by the central government on 24th March, state governments had started independently enacting social distancing measures. However, there is little empirical evidence on the efficacy of the initial state-level restrictions, in comparison to the national lockdown. Even fewer studies have commented on the role of public awareness in reducing mobility. This paper contributes by comparing the impact of two policy events on mobility: the first Covid-19 social distancing policy imposed by each state and the imposition of the lockdown. We further explore how the news of the first reported case in a state impacted public awareness. The above effects were estimated by using an event study Difference-in-Differences model with time-varying treatment. Results show that while people did seek information in response to the perception that Covid-19 had 'reached' their state, they did not reduce out-of-home mobility significantly. However, starting from the second day after the lockdown, time spent in residence increased significantly for each day by 3-4% for the next 21 days. This is in sharp contrast to the insignificant

1 We would like to thank Dr. Srikanta Kundu and Dr. Ritika Jain for their guidance. We are grateful to Dr. Charles Wyplosz for his encouragement, and the reviewer(s) for their comments. All errors are ours.
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effect of states' own first policy on mobility. The intervention of the central government had a much larger and persistent impact on mobility than the initial state-level policies, indicating that a unified, coordinated policy intervention is more effective than isolated, subnational efforts.

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1. Introduction

In response to the Covid-19 pandemic, state and central governments in India imposed social distancing policies, which have drastically reduced population mobility levels. The rationale for these non-pharmaceutical interventions (NPIs) is to slow down the rate of infections by reducing contact between individuals. By restricting non-essential economic and social activities, increasing numbers of people are forced to stay in their residences. Growing information and awareness about the disease also leads people to reduce their mobility, even in the absence of explicit restrictions.

In past epidemics of contagious, influenza-like diseases, countries have imposed human mobility restrictions to control transmission. Epidemiological studies have expressed mixed support for their effectiveness (Bajardi et al 2011; Bootsma and Ferguson 2007). Several empirical studies have examined the effects of Covid-19 social distancing policies around the world (Fang et al, 2020; Dahlberg et al 2020; Kurita et al 2020). The variation in timing of Covid-related restrictions among US states has been exploited by several studies to test their effects on mobility.[4] Using event study regressions on mobility data, Gupta et al (2020) found that state-level 'stay-at-home' orders produced smaller mobility reductions than closure orders or emergency declarations. Using aggregated Google mobility data, Wellenius et al (2020) found that emergency declarations led to a 10% increase in time spent inside residences, compared to 29% in response to stay-at-home orders. A subset of studies examined the interaction of political affiliation in US states with adherence to social distancing restrictions (Adolph et al. 2020; Andersen 2020; Painter and Qiu 2020). Studies also showed that with the rising risks of infection, people self-regulate their mobility resulting in higher compliance with government directives (Fang et al, 2020; Suwanprasert 2020).

On 11 March, the Indian Central government advised states to enact mobility restrictions authorized by the Epidemic Diseases Act (1897). The Act empowers state governments to impose sweeping social distancing policies; and states independently used this Act to implement varying policies in response to rising COVID-19 cases till 22nd March. These policies ranged from emergency declarations to complete lockdowns (equivalent to stay-at-home orders). However, India has an 'asymmetric' federal structure where the national government can exercise overarching authority (Tillin, L. 2007). The national government overruled the states, and imposed a nationwide lockdown on 24th March. This ordered a complete halt on all non-essential activities, and was recognized as the most 'stringent' lockdown in the world (Hale 2020).[5]

Indian studies of the restrictions' impact on mobility have focused exclusively on the effects of the national lockdown. Mukhopadhyay and Roy (2020) found that the lockdown disrupted usual migration patterns and initiated a reverse migration from cities to rural regions.[6] Lee and Sahai (2020) found that after the national lockdown intra-city mobility in Delhi reduced by 80%, working days by 73% and income by 53%. However, inter-state portability of food security benefits reduced within-state mobility by 12% (Choudhury et al 2020). As mentioned above, India's states had started enacting social distancing policies two weeks before the national lockdown. However, there is little empirical evidence on the efficacy of the initial

[4] Unlike India, the US federal government did not institute mandatory restrictions and allowed states to decide their own policies, with wide variation.
[5] This was implemented through the National Disaster Management Act, 2005
[6] Using Facebook's mobile users' data from the company's 'Data for Good' platform

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state-level restrictions, in comparison to the national lockdown. This paper adds to the literature by comparing the impact of two time-varying events on state-level mobility: the first Covid-19 policy imposed by each state, and the imposition of lockdown in each state. Even fewer studies have commented on the role of public awareness in reducing mobility. We further explore how the news of the first reported case in a state impacted public awareness about Covid-19, and the extent to which it was translated into mobility reductions. This analysis was conducted by constructing a dataset on state government policies, combined with newly available data on mobility patterns (see section 3).

2. Analytical framework

People move out of their residences mainly to supply labour and participate in consumption of goods and services (Gupta et al, 2020). Mobility patterns can be captured by visits to grocery stores, pharmacies, workplaces, malls and parks. A rise in mobility leads to a negative change in time spent in residences. Conversely, a fall in mobility leads to a positive change in time spent in residences. Hence, the time spent in residences can be a convenient indicator of changes in mobility patterns.

The surge in Covid-19 cases is expected to reduce mobility through two channels, the 'information channel' and the 'policy channel':

(i) *Information channel*: People voluntarily reduce mobility in response to information about the pandemic, independent of government policies. As the number of cases and risk of infection rises, they start limiting movement to essential purposes only (Fang et al, 2020). In India, the online, print and TV media rapidly spread news of detected cases, escalating fear as well as compliance among people (Choudhury et al 2020; Suwanprasert 2020).[7] Recent statistics indicate that a majority of Indian smartphone users access news primarily online (Aneez et al 2019). The aggregated volume of searches on issues related to COVID-19, as a proportion of total searches in a region, could be a proxy for the relative rise in awareness.

(ii) *Policy channel*: State governments imposed social distancing policies to minimise chances for the virus to spread from infected to uninfected patients. These restrictions were legally enforceable with violators being penalised. These measures conform to the 'containment and closure' category of the OxCGRT (Hale et al 2020), a widely used framework to categorize government responses to Covid-19. In India, these policies were classified into the following categories:

a. Restrictions on public gatherings
b. Closures: schools, recreational services (movie theatres, gyms, parks), bars and restaurants, all non-essential services
c. Internal travel restrictions: closure/reduction of public transport, closing state borders
d. Declaration of an epidemic/emergency
e. Lockdown: equivalent to 'stay-at-home' orders, where all the above restrictions are in effect

[7] This includes the number of cases, risks, symptoms, prevention and government policies.

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3. Data and variables

3.1 Change in time spent in residences

We compiled a dataset at the state-day level, recording observations for 30 states from 15th February to 26th April, 2020. Google open-source Community Mobility data at the state level was used to capture the changes in mobility.[8] The data records changes in mobility trends for six categories of locations: grocery and pharmacy stores, parks, transit stations, retail and recreational establishments, workplaces and residences. In 2018, there were an estimated 390.9 million mobile internet users in India (Agnihotri and Chetan, 2019). Around 77% of the smartphones have Google search engines pre-installed. This implies that changes in movement patterns detected should be representative of the overall population. Our primary variable of interest is percentage rise in time spent in residences relative to baseline, which conversely indicates the change in out-of-residence mobility.

The value of each category shows the change in mobility for each day of the week, relative to that day's baseline level (the median for all Tuesdays during Jan 3 – Feb 6, 2020). For example, the value on 24 March (Tuesday) was the percentage change in time spent in residences, relative to the median value for all Tuesdays in the period Jan 3 – Feb 6. In other words, a higher value of residential mobility on Tuesday may not mean a relative increase in the number of people staying at home compared to Monday. To deal with this high seasonality, the time series is smoothed using an exponential window function. A moving window of observations is used to calculate a new, 'smoothed' variable. The original variable (x_t) is averaged to generate a new variable (Y_t) such that each observation gives more weightage to the present day's fluctuation (equations 1 and 2).

$$Y_0 = x_0 \tag{1}$$

$$Y_t = \alpha x_t + (1 - \alpha)x_{t-1}, t > 0 \tag{2}$$

Where α is the smoothing factor, and $0< \alpha <1$. The smoothing factor is optimised to minimize the forecast error for each individual state.

3.2 Social distancing policies and cases

Information on Covid-19 mobility restrictions in Indian states were compiled from news reports and state government executive orders. The state-wise dates of each policy announced is given in appendix A2. Data on the first case in each state was taken from covid19india.org.

3.3 Google Search trends

We include Google Trends data on the relative frequency of online searches related to Covid-19. The data is based on a sample of the total searches for the topic COVID-19[9] in a state within the time range of our study, expressed as an index of relative popularity. A higher value of the index means that searches related to the topic increased, as a proportion of all searches (Rogers, 2016).

[8] The data was gathered by Google from users who have enabled GPS location history on their personal devices.
[9] This includes searches for different words associated with COVID-19 like "coronavirus" and "corona".

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4. Empirical model

Researchers note that disentangling the effect of any specific policy is difficult, as several states imposed multiple policies on the same day (Wellenius et al 2020). As states imposed several restrictions in quick succession or simultaneously, later actions were possibly influenced by public awareness. However, early actions were plausibly exogenous with respect to their impact on mobility (Gupta et al, 2020). This analysis focuses on the first response policy of each state and compares its effectiveness with the declaration of lockdown.

The specification aims to the estimate dynamic effect of a time-varying treatment which is gradually absorbed over time by the treatment unit. The popular strategy to estimate the effect of a treatment on the outcome variable is the two-way linear fixed effect regression, which accounts for unobserved unit-specific and time-specific confounding effects. The basic Difference in Differences (DID) model consists of a single treatment, two groups (control and treated) and two time periods, pre-treatment and post-treatment (Angrist and Pischke, 2009). The basic DID setup, however, assumes a homogenous impact of the treatment through time. But the effect of a treatment can vary dynamically within pre-treatment and post-treatment time periods. Recent studies have used event study DID regression models to incorporate these aspects. (Athey and Imbens 2018, Borusyak and Jaravel, 2017, Callaway and Sant'Anna, 2018, de Chaisemartin and D'Haultfœuille, 2019 and Goodman-Bacon, 2018). The Goodman-Bacon (2018) variant of the panel-event study extends the DID setup to situations where treatment timing can vary across units. This event study DID regression accounts for two features which are key to the current analysis. First, it reveals the time-heterogeneous impact of a treatment/event on population mobility. Second, the specification accounts for the fact that the treatments/events of interest occur at different time periods for each state.

We considered two treatments that vary in their dates of occurrence across states: the first mobility restriction imposed by the state and the declaration of lockdown. Event windows are defined as a range of 20 days before and 20 days after the date of a particular event. These 20-day windows ensure that the effect of non-event related changes in mobility are kept to a minimum.

The panel event study DID regression is given by equation (3), based on Clarke and Schythe (2020).

$$Y_{it} = \alpha + \sum_{j=-20}^{-2} \beta_j \,(W_{it} = j) + \sum_{k=0}^{20} \gamma_k (W_{it} = k) \; + \theta_i + \mu_t + \; \varepsilon_{it} \tag{3}$$

Y_{it} is the percent change in time spent in residences for state i at time t, which has undergone the single exponential smoothing process described in (1) and (2). W_{it} is the event window dummy for the i^{th} state at the t^{th} time period with $t = j$ for the lag periods with j = [-20, -2] and t=k for the lead periods with k = [0, 20]. In the model, θ_i is the set of state fixed effects, which captures the fixed differences in the level of outcomes across states that are time invariant. μ_t is the set of time fixed effects that are common across all states. β_j and γ_k are event study coefficients that captures the deviations from the regular trends that the states experience in the days before and after a given policy treatment respectively. α represents effects which are constant across all states. ε_{it} is the residual error term.

To check whether the first reported case led to a break in parallel trends in people's awareness, we also estimate the above model with search frequencies for Covid-19 as the dependent variable, as shown in equation (4).

$$S_{it} = \alpha + \sum_{j=-20}^{-2} \beta_j \,(W_{it} = j) + \sum_{k=0}^{20} \gamma_k (W_{it} = k) \; + \theta_i + \mu_t + \; \varepsilon_{it} \tag{4}$$

Here S_{it} is the search volume for state i at the t^{th} time period, and windows are defined around the date of the first reported case in each state. To check whether the information about first case translated into changes in mobility equation (4) was estimated with time spent in residence as the dependent variable.

5. Results

5.1 Descriptive results

Appendix A2 shows that states reported their first cases and instituted the first restrictions on different dates. Kerala reported the first COVID-19 case in India on 31st January. Some states – like Delhi - imposed restrictions more gradually, while others – like Arunachal Pradesh - adopted several policies within a shorter span of time. Kerala became the first state to invoke the EDA (1897), by declaring an epidemic/emergency on 11th March. Most states closed schools as their first policy response. The first state to declare a lockdown was Chattisgarh, on 20th March. It was the only state to impose a lockdown before the '*Janta* Curfew' on 22nd March which was effectively a voluntary nationwide lockdown. Nearly all states had enacted at least two mobility restrictions by 21st March.

As the number of cases and restrictions imposed by states began to increase, public awareness about Covid-19 began to rise. This is seen by the time series plots of state-wise search frequencies for Covid-19 (Appendix A3). However, these search volumes gradually declined with time. The same figures also show that mobility fell during the period of analysis, as the time spent in residences rose in all states. With the imposition of the national lockdown (shown by the dashed vertical line indicating March 24), time spent in residences rose sharply. States took varying amounts of time to elicit the maximum compliance to the national lockdown. Additionally, the maximum *levels* of time spent in residences also varied considerably. States' mobility levels did not stabilize at the maximum levels. The figure also shows roughly similar patterns for search frequencies, which increased till March 24th, followed by a much faster decline.

5.2 Event Study DID regression

The above patterns indicate that the national lockdown had a large impact on mobility levels and search trends, with varying lags. Figure 1 plots the estimated event study coefficients for search frequencies from equation (4), with the regression results shown in Appendix A4. The event is defined as the date on which the first case was reported in the state. There are two large, positive coefficients on the first and second days after the event, indicating a sharp break in parallel trends. One day after the first reported case, average search frequencies for Covid-19 went up by 5 percentage points after controlling for time and state-fixed effects. After the first day, none of the coefficients are significant. This implies that the news of the first confirmed case in a state led to a large number of people seeking information about Covid-19 online, but this heightened awareness did not persist beyond two days.

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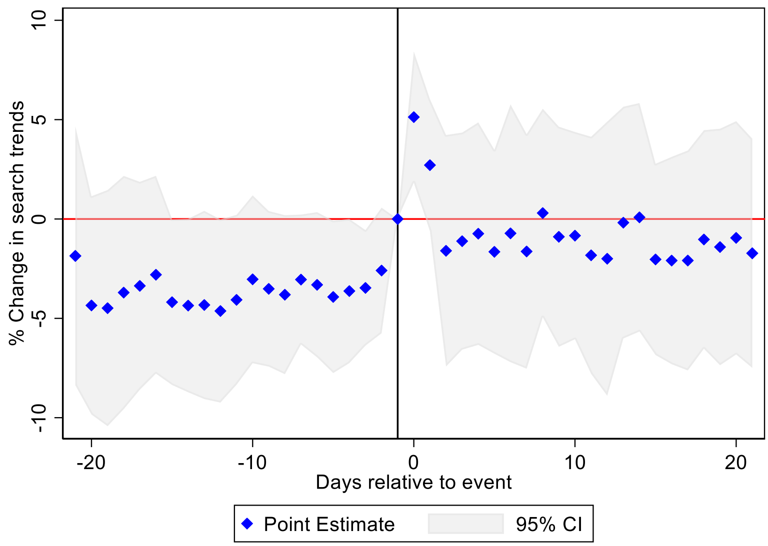
Fig.1 Event study coefficients plot: effect of first reported case on search frequencies



Note: Dependent variable is search frequencies. Regression results included in appendix A4.

As increase in people's information might translate into self-regulated changes in mobility, we estimate the above model with time spent in residences as the dependent variable. However, the report of the first case did not significantly change mobility patterns (as the first column in appendix A7 shows). These results show that while people did seek information in response to the perception that Covid-19 had 'reached' their state, they did not reduce out-of-home mobility significantly. This indicates that the 'information channel' was largely ineffective in reducing mobility. To check the effect of the 'policy channel', equation (3) was estimated with the two policy events mentioned above (section 2).

The second column of appendix A7 reports the coefficients of the event study regression with time spent in residence as the dependent variable. The event is defined as the date of the first Covid-19 related policy in each state. None of the coefficients are significant, reflecting that there was no break in parallel trends in mobility with the imposition of the first state-level policy.

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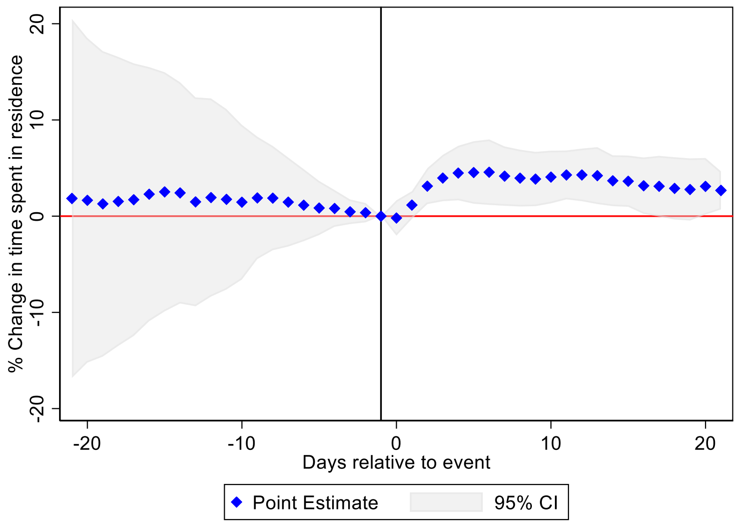
Fig 2. Event study coefficients plot: effect of lockdown on time spent in residences



Note: Dependent variable is time spent in residences. Regression results included in appendix A7, column 3.

The third column of appendix A7 reports the coefficients of the same event study regression model, with the event defined as the date on which a lockdown was imposed in the state. For the majority of states, this was the date of the national lockdown. As mentioned above, for eight states the lockdown was declared one to two days earlier. However, these policies coincided with the '*Janta* Curfew' declared on 22nd March. Hence, although the treatment/event varied over time across states, the impact of the event can be mainly attributed to the national lockdown.

The results show that starting from the second day after the policy, time spent in residence increased significantly for each day by 3-4% for the next 21 days. The cumulative effect of the lockdown was a 74 % rise in people's time spent in their residences from the baseline value, after 21 days. This is in sharp contrast to the insignificant effect of states' own first policy on mobility. We can conclude that the intervention of the central government had a much larger and persistent impact on reducing mobility than the initial state-level policies.

6. Conclusion and Discussion

One reason why heightened awareness about Covid-19 did not translate into voluntary mobility reductions may be related to people's assessment of the relative risks. Different age structures make certain populations more vulnerable to the disease (Suwanprasert, W; 2020). Levels of education and access to the internet vary widely among states, implying possibly high inequalities in how people get and use information.

Citizens' compliance with rules depends on the quality of institutions and level of trust in authority (van Rooj.et al, 2020; Goldstein & Wiedemann, 2020; Liu, N.N, 2015). Societies

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with institutions emphasizing collective decision-making witnessed better compliance with Covid-19 restrictions, than those with individualistic norms and cultures (Jinjarak et al 2020). Some, but not all, Indian states enacted supporting policies to ensure higher compliance with mobility restrictions, most importantly centred around food security (Choudhury et al 2020).

States also varied in how effectively they implemented the initial mobility restrictions, apart from the mere declaration. Enforcement measures like identifying and penalising violators become critical, such that sufficient deterrence fear is created (May, P. J, 2005). States pursued legal action against offenders, such as seizing vehicles and issuing fines. Police forces in several states also resorted to extra-legal violence against violators. Such coercive implementation strategies may be effective, but may create mistrust and antipathy towards authorities due to the disregard for human rights (Ray and Subramaniam 2020). These arguments point towards complex reasons why state-level policies had limited success.

The pandemic has raised questions about the efficacy of varying, state-level public health responses versus uniform, centralised decisions. The intervention of the Indian central government achieved a massive reduction in mobility across states, though it overrode states' autonomy and citizens' rights.[10] Findings from this study indicate that a unified, coordinated policy intervention is more effective than isolated, subnational efforts.

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Appendix

A1: Summary Statistics

Variable	Obs	Mean	Std.Dev.	Min	Max
Percent change in out-of-residence mobility	2232	12.476	12.816	-17	39.057
Search frequency	2232	40.079	30.007	1	100

A2: State-wise dates of first cases and policy responses

States/UT	First Case Date	Date of restricting public gathering	Date of Declaring Lockdown	Date of School Closure	Date of Recreational Service Closure	Date of Restaurant Closure	Date of Internal Travel Closure	Date of Declaration of Emergency
Andhra Pradesh	12-Mar	24-Mar	24-Mar	20-Mar	20-Mar	24-Mar	24-Mar	Not Taken
Arunachal Pradesh	03-Apr	24-Mar	24-Mar	18-Mar	18-Mar	24-Mar	18-Mar	Not Taken
Assam	01-Apr	24-Mar	24-Mar	16-Mar	16-Mar	24-Mar	22-Mar	Not Taken
Bihar	22-Mar	19-Mar	24-Mar	14-Mar	14-Mar	22-Mar	22-Mar	18-Mar
Chandigarh	19-Mar	17-Mar	23-Mar	14-Mar	17-Mar	23-Mar	14-Mar	16-Mar
Chhattisgarh	19-Mar	16-Mar	20-Mar	14-Mar	14-Mar	20-Mar	14-Mar	Not Taken
Goa	26-Mar	22-Mar	22-Mar	15-Mar	15-Mar	22-Mar	24-Mar	Not Taken
Gujarat	20-Mar	24-Mar	24-Mar	16-Mar	16-Mar	24-Mar	24-Mar	15-Mar
Haryana	04-Mar	16-Mar	24-Mar	16-Mar	16-Mar	24-Mar	24-Mar	18-Mar
Himachal Pradesh	21-Mar	18-Mar	24-Mar	15-Mar	15-Mar	24-Mar	21-Mar	13-Mar
J and K	07-Mar	24-Mar	24-Mar	12-Mar	24-Mar	24-Mar	24-Mar	18-Mar
Jharkhand	01-Apr	24-Mar	24-Mar	17-Mar	17-Mar	24-Mar	22-Mar	17-Mar

Karnataka	09-Mar	14-Mar	24-Mar	14-Mar	14-Mar	24-Mar	22-Mar	Not Taken
Kerala	31-Jan	22-Mar	24-Mar	11-Mar	14-Mar	24-Mar	24-Mar	11-Mar
Madhya Pradesh	21-Mar	24-Mar	24-Mar	15-Mar	15-Mar	23-Mar	24-Mar	Not Taken
Maharashtra	09-Mar	14-Mar	24-Mar	18-Mar	18-Mar	24-Mar	24-Mar	Not Taken
Manipur	24-Mar	22-Mar	24-Mar	13-Mar	22-Mar	22-Mar	18-Mar	Not Taken
Meghalaya	NA	24-Mar	24-Mar	17-Mar	17-Mar	24-Mar	24-Mar	Not Taken
Mizoram	25-Mar	24-Mar	24-Mar	18-Mar	18-Mar	24-Mar	10-Mar	Not Taken
Nagaland	NA	22-Mar	22-Mar	17-Mar	22-Mar	22-Mar	24-Mar	11-Apr
NCT of Delhi	02-Mar	17-Mar	24-Mar	13-Mar	17-Mar	20-Mar	24-Mar	14-Mar
Odisha	16-Mar	21-Mar	24-Mar	14-Mar	14-Mar	21-Mar	22-Mar	14-Mar
Puducherry	18-Mar	22-Mar	24-Mar	18-Mar	18-Mar	18-Mar	24-Mar	Not Taken
Punjab	09-Mar	17-Mar	22-Mar	14-Mar	14-Mar	14-Mar	19-Mar	Not Taken
Rajasthan	03-Mar	17-Mar	23-Mar	14-Mar	14-Mar	14-Mar	23-Mar	Not Taken
Tamil Nadu	07-Mar	24-Mar	24-Mar	17-Mar	17-Mar	24-Mar	17-Mar	Not Taken
Telangana	02-Mar	23-Mar	23-Mar	15-Mar	15-Mar	23-Mar	20-Mar	Not Taken
Tripura	07-Apr	24-Mar	24-Mar	16-Mar	16-Mar	24-Mar	24-Mar	Not Taken
Uttar Pradesh	04-Mar	24-Mar	24-Mar	14-Mar	17-Mar	24-Mar	24-Mar	Not Taken
Uttarakhand	15-Mar	23-Mar	23-Mar	15-Mar	15-Mar	23-Mar	24-Mar	15-Mar
West Bengal	18-Mar	24-Mar	24-Mar	16-Mar	17-Mar	22-Mar	22-Mar	Not Taken

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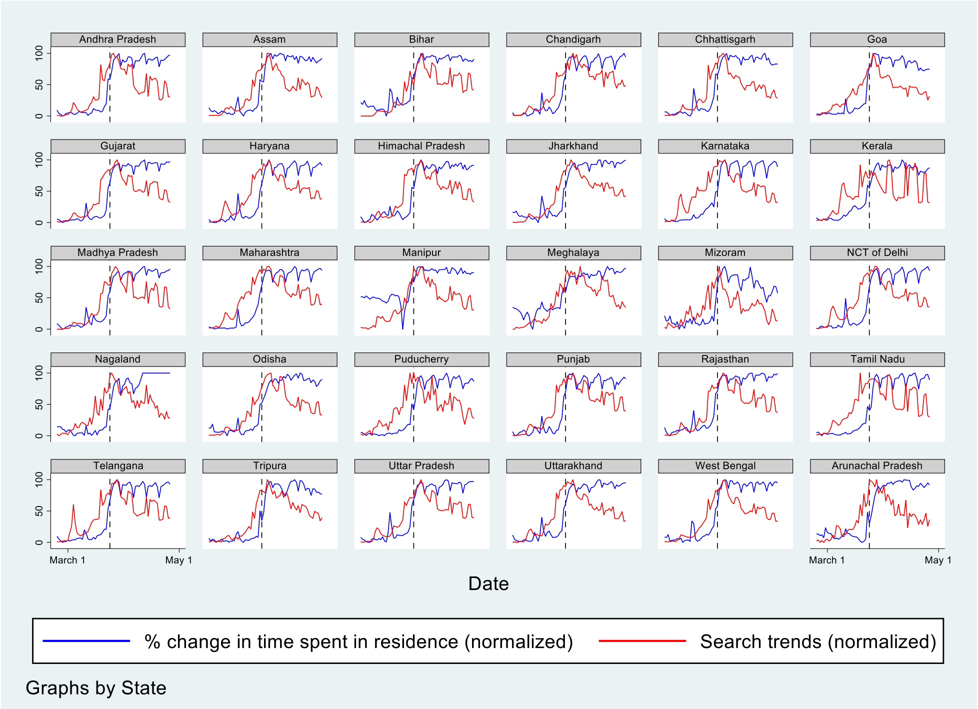
A3: State-wise time series plots of Percentage Change in time spent in residence and Search volume trends



Note: Values are normalised between 0 and 100. Vertical dashed line indicates the date of declaration of national lockdown.

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A4: Event study regression results corresponding to Figure 1(a)

Dependent Variable: Search frequencies for Covid-19

Regressors	First Case
Days Before Event=21	-1.857
	(3.19)
Days Before Event=20	-4.348
	(2.69)
Days Before Event=19	-4.485
	(2.91)
Days Before Event=18	-3.702
	(2.87)
Days Before Event=17	-3.364
	(2.56)
Days Before Event=16	-2.804
	(2.44)
Days Before Event=15	-4.184*
	(2.04)
Days Before Event=14	-4.356
	(2.14)
Days Before Event=13	-4.325
	(2.32)
Days Before Event=12	-4.630*
	(2.26)
Days Before Event=11	-4.068
	(2.09)
Days Before Event=10	-3.034
	(2.07)
Days Before Event=9	-3.515
	(1.92)

Days Before Event=8	-3.809
	(1.96)
Days Before Event=7	-3.048
	(1.60)
Days Before Event=6	-3.311
	(1.79)
Days Before Event=5	-3.921*
	(1.88)
Days Before Event=4	-3.625
	(1.78)
Days Before Event=3	-3.466*
	(1.42)
Days Before Event=2	-2.591
	(1.55)
Days After Event=0	5.129**
	(1.62)
Days After Event=1	2.711
	(1.63)
Days After Event=2	-1.599
	(2.85)
Days After Event=3	-1.114
	(2.67)
Days After Event=4	-0.738
	(2.74)
Days After Event=5	-1.650
	(2.51)
Days After Event=6	-0.720
	(3.18)
Days After Event=7	-1.633

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	(2.89)
Days After Event=8	0.301
	(2.57)
Days After Event=9	-0.894
	(2.71)
Days After Event=10	-0.837
	(2.55)
Days After Event=11	-1.826
	(2.92)
Days After Event=12	-1.998
	(3.38)
Days After Event=13	-0.183
	(2.86)
Days After Event=14	0.085
	(2.81)
Days After Event=15	-2.034
	(2.36)
Days After Event=16	-2.091
	(2.55)
Days After Event=17	-2.089
	(2.71)
Days After Event=18	-1.029
	(2.69)
Days After Event=19	-1.410
	(2.91)
Days After Event=20	-0.948
	(2.87)
Days After Event=21	-1.724
	(2.82)

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Constant	6.230^{*}
	(2.81)
N	2232
Date Fixed Effects	Yes
State Fixed Effects	Yes

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; Clustered standard errors in parentheses

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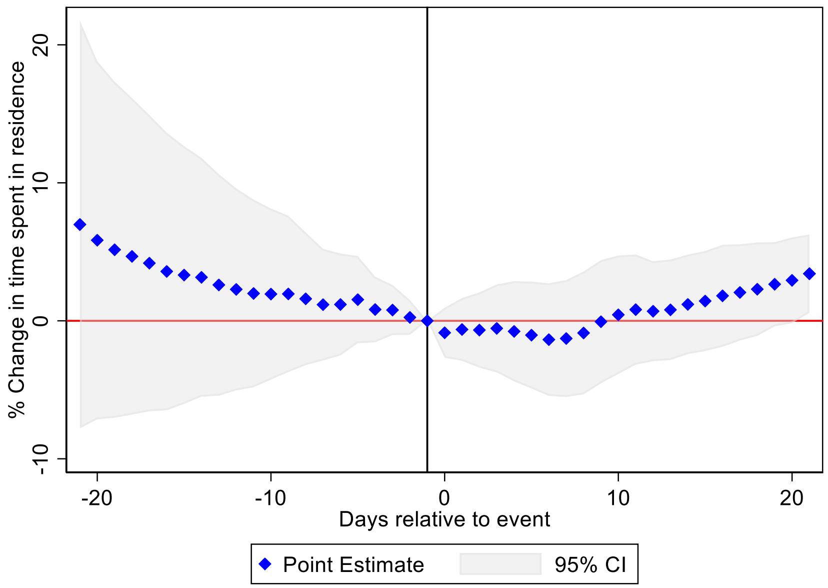
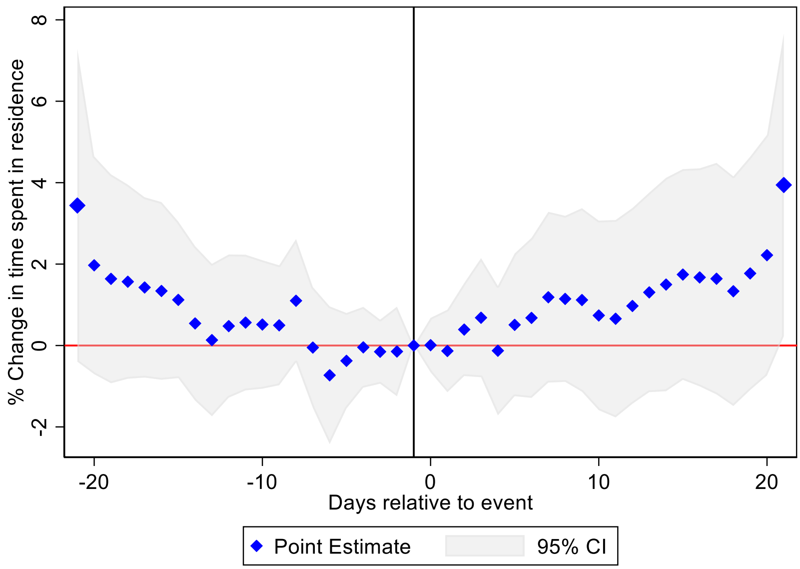
A5: Event study coefficients: Dependent Variable-Change in Mobility, Event-First policy enacted by each state



A6: Event study coefficients: Dependent variable-Change in Mobility, Event-First reported case in each state



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A7: Event study regression results corresponding to Figure 1, A5 and A6

Dependent Variable: Change in time spent in residence

Regressors	First Case	First Policy Implemented	Lockdown
Days Before Event=21	3.440	6.980	1.844
	(1.88)	(7.23)	(9.13)
Days Before Event=20	1.971	5.841	1.650
	(1.31)	(6.35)	(8.26)
Days Before Event=19	1.638	5.153	1.279
	(1.26)	(5.96)	(7.77)
Days Before Event=18	1.568	4.673	1.532
	(1.17)	(5.62)	(7.35)
Days Before Event=17	1.427	4.179	1.712
	(1.08)	(5.26)	(6.94)
Days Before Event=16	1.341	3.578	2.285
	(1.07)	(4.93)	(6.47)
Days Before Event=15	1.122	3.318	2.528
	(0.94)	(4.57)	(6.09)
Days Before Event=14	0.544	3.155	2.421
	(0.93)	(4.24)	(5.63)
Days Before Event=13	0.134	2.601	1.487
	(0.92)	(3.93)	(5.31)
Days Before Event=12	0.477	2.278	1.938
	(0.86)	(3.58)	(5.04)
Days Before Event=11	0.564	1.984	1.752
	(0.82)	(3.33)	(4.59)
Days Before Event=10	0.516	1.939	1.456
	(0.77)	(3.04)	(3.94)
Days Before Event=9	0.496	1.950	1.900
	(0.72)	(2.77)	(3.11)

Days Before Event=8	1.100	1.597	1.877
	(0.74)	(2.35)	(2.65)
Days Before Event=7	-0.050	1.172	1.464
	(0.73)	(1.98)	(2.26)
Days Before Event=6	-0.731	1.185	1.143
	(0.83)	(1.81)	(1.83)
Days Before Event=5	-0.375	1.537	0.851
	(0.57)	(1.55)	(1.38)
Days Before Event=4	-0.044	0.820	0.804
	(0.49)	(1.16)	(0.93)
Days Before Event=3	-0.152	0.783	0.457
	(0.39)	(0.89)	(0.62)
Days Before Event=2	-0.147	0.249	0.363
	(0.54)	(0.62)	(0.49)
Days After Event=0	0.009	-0.864	-0.188
	(0.33)	(0.89)	(0.90)
Days After Event=1	-0.134	-0.626	1.155
	(0.50)	(1.11)	(0.70)
Days After Event=2	0.392	-0.670	3.122**
	(0.56)	(1.34)	(0.92)
Days After Event=3	0.683	-0.549	3.964**
	(0.72)	(1.56)	(1.18)
Days After Event=4	-0.128	-0.763	4.483**
	(0.78)	(1.78)	(1.39)
Days After Event=5	0.508	-1.036	4.539**
	(0.86)	(1.90)	(1.59)
Days After Event=6	0.679	-1.361	4.571*
	(0.96)	(2.00)	(1.67)
Days After Event=7	1.186	-1.281	4.164**

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	(1.03)	(2.07)	(1.51)
Days After Event=8	1.146	-0.879	3.956*
	(1.00)	(2.18)	(1.44)
Days After Event=9	1.118	-0.064	3.859**
	(1.10)	(2.19)	(1.38)
Days After Event=10	0.739	0.438	4.066**
	(1.14)	(2.10)	(1.34)
Days After Event=11	0.658	0.813	4.284**
	(1.19)	(1.95)	(1.24)
Days After Event=12	0.972	0.696	4.289**
	(1.18)	(1.77)	(1.34)
Days After Event=13	1.306	0.798	4.214**
	(1.20)	(1.78)	(1.45)
Days After Event=14	1.498	1.191	3.684**
	(1.28)	(1.76)	(1.29)
Days After Event=15	1.744	1.435	3.635**
	(1.27)	(1.77)	(1.30)
Days After Event=16	1.673	1.815	3.164*
	(1.31)	(1.81)	(1.44)
Days After Event=17	1.642	2.057	3.107
	(1.39)	(1.71)	(1.54)
Days After Event=18	1.334	2.293	2.890
	(1.38)	(1.66)	(1.58)
Days After Event=19	1.771	2.650	2.775
	(1.40)	(1.49)	(1.58)
Days After Event=20	2.219	2.937	3.100*
	(1.45)	(1.52)	(1.44)
Days After Event=21	3.944*	3.414*	2.678**
	(1.82)	(1.39)	(0.97)

Constant	-1.676	-5.814	-0.677
	(1.57)	(7.09)	(9.00)
N	2232	2232	2232
Date Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; Clustered standard errors in parentheses

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Optimal unemployment benefits in the pandemic[1]

Kurt Mitman[2] and Stanislav Rabinovich[3]

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How should unemployment benefits vary in response to the economic crisis induced by the COVID-19 pandemic? We answer this question by computing the optimal unem- ployment insurance response to the COVID-induced recession. We compare the optimal policy to the provisions under the CARES Act-which substantially expanded unemployment insurance and sparked an ongoing debate over further increases-and several alternative scenarios. We find that it is optimal first to raise unemployment benefits but then to begin lowering them as the economy starts to reopen - despite unemployment remaining high. We also find that the $600 UI supplement payment implemented under CARES was close to the optimal policy. Extending this UI supplement for another six months would hamper the recovery and reduce welfare. On the other hand, a UI extension combined with a re-employment bonus would further increase welfare compared to CARES alone, with only minimal effects on unemployment.

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1 Introduction

The Coronavirus Aid, Relief, and Economic Security Act (CARES) Act, passed in response to the COVID-19 pandemic and the ensuing economic crisis, included an aggressive expansion of unemployment insurance (UI). Specifically, the Federal Pandemic Unemployment Compensation added $600 to the weekly benefit amount of all UI recipients through the end of July 2020. As documented by Ganong et al. (2020), this resulted in more than a 100% replacement rate of lost earnings for many job losers. This expansion of UI generosity paralleled an unprecedented increase in jobless claims starting in March 2020—due to the pandemic and the subsequent economic lockdown. The efficacy of this UI expansion — and the desirability of extending it beyond July 2020 — is the subject of an ongoing heated debate in the U.S. Congress. Proponents are pinpointing the dire need for an additional safety net, and opponents argue that it provides work disincentives in an already recovering economy. In this paper, we ask what the optimal UI response is to the economic crisis and how it compares to the current implementation under the CARES act and alternative policy proposals.

We answer this question quantitatively in a search model in which unemployment insurance may be contingent on labor market conditions. We model the COVID-19 crisis as the destruction of job matches coupled with a deterioration of workers' search efficiency. The latter consists of a sequence of adverse shocks to search efficiency, which leads the job-finding rate to initially halt and then gradually recover. This adverse shock can be interpreted as the combination of reduced labor demand, reduced ability to search due to shelter-in-place restrictions, increased cost of search due to infection risk, or reallocation costs associated with sector-specific effects of the epidemic.[1]

We find that the optimal policy calls for raising the replacement rate of unemployment benefits dramatically in response to the fall in search efficiency, and then lowering them once search efficiency starts to recover. Importantly, this means that the rise and fall in the optimal UI replacement rate closely track the shock to search efficiency, not the unemployment rate. This distinction is significant because the unemployment rate is a slow-moving variable that remains persistently high, even as the economy is reopening. Indexing UI to the unemployment rate would (sub-optimally) keep benefits high for longer than our optimal policy implies, thereby impeding the economic recovery and reducing consumer welfare. As a by-product, our results show that the policy implemented under the CARES Act—with

[1]We abstract from heterogeneity in re-employment probabilities amongst those separated. However, see Gregory et al. (2020) for evidence that the separation shocks induced by the pandemic may have disproportionately affects workers that take significantly longer to find stable jobs in the future.

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its expiration set for July 31, 2020—is close to the optimal policy. We also conduct counterfactual exercises to experiment with alternative policy proposals following July 2020. A policy of extending the elevated replacement rate for an additional six months would lead to more protracted high unemployment and lower welfare. On the other hand, a re-employment bonus providing \$450 a week to both re-employed and unemployed would deliver higher welfare than the current UI supplement alone, despite leading to a slightly slower recovery of unemployment.

2 Model

Time is discrete, and the time horizon is infinite. The economy is populated by a continuum of infinitely-lived risk-averse workers, with utility

$$\mathcal{U} = \mathbb{E}_0 \sum_{t=0}^{\infty} \beta^t \left[\ln(x_t) - \frac{1}{\zeta_t} c(S_t) \right] \tag{1}$$

where x_t denotes period-t consumption, and S_t denotes period-t search effort, incurred only when unemployed and restricted to be between 0 and 1. The economy is subject to aggregate shocks to ζ_t; a lower ζ_t implies a higher cost of finding a job, which can be interpreted as reduced ability to search due to shelter-in-place restrictions, increased cost of search due to infection risk, or reallocation costs associated with sector-specific effects of the epidemic. We assume that ζ_t follows an AR(1) process

$$\ln \zeta_t = \rho_\zeta \ln \zeta_{t-1} + \sigma_\zeta \epsilon_t, \quad \epsilon_t \sim \mathcal{N}(0,1) \tag{2}$$

and denote the history of of ζ-shocks up to period t as $\mathcal{Z}_t = \{\zeta_1, ...\zeta_t\}$. The cost function $c(S)$ is strictly increasing, strictly convex, and satisfies $c'(0) = 0$, $c'(1) = \infty$. In the numerical analysis below, we will assume the functional form adopted in Mitman and Rabinovich (2015)

$$c(S) = A \left[\frac{(1-S)^{-(1+\psi)} - 1}{1+\psi} - S \right]. \tag{3}$$

Workers can be either employed or unemployed. When employed, they separate from their job the next period with an exogenous probability δ, and when unemployed, they find a job the next period with the endogenous probability S_t. The law of motion for aggregate

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employment, denoted l_t, is then

$$l_t = (1-\delta)\, l_{t-1} + S_t\, (1 - l_{t-1}) \tag{4}$$

When employed, workers receive exogenous income w and pay a tax τ; when unemployed, they receive $h+b_t$, where h is an exogenous value of home production and b is the government-provided unemployment benefit, which is the policy choice of interest. This unemployment benefit b_t can potentially be contingent on the entire past history of shocks, $\mathcal{Z}_t$.[2]

Unemployed workers choose S_t at each point in time to maximize expected utility, taking as given the government policy $b_t(\mathcal{Z}_t)$. We show in Appendix A.1 that the worker's optimal search behavior leads to the Euler equation for search intensity,

$$\frac{1}{\zeta_t} c'(S_t) = \ln(w-\tau) - \ln(h+b_t) + \beta \mathbb{E}_t \left[\frac{1}{\zeta_{t+1}} c(S_{t+1}) + (1-\delta-S_{t+1}) \frac{1}{\zeta_{t+1}} c'(S_{t+1}) \right]. \tag{5}$$

The Euler equation equates the marginal cost of additional search to the marginal benefit; the latter is the combination of the consumption gain from becoming employed and the benefit of economizing on search costs in the future. Given a policy path $b_t(\mathcal{Z}_t)$, the equilibrium is fully characterized by law of motion (4) and Euler equation (5).

3 Optimal policy

In the optimal policy analysis, we consider the optimal path of history-contingent b_t, l_t and S_t chosen by a benevolent, utilitarian government with commitment power. Such a government maximizes the expected value of

$$\mathbb{E}_0 \sum_{t=0}^{\infty} \beta^t \left[l_t \ln(w-\tau) + (1-l_t) \ln(h+b_t) - \frac{1}{\zeta_t} (1-l_{t-1})\, c(S_t) \right] \tag{6}$$

We assume that the government budget needs to be balanced in expectation, so that

$$\mathbb{E}_0 \sum_{t=0}^{\infty} \beta^t \left[l_t \tau - (1-l_t)\, b_t \right] = 0 \tag{7}$$

In other words, the expected present value of unemployment benefits cannot exceed the expected present value of tax receipts. The maximization of (6) is therefore subject to the budget constraint (7), the law of motion for employment (4), and the optimal search behavior

[2] For tractability, we abstract from policies that can depend on individual worker histories.

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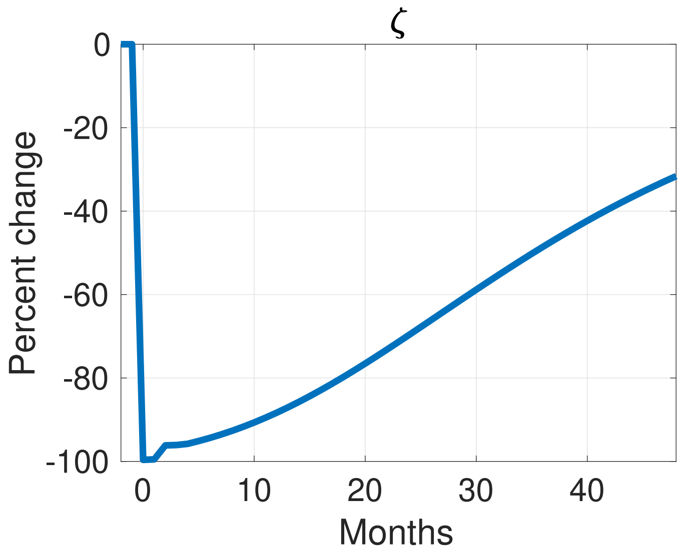

Figure 1: Simulated path for ζ_t that mimics the COVID-19 shock to the economy.

of workers, (5).

4 Quantitative Analysis

We calibrate the model to match salient features of the U.S. labor market prior to the onset COVID-19 pandemic. The model period is one week. We set the discount factor equal to $\beta = 0.99^{\frac{1}{2}}$ to match a 4% annual discount rate. We set $\delta = 0.0081$ to match the weekly job separation rate. We jointly estimate the disutility parameters in the search cost function $A = 3$ and $\psi = 1.9$ so that the model is consistent with the average unemployment rate and empirical estimates of the elasticity of unemployment duration with respect to unemployment benefits from Meyer (1990).[3]

We treat the COVID pandemic in the model as an unexpected shock to the economy. Starting from a steady state at $t = 0$, the economy is hit by a one-time increase in the separation rate, $\delta_0 > \delta$, combined with a sequence of negative ζ_t shocks. Agents have perfect foresight of the entire future path of ζ_t - making this effectively an "MIT shock" to the

[3]We note that there is an ongoing and active debate regarding the effects of unemployment benefits (levels and duration) on worker search effort (micro effects) and firm vacancy creation (macro effects). In innovative work using administrative data from Missouri, Johnston and Mas (2018) find significant affects of potential benefit duration on worker search effort, as measured through exits into employment.

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economy (Boppart et al. (2018)). We think of ζ_t as encompassing policy responses and the decline in economic activity resulting from the spread of the virus. For example, it reflects NPI's, such as orders to limit restaurants to take-out only and stay-at-home orders, as well as reluctance or inability to search due to the fear of becoming infected (consistent with the evidence provided by Wiczer et al. that observable measures of search intensity declined during this period, along with posted job openings).

We choose the size of the separation shock to generate a 15% drop in employment by the end of April 2020. We calibrate the path for ζ, shown in Figure 1, to match the evolution of NPIs. For the first two months, we set ζ such that it's roughly 200 times more costly to find a job than pre-COVID. The fact that many sectors were effectively closed by policy justifies this extreme increase in the cost (moreover, there was a substantial drop in job vacancies, see e.g. Kahn et al. (2020)). For the next two months, we assume that the cost falls by one order of magnitude, to reflect the reversal of NPIs. After that, ζ mean reverts to its pre-COVID level with a monthly persistence of 0.96. The persistence of ζ is calibrated to be relatively high, to match the slow increase in visit to establishments and hours worked (Bognanni et al., 2020) even after NPI's are lifted. For research that explicitly models the interaction between employment and the spread of the virus see Kapicka and Rupert (2020).

We then perform a series of computational experiments. In 4.1, we simulate the response of unemployment in response to the actual policy implemented under the CARES Act. In 4.2, we compute the optimal policy response to the shocks, which is the solution to the problem described in Section 3. In 4.3, we perform other counterfactual experiments, to assess the effects of policy alternatives being discussed.

4.1 Baseline: CARES Act

Figure 2 shows path of the UI replacement rate and the response of employment under the implemented \$600 weekly CARES UI supplement through July 2020. Consistent with the data, the COVID shock combined with the UI extension generates a large and protracted fall in employment.

4.2 Optimal policy response

Next, in Figure 3, we plot the path of the UI replacement rate prescribed by the optimal policy, as characterized above in Section 3. There are important differences as well as similarities between the optimal policy and the one implemented under CARES. First, while the optimal policy still calls for a significant rise in the replacement rate (60%) on impact in

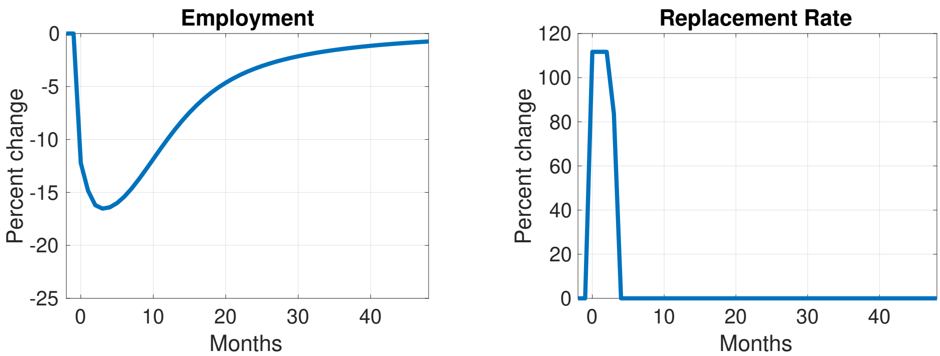

Figure 2: Employment and Benefits under the CARES Act.

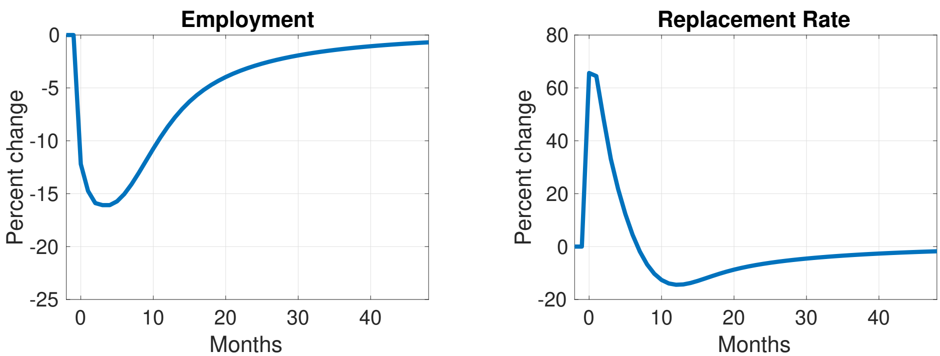

Figure 3: Employment and Benefits under the Optimal Policy.

response to the shock, this is lower than the rise in the replacement rate under CARES. Second, the optimal policy features a rapid fall in benefits around the point where ζ_t (see Figure 1) starts to recover. Importantly, this drop-off in benefits precedes the substantial recovery in employment. Overall, the CARES policy turns out to be close to optimal, largely because of the timing of the UI benefit decline. We find that the two policies are similar in terms of the employment recovery, though it is somewhat faster under the optimal policy. The welfare gain from implementing the optimal policy rather than CARES, in consumption-equivalent variation terms, is 0.1% of lifetime consumption.

4.3 Alternative policies

Next, we consider two alternative policy proposals. First, we consider the proposal (e.g. as included in a provision of the HEROES Act) to extend the $600 weekly UI supplement beyond

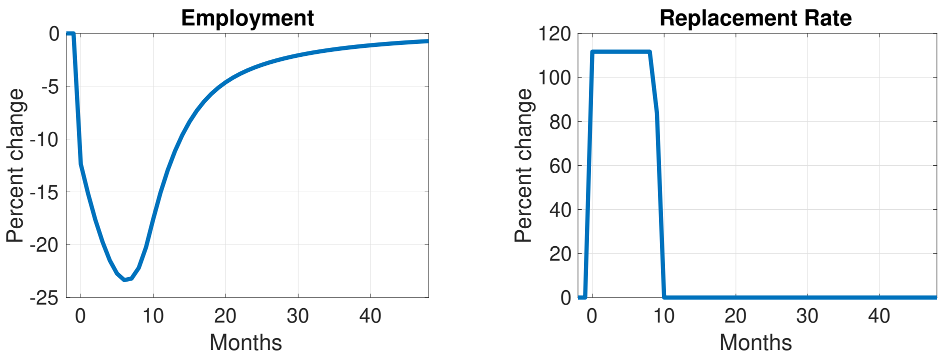

Figure 4: Employment and Benefits under an extension of the $600 weekly CARES UI supplement for all unemployed through December 31, 2020.

July 2020. Figure 4 displays the corresponding path of the replacement rate and the implied employment trajectory. Extending the $600 weekly UI supplement through December 2020 substantially delays the recovery of employment. Further, it generates a deeper trough in the employment drop relative to both the optimal policy and the original CARES act. With regard to the normative implications, we find that the extended CARES policy entails a 0.1% welfare loss in lifetime consumption-equivalent terms relative to the optimal policy.

Second, we consider a recent proposal to extend the weekly UI supplement, but to have it be implemented additionally as a re-employment bonus that newly hired workers could keep. The motivation behind the policy is to remove the moral hazard distortion from the high effective replacement rates under the CARES supplement. Following the bonus proposal being considered. we assume that from August 1, 2020 through December 31, 2020 unemployed individuals receive a weekly $450 supplement. Newly hired workers during this time period keep receiving the $450 supplement in addition to their weekly wage. The dynamics of employment and the benefits/bonus policy are plotted in Figure 5. Employment falls slightly more than under the optimal and CARES scenarios, but by significantly less than in the scenario where the $600 CARES UI supplement is extended through December 31, 2020. The bonus program can therefore effectively overcome the majority of the moral hazard distortion induced by the higher benefit replacement rate. In terms of normative implications, we find that the CARES Bonus program delivers roughly the same welfare (in CEV terms) as the optimal policy, despite leading to a slightly slower recovery of unemployment.[4]

Figure 6 plots the relative sizes of the unemployment rate increase and recovery under the

[4]The slower recovery occurs because, under risk aversion, a lump-sum payment lowers search effort even when the payment accrues to both employed and unemployed, i.e. the flow surplus from being employed is $\ln(w-\tau+\Delta)-\ln(h+b_t+\Delta)<\ln(w-\tau)-\ln(h+b_t)$ for $\Delta>0$.

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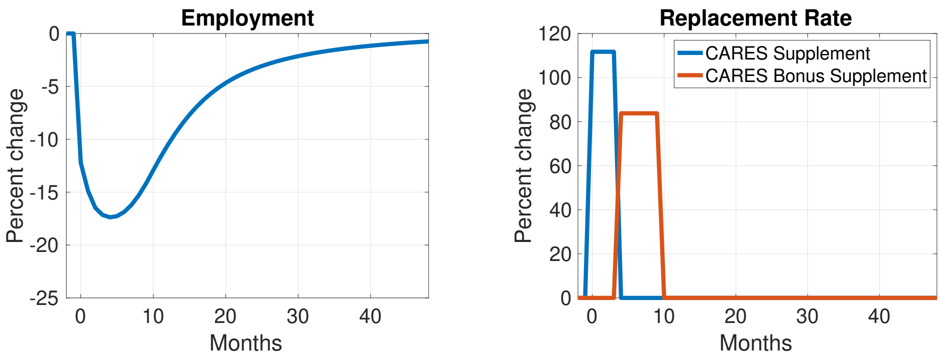

Figure 5: Employment and Benefits under a \$450 CARES UI supplement/re-employment bonus for all unemployed and newly hired through December 31, 2020.

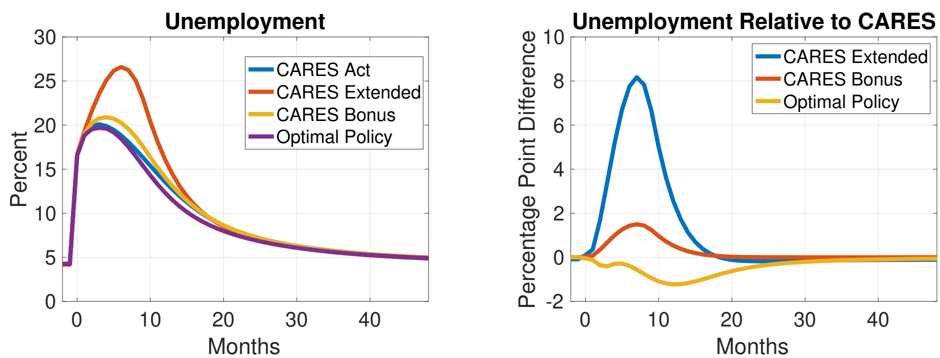

Figure 6: Unemployment rate under the four different scenarios: baseline CARES Act, the optimal policy, an extension of CARES \$600 weekly payment through December 31, 2020, and a CARES "Bonus" program through December 31, 2020.

four scenarios considered. The right panel illustrates the relative size of the unemployment rate, under the optimal policy, the extended CARES policy, and the re-employment bonus, as compared to the baseline CARES policy.

4.4 A more optimistic recovery scenario

For robustness, we also consider a more optimistic recovery scenario. We lower the persistence of the ζ_t process from 0.99 to 0.9 on a weekly basis. The path of ζ_t is illustrated in Figure 7. With this lower persistence, the cost of finding of job is essentially back to the steady state level within one year of the onset of the pandemic. Figure 8 illustrates the optimal policy response in this case, and Figure 9 compares the unemployment trajectory across the

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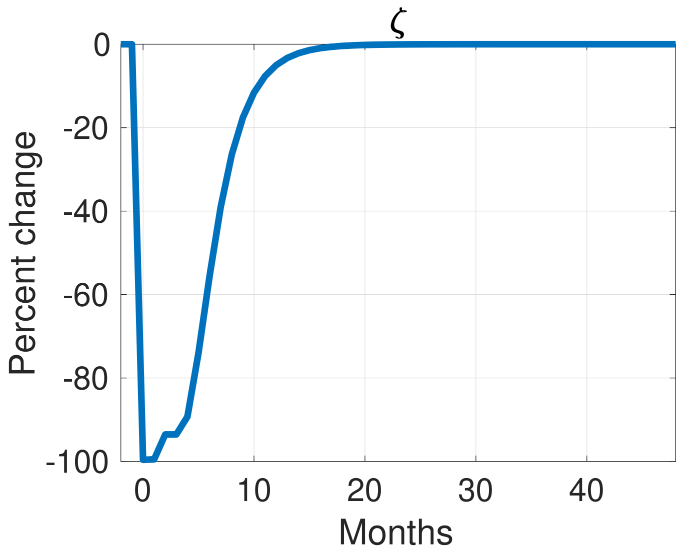

Figure 7: Simulated path for ζ_t: optimistic recovery scenario.

different policy alternatives. Not surprisingly, we find significantly faster recoveries under all policy alternatives — unemployment is back to steady state levels within 18 months of the shock. Interestingly, we find that the moral hazard distortion of the extended CARES policy is much *stronger* under this more optimistic scenario, as can be seen by comparing Figures 6 and 9. The easy availability of jobs associated with the faster recovery worsens the moral hazard distortions. If agents know that the cost of finding a job will be very low after the UI supplement runs out, the cost of delaying search (and collecting the high UI supplement) is low. On the other hand, in the more pessimistic baseline scenario, the more sluggish recovery makes households more willing to accept jobs even if the replacement rate is higher than the wage, because they are afraid of being unemployed after the supplement runs out, when it's still costly to find a job. The weak recovery in our baseline case thus served as a discipline device mitigating the moral hazard distortion.

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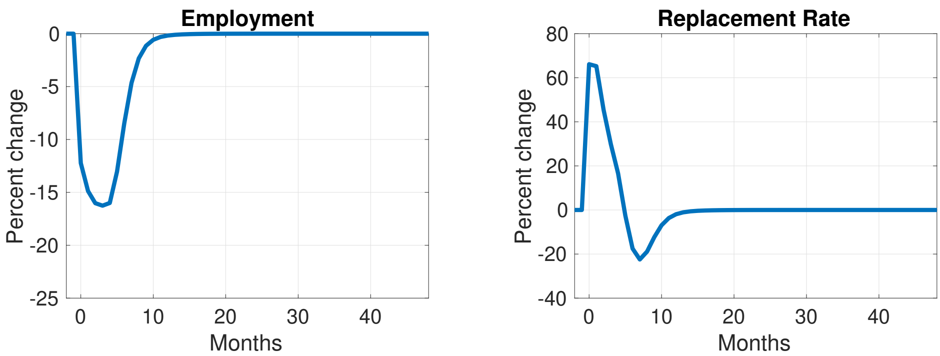

Figure 8: Employment and benefits under the optimal policy: optimistic scenario.

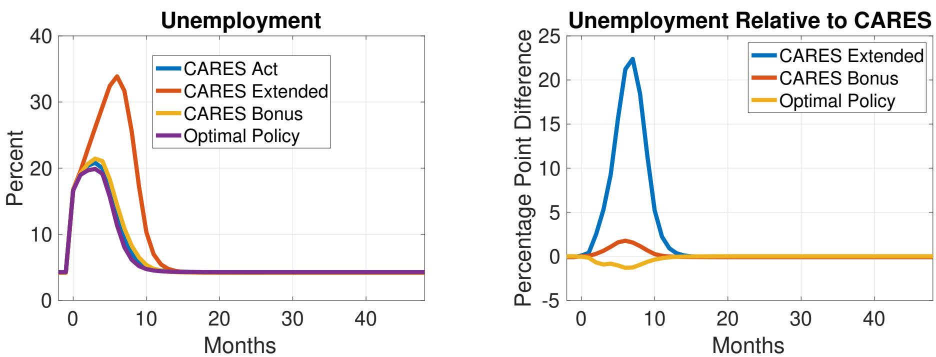

Figure 9: Comparison across policies under optimistic scenario: baseline CARES Act, the optimal policy, an extension of CARES $600 weekly payment through December 31, 2020, and a CARES "Bonus" program through December 31, 2020.

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5 Discussion

We assessed the optimal UI policy and compared it to the one currently implemented, as well as the most prominent alternative proposals. We found that the UI supplement applied under the baseline CARES Act policy performs quite well. The UI supplement combined with a re-employment bonus would perform even better, despite a somewhat slower employment recovery. On the other hand, a blanket extension of the UI supplement for another six months would substantially hamper the recovery and reduce welfare. The broad lesson is that expectations matter. The optimal policy starts lowering the UI payment when the economy begins to reopen - *before* the recovery of employment. A policymaker that indexes UI benefits to the level of unemployment would keep them high for too long, generating hysteresis (see, e.g., Mitman and Rabinovich (2019)). Furthermore, expectations of weak labor market conditions in the future mitigate the moral hazard problem today, as we showed by comparing alternate recovery scenarios. High future costs of search make it easier to incentivize current search effort, creating a further reason for a temporary UI expansion.

We have focused on the amount and timing of unemployment benefits, and thus abstracted from two other important aspects of the current crisis: the distinction between temporary and permanent separations, as examined in Gregory et al. (2020) and Birinci et al. (2020); and the epidemiological side of the discussion, as applied to a search model by e.g. Kapicka and Rupert (2020). Combining these unique features of the recession with our analysis of unemployment insurance is an important extension. We have also abstracted from two general equilibrium feedback mechanisms. First, we have ignored potential aggregate demand effects induced by providing transfers to the unemployed that could speed the recovery (Kekre (2019); Ravn and Sterk (2016); Den Haan et al. (2018)). Our view is that the COVID-19 pandemic (and ensuing policy response with lockdown orders) represents a supply shock and thus that normal demand channels will be muted (see Guerrieri et al. (2020) for an alternative view). Second, we have abstracted from firm labor demand and the response of wages and labor force participation to benefit policy (see, e.g.,Hagedorn et al. (2013, 2015)). We leave these for future work.

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A Supplementary derivations

A.1 Details on the worker problem

Throughout, let $\mathcal{Z}_t = \{\zeta_1, ...\zeta_t\}$ denote the history of shocks. Let $W_t = W_t(\mathcal{Z}_t)$ be the value of a worker entering period t employed, and $U_t = U_t(\mathcal{Z}_t)$ the value of a worker entering period t unemployed. These values satisfy the Bellman equations

$$W_t = (1-\delta)\left[\ln(w-\tau) + \beta\mathbb{E}_t W_{t+1}\right] + \delta\left[\ln(h+b_t) + \beta\mathbb{E}_t U_{t+1}\right] \tag{8}$$

$$U_t = \max_S -\frac{1}{\zeta_t}c(S) + S\left[\ln(w-\tau) + \beta\mathbb{E}_t W_{t+1}\right] + (1-S)\left[\ln(h+b_t) + \beta\mathbb{E}_t U_{t+1}\right] \tag{9}$$

where the period-t expectation is taken with respect to ζ_{t+1} and dependence on $\mathcal{Z}_t$ is suppressed for notational convenience. From (9), the first-order necessary condition for the optimal $S = S_t$ is

$$\frac{1}{\zeta_t}c'(S_t) = \ln(w-\tau) - \ln(h+b_t) + \beta\mathbb{E}_t\left[W_{t+1} - U_{t+1}\right] \tag{10}$$

Subtracting (9) from (8) also gives

$$W_t - U_t = \frac{1}{\zeta_t}c(S_t) + (1-\delta-S_t)\left\{\ln(w-\tau) - \ln(h+b_t) + \beta\mathbb{E}_t\left[W_{t+1} - U_{t+1}\right]\right\} \tag{11}$$

Combining (10) with (11) gives (5).

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