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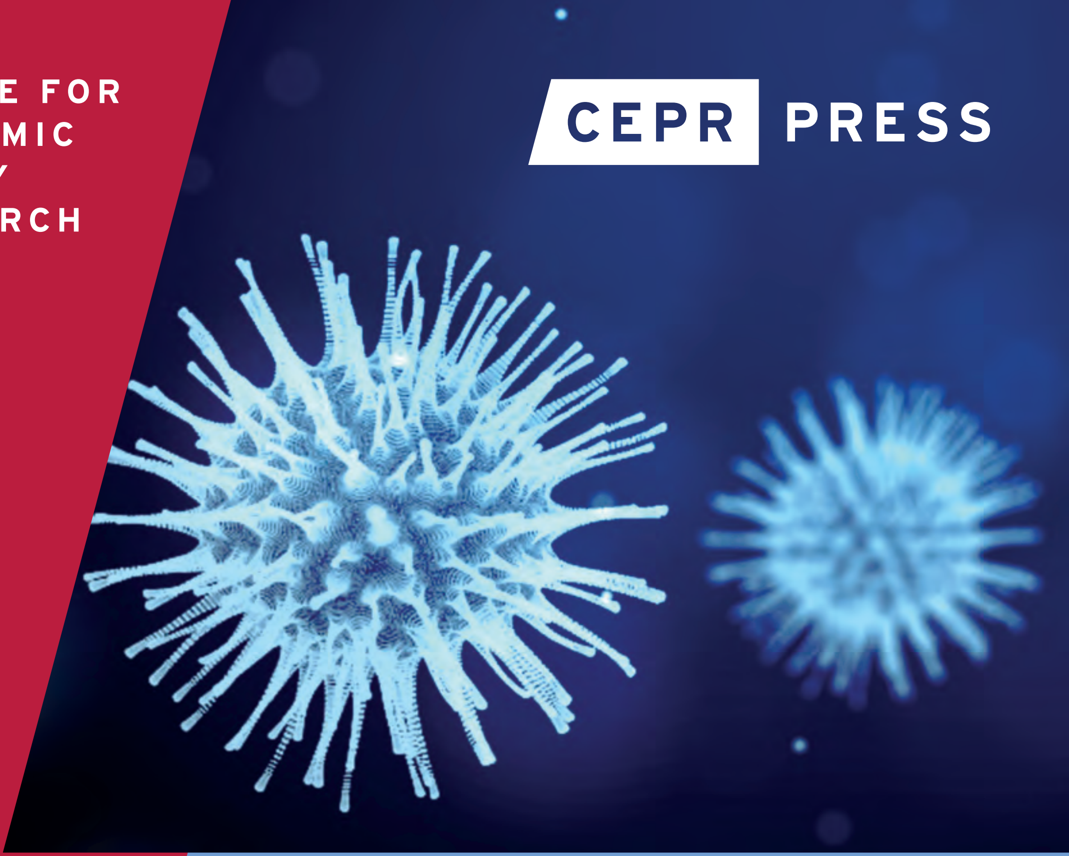

COVID ECONOMICS

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.

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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

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*Journal of Econometrics**

Journal of Economic Growth

Journal of Economic Theory

*Journal of the European Economic Association**

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*Journal of Labor Economics**

Journal of Monetary Economics

Journal of Public Economics

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*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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Tracking the economic impact of COVID-19 and mitigation policies in Europe and the United States[1]

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Date submitted: 2 July 2020; Date accepted: 2 July 2020

We use high-frequency indicators to analyze the economic impact of COVID-19 in Europe and the United States during the early phase of the pandemic. We document that European countries and U.S. states that experienced larger outbreaks also suffered larger economic losses. We also find that the heterogeneous impact of COVID-19 is mostly captured by observed changes in people's mobility, while, so far, there is no robust evidence supporting additional impact from the adoption of non-pharmaceutical interventions. The deterioration of economic conditions preceded the introduction of these policies and a gradual recovery also started before formal reopening, highlighting the importance of voluntary social distancing, communication, and trust-building measures.

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1 We thank Giovanni Dell'Ariccia, Sole Martinez Peria, Andrea Pescatori, Petia Topalova, and IMF colleagues for help with data and useful discussions. We are grateful to Dalya Elmalt and Mu Yang Shin for excellent research assistance and to Alberto Sanchez for suggesting useful data sources.

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1. Introduction

As the COVID-19 pandemic spread across the globe, many countries adopted Non-Pharmaceutical Interventions (NPIs), such as school and business closures and shelter-in-place orders, to mitigate the outbreaks. NPIs are controversial due to uncertainty about their efficacy in containing the outbreak and potential negative economic effects (Correia, Luck, and Verner 2020; Lilley, Lilley, and Rinaldi 2020). Providing evidence of their effects is crucial in the reopening phase, where governments ponder lifting NPIs and restoring the economy to its normalcy. In fact, fine-tuning mitigation policies may greatly reduce the economic and human costs of the pandemic as shown by a fast-growing quantitative literature (see Acemoglu et al. 2020; Alvarez, Argente, and Lippi 2020; Favero, Ichino, and Rustichini 2020; Jones, Philippon, and Venkateswaran 2020, among others). Despite its urgency and importance, empirical evidence on mitigation policies is still scant.

Mandatory mitigation measures may exacerbate the economic impact of the pandemic, at least in the short run, by halting some activities, in particular those requiring face-to-face interaction. However, if most of these activities are already disrupted by voluntary behavior of consumers and workers that do not consume certain goods and services and perform certain tasks for the fear of contagion (as highlighted by Eichenbaum, Rebelo, and Trabandt 2020), then the additional damage of coercive policies may be negligible. Similarly, if these policies are established but compliance is low, their impact may be limited.

While it is clear that COVID-19 is causing economic disruption at unprecedented speed and scale (Baldwin and Weder di Mauro 2020; Gopinath 2020), the actual size of its economic impact and the relative importance of the underlying channels are still unknown. This poses an additional empirical challenge for the assessment of the economic impact of the NPIs. For instance, if most of the impact of the pandemic were due to the heightened uncertainty (Baker et al. 2020c) at the global level, then economic activity in a particular country or region may not respond to local health conditions or policies. This hypothesis is supported by earlier work by Carvalho et al. (2020) and Kahn et al. (2020) who find no evidence of a positive correlation between economic losses and the onset and severity of the pandemic, using, respectively, Spanish consumption data and U.S. labor market indicators.

In this paper, we use high-frequency indicators (HFIs) to provide a close-to-real-time assessment of the economic effects of the pandemic and NPIs. In the context of fast and massive economic disruptions due to the COVID-19 pandemic, the relatively slow frequency of most macroeconomic indicators represents a challenge for policymakers tasked with mitigating the economic impact of the crisis. In comparison, HFIs—such as electricity usage, unemployment insurance claims, measures of mobility based on location data, and other economic data collected by the private sector (Chetty et al. 2020)—are available with a short time lag and can be used to track economic activity as close as possible to "real time".

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Importantly, some of these indicators are available at daily frequency, which is useful for identifying abrupt changes in people's behavior and economic activity. Exploiting variation in the timing of the NPIs across regions, we can show the extent to which NPIs affect mobility and economic activity by comparing the timing of their changes with the date when the policies are introduced.

We ask the following questions. First, is the COVID-19 pandemic a truly common shock, or do countries or regions that experience more extensive outbreaks also suffer more economically—in which case, what makes an economy more vulnerable to the COVID-19 shock? Second, how do people's mobility respond to the outbreak? Third and most importantly, what is the role of mobility in transmitting the COVID-19 shock to the economy and how does it compare to the role of *de jure* NPIs?

Our main insight is that outbreaks and people's mobility matter a great deal while *de jure* NPIs seem to matter less. We find that the economic impact of COVID-19 is mostly captured by changes in people's mobility, while, so far, there is no robust evidence supporting additional impact from NPIs, during both the lockdown and reopening periods, especially in the United States.

More specifically, we find that European countries and U.S. states that have experienced larger outbreaks have experienced larger economic losses. Energy usage in Europe suggests that weekly output has declined by between 20 to 29 percent in mid-April in the median country and about twice as much in the hardest hit countries, such as Italy and Spain. Focusing on the heterogeneous impact of the shock across U.S. states, we find that states that are poorer and have lower share of workers that can work from home are more vulnerable.

We also find that most of the variation between states or countries is captured by the observed changes in people's mobility, while the timing of *de jure* NPIs have no discernable effect on economic outcomes between March and mid-April. In fact, the decline in economic activity or mobility precedes rather than follows the introduction of such mitigation policies. This evidence is a warning against optimistic projections that the economic recovery will start once NPIs are lifted. The economy may not rebound unless workers and consumers feel safe about resuming their normal behavior. Consistent with this, we show that mobility and economic activity recovery started happening before the easing of NPLs. Moreover, there is no sharp acceleration in mobility and economic activity after the reopening.

The rest of the paper is structured as follows: we start by presenting the HFIs used in the analysis (Section II). We then discuss the effect of COVID-19 on economic activity in Europe (Section III) and the United States (Section IV). In Sections V and VI, we zoom in on the role of mitigation policies during the lockdown and the early phase of the reopening. We then discuss a few caveats to our analysis and conclude.

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2. High-Frequency Indicators and Other Data

To monitor economic activity across European countries and U.S. states, we collect a variety of indicators, depending on data availability, from January 2020 to early May 2020.

First, we use data on electricity usage. This is a very useful high-frequency indicator of economic fluctuations (Chen et al. 2019; Cicala 2020) because electricity is an input in most economic activities and it is difficult to substitute in the short run. Data on electricity usage are available within the same day from the European Network of Transmission System Operators for Electricity (ENTSO-E) for 32 European countries. They are available from the U.S. Energy Information Administration for 64 Balancing Authorities (BAs) in the United States, who are responsible for monitoring and balancing the generation, load, and transmission of electric power within their region. We use GIS data to map BA regions to U.S. states. Since energy consumption exhibits substantial day-of-the-week fluctuations, we measure electricity usage with respect to the same day of the same week in 2019.[1]

Second, for the United States, we collect data on unemployment insurance (UI) claims, which are available at weekly frequency for all the states from the U.S. Department of Labor with only a one-week lag. These administrative data closely track labor market developments, so that an increase in UI claims is one of the earliest signs of rising unemployment and a weakening economy. We complement our analysis with data on hours worked, number of employees, and number of businesses from more than 100,000 local businesses (and their hourly employees) from the time-tracking tool Homebase (Bartik et al. 2020).[2] This company covers primarily individual-owned restaurants and small- and medium-sized businesses in food service, retail, and other sectors that employ many hourly workers. Daily changes in hours worked, number of employees, and number of open businesses are computed by comparing a given day with the median of the same day of the week for the period January 4–31, 2020.

We focus on these indicators rather than other HFIs, such as hotel reservations or flight cancellations, as we aim to capture the overall pace of economic activity rather than focus on the hardest-hit sectors.[3] We also abstract from long-term and persistent macroeconomic effects (Jorda, Singh, and Taylor 2020).

To track the severity of the pandemic in each country or region, we use the number of COVID-19 cases or deaths as a proxy for the severity of the outbreaks. We gather daily data on confirmed cases

[1] For example, we compare the electricity usage on Tuesday March 31, 2020 with that on Tuesday April 2, 2019.

[2] Homebase data should be interpreted with caution as the coverage is predominantly in small businesses and services.

[3] Proprietary consumer data or asset prices can also provide useful information (Baker et al. 2020a, 2020b; Alfaro et al. 2020).

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and deaths from the European Centre for Disease Prevention and Control (ECDC) for the European countries and from the COVID Tracking Project for U.S. states.

We use the Google Community Mobility Index to measure people's mobility. This index is available on a daily basis both for European countries and U.S. states. Based on the number of times individuals visit certain places, daily change in mobility are computed with respect to the median value in the corresponding day of the week during the period of January 3 to February 6, 2020. The index is available for six categories (transit stations, workplaces, retail stores and recreation places, groceries and pharmacies, residential, and parks). To capture *de facto* social distancing, we focus on places which are the usual focus of social and economic life (transit stations, workplaces, retail stores and recreation places, groceries and pharmacies), excluding residential and parks.

Finally, we use data on the timing of adoption for several NPIs, including social distancing, closure of nonessential services, closure of public venues, school closures, and shelter-in-place orders, obtained from Hale et al. (2020) for Europe and from Keystone Strategy for the United States. Several other variables (e.g., population, employment) are drawn from standard sources (U.S. Census Bureau, Bureau of Economic Analysis, World Economic Outlook).

3. COVID-19 AND ECONOMIC ACTIVITY IN EUROPE

We compare current weekly electricity usage to the same week in 2019 for 32 European countries (counting only workdays and excluding weekends). Since early March, electricity usage has been declining in most countries, despite lower energy prices: in the median country in our sample, energy consumption was about 5 percent lower than in 2019 during weekdays. The decline has accelerated in April, as the health crisis and mitigation measures have become more widespread, with energy consumption about 15 percent lower than in 2019 during the weeks in the middle of April.[4] The decline is largest in Italy—the first European country to experience an extensive outbreak and one of the hardest-hit so far: electricity usage has plummeted by almost 30 percent compared to 2019.

Cross-country analysis confirms the relationship between the extent of the health crisis and energy decline. Countries that have been experiencing a more severe outbreak, as measured by deaths per capita, and a sharper decline in people's mobility have also reduced their energy consumption more

[4] A back-of-envelope calculation suggests that this decline in electricity usage corresponds to a weekly output loss of approximately 20 to 29 percent (in annualized terms). Approximately 30 percent of electricity is used by households in Europe. Therefore, assuming that neither the mix of input used in productive processes, nor the amount of electricity consumed domestically have changed during the pandemic, a 1 percent drop in electricity usage would correspond to a 1.43 (=1/0.70) percent drop in production. Alternatively, we estimate the elasticity of electricity with respect to GDP using annual data and exploiting banking crises as shocks to economic activity (see Table A1). We obtain coefficient values ranging from 0.53 to 0.78, implying that, historically, a 1 percent drop in electricity usage is associated with 1.3 to 1.9 percent drop in output.

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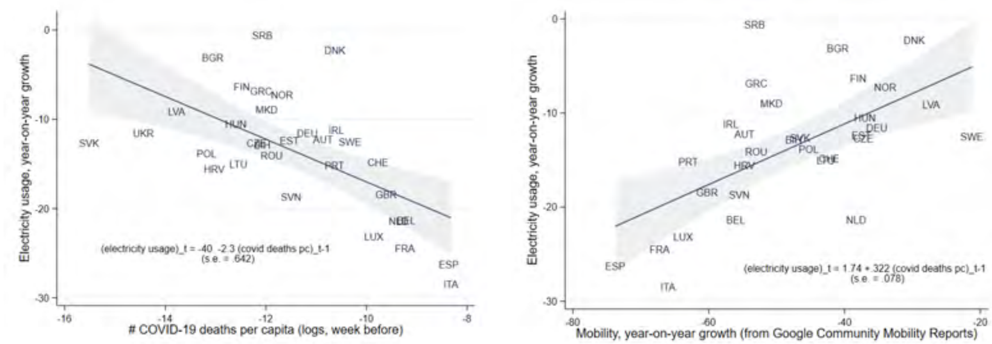
(Figure 1).[5] The estimated coefficient in Panel A suggests that during the acute stage of the pandemic, a doubling of the COVID-19 outbreak leads to a decrease in energy consumption of approximately 2.3 percent. This is a non-trivial amount, given that the number of cases doubled every 2 to 3 days during the early phase of the epidemic. Mobility has, in general, stronger explanatory power than deaths per capita (Panel B; also see Table A2). These figures come with all the well-known caveats of extrapolation from cross-sectional results to the aggregate (Nakamura and Steinsson 2018), together with the additional issue of potentially severe non-linearities (the small sample size thus far limit a thorough assessment of these non-linearities).

FIGURE 1. COVID-19, Electricity Usage, and Mobility in Europe



A. Electricity usage and COVID-19 deaths B. Electricity usage and mobility

Source: [ENTSO-E](), [ECDC](), [Google Community Mobility Reports]().
Notes: Panel A plots the percent change in weekly electricity usage relative to the same week in 2019 and the number of COVID-19 deaths per capita in 32 European countries. Panel B plots the percent change in weekly electricity usage relative to the same day of the same week in 2019 and the percent change in visits public places (retail and recreation, grocery and pharmacy, transit stations, and workplaces) within a geographic area relative to the pre-COVID-19 period. In both charts, the solid line plots a linear fit and the gray area shows the 95 percent confidence interval bands. The sample is the week ending on April 11, 2020.

These results are robust to controlling for weather conditions or differences in sectoral composition of output—other important factors for electricity usage. We proxy for weather conditions with the average temperature difference between 2020 and the same week of 2019. To capture the heterogenous sectoral composition of output, we use either the share of manufacturing in national production or the expected GDP loss for a six-week lockdown as calculated by Barrot, Basile, and

[5] The figure plots data for the week of April 11 when electricity usage reached its bottom. Results for other weeks between mid-March and late April are similar, also see Table A4.

Sauvagnat (2020) using differences in sectoral composition and propensity to telework across countries (see Table A2).[6]

4. COVID-19 AND ECONOMIC ACTIVITY IN THE UNITED STATES

Electricity usage has also decreased sharply in the United States: average daily usage in early April was 5 percent lower than it was during the same period in 2019. More strikingly, 30 million new unemployment insurance claims have been filed in the first six weeks since the pandemic, implying a dramatic reduction in employment and labor force participation (see Bick and Blandin 2020; Cajner et al. 2020; Coibion, Gorodnichenko, and Weber 2020, among others). It took almost one year to reach that number in the wake of the Lehman Brothers' bankruptcy.

Cross-sectional analysis shows that the decline in electricity usage and job losses, as measured by the weekly filings for unemployment insurance between March 8 and April 25, are concentrated in states that have been hit harder by COVID-19, as measured by the number of COVID-19 deaths per capita over the same period (Figure 2).[7] This evidence is corroborated when looking at the change in the number of hours worked (Figure A1, panel A) and it is in line with recent evidence shown for U.S. cities during the 1918 flu pandemic (Correia, Luck, and Verner 2020).[8]

We then exploit both the time and cross-sectional dimensions of the data and estimate a model with time and state fixed effects. In this case, we observe that—*within state*—as the number of COVID-19 cases increases, electricity usage decreases while filings for UI claims increases. The results are economically significant and summarized in Figure A2. For electricity usage, the average elasticity is 0.8 for continental U.S. states, indicating that a doubling of the number of cases leads to a decrease in energy consumption of 0.8 percent. For UI claims, the average elasticity is 0.11, indicating that a doubling of the COVID-19 positive cases is associated with 11 percent more claims. However, this elasticity weakens over time—in the first week during which the number of claims spiked, the elasticity was close to 0.3. The same pattern is present if we look at the decline in the number of hours

[6] The sectoral composition of output controls for sectoral heterogeneity in electricity usage (e.g. manufacturing is more energy intense than other sectors) and exposure to the COVID-19 shock (e.g. some industries are hit harder by mitigation policies due to the lack of teleworking arrangements).

[7] In our analysis for Europe, we are not able to look at the labor market given that high-frequency data like weekly UI claim filings are not available.

[8] Related evidence on labor market outcomes is also discussed by Doerr and Gambacorta (2020) and Béland et al. (2020), among others. Note that, the results on the UI claims differ from those shown by Kahn et al. (2020), who look at the UI claims only until April 11 and, more importantly, measure the extent of the pandemic by the number of cases per capita in the week of March 14, when the outbreak had not yet reached all U.S. states.

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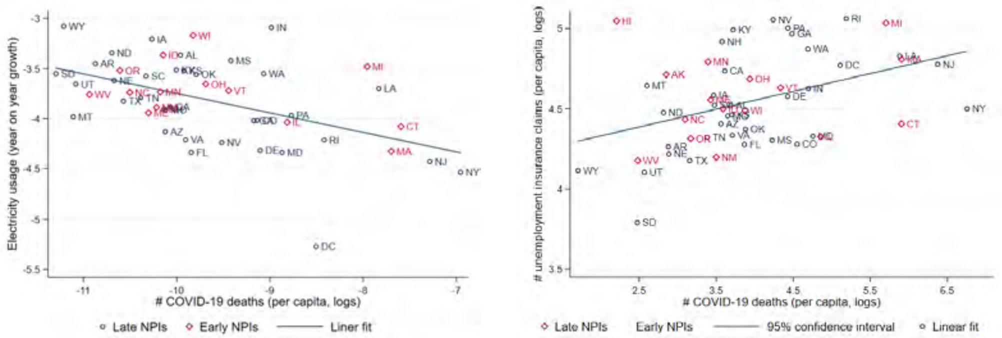
worked (Figure A3), suggesting that the labor market has reacted very fast to the outbreak and the related social distancing measures put in place to contain the pandemic.[9]

FIGURE 2. COVID-19, Electricity Usage, and UI Claims in the United States



A. Electricity usage and COVID-19 deaths B. UI claims and COVID-19 deaths

Source: U.S. Energy Information Administration, U.S. Department of Labor, U.S. Census Bureau, https://covidtracking.com, Google Community Mobility Reports.
Notes: This figure plots electricity usage (in logs of megawatt hours, compared to the same day of the same week in 2019, Panel A) and the total number of unemployment insurance claims per capita (in logs, Panel B) against the number of COVID-19 deaths per capita (in logs). The sample period is March 1-April 5 (Panel A) and March 8-April 25 (Panel B). The solid line plots a linear fit. Panel A controls for the share of service industry. The slope is -0.19 (s.e.=0.05) in Panel A and 0.11 (s.e.=0.04) in Panel B. States are divided between early (red labels) and late (blue labels) NPI adopters. The NPIs considered are social distancing, closure of nonessential services, closure of public venues, school closures, and shelter-in-place orders. A state is considered an early NPIs adopter if all these five policies have been implemented within a week from the day in which the first death in the state has been recorded.

Finally, the labor market reaction to the pandemic is heterogenous across U.S. states, not only in relation to the intensity of the COVID-19 shock, but also depending on institutional and economic characteristics. In particular, for a given severity of the outbreak, job losses have increased more in poorer states, in states with a lower employment share in hotels and leisure, as well as a lower share of jobs that can be done from home (as measured by Dingel and Neiman 2020), and in states that do not have in place laws for paid sick leave (Figure A2, panel A).[10] The impact on electricity usage is also stronger among states with a lower share of jobs that can be done at home (Figure A2, panel B).

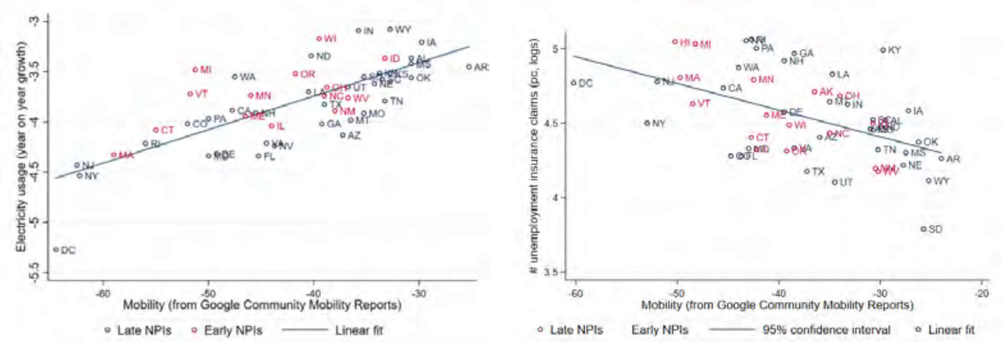
[9] Similar findings are also valid if we use the change in the number of employees working or open local businesses. Results available upon request.

[10] All these results are reported in Table A3. Findings are similar if we look at the change in (i) hours worked, (ii) the number of employees, and (iii) open businesses in the Homebase sample of local businesses. Results are available upon request.

5. COVID-19, ECONOMIC CONTRACTION, AND MITIGATION EFFORTS

We now turn to the channel through which the COVID-19 shock transmits to the economy, with a focus on the role of people's mobility and NPIs—two related but distinct issues. People's mobility is a *de facto* measure of mitigation efforts and captures *de jure* NPIs, such as school closures and shelter-in-place orders, but also compliance and voluntary social distancing by individuals.

FIGURE 3. Mobility, Electricity Usage, and UI Claims in the United States



A. Electricity usage and mobility B. UI claims and mobility

Source: U.S. Energy Information Administration, U.S. Department of Labor, U.S. Census Bureau, https://covidtracking.com, https://github.com/Keystone-Strategy/covid19-intervention-data, Google Community Mobility Reports.

Notes: This figure plots electricity usage (in logs of megawatt hours, compared to the same day of the same week in 2019, Panel A) and the total number of unemployment insurance claims per capita (in logs, Panel B) against the percent change in visits to various places (grouped under four categories: retail & recreation, grocery & pharmacy, transit stations, and workplaces) within a geographic area relative to the pre-COVID-19 period, at the state level. The sample period is March 1-April 4 (Panel A) and March 8-April 25 (Panel B). The solid line plots a linear fit. Panel A controls for the share of service industry. The slope is 0.033 (s.e.=0.004) in Panel A and -0.017 (s.e.=0.004) in Panel B. States are divided between early (red labels) and late (blue labels) NPI adopters. The NPIs considered are social distancing, closure of nonessential services, closure of public venues, school closures, and shelter-in-place orders. A state is considered an early NPIs adopter if all these five policies have been implemented within a week from the day in which the first death in the state has been recorded.

We find that mobility is positively associated with electricity usage (Figure 3, panel A) and negatively associates with UI claims (Figure 3, panel B). In contrast, the relationship between *de jure* NPIs and economic contraction is weaker. Figure 2 shows that in the United States early NPIs adopters do not perform, on average, worse than late adopters neither in terms of electricity usage nor the number of UI claims. The cross-sectional correlation between electricity usage across European countries and the stringency of mitigation policies (Hale et al. 2020) is statistically significant only in the early weeks of the pandemic but not in April (Table A4). In the United States, the timing of *de jure* NPIs

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is not significantly associated with the number of UI claims per capita, whether we control for the size of the local outbreak and other state-level characteristics or not (Table A5).[11]

In other words, *de jure* NPIs are only part of the story. Compliance and voluntary social distancing matter. This is also in line with the Swedish experience, albeit the situation is still unfolding: the observed decline in electricity usage in Sweden—which has adopted relatively less strict mitigation policies but where many have been practicing social distancing by choice—is fairly similar to that in neighboring countries although the decline in mobility is smaller (Figure 1, panel B). Data from Denmark and Sweden also show that consumer spending dropped by 25 percent in Sweden compared to a slightly higher drop of 29 percent in Denmark, which was similarly exposed to the pandemic but adopted much stricter containment measures (Andersen et al. 2020). Furthermore, the similar economic outcomes seem to be accompanied by higher death rates than other Nordic countries (Bricco et al. 2020).

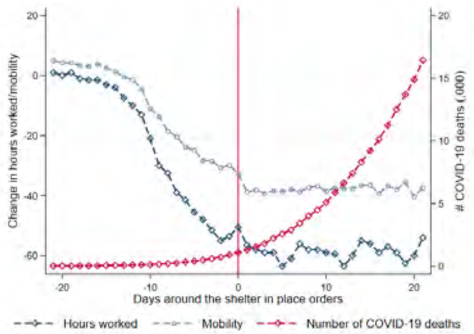
Furthermore, using daily data on a large sample of local businesses,[12] we find that the sharp decline in hours worked—relative to January—begins well before the introduction of *de jure* NPIs at the state level (Figure 4, panel A), and it is quite common across states, as shown also by Bartik et al. (2020). Similarly, by the time stay-at-home orders were adopted in Europe, the decrease in mobility and electricity usage was already sizeable (Figure 4, panel B). Interestingly, relative to local COVID-19 caseloads, mobility dropped earlier in the United States than in Europe although NPIs were adopted around the same phase of the epidemic. The United States reached 1,000 COVID-19 cases 11 days after Europe. The first stay-at-home order in the United States (in California) was issued 10 days after the first stay-at-home order in Europe (in Italy). But mobility in the United States fell by 20 percent compared to January 2020 just 4 days after Europe (Figure 5). Moreover, the early NPIs—school closures in many cases—triggered the decline in mobility and economic activity in Europe (Figure 5, panel D) but even they seem to have been anticipated in the United States (Figure 5, panel C).

A likely explanation of this difference is that Americans "learnt" from the European experience and practiced voluntary distancing and closures before *de jure* NPIs were adopted. Increased news coverage on COVID-19 during the second week of March is also consistent with this increased "awareness" explanation: on March 11, for instance, the WHO declared COVID-19 a pandemic, the NBA suspended its games, and Hollywood star Tom Hanks revealed that he had tested positive.

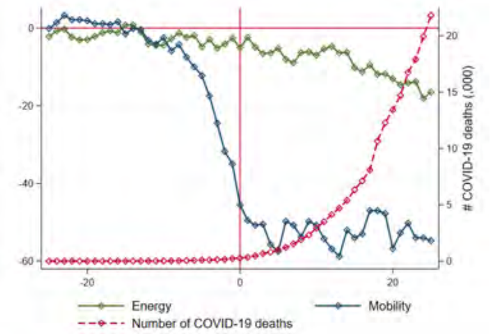
[11] As before, findings are similar if we look at the change in (i) hours worked, (ii) the number of employees, and (iii) open businesses in the Homebase sample of local businesses. Results are available upon request. In a similar vein, personal vehicle travel declined both in states that imposed stay-at-home orders early in March and in those that imposed such orders later, although the decline in the former was slightly more (Cicala et al. 2020).

[12] The data come from Homebase. Sectors that are hit harder and earlier by the pandemic, such as restaurants, may be overrepresented in this data source.

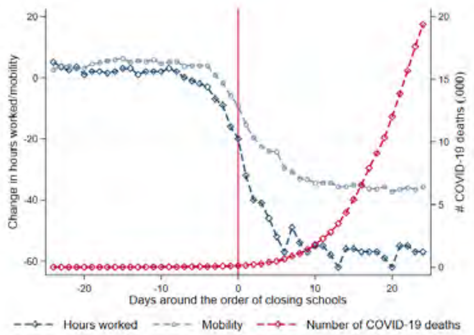
FIGURE 4. COVID-19, NPI Timing, Mobility, and Economic Activity



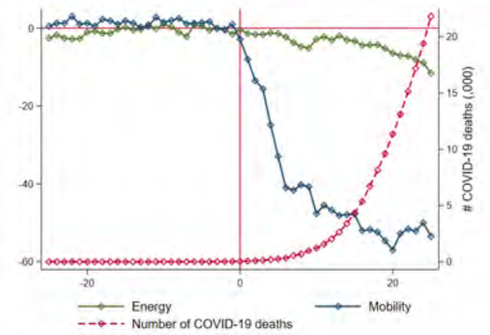
A. Shelter-in-place orders in the United States



B. Stay-at-home orders in Europe



C. School closures in the United States

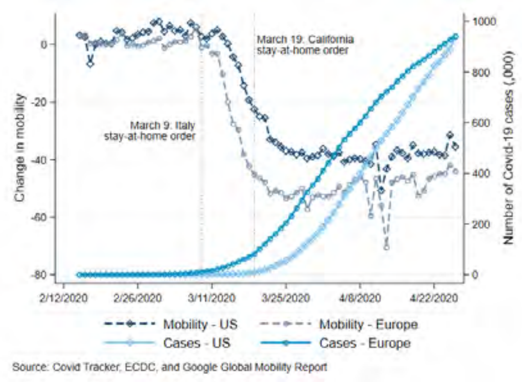


D. School closures in Europe

Source: https://covidtracking.com, https://github.com/Keystone-Strategy/covid19-intervention-data, Google Community Mobility Reports, Homebase, ECDC, ENTSO-E, Hale et al. (2020).

Notes: Panels A and C plot the changes in hours worked for a large sample of small businesses and in mobility (both relative to the pre-COVID-19 period) for the median U.S. state, and the cumulative number of COVID-19 deaths for all U.S. states in the sample. The x-axis is the number of days before/after the introduction of NPIs (shelter-in-place in Panel A and school closures in Panel C). The sample only includes states that have adopted the policy by April 30. Figures based on other NPIs, such as closure of non-essential business or public venues, are qualitatively and quantitatively similar. Panels B and D plot the median change in electricity usage—with respect to the previous year—the median change in mobility relative to the pre-COVID-19 period, across European countries, and the cumulative number of COVID-19 deaths for all European countries. The x-axis reports the number of days before/after the introduction of NPIs (stay-at-home orders in Panel B and school closures in Panel D). The sample only includes European countries that have adopted the policy by April 10. NPIs introduction and classification is based on Hale et al. (2020).

FIGURE 5. COVID-19, NPI Timing, and Mobility in Europe versus the United States



Source: https://covidtracking.com, Google Community Mobility Reports, Homebase, ECDC, ENTSO-E.
Notes: The chart plots the cumulative number of COVID-19 cases and changes in mobility relative to the pre-COVID-19 period in the United States and Europe. The vertical lines are March 9 and March 19, 2020—the dates when state-at-home orders were issued in Italy and California, respectively.

These findings suggest that avoiding or delaying NPIs may not fully shield an economy from the COVID-19 shock,[13] and that the depression of economic activity may persist even after mandatory lockdown measures are lifted if people continue to voluntarily limit their mobility.

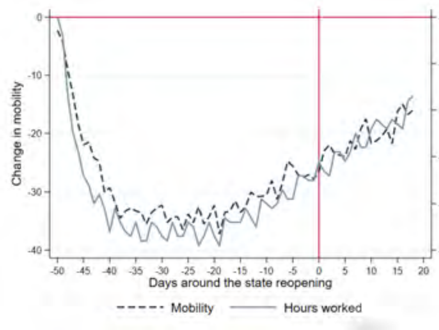
6. COVID-19, Lifting of Mitigation Measures, and Economic Recovery

The lifting of mitigation policies in many countries and states provides additional evidence on the role of *de jure* NPIs and voluntary social distancing. In the 45 US states that have allowed nonessential businesses to reopen since late April, mobility and hours worked show a gradual recovery starting about two weeks before the reopening (Figure 6, panel A).[14] The gradual increase in mobility before official reopening may be due to lockdown fatigue and lower perceived risk from infection (for instance, because daily case numbers start coming down, or the outbreak is no longer the top story in the news headlines, or people update their beliefs about the virus every day they or someone they know do *not* get infected). The dynamics of hours worked can be partially explained by the fact that some services started operating even before the reopening (e.g., food deliveries and curbside pick-up). In addition, small businesses may have to start re-hiring to get ready for the reopening.

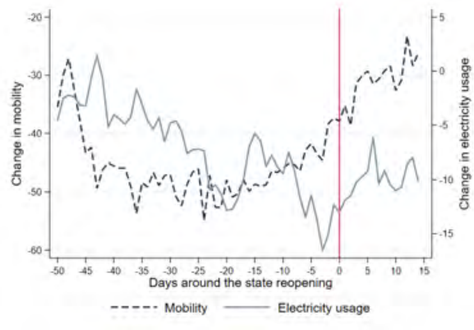
[13] This could be because people's behavior changes even in the absence of mandatory restrictions and/or due to spillovers from other regions (for instance, through supply chain disruptions or reduced demand for travel).

[14] While data on hours worked from Homebase have to be interpreted with caution as they refer to hourly workers predominantly in small businesses and services, consumer spending shows a similar pattern (see Chetty et al. 2020).

FIGURE 6. Reopening, Mobility, and Economic Activity



A. Reopening in the United States



B. Reopening in Europe

Source: https://tracktherecovery.org/, Google Community Mobility Reports, Homebase, ENTSO-E, Hale et al. (2020).
Notes: Panel A plots the changes in hours worked for a large sample of small businesses and in mobility (both relative to the pre-COVID-19 period) for the median U.S. state. The x-axis is the number of days before/after the reopening of nonessential businesses. The sample only includes 45 states that have reopened by May 30. A figure based on the lift of the state-at-home orders is qualitatively and quantitatively similar. Panel B plots the median change in electricity usage—with respect to the previous year—the median change in mobility relative to the pre-COVID-19 period, across 21 European countries. The x-axis reports the number of days before/after the relaxation of NPIs (the day 0 is the first time that the Oxford stringency index (Hale et al. 2020) declines by at least 5 points.

A similar picture emerges from analyzing the easing of NPIs in European countries (Figure 6, panel B). In this exercise, we date the reopening as the day of the first reduction in the stringency index of mitigation policies (Hale et al. 2020) by at least 5 points. We again observe that mobility starts improving about two weeks before reopening. The trend in electricity usage is less clear. There is a first pick-up about 20 days before the easing of the stringency index in tandem with mobility, followed by a modest decrease and a second pick-up three days before the reopening. This second pick-up continues for about a week after the reopening and then appears to flatten.

Two main findings stand out. First, there are strong anticipation effects in both mobility and economic activity. Second, there is no clear evidence of any acceleration soon after the easing of restrictions. These findings suggest that, in the reopening phase, people's behavior matters more for the resumption of activities than the timing of the reopening. In addition, the evidence of anticipation effects in mobility and economic activity—seen also in the lockdown phase (Figure 5)—suggests caution in interpreting changes in economic activity around changes in *de jure* NPIs.

7. Discussion and Conclusion

The use of cases or death counts as a measure of the COVID-19 shock at the local level must be accompanied by three caveats. First, the reported numbers depend on testing policies and capabilities, which might be different across countries and states and evolve over time.[15] Second, there is an interaction between mitigation policies and new case and death counts, as well as economic activities. As a result, national and local authorities may face a trade-off between slowing the pandemic and preserving economic activity, at least in the short run. Third, the exact reasons why some areas have been experiencing earlier or more intense outbreaks are still largely unknown. Therefore, hardest-hit areas might be different from other areas and, importantly for any empirical analysis, what makes an area susceptible to large outbreaks could be correlated with what also makes the economic impact sizeable (e.g., the prevalence of nonessential service jobs, industry composition, etc.).

Our analysis relies on the heterogeneous timing and intensity of the COVID-19 outbreak across different European countries and U.S. states to provide useful evidence to guide policymaking to "flatten the recession curve" (Gourinchas 2020).

First, the sharp decline in electricity usage and the unprecedented spike in UI claims highlight that this crisis is novel not only for its magnitude, but also for the speed at which the economy and specifically the labor market are affected. With entire sectors of the economy on a lockdown and the need to "flatten the curve," millions of workers have immediately lost their jobs. These numbers are a call for an unprecedented policy response, which should be more similar in spirit to the reaction to wars and natural disasters, rather than a standard macroeconomic stimulus to support demand. A mix of monetary, fiscal, and financial measures should be aimed at minimizing disruptions and scarring from the lockdown, by providing sizable, targeted support to households and businesses to cope with the "hibernation" of the economy and to be able to jump-start soon after the health crisis will be over.

Second, our evidence suggesting that the heterogeneous impact of COVID-19 is mostly due to observed mobility instead of the adoption of *de jure* NPIs is a warning against optimistic projections that the economic recovery will start once NPIs are officially lifted. The economy may not rebound unless workers and consumers feel safe about resuming their normal activities. Early evidence on reopening of non-essential business activities and the easing of NPIs indicates that this is in fact the case. As countries move forward with loosening of mitigation policies, analyses such as ours could guide decisions not only on the pace and breadth of lifting mitigation policies but also on other measures that may be needed to restore confidence and trust for people to get back to pre-COVID-19 behaviors.

[15] Our results are robust to controlling for the total number of tests, which partially mitigates this concern.

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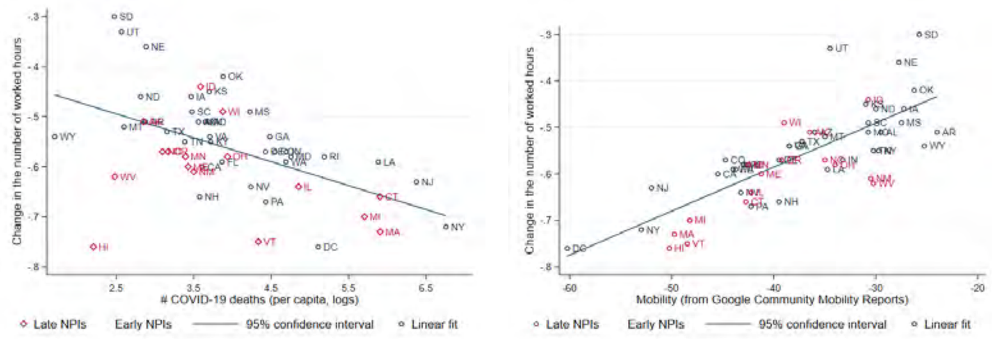
APPENDIX FIGURES AND TABLES

FIGURE A1. COVID-19, Mobility, and Hours Worked in the United States



A. COVID-19 and hours worked B. Mobility and hours worked

Source: U.S. Department of Labor, Homebase, https://covidtracking.com, https://www.kff.org/other/state-indicator/paid-family-and-sick-leave/.

Notes: This figure plots the change in the number of worked hours in local businesses (measured with respect to the period Jan 4-31, 2020) against the number of COVID-19 deaths per capita (in logs, Panel A) and the percent change in visits to various places (grouped under four categories: retail & recreation, grocery & pharmacy, transit stations, and workplaces) within a geographic area relative to the pre-COVID-19 period, at the state level. The sample period is March 8-April 25. The solid line plots a linear fit. The slope is -0.048 (s.e.=0.011) in Panel A and 0.010 (s.e.=0.001) in Panel B. States are divided between early (red labels) and late (blue labels) NPI adopters. The NPIs considered are social distancing, closure of nonessential services, closure of public venues, school closures, and shelter-in-place orders. A state is considered an early NPIs adopter if all these five policies have been implemented within a week from the day in which the first death in the state has been recorded.

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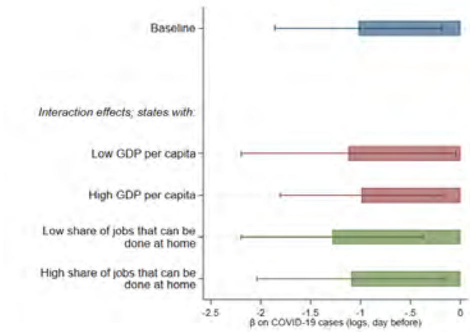
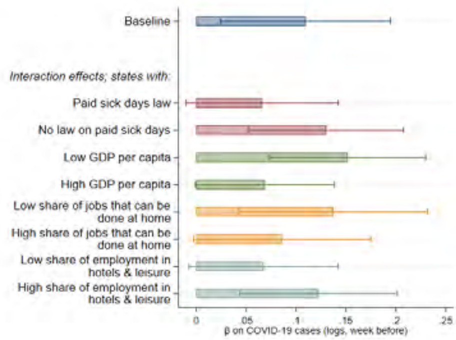
FIGURE A2. COVID-19, Electricity Usage, and UI Claims: State Heterogeneity



A. COVID-19 and electricity usage



B. COVID-19 and UI claims

Source: U.S. Department of Labor, U.S. Energy Information Administration, Bureau of Economic Analysis, https://covidtracking.com, https://www.kff.org/other/state-indicator/paid-family-and-sick-leave/, Dingel and Neiman (2020).

Notes: Results of estimating the equation: $y_{s,t} = \alpha_s + \gamma_t + \beta * COVID_{s,t-1} + \delta * X_s * COVID_{s,t} + \varepsilon_{s,t}$, where s is a U.S. state, t is a day between March 1 and April 4, 2020 (Panel A) or a week between March 7 and April 25, 2020 (Panel B). $y_{s,t}$ is electricity usage (Panel B, in logs of MWHs, relative to the same day of the week of the same week in 2019) or the number of unemployment insurance claims in a that week (in logs) (Panel A). $COVID_{s,t-1}$ is the number of COVID-19 cases in the previous the day (Panel A) or week (Panel B) (in logs), X_s is a vector of state-level characteristics, $\alpha_s$and γ_t are, respectively, state and week fixed effects. The sample is a balanced panel with t=49, n=50 (Panel A) or t=7, n=51 (Panel B). The top bar plots the coefficient of the baseline regression (β), while the other bars plot the coefficients ($\beta + \delta$) separately for states with and without paid sick days laws; low and high GDP per capita; low and high share of jobs that can be done from home; and low and high share of employment in hotels and leisure. Low is defined by the first quartile of the state distribution. The bars show the associated 90 percent confidence intervals. Standard errors are clustered by state.

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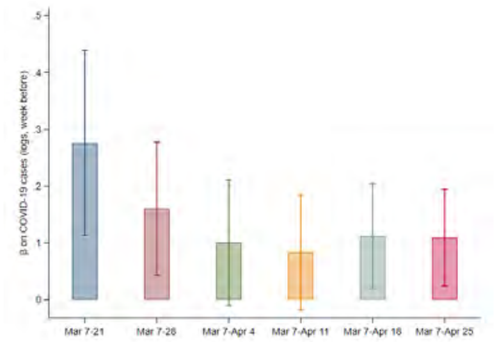
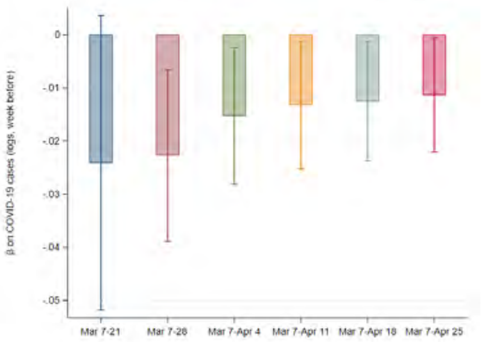
FIGURE A3. COVID-19, UI Claims, and Hours Worked: Changes over Time



A. COVID-19 and UI claims



B. COVID-19 and hours worked

Source: U.S. Department of Labor, Bureau of Economic Analysis, Homebase, https://covidtracking.com, https://www.kff.org/other/state-indicator/paid-family-and-sick-leave/, Dingel and Neiman (2020).

Notes: Results of estimating the equation: $y_{s,t} = \alpha_s + \gamma_t + \beta_t * Covid_{s,t-1} + \varepsilon_{s,t}$, where s is a U.S. state, t is a week between March 7 and April 25, 2020. $y_{s,t}$ is the number of unemployment insurance claims in a that week (in logs) (Panel A) or the change in the number of worked hours in local businesses (measured with respect to the period Jan 4-31, 2020, Panel B). $COVID_{s,t-1}$ is the number of COVID-19 cases in the previous week (in logs), and $\alpha_s$and γ_t are, respectively, state and week fixed effects. The sample is a balanced panel with t=7, n=51 (Panel B). Each bar plots the β coefficients estimating the equation above separately over different time periods, as indicated on the x-axis. The bars show the associated 90 percent confidence intervals. Standard errors are clustered by state.

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TABLE A1. Electricity and Output

Dep. Vars.: (in delta log per capita)	Electricity			GDP			Electricity		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
GDP	0.0801	0.2703***	0.2861***				0.7756**	0.5287*	0.5602**
	(0.101)	(0.068)	(0.063)				(0.333)	(0.279)	(0.271)
Banking crisis (t to t-2)				-0.0322***	-0.0374***	-0.0368***			
				(0.006)	(0.011)	(0.010)			
Time Frame	2001 to 2019	1981 to 2019	1961 to 2019	2001 to 2019	1981 to 2019	1961 to 2019	2001 to 2019	1981 to 2019	1961 to 2019
Estimator	OLS			OLS (First Stage)			IV		
Observations	694	1,329	1,554	700	1,475	2,065	694	1,329	1,554
F-stat				28	12	12			
R2	0.131	0.084	0.102	0.322	0.086	0.073			
R2-within	0.0008	0.0322	0.0374	0.0473	0.0189	0.0166			

Source: EAI, ENTSO-E, WEO, Laeven and Valencia (2020).
Notes: The table presents the results of estimating the linear regression:
$\Delta Electricity_{c,t} = \beta * \Delta GDP_{c,t} + \gamma_c + \alpha_c * t + \varepsilon_{c,t}$. where c and t indicate a country and a year in our sample, γ_c are country fixed effects, and α_c capture country-specific time trends. Estimating the parameter β allows us to infer the unobserved drop in GDP caused by the COVID-19 shock as: $\Delta GDP_{c,COVID} = \frac{\Delta Electricity_{c,COVID}}{\beta}$. Estimates of β with OLS are reported in columns (1), (2), and (3) which refer to three different sample periods, all ending in 2019 and starting, respectively, in 2001, 1981, and 1961. As an alternative empirical strategy, we instrument the changes in GDP with the banking crises reported by Laeven and Valencia (2020, Systemic Banking Crises Revisited). Banking crises are a useful instrument as they are unlikely to affect energy production directly but only through their effect on economic activity, as they are often followed by sharp recessions. We therefore estimate a two-stage least squares model where we instrument delta logs of GDP with a dummy equal to one if that country experienced a banking crisis in that year or in the previous two (different timing choices lead to less power in the first stage). The first stage of the model is presented in columns (4), (5), and (6) for the three different time periods. The second stage results are reported in columns (7), (8), and (9). All variables are in delta log per capita except for the banking crisis dummies. Robust standard errors are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

TABLE A2. COVID-19 and Electricity Usage in Europe: Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var.:	Electricity consumption: week ending on Apr 11									
Log of deaths per capita	-2.3924***	-2.3349***	-4.6048***	-2.3924***	-1.5626**					
(stock, previous week)	(0.643)	(0.822)	(0.860)	(0.643)	(0.748)					
Mobility						0.3225***	0.3250***	0.4717***	0.3225***	0.3320***
						(0.079)	(0.072)	(0.085)	(0.079)	(0.097)
Share of Manufacturing in		6.5321					9.3654			
Production		(14.724)					(7.631)			
Expected lockdown impact,			1.8036					-1.6722*		
Barrot et al. 2020			(1.073)					(0.872)		
Average Temperature					-0.6184***					-0.7498***
					(0.214)					(0.197)
Average Temperature (same					0.3576					0.8145***
week 2019)					(0.242)					(0.196)
Observations	32	25	15	32	30	31	25	15	31	29
R2	0.374	0.421	0.652	0.374	0.484	0.364	0.530	0.657	0.364	0.608

Source: ENTSO-E, ECDC, Google Community Mobility Reports, OECD, Barrot, Basile, and Sauvagnat (2020), NOAA

Notes: Results of estimating the equations: $y_c = \alpha + \beta * COVID_c + \delta * X_c + \varepsilon_c$ and $y_c = \alpha + \beta * Mobility_c + \delta * X_c + \varepsilon_c$, where y_c is the year-on-year change in weekly (workday) electricity consumption in one of 32 European countries during the week ending on April 11, 2020; $COVID_c$ is the log of the total deaths due to COVID-19 per capita; $Mobility_c$ is the change in mobility from Google Community Report. X_c is a country-level control, which is either the share of manufacturing in national production in 2017 (OECD), or the expected impact of a six-week lockdown calculated by Barrot et al. (2020) using data on sectoral composition of output and propensity to work-from-home, or the average temperature in the country in that week and same week in 2019.

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TABLE A3. COVID-19 and UI claims in the United States

Dep. Var.: Unemployment insurance claims (logs)	(1)	(2)	(3)	(4)	(5)
COVID-19 cases (logs, week earlier)	0.1094**	0.1300***	0.0686	0.0858	0.1222**
	(0.051)	(0.046)	(0.042)	(0.053)	(0.047)
Interaction with:		-0.0641***			
Paid sick day laws in place		(0.023)			
			0.0829***		
Low per capita GDP			(0.024)		
				0.0513*	
Low share of jobs that can be done at home				(0.028)	
					-0.0549***
Low employment share in hotels & leisure					(0.020)
	356	356	356	314	356
Observations	0.952	0.954	0.955	0.955	0.953
R2-adjusted	0.0234	0.0646	0.0975	0.0615	0.0557
R2-within	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes

Source: U.S. Department of Labor, U.S. Energy Information Administration, Bureau of Economic Analysis, https://covidtracking.com, https://www.kff.org/other/state-indicator/paid-family-and-sick-leave/, Dingel and Neiman (2020).

Notes: Results of estimating the equation: $y_{s,t} = \alpha_s + \gamma_t + \beta * COVID_{s,t-1} + \delta * X_s * COVID_{s,t} + \varepsilon_{s,t}$, where s is a U.S. state, t is a week between March 7 and April 25, 2020. $y_{s,t}$ is the number of unemployment insurance claims in a that week (in logs). $COVID_{s,t-1}$ is the number of COVID-19 cases in the previous week (in logs), X_s is a vector of state-level characteristics, $\alpha_s$and γ_t are, respectively, state and week fixed effects. The sample is a balanced panel with t=7, n=51. The “low” category (for: per capita GDP, share of jobs that can be done from home, and employment share in hotel and leisure) is defined by the first quartile of the state distribution. Standard errors are clustered by state.

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TABLE A4. COVID-19 and Electricity Usage in Europe: Different Weeks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Dep. Var.: Electricity consumption	week ending on Mar 14				week ending on Mar 28				week ending on Apr 4				week ending on Apr 11				week ending on Apr 18			
Log of deaths per capita (stock, previous week)	-1.7416*		-1.2450		-2.4438**		-2.2885***		-2.5632**		-1.8815**		-2.3924***		-1.8467**		-1.1120		0.2260	
	(0.978)		(0.875)		(0.915)		(0.811)		(1.000)		(0.872)		(0.643)		(0.714)		(0.696)		(0.737)	
Mobility		0.2586***	0.1955***			0.3149***	0.2150**			0.5226***	0.4061***			0.3225***	0.2160**			0.4608***	0.4750***	
		(0.066)	(0.065)			(0.110)	(0.090)			(0.103)	(0.092)			(0.079)	(0.080)			(0.102)	(0.122)	
Stringency Index				-0.1911**				-0.1121				-0.1523				-0.0832				-0.1945
				(0.077)				(0.132)				(0.170)				(0.139)				(0.123)
Observations	32	31	31	29	32	31	31	29	32	31	31	29	32	31	31	29	32	31	31	29
R2	0.132	0.291	0.334	0.249	0.226	0.207	0.375	0.033	0.250	0.454	0.551	0.039	0.374	0.364	0.524	0.018	0.056	0.460	0.462	0.081

Source: ENTSO-E, ECDC, Google Community Mobility Reports, Hale et al. (2020)

Notes: Results of estimating equations: $y_c = \alpha + \beta * X_c + \varepsilon_c$ where y_c is the year-on-year change in weekly (workday) electricity consumption in one of 32 European countries during a week between March 8 and April 18. X_c is either the log of the total deaths due to COVID-19 per capita, or the change in mobility from Google Community Report, or the Index of Stringency of COVID-19 Government Intervention from Hale et al (2020).

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Table A5. Unemployment Insurance Claims, NPIs, and COVID-19 in the United States

Dep. Var.: Unemployment insurance claims per capita (logs)	(1)	(2)	(3)	(4)	(5)	(6)
Early NPIs	0.0406					
	(0.075)					
Early nonessential service closure		0.0205				
		(0.071)				
Early public venue closure			0.0169			
			(0.113)			
Early social distancing				-0.0780		
				(0.072)		
Early school closure					0.0954	
					(0.177)	
Early shelter in place						0.0700
						(0.073)
Covid-19 deaths per capita (logs)	0.1184***	0.1181**	0.1169***	0.1089**	0.1145***	0.1209***
	(0.043)	(0.044)	(0.044)	(0.045)	(0.042)	(0.043)
Observations	51	51	51	51	51	51
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.260	0.257	0.256	0.273	0.264	0.270

Source: U.S. Department of Labor, U.S. Census Bureau, https://covidtracking.com, https://github.com/Keystone-Strategy/covid19-intervention-data.
Notes: The table reports the estimated coefficient of a regression which the total number of unemployment insurance claims per capita (in logs) is function of NPIs, the total number of COVID-19 death per capita, per capita GDP (in logs), the employment share in hotels and leisure, and a dummy for the presence of paid sick days laws, at the state level. The sample is a cross-section of 51 U.S. states, with variables measured from March 8 to April 25, 2020. The NPIs considered are: (i) social distancing, (ii) closure of nonessential services, (iii) closure of public venues, (iv) school closures, and (v) shelter-in-place orders. For each NPI, a state is considered an early adopter if the policy has been implemented within a week from the day in which the first death in the state has been recorded. A state is considered an early NPIs adopter if all these five policies have been implemented within a week from the day in which the first death in the state has been recorded. Results obtained excluding control variables are qualitatively and quantitatively similar.

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Sudden stop: When did firms anticipate the potential consequences of COVID-19?[1]

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COVID-19 hit firms by surprise. In a high frequency, representative panel of German firms, the business outlook declined and business uncertainty increased only when the spread of the COVID-19 pandemic led to domestic policy changes: The announcement of nation-wide school closures on March 13 caused by far the largest change in business perceptions. In contrast, business perceptions hardly reacted to any other potential source of information: Firms did not learn from foreign policy measures, even if they relied on inputs from China or Italy. The local, county-level spread of COVID-19 cases affected expectations and uncertainty, albeit to a much lesser extent than the domestic policy changes.

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2 LMU Munich and CESifo.
3 ifo Institute and LMU Munich.
4 ifo Institute, LMU Munich, IZA, and CESifo.

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1. Introduction

After an initial outbreak in China in late December 2019, the COVID-19 pandemic started spreading around the world by mid-February 2020. As the pandemic progressively spread from China to further countries, firms in the rest of the world could, in principle, account for the possibility that the pandemic would spill over to other economies, affecting their own production and demand. Did firms anticipate this possibility, enabling them to take precautionary measures, or were firms unexpectedly hit by the crisis when it reached their domestic market? At what point did firms start to realize that they would be affected by the pandemic?

This paper tackles these questions using panel data from a representative and large German business survey. We show that, despite the previous spread in Asia, the COVID-19 crisis hit German firms almost completely by surprise. Based on detailed information on the day of filling the survey, businesses report a worsening outlook and increasing uncertainty only after the beginning of March, when the curfew in Northern Italy was imposed and the first schools were closed in Germany. The largest drop of business expectations and the largest increase in uncertainty follows after March 13, when the German government announced a nation-wide school closure. In combination, these two events led to an unprecedented drop in the business outlook of six standard deviations, and a comparably large increase in business uncertainty.

In contrast to the salient European policy measures, other potential sources of information about the severity of the COVID-19 crisis seem to have at best small effects on expectations and uncertainty. The spread of COVID-19 cases at the firm's location has some explanatory power, but the overall magnitude is small relative to timing effects. Also, whether or not firms process inputs from China or Italy, the countries most prevalent in the news about the pandemic, appears to matter comparatively little for business expectations or uncertainty.

These results suggest that news about policy events in the home market are the main cause of the heightened uncertainty and sluggish short-run economic development induced by COVID-19 that have been described—but not explained—by Altig et al. (2020) and Bloom et al. (2020) for the US and the UK.[1] By highlighting the crucial role of domestic policy events for firms' expectations—and the smaller, but significant impact of the local COVID-19 spread—, our work provides new evidence that the experiences of economic agents are a prime source for their expectations. Here, our work is the first to show how local news matter for firms' expectations, as the effect of experience on expectations has thus far been documented primarily for households (see, e.g., Ehrmann and Tzamourani, 2012; Malmendier and Nagel, 2016; Cavallo et al., 2017; Kuchler and Zafar, 2019).

In a broader sense, this paper contributes to the survey evidence on firms' expectations and decision making in the COVID-19 crisis (e.g., Balleer et al., 2020; Bartik et al., 2020; Buchheim et al., 2020; Hassan et al., 2020). This literature, however, is predominantly concerned with firms' responses to the crisis along different dimensions. As such, it does not consider the determinants of firms' expectations before the widespread shutdowns, which is at the heart of this paper.

[1]Relatedly, Giglio et al. (2020) and Fetzer et al. (forthcoming) document changes in economic beliefs and anxiety among retail investors and households, respectively.

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We proceed as follows. Section 2 describes the firm-level survey data as well as the data on salient events and the local spread of COVID-19 across Germany. Section 3 presents the results, and Section 4 concludes.

2. Data and Empirical Strategy

ifo Business Survey The main data source of this paper is the ifo Business Survey (IBS) as described by Sauer and Wohlrabe (2020).[2] The IBS is a long-standing monthly panel among a representative sample of German firms across all sectors of the economy, and covers various dimensions of firms' business activity, including their current and expected business conditions and their uncertainty associated with these expectations. We also obtained access to the exact return date of each survey questionnaire as well as information on the location of the firm at the county level. This data is used to merge the IBS to data on the local spread of COVID-19 as described below. As the survey usually runs during the first three weeks of each month, we lack observations for each months' final week. We use the responses of firms that filled the survey online between January and April 2020 and harmonize the data following Link (2020). The overall sample encompasses 19,273 firm responses. To get a sense for the monthly responses, consider the April wave: Here, our sample includes 4,867 firms, with 1,694 firms in manufacturing, 363 in construction, 1,132 in retail and wholesale, and 1,678 in the remaining service industries.

Our main variables of interest are firms' realized business conditions as well as expectations for the next six months, and firms' perceived uncertainty in predicting their future business development. Firms provide these assessments on a visual analogue scale, ranging from 0 ["bad"/"low uncertainty"] to 100 ["good"/"high uncertainty"].[3]

In addition to these standard questions, the April wave of the IBS contained a series of additional COVID-19-related questions including a question on the expected impact of the crisis on firms' revenues in the year 2020 indicated as a percentage increase/decrease.[4] Moreover, manufacturing firms were asked in April whether they were depending on important input goods from abroad before the pandemic. Firms answering in the affirmative were asked a follow-up question on whether they were depending on shipments from China, Italy, or another severely affected country.

Timing of COVID-19 Containment Measures and Infection Data We assess the relevance of several channels through which the spread of COVID-19 may have affected firms' perceptions. First, firms' expectations may be informed by salient news about policy measures both abroad and

[2]The IBS provides input for the ifo Business Climate Index, which is the most recognized leading indicator for the German business cycle; see Sauer and Wohlrabe (2020) for details. According to a meta-study by Sauer and Wohlrabe (2019), the survey is usually answered by senior management such as firm owners, members of the executive board, or department heads.

[3]Appendix Table A1 shows summary statistics for the main outcome variables for each survey month. The survey also elicits business conditions and expectations on a three point scale, encoded as "more unfavorable" (-1), "roughly the same" (0) or "more favorable" (1). As our findings are similar when using these variables, we only focus on results for the more detailed scale.

[4]The exact wordings of all COVID-19-related questions that we use are listed in Appendix C.

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Table 1: Intervals between Major Policy Events and Number of Firms in IBS

	Start Date	End Date	Obs in IBS
Baseline period	Jan 1	Jan 21	4392
Wuhan lockdown (CHN)	Jan 22	Feb 3	354
Diamond Princess quarantine (JPN)	Feb 4	Feb 15	3879
Hubei hard curfew (CHN)	Feb 16	Feb 21	897
Municipalities lockdown (ITA)	Feb 22	Feb 29	0
Regional curfew (ITA)	Mar 1	Mar 4	1376
Local school closure (GER)	Mar 5	Mar 7	966
Northern Italy curfew (ITA)	Mar 8	Mar 9	757
Nation-wide curfew (ITA)	Mar 10	Mar 12	564
Nation-wide school closure (GER)	Mar 13	Mar 21	1017
Nation-wide curfew (GER)	Mar 22	Apr 14	3829
Lockdown easing announced (GER)	Apr 15	Apr 24	1155

Notes: This table shows different periods of the COVID-19 crisis defined as the interval between major policy events and indicates the number of firms that responded to the IBS in the respective period.

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in Germany. For this purpose, we define time indicator variables for all firms replying to the IBS in the period between two salient policy event. These salient policy events are a subset of COVID-19 related policies (shutdown or quarantine measures) that are selected according to the following protocol: (i) Order shutdown events in Asia, Italy, and Germany according to their severity—i.e., the geographical unit affected (local, state/province, or nation-wide shutdown)—and according to distance from Germany. (ii) Select a new event if it is either more severe than the past event or closer to Germany. Table 1 provides an overview of these policy events, the associated time intervals, and the number of firms replying to the IBS in each interval.

Second, firms may perceive the COVID-19 pandemic as more severe if it spread more strongly through their region. The regional exposure varied strongly across Germany as COVID-19 was, at least initially, predominately spread at specific events, such as a carnival celebration in the state of North-Rhine Westphalia, and by tourists returning from skiing vacations. We assess the exposure of firms to the local spread by merging them to the official daily data on the number of infections at the county level provided by the Robert Koch Institute, the German government agency and research institute responsible for disease control and prevention.[5]

Empirical Strategy To assess the determinants of German firms' perceptions in the first months of the COVID-19 pandemic, we regress the different measures for a firm's business outlook on the above-described COVID-19 time indicator variables, leaving out the period until January 21 as baseline period. Estimations also account for the number of COVID-19 cases (in logs) in a firm's county at the time of answering the survey. Given that salient news about the local spread of COVID-19 may have had a different impact on firms' perceptions at different phases of the pandemic, we interact this variable with monthly indicator variables. In addition, we control for

[5]Infection data are obtained from https://npgeo-corona-npgeo-de.hub.arcgis.com

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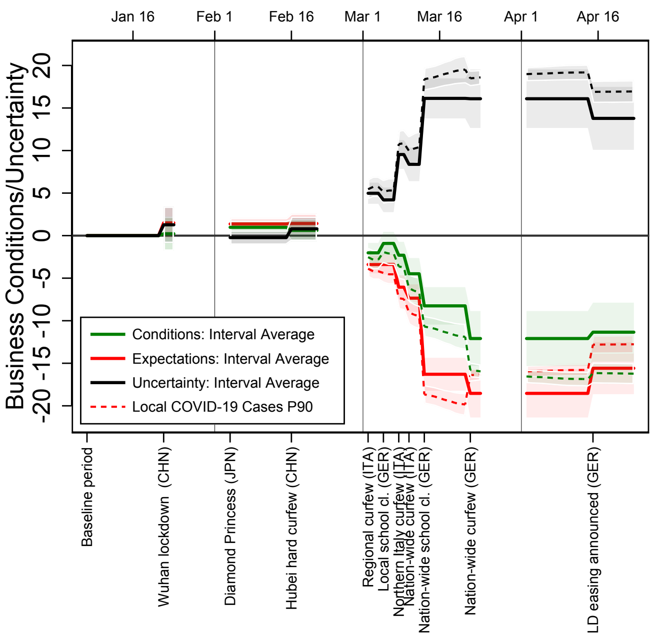
Figure 1: Effect of COVID-19 on Business Conditions, Expectations, and Uncertainty



Notes: The solid lines show the effect of COVID-19-related policy measures on firms' business outlook and uncertainty after controlling for the local spread interacted with month dummies, firm size, and fixed effects at the levels of counties and 70 two-digit industries. The effects are estimated relative to the baseline period before January 22. The dashed lines add the predicted effect of the local COVID-19 cases for a firm at the 90th percentile of cases at a given date. The data gaps correspond to periods that are not covered by the survey. The shaded areas are 90% confidence bounds. The estimates refer to Appendix Table B1.

firm size and include fixed effects at the levels of counties and two-digit industries.

3. Results

Main Findings Figure 1 summarizes the main findings with respect to the effect of the COVID-19 pandemic on business conditions, business expectations, and business uncertainty since January 2020. The full set of estimated coefficients is also shown in Appendix Table B1. The solid lines show the coefficients of the respective time periods, i.e., the overall effect of the pandemic on the respective dependent variable in the intervals between two policy events without adding the direct effect of the local spread of COVID-19. The dashed lines add the effects of the local spread to the timing coefficients, evaluated at 90th percentile of firms with respect to the infection count in their respective county. Confidence intervals are depicted at the 90% level.

The spread of COVID-19 throughout Asia and the severe lockdown measures in China had no discernible effect on the current and expected business conditions of German firms and were not reflected by any increase in uncertainty until the end of February. Business conditions and expectations only started to depreciate once the infection rates in Europe increased and Italy implemented shutdown measures by the beginning of March. This was accompanied by a significant increase in business uncertainty. Along with the rapid spread of the virus and the implementation of various containment measures in subsequent weeks, firms' business outlook rapidly deteriorated, reaching unprecedentedly low levels by the end of March. The strongest plunge in expectations followed after the German government announced nation-wide school closures on March 13, which likewise led to a substantial increase in uncertainty. The implementation of a nation-wide curfew on March 22 was followed by a further decline in firms' outlook. After April 15, when a first easing of the severity of restrictions was announced, all measures of the business outlook improved only slightly, but stayed close to their historically bad levels.[6]

Firms located in regions with higher infection numbers reported significantly worse business conditions and expectations as well as higher uncertainty during March, as shown by the dashed lines. Compared to the timing effects of salient policy events, however, the magnitude of the local spread of COVID-19 infections is relatively small. In April, business conditions of firms in highly-affected regions remained comparatively worse, while the influence of the local spread on expectations and uncertainty turned insignificant, see Appendix Table B1.

Overall, both realized and expected business conditions showed an unprecedented drop within only a few weeks of time. Relative to the month before, average business conditions and expectations deteriorated by approximately six standard deviations, while uncertainty increased by a similar magnitude.[7]

Heterogeneity between Sectors Overall, firms' perceptions and uncertainty followed a similar time path in the manufacturing, services, and retail industries (see Appendix Figure B1), though at different magnitudes. In all three sectors, expectations and conditions strongly deteriorated during March, with the service sector experiencing the strongest plunge. Since many businesses in the service sector were obliged to cease any in-person client interaction during the lockdown, the larger magnitude of adverse effects does not come as a surprise. Also, business conditions in the service sector began to worsen already in early March, possibly reflecting a growing reluctance of consumers to spend. In contrast, the business expectations and uncertainty of construction firms were largely unaffected until mid-March, but strongly deteriorated after nation-wide school closure

[6] A related question is whether the policy measures affect expectations directly or indirectly—e.g., via stock market developments. In Germany, the two largest drops of the DAX—the most important German stock market index—occured on March 9 and 12, just before the Italian curfew and the announcement of the German school closures, respectively. For these events, we cannot identify whether firms learn from the policy announcements or the stock market reactions. However, there is suggestive evidence that the policy measures are more important for firms' business perceptions: For one, the cumulative drop in the DAX of more than 15 percentage points between February 17 and March 6 (a Friday), that is of equal size as the cumulative drop between March 9 and 12, is hardly reflected in firms' expectations and uncertainty. Moreover, the announcement of the nation-wide curfew for Germany on March 22 affected firms' business outlook, but had no discernible effect on stock prices.

[7] The standard deviation of monthly means of realized and expected business conditions conditions amounted to 2.5 and 2.9 between 2012 and 2019, respectively. The standard deviation of uncertainty since the introduction of the survey question in 2017 is 2.9.

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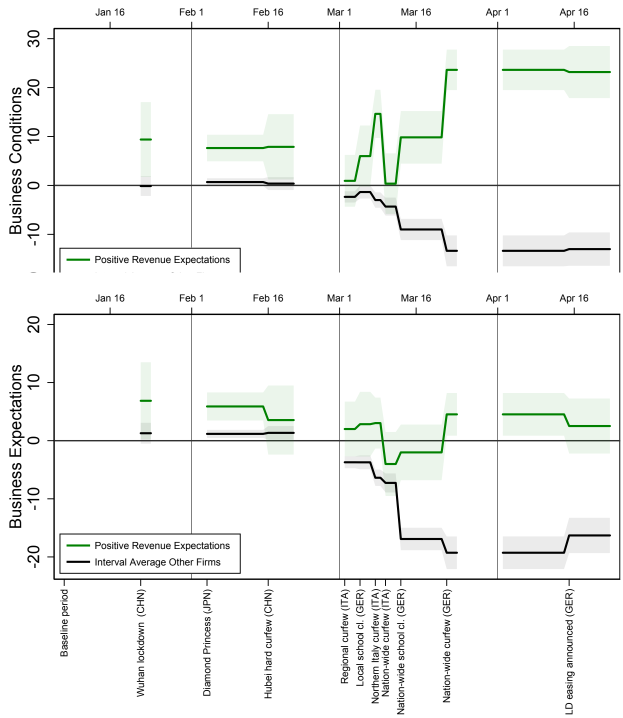
Figure 2: Effect of COVID-19 on Business Outlook: Positively Affected Firms



Notes: The solid lines show the effect of COVID-19-related policy measures on firms' realized and expected business conditions after controlling for the local spread interacted with month dummies, firm size, and fixed effects at the levels of counties and 70 two-digit industries. The effects are estimated relative to the baseline period before January 22 for two groups: firms that report in April 2020 to expect a positive effect of the COVID-19 crisis on their total revenues in 2020 (4.9% of all firms) and all other firms. The data gaps correspond to periods that are not covered by the survey. The shaded areas are 90% confidence bounds. The estimates refer to Appendix Table B1.

was announced on March 13 as well.

Effect on Expectations of Positively Affected Firms While firms' business outlook plummeted across the vast majority of industries, a small share of firms benefited from the COVID-19 crisis. We categorize firms as advantaged if they expected positive overall revenue effects of the crisis in a special survey question in April. According to this metric, only 4.9% of all firms benefited from the crisis. Unsurprisingly, the vast majority of these firms operate in the food and pharmaceutical industry, are supermarkets, or are active in the information technology or telecommunication services. The differential effects displayed in Figure 2 demonstrate that firms in advantaged sectors reported strongly appreciated business conditions throughout almost all time periods relative to the levels before January 22, whereas conditions for the remaining firms strongly deteriorated. In parallel, business expectations of advantaged firms only appreciated during February and stayed

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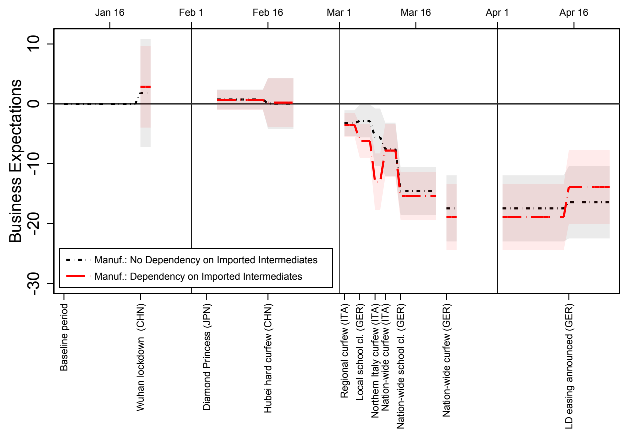
Figure 3: Effect of COVID-19 on Expectations: Role of Dependency on Imported Intermediates



Notes: The solid lines show the effect of COVID-19-related policy measures on manufacturing firms' expected business conditions after controlling for the local spread interacted with month dummies, firm size, and fixed effects at the levels of counties and 22 two-digit industries. The effects are estimated relative to the baseline period before January 22. Firms are grouped according to their dependency on important intermediates from abroad prior to the crisis. The data gaps correspond to periods that are not covered by the survey. The shaded areas are 90% confidence bounds. The estimates refer to Appendix Table B2.

relatively flat during March and April, which possibly reflects that firms expect the positive effects on their businesses to be temporary. Business uncertainty also did not increase for firms that benefit from the crisis, except for the period before the nation-wide curfew in Germany (see Appendix Figure B2). Once it became evident that these firms were not restricted during the curfew, uncertainty dropped to pre-crisis levels.

International Trade Links Next, we examine whether expectations differ for firms that are internationally connected. The hypothesis is that the perceptions of import- and export-dependent firms deteriorate earlier, as China—the origin of the pandemic—and Italy—one of the most affected countries early on—are important markets for German firms. To investigate this hypothesis, we assess whether responses of manufacturing firms differ between firms that were relying on imports of intermediate goods before the COVID-19 pandemic and firms that do not.[8]

Figure 3 shows that, contrary to the hypothesis, firms' expectations are, throughout February, virtually identical for firms that strongly depended on intermediates from abroad and for firms that did not. This is even though the shutdown in China already affected Chinese exports at that time.[9] What is more, the result also holds for the subset of firms that depend on important intermediates from China specifically (see Appendix Figure B4). Hence, firms failed to anticipate negative effects

[8]in April, the subset of manufacturing firms was asked whether they depended on important intermediaries from abroad in general as well as from China, Italy, or any other country that was strongly affected by the COVID-19 pandemic before the crisis. Empirically, we interact these dummy variables on import dependency with the time intervals.

[9]The same holds for firms' business conditions. The results are available upon request.

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of the pandemic before it had reached Europe, even if they could have learned about them from their suppliers.

In early March, the expectations of import-dependent firms suffered a slightly stronger decline that is only close to significance (t-statistic: 1.57). With increasing restrictions in Italy, expectations of firms depending on Italian intermediates started to drop approximately one week ahead of those of other firms (see Appendix Figure B4). No difference remained from mid-March onwards.

Finally, import-dependent firms faced a slightly, albeit insignificantly, higher level of uncertainty throughout the first months of the pandemic, plausibly reflecting the additional uncertainty generated by trade restrictions implemented in the wake of the pandemic (see Figure B3 in the Appendix).

Overall, the findings suggest that German firms failed to anticipate the crisis until the pandemic reached their domestic market, even if they had the opportunity to learn from their suppliers. In line with this finding, a firm's pre-crisis export share does not hold much explanatory power for the drop in business expectations or the rise in uncertainty, either (see Appendix Figure B5).

4. Conclusion

Based on a large and representative survey of German firms, this paper examines the point in time when firms became aware of the adverse economic implications of the COVID-19 pandemic. We show that firms were unexpectedly hit by the COVID-19 pandemic when it reached Europe, leaving firms with little time to prepare for the lockdown. Despite the prior spread of the pandemic in Asia, business outlooks only began to worsen in March, when Italy imposed its first regional curfew and first schools were closed in Germany. Once the crisis reached their domestic market, firms' business outlook rapidly deteriorated across March, with the strongest plunge occurring after the German government had announced a nation-wide school closure on March 13. Both business conditions and business expectations then stabilized on historically low levels in April.

Other channels through which the COVID-19 pandemic may affect firms' business outlook play a minor role. While the spread of COVID-19 infections in a firms' county exhibits a negative effect on firms' business outlook, the magnitude of effects is by far smaller than that of timing effects. Also, whether firms process intermediate goods from China or Italy, the countries that were most prevalent in the news before the crisis reached the domestic market, has only limited explanatory power for business expectations and uncertainty.

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Appendix

A. Summary Statistics

Table A1: Summary Statistics

	Overall				January		February		March		April	
	Min	Max	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Business Conditions	0	100	47.55	23.94	52.68	21.44	53.74	21.09	47.35	23.00	36.64	25.92
Business Expectations	0	100	44.21	20.50	49.92	16.84	51.44	17.36	40.78	21.00	34.98	21.68
Business Uncertainty	0	100	62.56	25.02	55.64	22.48	55.12	23.10	65.42	25.29	73.70	24.24
ln(COVID-19 Cases in County)	0	8.59	1.89	2.58	0	0	0.06	0.28	1.60	1.65	5.84	1.08
ln(Employees)			3.96	1.76								
Dependency on Imports	0	1	0.57	0.49								
Dependency on Imports from Italy	0	1	0.32	0.47								
Dependency on Imports from China	0	1	0.32	0.47								
Expected COVID-19 Revenue Effect	-1	3	-0.20	0.21								
Observations	19,273				4,746		4,776		4,884		4,867	

Notes: Table shows summary statistics of the IBS waves January through April 2020 that are used in our analyses. The data on the number of COVID-19 cases in each firm's county at the date of the survey response are obtained from the Robert Koch Institute. The *Expected COVID-19 Revenue Effect* and the different dummies on dependency on imports (manufacturing firms, only) are elicited in special IBS questions in April 2020.

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B. Supplementary Tables and Figures

Table B1: Effect of COVID-19 on Business Conditions, Expectations, and Uncertainty

	Business Conditions		Business Expectations		Business Uncertainty	
	(1)	(2)	(3)	(4)	(5)	(6)
Time indicators (baseline period: Jan 1 - Jan 21):						
Wuhan lockdown (CHN)	0.22	0.37	1.50	1.71	1.27	1.21
	(1.22)	(1.23)	(1.07)	(1.08)	(1.30)	(1.31)
Diamond Princess quarantine (JPN)	0.98**	0.89*	1.37***	1.32***	-0.23	-0.19
	(0.48)	(0.48)	(0.42)	(0.42)	(0.51)	(0.52)
Hubei hard curfew (CHN)	0.61	0.58	1.42**	1.46**	0.80	0.81
	(0.81)	(0.81)	(0.71)	(0.72)	(0.86)	(0.87)
Regional curfew (ITA)	-2.02***	-2.17***	-3.40***	-3.43***	4.98***	4.97***
	(0.71)	(0.71)	(0.62)	(0.62)	(0.75)	(0.76)
Local school closure (GER)	-0.91	-1.04	-3.39***	-3.42***	4.20***	4.22***
	(0.82)	(0.82)	(0.72)	(0.72)	(0.88)	(0.88)
Northern Italy curfew (ITA)	-2.30**	-2.64***	-6.06***	-6.19***	9.53***	9.64***
	(0.96)	(0.96)	(0.85)	(0.85)	(1.02)	(1.03)
Nation-wide curfew (ITA)	-4.48***	-4.47***	-7.35***	-7.44***	8.38***	8.37***
	(1.12)	(1.12)	(0.99)	(0.99)	(1.20)	(1.20)
Nation-wide school closure (GER)	-8.25***	-8.46***	-16.29***	-16.47***	16.13***	16.05***
	(1.34)	(1.34)	(1.18)	(1.18)	(1.43)	(1.43)
Nation-wide curfew (GER)	-12.09***	-12.32***	-18.54***	-18.65***	16.09***	16.19***
	(1.96)	(1.96)	(1.73)	(1.72)	(2.09)	(2.09)
Lockdown easing announcement (GER)	-11.35***	-11.48***	-15.59***	-15.55***	13.78***	13.81***
	(2.11)	(2.10)	(1.86)	(1.85)	(2.25)	(2.25)
Interaction terms: time indicators for sectors with positive revenue effects:						
Wuhan lockdown (CHN) × 1(Pos. aff. ind.)		3.15		-8.49		1.42
		(8.69)		(7.67)		(9.30)
Diamond Pr. (JPN) × 1(Pos. aff. ind.)		14.03***		8.14**		-5.65
		(4.28)		(3.78)		(4.59)
Hubei curfew (CHN) × 1(Pos. aff. ind.)		5.08		-1.23		-1.41
		(6.14)		(5.42)		(6.57)
Reg. curfew (ITA) × 1(Pos. aff. ind.)		18.78**		2.40		7.91
		(9.40)		(8.29)		(10.06)
Local school cl. (GER) × 1(Pos. aff. ind.)		27.17		-3.18		1.19
		(21.29)		(18.79)		(22.79)
Northern ITA curfew × 1(Pos. aff. ind.)		23.86***		8.50*		-5.39
		(5.50)		(4.86)		(5.71)
Nation-wide curfew (ITA) × 1(Pos. aff. ind.)		7.09		13.14		3.13
		(10.55)		(9.31)		(11.30)
Nation-wide school cl. (GER) × 1(Pos. aff. ind.)		20.06***		13.16**		9.49
		(6.33)		(5.59)		(6.78)
Nation-wide curfew (GER) × 1(Pos. aff. ind.)		36.46***		12.66***		-9.57**
		(4.42)		(3.90)		(4.63)
LD easing announcement (GER) × 1(Pos. aff. ind.)		20.93***		-3.97		-0.49
		(6.46)		(5.46)		(6.62)
Local spread of COVID-19:						
ln(COVID cases county) × 1($t \in$ 04 Feb, 21 Feb)	1.48	1.40	1.75	1.73	-1.50	-1.51
	(1.31)	(1.31)	(1.15)	(1.15)	(1.40)	(1.40)
ln(COVID cases county) × 1($t \in$ 03 Mar, 21 Mar)	-0.74**	-0.73**	-0.72**	-0.71**	0.68*	0.67*
	(0.33)	(0.33)	(0.29)	(0.29)	(0.35)	(0.35)
ln(COVID cases county) × 1($t \in$ 22 Mar, 23 Apr)	-0.74**	-0.74**	0.43	0.43	0.48	0.47
	(0.33)	(0.33)	(0.29)	(0.29)	(0.35)	(0.35)
ln(Employees)	0.81***	0.77***	0.26***	0.24**	0.53***	0.53***
	(0.11)	(0.11)	(0.10)	(0.10)	(0.12)	(0.12)
Constant	49.39***	49.56***	48.85***	48.90***	53.42***	53.42***
	(0.54)	(0.54)	(0.48)	(0.48)	(0.58)	(0.58)
Observations	17,939	17,939	17,960	17,960	17,961	17,961
R^2	0.259	0.263	0.218	0.219	0.231	0.232
County FE	yes	yes	yes	yes	yes	yes
Industry FE (2 digit)	yes	yes	yes	yes	yes	yes
Sample of Firms	Total	Total	Total	Total	Total	Total

Notes: This table summarizes the effect of COVID-19 on firms' business conditions, business expectations and business uncertainty which are elicited on a visual analogue scale between 0 and 100. The period indicators are defined in Table 1. In Columns (2), (4), and (6) the period indicators are interacted with an indicator that equals one if the firm is operating in an industry that is benefiting from the pandemic (supermarkets and pharmaceutical industry). Data on the county-level counts of COVID-19 cases are received from the Robert Koch Institute and interacted with dummies for different phases of the pandemic. Further controls include the log number of employees and fixed effects at the levels of counties and 70 two-digit industries. Firms are grouped according to their dependency on important intermediates from abroad prior to the crisis. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

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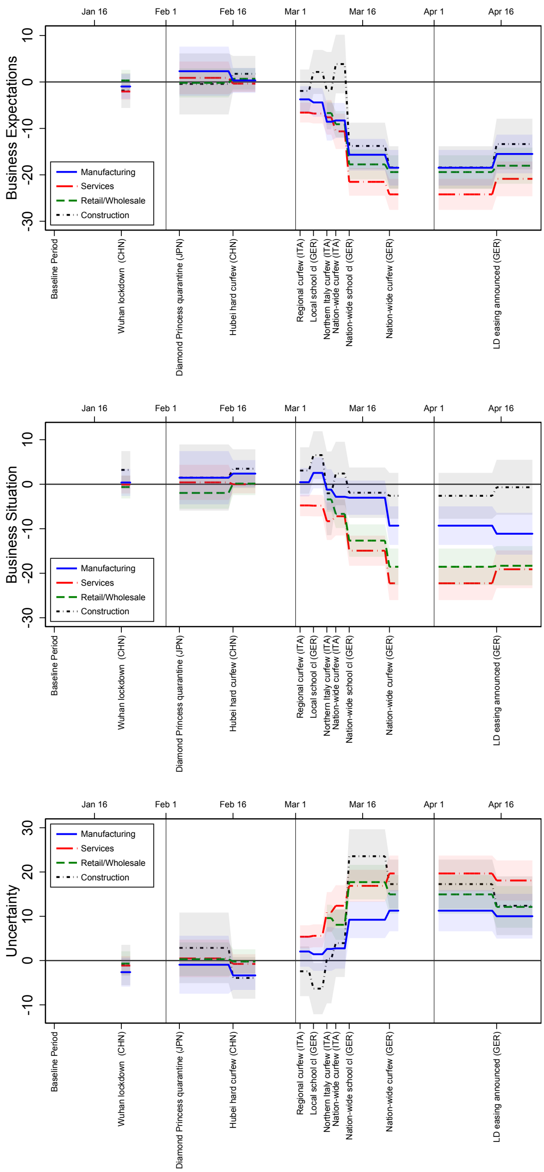
Figure B1: Effect of COVID-19 on Business Outlook and Uncertainty in Different Industries



Notes: The solid lines show the effect of COVID-19-related policy measures on firms' business outlook and uncertainty after controlling for the local spread interacted with month dummies, firm size, and fixed effects at the levels of counties and 70 two-digit industries. The effects are estimated relative to the baseline period before January 22 and separately for firms in manufacturing, services, retail/wholesale, and construction industries. The data gaps correspond to periods that are not covered by the survey. The shaded areas are 90% confidence bounds.

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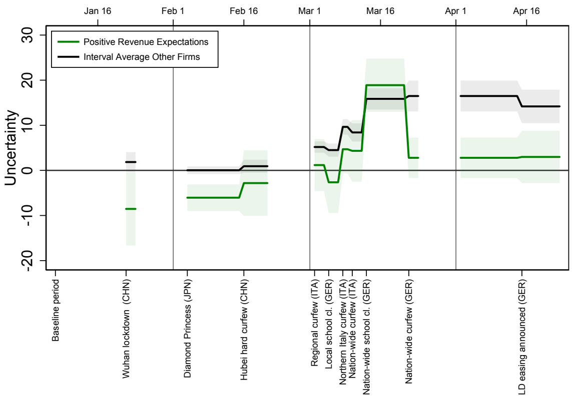
Figure B2: Effect of COVID-19 on Uncertainty: Positively Affected Firms



Notes: The solid lines show the effect of COVID-19-related policy measures on firms' business uncertainty after controlling for the local spread interacted with month dummies, firm size, and fixed effects at the levels of counties and 70 two-digit industries. The effects are estimated relative to the baseline period before January 22 for two groups: firms that report in April 2020 to expect a positive effect of the COVID-19 crisis on their total revenues in 2020 (4.9% of all firms) and all other firms. The data gaps correspond to periods that are not covered by the survey. The shaded areas are 90% confidence bounds. The estimates refer to Appendix Table B1.

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Table B2: Manufacturing Firms: Results by International Trade Links

	Business Conditions		Business Expectations		Business Uncertainty	
	(1)	(2)	(3)	(4)	(5)	(6)
Time indicators (baseline period: Jan 1 - Jan 21):						
Wuhan lockdown (CHN)	2.51	18.33***	4.35	1.82	0.52	-2.52
	(3.16)	(6.18)	(2.80)	(5.50)	(3.27)	(6.24)
Diamond Princess quarantine (JPN)	1.86**	2.67**	1.30**	0.73	-0.98	-1.00
	(0.73)	(1.13)	(0.65)	(1.01)	(0.74)	(1.15)
Hubei hard curfew (CHN)	-0.37	0.82	0.52	0.06	2.31	2.29
	(1.80)	(2.90)	(1.59)	(2.58)	(1.83)	(2.93)
Regional curfew (ITA)	-0.24	-0.97	-2.84***	-3.22**	4.28***	3.27**
	(0.96)	(1.46)	(0.85)	(1.30)	(0.97)	(1.48)
Local school closure (GER)	1.60	2.70	-3.58***	-2.86*	3.22**	3.68*
	(1.28)	(1.88)	(1.14)	(1.67)	(1.30)	(1.90)
Northern Italy curfew (ITA)	-1.45	-0.69	-7.54***	-5.59*	5.68**	8.02**
	(2.23)	(3.28)	(1.98)	(2.92)	(2.28)	(3.32)
Nation-wide curfew (ITA)	-3.34	-1.99	-6.27***	-7.58***	5.80***	6.34**
	(2.07)	(3.03)	(1.84)	(2.70)	(2.10)	(3.06)
Nation-wide school closure (GER)	-3.50	-1.97	-14.03***	-14.55***	10.75***	10.41***
	(2.20)	(2.73)	(1.96)	(2.43)	(2.25)	(2.76)
Nation-wide curfew (GER)	-10.54***	-12.49***	-16.12***	-17.47***	16.99***	18.91***
	(3.57)	(3.77)	(3.17)	(3.36)	(3.62)	(3.80)
Lockdown easing announcement (GER)	-12.17***	-12.59***	-12.99***	-16.45***	15.52***	15.88***
	(3.86)	(4.12)	(3.43)	(3.68)	(3.92)	(4.16)
1(Dependency on imported intermediates)		-0.72		-1.20		2.54**
		(1.12)		(1.00)		(1.14)
Interaction effects: time indicators for firms depending on imported intermediates:						
Wuhan lockdown (CHN) × 1(Dep. imports)		-25.98***		2.23		4.00
		(7.74)		(6.89)		(7.95)
Diamond Pr. (JPN) × 1(Dep. imports)		-0.97		1.10		0.58
		(1.54)		(1.37)		(1.56)
Hubei curfew (CHN) × 1(Dep. imports)		-3.74		1.35		0.17
		(3.97)		(3.53)		(4.04)
Reg. curfew (ITA) × 1(Dep. imports)		1.50		0.89		1.46
		(1.91)		(1.70)		(1.93)
Local school cl. (GER) × 1(Dep. imports)		-1.99		-2.15		0.25
		(2.59)		(2.31)		(2.62)
Northern ITA curfew × 1(Dep. imports)		-5.03		-6.24		-0.60
		(4.48)		(3.98)		(4.52)
Nation-wide curfew (ITA) × 1(Dep. imports)		-3.75		0.97		-1.05
		(3.98)		(3.55)		(4.02)
Nation-wide school cl. (GER) × 1(Dep. imports)		-6.22**		0.36		4.52*
		(2.68)		(2.38)		(2.73)
Nation-wide curfew (GER) × 1(Dep. imports)		0.43		-0.22		0.03
		(1.54)		(1.37)		(1.56)
LD easing announcement (GER) × 1(Dep. imports)		-2.75		3.77*		4.31*
		(2.44)		(2.18)		(2.48)
Local spread of COVID-19:						
ln(COVID cases county) × 1($t \in$ 04 Feb, 21 Feb)	4.69*	4.89*	1.55	2.33	2.86	0.83
	(2.71)	(2.95)	(2.40)	(2.62)	(2.75)	(2.98)
ln(COVID cases county) × 1($t \in$ 03 Mar, 21 Mar)	-0.16	0.40	-0.70	-0.46	0.90	0.63
	(0.59)	(0.63)	(0.52)	(0.56)	(0.60)	(0.64)
ln(COVID cases county) × 1($t \in$ 22 Mar, 23 Apr)	-0.01	0.31	0.35	0.62	-0.24	-0.62
	(0.62)	(0.64)	(0.55)	(0.57)	(0.63)	(0.64)
ln(Employees)	0.17	0.28	0.16	0.23	0.54***	0.42*
	(0.19)	(0.21)	(0.17)	(0.19)	(0.19)	(0.22)
Constant	46.73***	46.53***	47.48***	47.65***	59.82***	59.36***
	(1.05)	(1.26)	(0.93)	(1.12)	(1.07)	(1.28)
Observations	6,457	5,849	6,449	5,841	6,432	5,833
R^2	0.238	0.258	0.256	0.268	0.261	0.279
County FE	yes	yes	yes	yes	yes	yes
Industry FE (2 digit)	yes	yes	yes	yes	yes	yes
Sample of Firms	Manuf.	Manuf.	Manuf.	Manuf.	Manuf.	Manuf.

Notes: This table summarizes the effect of COVID-19 on manufacturing firms' business conditions, business expectations, and business uncertainty which are elicited on a visual analogue scale between 0 and 100. The period indicators are defined in Table 1. In Columns (2), (4), and (6) the period indicators are interacted with an indicator that equals one if the a firm reported to have been depending on imports of important intermediaries before the pandemic. Data on the county-level counts of COVID-19 cases are received from the Robert Koch Institute and interacted with dummies for different phases of the pandemic. Further controls include the log number of employees and fixed effects at the levels of counties and 70 two-digit industries. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

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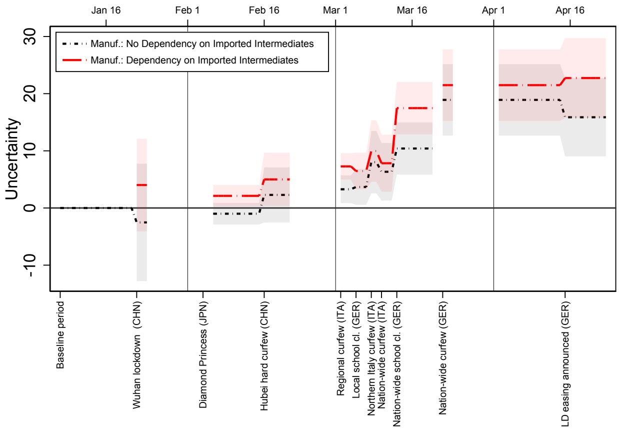
Figure B3: COVID-19 Effect on Uncertainty: Role of Dependency on Imported Intermediates



Notes: The solid lines show the effect of COVID-19-related policy measures on manufacturing firms' business uncertainty after controlling for the local spread interacted with month dummies, firm size, and fixed effects at the levels of counties and 22 two-digit industries. The effects are estimated relative to the baseline period before January 22. Firms are grouped according to their dependency on important intermediates from abroad prior to the crisis. The data gaps correspond to periods that are not covered by the survey. The shaded areas are 90% confidence bounds. The estimates refer to Appendix Table B2.

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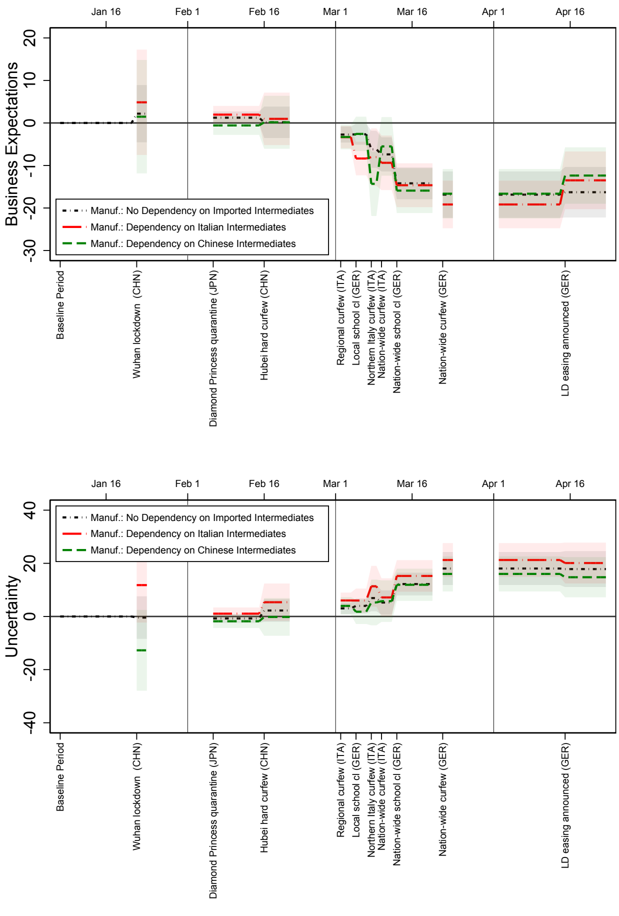
Figure B4: COVID-19 Effect on Expectations and Uncertainty: Role of Dependency on Intermediates from China or Italy



Notes: The solid lines show the effect of COVID-19-related policy measures on manufacturing firms' expected business conditions and uncertainty after controlling for the local spread interacted with month dummies, firm size, and fixed effects at the levels of counties and 22 two-digit industries. The effects are estimated relative to the baseline period before January 22. Firms are grouped according to their dependency on important intermediates from China or Italy prior to the crisis. The data gaps correspond to periods that are not covered by the survey. The shaded areas are 90% confidence bounds.

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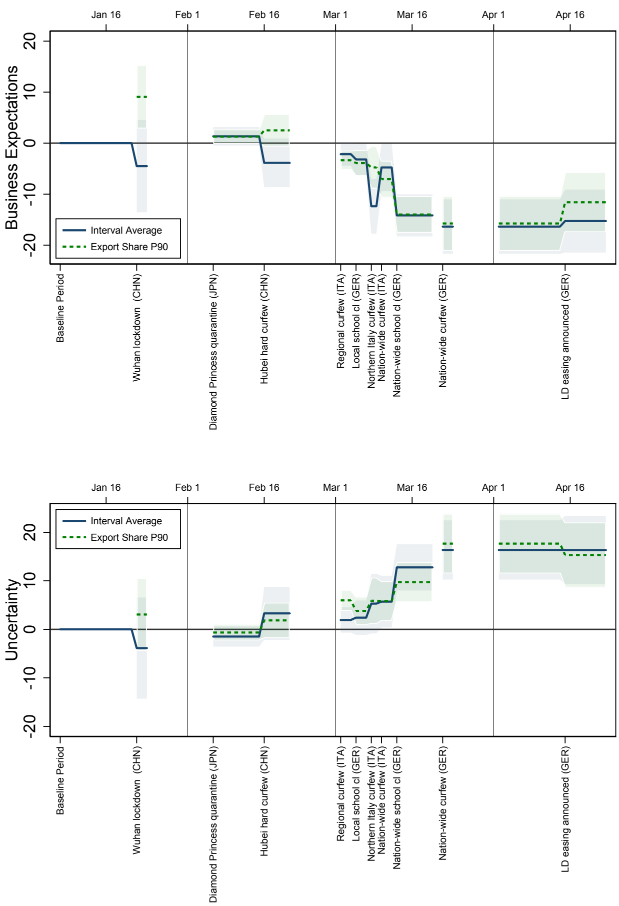
Figure B5: Role of Export Share for Business Expectations and Uncertainty



Notes: The solid lines show the effect of COVID-19-related policy measures on manufacturing firms' expected business conditions and uncertainty after controlling for the local spread interacted with month dummies, firm size, and fixed effects at the levels of counties and 22 two-digit industries. The effects are estimated relative to the baseline period before January 22. The dashed lines add the predicted effect of export exposure for a firm at the 90th percentile of export shares. The data gaps correspond to periods that are not covered by the survey. The shaded areas are 90% confidence bounds.

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C. Special Questions on COVID-19

The wording of the special questions of the April IBS survey used in this paper was as follows:

Question 1:

Welchen Effekt der Corona-Pandemie auf Ihren Umsatz erwarten Sie im laufenden Jahr?

☐ keinen Effekt ☐ Anstieg um ____ % ☐ Rückgang um ____ %

English translation (by authors):
Which effect of the Corona pandemic do you expect on your revenues in the current year?

☐ No effect ☐ Increase of ____ % ☐ Decline of ____ %

Question 2 [Manufacturing Firms Only]:

a) Waren Sie vor Ausbruch der Corona-Pandemie auf wichtige Warenlieferungen aus dem Ausland angewiesen?

☐ Ja ☐ Nein

b) Wenn ja, stammten diese wichtigen Warenlieferungen aus China, Italien oder einem anderem inzwischen vom Corona-Virus besonders stark betroffenem Land?

☐ China ☐ Italien ☐ Sonstige, und zwar: ________

English translation (by authors):
a) Did you rely on important shipments of goods from abroad before the Corona pandemic?

☐ Yes ☐ No

b) If yes, did those important shipments originate from China, Italy, or any other heavily affected country?

☐ China ☐ Italy ☐ Other countries: ________

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COVID-19: Pandemics, recessions, and suicide - lessons from the past and points to the future

Eudora Maria de Castro Ribeiro[1]

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Fear and imposed isolation worldwide due to the outbreak of the coronavirus disease (COVID-19) have raised alarms about its impact on mental health on a global scale. Associated with the respiratory disease and the public health threat, confinement, isolation, and social distance were presented as the only effective measures to prevent the spread of the virus. For those who already have psychological disorders, it is one additional factor of distress and tension. For those who do not have them, this is, in the overwhelming majority, a whole new situation and a possible cause of anxiety, stress, and depression. Besides, the severe anticipated global recession, following the lockdown of economies resulting from the virus containment strategies, should lead to substantial increases in unemployment rates and indebtedness levels, which are both risk factors for suicide. History has shown us that previous pandemics and recessions harmed population mental health, having a negative impact, namely in suicidal behaviors. The present literature review intends to alert to the prevention of suicide amid the COVID-19 pandemic, based on past similar scenarios of epidemics, such as the Spanish flu and SARS, and recessions, namely the Great Recession, the Asian economic crisis (1997-1998) and the Russian economic crisis (early 1990s). A positive sign was the fact that at the end of March, several organizations and entities worldwide, namely in Portugal, had (already) made available resources to tackle population stress and avoid negative impacts on mental health. Moreover, specialized publications had warned about the possible effects

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of COVID-19 on suicidal behaviors. Two months after, the subject was still active and alive on the Web. The literature also shows that the recipe to mitigate depressions and suicide behaviors in times of pandemics and recessions seems to be known: investment in mental healthcare, namely suicide prevention services, and in active employment policies.

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COVID-19: Pandemics, recessions, and suicide – Lessons from the past and points to the future

Introduction

On March 11, the World Health Organization (WHO) declared Coronavirus COVID-19 a pandemic. At the media briefing, the WHO Director-General, Tedros Adhanom Ghebreyesus, said that over the previous two weeks, the number of cases of COVID-19 outside China had increased 13-fold, and the number of affected countries had tripled (WHO, 2020a). Tedros Adhanom Ghebreyesus also noted that it was “the first pandemic caused by a coronavirus" and that WHO was "deeply concerned both by the alarming levels of spread and severity and by the alarming levels of inaction."

The coronavirus disease (COVID-19) pandemic was caused by the SARS-CoV-2 virus (WHO, 2020b), a new coronavirus that was detected for the first time in China, in Wuhan city, in December 2019. SARS-CoV-2 stands for Severe Acute Respiratory Syndrome-Coronavirus 2 (DGS, 2020), and COVID-19 (Coronavirus Disease) is the name of the illness that is caused by infection with SARS-CoV-2, meaning Coronavirus Disease 2019, in reference to the year in which it was discovered. There is another coronavirus that causes Severe Acute Respiratory Syndrome, called SARS-CoV, identified in 2002.

According to Merriam-Webster (2020), after WHO officially changed its designation of COVID-19 from an epidemic to a pandemic, “a considerable number of people” turned to the dictionary, “in order to ascertain the difference between the two -demics." An epidemic is “an outbreak of disease that spreads quickly and affects many individuals at the same time," while a pandemic is a type of epidemic with greater range and coverage: “an outbreak of a disease that occurs over a wide geographic area and affects an exceptionally high proportion of the population" (Merriam-Webster, 2020).

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COVID-19, mental health and suicide

The flow of media information about the virus, the respiratory disease, the number of dead and infected people worldwide, and the forecasts about the future, either in terms of public health or in terms of socio-economical consequences, was broad and robust, especially from mid-March, at least in Portugal. A Google search on “suicide COVID," at the end of March, showed that several organizations had made available resources to tackle population stress and avoid negative impacts on mental health. Also, specialized publications had already made warnings about the possible effects of COVID-19 on suicidal behaviors.

As WHO (2011) states, "mental health is an indivisible part of public health and significantly affects countries and their human, social and economic capital," not being "merely the absence of mental disorders or symptoms" (p.1). On March 18, WHO noted that the time of crisis due to COVID-19 was generating stress throughout the population. Therefore, it made available the resource “Mental health and psychosocial considerations during the COVID-19 outbreak”, in order “to support mental and psychosocial well-being in different target groups during the outbreak” (WHO, 2020c, p.1), with recommendations for the general population, healthcare workers, team leaders or managers in health facilities, carers of children, older adults, people with underlying health conditions and their carers and people in isolation.

SAVE - Suicide Awareness Voices of Education (2020) also made available a resource entitled “Preventing suicide during and after the COVID-19 pandemic”, including several tips to deal with the pandemic in both moments. According to SAVE, “both Covid-19 and the recommended strategies to “flatten the curve” or slow the spread of the disease, increase the risk of suicide among those already most vulnerable and, for others, it creates a risk that was low or did not exist prior to the pandemic." The SAVE’s resource includes information about how a pandemic might affect people with mental illnesses that may be especially impacted by the present pandemic, namely, anxiety, obsessive-compulsive disorder, depression, bipolar disorder, and schizophrenia.

The American Foundation for Suicide Prevention (2020) also had published an article on its website about taking care of mental health in the face of uncertainty. The article presents strategies for that purpose and notes that those who already struggle with mental wellness might feel more depressed or less motivated to carry out daily activities. The National Suicide Prevention Lifeline (2020) provided, likewise, a page on its website on “Emotional Well-being During the COVID-19 Outbreak”, presenting coping tips to people feeling distressed related to the disease, as well as a list of helpful resources and reliable sources of information.

The Psychology Today (2020) website asked, on its turn, in the title of an article published on March 22: “Will COVID-19 Make the Suicide Crisis Worse?”, proceeding that mass unemployment could bring about historically high suicide rates. The article noted that if the unemployment rate of the United States (U.S.) jumps as it did after the 1929 stock market crash, in 2021, there could be 6,000 excess deaths by suicide, comparing to 2018, which "would be additional victims of the coronavirus emergency and its economic impact" (Psychology Today, 2020). Medpage Today (2020) also posed a similar question on an article of March 18: “Mental Health Effects of COVID-19 Pandemic: A Ripple or a Wave?". The article mentions that it is reasonable to anticipate that the impact of the virus "will have a rippling effect on national and worldwide suicide events, especially based on current hysterical public reactions."

In Portugal, the Order of Psychologists (2020) made available a set of supporting documents related to COVID-19, such as: “3 steps to deal with anxiety”, “How to deal with a situation of isolation," "Helping children cope with stress during the outbreak of COVID-19”, among others. The television news and entertainment programs quickly invited psychologists to explain to viewers how to deal with social isolation and uncertainty about the present and the future arising from the pandemic. Moreover, even among Portuguese comedians, there were those who warned about the rise of suicide due to the outbreak of COVID-19. "Suicides will rise exponentially, if not fueled by the economic crisis, they will be triggered by mental illness that results from

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confinement and social detachment, fear and paranoia," wrote Duarte (2020) in the blog "Por falar noutra coisa" ("Talking about something else").

In April, Reger et al. (2020) have alerted, in an article published on JAMA, that secondary consequences of social distancing could increase the risk of suicide, stressing the importance to consider changes in a variety of economic, psychosocial, and health-associated risk factors. That is to say, economic stress, social isolation, decreased access to community and religious support, barriers to mental health treatment, illness and medical problems, outcomes of national anxiety, health care professional suicide rates, and seasonal variation in rates. About this last factor, the authors noted that in the northern hemisphere, suicide rates tend to peak in late spring and early summer, adding that it would probably coincide with the COVID-19 peak.

As Santos (2014) notes, in most countries, a seasonal increase in suicide rates has not yet been proven, despite being higher in spring and summer in Southern Europe and winter in Scandinavian countries. The author refers that in the district of Beja, Portugal, there has been a predominance of suicides in June, July, and August in the last 20 years. Santos stresses that although it has not yet been possible to prove the relationship with temperature or wind direction scientifically, "it cannot be excluded that high temperatures in the countries of Southern Europe may influence suicide" (p.114). In this sense, Postolache et al. (2010) have confirmed that mood disorders are associated with a greater seasonality of suicide with peaks in spring, using the Danish registers between 1970 and 2001, covering the entire population of Denmark.

Also, according to Gao et al. (2019), there is "strong evidence that rising temperature has a positive relationship with increased risk of suicide, especially completed suicide" (p.1028). The authors remark that a possible mechanism, which may be at the origin of suicide peaks in spring and summer is that, after the cold nights, the experience of warm during the day in the human body can cause the overactive temperature-responsive brown fat tissue, an intensification of anxiety and mental activity, and, finally, increase the risk of suicide (p.1022). Another mentioned explanation is that high temperature can increase impulsive and aggressive behavior through high levels of serotonin.

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About two months after the first Google search, a new search for "suicide COVID" on June 3 showed a new round of results, pointing out that the narrative of the virus, of the pandemic and their effects on mental health, could just be starting. “The next Covid Crisis could be a wave of suicides," wrote Bloomberg (2020) on May 8, on a reference to a research done by the Well Being Trust and researchers affiliated with the American Academy of Family Physicians. According to the study, across nine different scenarios, additional deaths of despair in the U.S. – deaths due to drug, alcohol, and suicide – would “range from 27,644 (in a scenario of quick recovery, smallest impact of unemployment on deaths of despair) to 154,037 (in a scenario of slow recovery, greatest impact of unemployment on deaths of despair), with somewhere in the middle being around 68,000” (Petterson et al., 2020, p.3). According to Petterson et al., deaths of despair “should be seen as the epidemic within the pandemic," in the context of COVID-19 (idem).

An article published on The Conversation (2020), on May 7, also asked: “Will the number of lives saved as a result of the COVID-19 restrictions be outweighed by the deaths from an economic recession?”. In an attempt to estimate the numbers involved in Australia, the article refers that an increase in the unemployment rate to 15% followed by a decline over ten years would be associated with 2,761 extra suicides and additional 4,015 deaths from loneliness. These numbers were estimated both for a scenario of easing the restrictions allowing the virus to slowly spread, in order to achieve the so-called herd immunity – leading to 141,000 deaths from COVID-19 –, and for a scenario of maintenance of restrictions until the virus is contained – resulting in less than 27,000 deaths from COVID-19. The conclusion of “the calculus of death” was that “regardless of the strategy, the estimated number of deaths from COVID-19 far exceeds the estimated number of deaths from suicide and loneliness” (The Conversation, 2020).

On June 1, a “special report” published on the website of the American Psychological Association (APA, 2000) mentioned that “how the pandemic will affect suicide rates is still unknown, but there’s much psychologists can do to mitigate its impact," including screening their patients, developing safety plans for the pandemic and defending improved mental health services. The article states that millions of people have lost their jobs, some have lost their homes or businesses, and “families cooped up together because of stay-at-home orders are chafing under the stress." Besides, “disrupted routines and the potential for contracting a life-threatening disease may be exacerbating preexisting problems such as mental illness or substance use," the article adds.

Nevertheless, despite the concerns, the final suicide data may vary according to national or regional specificities. An article published on The Guardian (2020), on May 14, titles that “Japan suicides decline as Covid-19 lockdown causes shift in stress factors”, explaining that the suicide rate fell 20% in April compared with the same time last year, the biggest drop in five years. The fact of people spending more time at home with their families, fewer people commuting to work, and delays to the start of the school year are the factors presented for the drop. However, a prolonged economic downturn caused by the pandemic could lead to a rebound in cases, as said by Yukio Saito, a former chair of the Japanese Association for Suicide Prevention, quoted by The Guardian.

Disease, fear, and suicide

History has demonstrated that periodically, humanity is affected by epidemics, and recent history of pandemics shows us that the 20th century witnessed two pandemics after the ‘Spanish Influenza’ of 1918: the ‘Asian flu’ of 1957 and the ‘Hong Kong flu’ of 1968. On its turn, the 21st century has experienced four pandemic outbreaks before COVID-19: H1N1 in 2009, Severe Acute Respiratory Syndrome (SARS) in 2002, Middle East Respiratory Syndrome (MERS) in 2012, and Ebola, which peaked in 2013-14 (Baldwin and Mauro, 2020a).

The actions to combat H1N1 in 2009 resemble what was done to fight the Spanish flu in 1918 (Bertucci, 2009), and we may now say that also the measures to contain the spread of COVID-19 are very similar to those taken in the 20th century, just like the feelings of fear and anxiety experienced by the population. Bertucci (2009) explains that because Spain, a neutral country during World War I, did not censor the news about the new epidemic, some

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people have mistakenly deducted that the disease was originated in the country or made more victims there. The author writes that "in 1918, Brazilian cities stopped from the beginning of October", people watched public places, such as schools, parks, theaters and cinemas being closed, while religious meetings were drastically reduced, funerals with accompaniment on foot were prohibited, "kisses and hugs were condemned, and shaking hands became an unwanted act" (Bertucci, 2009, p.230). The military was specifically advised to avoid shaking hands, limiting themselves to continence, and among the civilian population, in addition to the prohibited visits, "kisses and hugs were considered almost acts of betrayal" (Bertucci, 2002, p.119).

Bertolli (2003) also describes the daily routine in the Brazilan city of São Paulo installed by the Spanish flu, noting that along with fear and tragedy, there was a range of contradictory attitudes, from suicide to irascible struggle for life. According to Bertucci (2009), "the dizzying increase in the number of the sick and dead generated a feeling of generalized helplessness, which made the fear grow immensely" (p.231). The author even cites a piece of news, which was published by São Paulo's newspapers, mentioning the attempted suicide of a worker when he assumed he had Spanish flu because of a headache, "which gives a sense of the dread that the disease aroused in the population" (idem).

In the U.S., Wasserman (1992) points that the epidemic of Spanish flu affected all regions, significantly reduced social interaction, isolated members of the mass public, and created a "high level of fear among those afflicted and others who may have had contact with them" (p.244). According to Galishoff (1969), on October 10, 1918, Newark ordered the closure of all schools, churches, theatres, moving picture houses, dance halls, saloons, and sporting arenas, in compliance with a state ban against all public gatherings declared by the New Jersey Board of Health (p.251).

About the fear and dread experienced by the populations, Santos (2006) writes that in the memorialists' texts, some characteristics are highlighted, illustrating the fear of death present in Western societies, especially when associated with wars, famines, and epidemics. "According to the description of the narratives, fear of the flu and the death of relatives, friends, and neighbors

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led to a 'relaxation' of social norms during and after the epidemic period" (Santos, 2006, p.142). The author adds that it is something similar to what happened with the epidemics of plague or even other contagious diseases. In this sense, Bertucci (2002) writes that in October 1918, the requests for the inhabitants of the city of São Paulo to remain calm redoubled:

> *"It was as if everyone heard the medicinal precepts of past centuries, which considered discouragement and fear as a predisposition to contagion. Even an old legend about the plague, which killed more because of the fear it aroused, was reissued" (p.114).*

Also about the black plague in the middle of the 14th century, Farrell (2003) writes, referring to the devastated Italian city of Siena, in 1348, that, on the one hand, there were so many people sick and dying that there were few of them to take care of the harvests, conduct the courts or police the streets. On the other hand, the healthy ones abandoned their responsibilities, some to flee or hide, others to live on the spree, during what they thought to be their last days.

Santos (2006) notes that when comparing the epidemics, there are similarities in the reports about the plague in Europe and the Spanish flu in Brazil in 1918: "The same loss of community ties, the rupture of social norms, the flight, the fear, and the surprising joy" (p.141). The author points that according to the descriptions of plague and flu survivors and victims, "fear and the overwhelming presence of death led to changes in social norms during epidemics – even exposing people to the dangers of contagion" (idem). In this sense, Queiroz (2004) refers that once established the belief of the announced total catastrophe, it is not uncommon to break with established norms and values, from which are born conducts labeled by common sense as "collective madness" or "mental epidemics" (p.69).

Bertucci (2002) describes that the tragedies that took place "in the delirium of fever" during the Spanish flu were repeated with a frequency that alarmed the inhabitants of the city of São Paulo. The author mentions "shots, stab wounds, blows, drowning, jumping to death," and notes that "people with the flu attempted suicide or killed whoever was closest" (p.128). Additionally, some

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patients jumped to their deaths from the windows of their homes or hospitals: “The stories were many, doctors were heard about it, treaties would be written about the nervous disorders caused by the Spanish flu" (Bertucci, 2002, p.131).

Besides the fear and despair associated with the outbreaks of the epidemic, there is some research about the long-term impact of the Spanish flu of 1918 on mental health. According to Phillips (1984), "post-'flu debility and lassitude were by no means unusual” (p.346), “bouts of Spanish flu also produced anemia and affected the nervous system," "post-influenza melancholia was common” and several cases of suicide were attributed to that “post-flu melancholia” (p.348). In the same sense, Mamelund (2003) writes that Spanish flu survivors were reported to have sleeping problems, depressions, mental distractions, low blood pressure, dizziness and difficulties to cope at work and with everyday life “for weeks, months or even years after 1918-19” (p.5). By looking at data from the Norwegian asylum hospitals, Mamelund also found an excess of first time hospitalized patients with mental diseases caused by influenza and pneumonia each year from 1918 to 1923 when compared to the average of the years 1915-17 and 1924-26.

Okusaga et al. (2011) say that “anecdotal reports of mood disorder following infection with common respiratory viruses with neurotropic potential have been in existence since the last century” (p.1). The authors have measured influenza A, B, and coronavirus antibody titers in 257 subjects with recurrent unipolar and bipolar disorder and healthy controls. Okusaga et al. have concluded that seropositivity for influenza A, B, and coronaviruses were associated with a history of mood disorders, but not with the diagnosis of unipolar or bipolar depression. Moreover, seropositivity for influenza B was significantly associated with a history of suicide attempt and history of psychotic features.

In 2009, after early outbreaks of H1N1 in North America in April (WHO, 2010), Balinska and Rizzo (2009), reflecting on the possible consequences of a future pandemic on the general population, pointed that there should be made a distinction between behavioral and emotional implications in a pandemic situation. On the one hand, according to the authors, population behavioral reactions include panic, non-compliance, resistance to travel restrictions,

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breaking of quarantine and isolation, and civil unrest. On the other hand, emotional responses could lead to increased anxiety, depression, suicide rates, traumatic stress reactions, and complicated grief.

More recently, Mamelund (2017) noted that "the emotional stresses during historical influenza epidemics are impossible to measure in statistical terms, but the suffering of bereavement from the sudden loss of loved ones cannot be ignored" (p.8). According to the author, "a significant rise in suicides was reported from several countries across the globe" (idem) during the 1918-1920 pandemic, and many can be related to mental disturbances resulting from the fear of contracting the disease, once that a stricken person could die in three days, or from the stress of infection with the flu itself. However, Mamelund also notes that "the unbearable loss of a spouse, children, or close relatives also contributed, as did a fall in social integration due to school closures, curtailment of public events, and so on" (p.10). In the U.S., the author refers that an increase of one unit in excess flu mortality (one more death per year per 1,000 population) raised the rate of suicide by 10%, although noting that the statistic takes into account the possible confounding role of World War I casualties and the decline in alcohol consumption between 1910 and 1920.

Nevertheless, Wasserman (1992) have concluded that World War I did not influence suicide, the Great Influenza Epidemic (Spanish flu) caused it to increase and the continuing decline in alcohol consumption depressed suicide rates. The author used a natural experiment approach to estimate the impact of exogenous social and political events on suicide behavior in the U.S. between 1910 and 1920. Wasserman notes, however, that for those infected by influenza "it is impossible to determine whether social isolation, individual pain, fear and discomfort, or a combination of these factors induced suicide," as it is also impossible to determine the influence of fears caused by the epidemic on the larger public and their suicide behavior, which he considers a "public health tragedy" (p.244).

There is also some evidence that deaths by suicide increased among older people in Hong Kong during the 2003 SARS epidemic. Based on a complete suicide database, Cheung et al. (2008) concluded that the SARS epidemic was

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likely associated with the excess of older adult suicides in 2003, especially in April, and some of those suicides may have been brought forward from summer due to SARS impact. Another conclusion was that the elderly suicide rates in the post-SARS year remained higher than before the epidemic outbreak, appearing that “the vulnerable older adults had not fully recovered from the fear and anxiety resulting from the epidemic” (Cheung et al., 2008, p.1236). The authors also note that the recovery of public mental health after the epidemic outbreak, or other social crisis, "was lagging behind the recovery of the economy" (p.1237), once that despite the reduction of unemployment rate and Gross Domestic Product (GDP) growth after SARS outbreak, the suicide rates took a longer period to return to the level before the outbreak.

Epidemics, economics and mental health

The Influenza A H1N1 virus caused both the pandemic of Spanish flu in 1918-19 and the 2009 flu pandemic. However, while the first occurred in a period of precarious sanitary conditions, which worsened the situation faced by the world population, the second happened in a time of technological advances and greater hospital infrastructure. Therefore, there was a more qualified preparation for coping (Jaskulski et al., 2012). Nevertheless, besides the development level of health structures and knowledge, did social and economic inequalities affect the pandemic's impact?

According to Mamelund (2017), although reducing social inequality in health is central to all international public health work today, it does not figure in any international or national contingency plans against pandemic influenza. As stated by the author, that “is striking, since mortality from pandemic influenza seems to hit the socioeconomically disadvantaged the hardest," adding that this was true not only in 1918 but also in 2009, when there was a second, though milder, H1N1 pandemic (p.10). Concerning the Spanish Influenza, Bertolli (2003) has demolished the myth that the epidemic manifested itself in a “democratic” way, making victims without discriminating between poor and rich. Queiroz (2004) notes that it was shown that the highest mortality rate reached the concentrated proletariat in peripheral and “pestilent” areas of the city of São Paulo, in Brazil, deprived locals of health infrastructure and professionals.

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Furthermore, the highest suicide rates and child orphanhood associated with the epidemic were also computed in the disadvantaged social groups (p.69).

A century after the Spanish flu, also Mamun and Ullah (2020) note that the majority of COVID-19-related suicides in Pakistan occur due to the lockdown related economic recession or, in other words, due to poverty, and not due to fear of COVID-19. According to the authors, a total of 29 suicide cases were reported in Pakistani press media, since January 2020, 12 of which were completed suicides and four attempts related to COVID-19 issues. The authors have extracted suicide data from press reports since the national suicide database is unavailable. Mamun and Ullah refer that most of the victims had suffered from the economic recession, and only four feared COVID-19 infection. Mamun and Ullah underline that the COVID-19 crisis threatens to hit hard undeveloped and developing countries like Pakistan, “not only as a public health crisis in the short term but as a devastating economic and social crisis over months and years to come" (p.165).

Regarding the present crisis, it would be interesting to assess in the future, with complete data and a greater temporal distance from the outbreak, whether COVID-19 affected the populations differently, according to socioeconomic factors, globally and nationally, making victims without discriminating between poor and rich, by instance.

Gloomy perspectives for the world economy

Besides the more direct health impact of the current pandemic, there is also an economic level of the disease impact that can negatively affect populations’ health. There were several alerts, at least since March, to the economic impact of COVID-19, anticipating a hard recession on a global scale, and are well know the consequences that recessions can have on mental health and suicide rates.

According to Baldwin and Mauro (2020a), when it comes to the economic shocks, we should distinguish three sources, two of which are tangible. Firstly, the medical shocks, since that sick workers are not contributing to the GDP. Secondly, the economic impact of public and private containment measures, like school and factory closures, travel restrictions, and quarantines. And thirdly, “literally ‘all in our heads’” (p.11). At this level, more related with emotions and fear, it is interesting to note that the CBOE Volatility Index (VIX), also known by "Fear Gauge" or "Fear Index" (Investopedia, 2020), surged almost 43% to close at a record high, on March 16, 2020, surpassing the peak level during the financial crisis, more than a decade ago (CNBC, 2020). Moreover, this can be revealing since “investors, research analysts and portfolio managers look to VIX values as a way to measure market risk, fear and stress before they take investment decisions” (Investopedia, 2020).

According to McKibbin and Fernando (2020), “even a contained outbreak could significantly impact the global economy in the short run” (p.116). In the most extreme scenario of a temporary global pandemic, the authors anticipate that the average GDP loss in 2020 would be 6.7%, with an 8.4% loss for the U.S. and the euro area excluding some countries – and including Spain, Netherlands, Belgium, Luxemburg, Ireland, Greece, Portugal, Finland, Cyprus, Malta, Slovakia, Slovenia, and Estonia. (p.139). For the U.K. economy, Wren-Lewis (2020) anticipated, at the beginning of March, that with all schools closed for three months and many people avoiding work when they were not sick, the largest impact for the U.K. GDP loss over a year would be less than 5%. However, if people started worrying sufficiently to cut back on social consumption, the economic impact would be more severe. The author underlined that much of the consumption is social, and “the biggest impacts on GDP occur when we have people reducing their social consumption in an effort not to get the disease” (p.111).

In a different sense, Maital and Barzani (2020) pointed, on an essay released on March, that the major economic impact of the COVID-19 pandemic would be on the supply-side of the global economy, even though the remedies considered and applied at the time were largely on the demand side. "The global armory of tools against supply-side disruptions and shocks is very very limited” (p.2), they wrote. Maital (2020) also noted that most economic downturns occur on the demand side, specifying that some shock occurs, people cut back, spend less, invest less, governments slash spending, exports fall, and the fall in demand slows the economy. “This is standard, and it describes every single economic downturn," he observed, adding that “COVID-

19 is unique because it is the first major supply-side disaster since the global economy's architecture was redesigned and rebuilt at Bretton Woods, in July 1944" (Maital, 2020).

In an analysis also disclosed in March, Gopinath (2020a), the IMF Chief Economist, has explained, in turn, that the coronavirus epidemic involves both supply and demand shocks, noting that, on the one hand, business disruptions had lowered production, creating shocks to supply, and, on the other hand, the reluctance of consumers and businesses to spend had lowered demand (p.43). "While the drop in manufacturing is comparable to the start of the global financial crisis, the decline in services appears larger this time – reflecting the large impact of social distancing," wrote Gopinath (p.41).

Baldwin and Mauro (2020a) argued, at the beginning of March, that COVID-19 was most definitely spreading economic suffering worldwide, and the virus could be as contagious economically as it was medically. In middle March, Baldwin and Mauro (2020b) noted that the size of the economic damage was still very uncertain, but would certainly be large and governments needed to focus on mitigating that damage. "This is the time to bring out the big artillery; this is not a time to be timid, but to do whatever it takes, fast" (p.2), they stated. In this sense, Sułkowski (2020) notes that "the likely consequence of the growing crisis of world economy will be the increase in the intervention role of states and international financial institutions," given the introduction of shielding economy packages by the governments of many countries (p.6). Furthermore, the author questions if "strengthening the central government will give rise to de-globalization tendencies," considering that recession and structural changes in many economies may strengthen tendencies towards economic nationalism (Sułkowski, 2020, p.8).

At the beginning of April, the IMF predicted that as a result of the pandemic, the global economy was projected "to contract sharply by -3% in 2020, much worse than during the 2008–09 financial crisis" (IMF, 2020a, p.vii). It was a downgrade of 6.3 percentage points from January. In the World Economic Outlook of April, the IMF (2020a) noted that the current crisis is like no other for three reasons: larger shock, continued severe uncertainty about the duration

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and intensity of the shock, like in a war or political crisis, and a very different role for economic policy. The IMF explained that in a normal crisis, policymakers try to encourage economic activity by stimulating aggregate demand as quickly as possible. In contrast, this time, since the crisis is, to a large extent, the consequence of needed containment measures, stimulating activity becomes "more challenging and, at least for the most affected sectors, undesirable" (p. v). At that time, the IMF Chief Economist noted that with countries implementing necessary quarantines and social distancing practices to contain the pandemic, the world had been put in a Great Lockdown, adding that the 3% contraction of the global economy made "the Great Lockdown the worst recession since the Great Depression, and far worse than the Global Financial Crisis" (Gopinath, 2020b).

About three months later, in the World Economic Outlook of June, the IMF (2020b) presented a more pessimistic perspective, anticipating a contraction of 4.9% of the global economy in 2020, 1.9 percentage points worse than the April forecast. Pointing in the title that this is "a crisis like no other," with "an uncertain recovery," the IMF explained that the COVID-19 pandemic had a more negative impact on activity in the first half of 2020 than anticipated, and the recovery was projected to be more gradual than previously forecast (IMF, 2020b, p.1). According to the IMF, the pandemic has worsened in many countries and leveled off in others; there was a "synchronized, deep downturn" with few exceptions; the "consumption and services output have dropped markedly"; mobility remained depressed and "the steep decline in activity comes with a catastrophic hit to the global labor market" (p.2). In the IMF's June forecasts, the U.S. GDP is projected to contract 8% in 2020 (growth of 2.3% in 2019), and the euro area economy is expected to fall 10.2% (growth of 1.3% in 2019).

Recessions, unemployment, mental health, and suicide

There is ample literature about the association between recessions, their impact on mental health, and additional suicides, and there is mainly evidence of the impact on suicide rates among working-age men. Christodoulou and Christodoulou (2013) note that "economic crisis are chronic stress situations and as such are likely to have psychological and psychopathological

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consequences," producing adaptive responses, such as normal sadness, and dysfunctional responses, mostly depression and suicidal potential (p.282).

According to WHO (2011), an economic crisis affects the factors determining mental health, since that protective factors are weakened while risk factors are strengthened (p.3). Among the protective factors, there is social capital and welfare protection, a healthy workplace, and living and healthy lifestyles. The risk factors include poverty, poor education, deprivation and high debt, unemployment, job insecurity and job stress, alcohol and/or drug use (WHO, 2011, p.4). As WHO notes, “substantial research has revealed that people who experience unemployment, impoverishment and family disruptions have a significantly greater risk of mental health problems, such as depression, alcohol use disorders and suicide” (p.6).

Reeves et al. (2014) have quantified the suicide impact of the Great Recession, which began in 2007, concluding that it was associated with at least 10,000 additional economic suicides between 2008 and 2010, with nearly all European societies experiencing rising suicide rate. The authors note that economic shocks can worsen mental health and, potentially, lead to suicide, through three major ways: job loss, indebtedness (a consequence of unemployment), and foreclosure on mortgages (caused by debt and unemployment). According to Reeves et al., a clue that suicide increases during the economic crisis are avoidable are the marked cross-national variations in affected countries, although noting that suicide rates may vary across nations once the depth and nature of recessions also vary. They refer that some European countries seem to have avoided the association between recession, unemployment, debt, housing insecurity, and suicide risk, namely Austria, where the suicide rate has not increased despite rising unemployment.

Chang et al. (2013) have also concluded that after the 2008 economic crisis, rates of suicide increased in 27 European and 18 American countries studied, especially in men and in countries with higher levels of job loss. They have found that “there were about 4,900 excess suicides in the year 2009 alone compared with those expected based on previous trends (2000-07)” (p.4). In 2009, all age suicide rates in European and American men were, respectively,

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4.2% and 6.4% higher than expected if past trends had continued. Chang et al. stress that several countries experienced a downward trend in suicide before the 2008 economic crisis, especially in men, trends that were reversed, suggesting the impact of the crisis. They argue that their data show an association between the magnitude of rises in unemployment and increases in suicide in men, suggesting a dose-response effect (p.4).

In the same sense, Nordt et al. (2015) have concluded that in all world regions, the relative risk of suicide associated with unemployment was elevated by about 20-30% between 2000 and 2011, and there were about 5,000 excess suicides associated with unemployment in 2009 since the economic crisis in 2008. The authors have analyzed public data for suicide, population, and economy from the WHO mortality database and the IMF's World Economic Outlook database from 2000 to 2011, selecting 63 countries. They have also found that a higher suicide rate preceded a rise in unemployment (lagged by six months).

Gavrilova et al. (2000) additionally found that rises in suicide mortality during the economic crisis in Russia, in the early 1990s, were more prominent in young and middle age groups than in age groups older than 70 years, for whom they found no significant change in mortality from suicide and alcohol poisoning since 1992. Alcohol abuse, economic and social instability, decrease in household real income, and stress and depression caused by the economic crisis are presented as plausible reasons for the rise of violent deaths, including suicides, homicides, and alcohol poisoning. Furthermore, the authors note that males responded to the growing economic and social instability through the increase of suicide rates, while women have become more often the victims of homicide. In this sense, Antunes (2015) synthesizes that published scientific production shows the health effects of economic crises, stressing that besides suicide-related mortality, also infant mortality and homicides increase, while mortality from road accidents decreases. The author exemplifies that in Portugal, suicide deaths exceeded those from road accidents for the first time in 2010, according to data from the National Statistics Institute.

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Chang et al. (2009) have also concluded that the Asian economic crisis (1997–1998) was associated with a rise in suicide mortality in some of the affected countries, namely in Japan, Hong Kong, and Korea, with a combined 10,400 excess suicides in 1998, compared to 1997. According to the authors, amongst conventional socioeconomic variables related to the variations in national suicide rates, changes in unemployment rates were most closely associated with rises in suicide, and working-age men were mostly affected.

Moreover, the rise in unemployment rates seems to be associated with the increase in the number of suicides not only nationally but also regionally. Between 2008 and 2010, Barr et al. (2012) found about 1,000 excess suicides in England (846 among men and 155 among women) than would have been expected based on historical trends, and they have concluded that English regions with greater rises in unemployment have experienced higher increases in suicides, especially among men. The authors estimate that each 10% increase in the number of unemployed men was associated with a 1.4% increase in male suicides. Although Barr et al. admit that their study cannot ascertain whether the association between job loss and suicide increase in the 2008-10 economic recession in England is causal, they point out that "the strength of the effect size, timing, consistency, coherence with previous research, existence of plausible mechanisms, and absence of any obvious alternative explanation suggest that it is likely to be" (p.3).

Blakely et al. (2003) have also found "a strong association between being unemployed (and in the non-active labor force) with suicide for male and female adults," stressing that the association was comparatively unaffected after controlling for income, education, car access, deprivation, and marital status (p.598). The study had the participation of 2.04 million respondents to the New Zealand 1991 census aged 18-64 years. According to the authors, unemployed men and women with 25-44 years and unemployed men with 45-64 years were associated with a two to threefold increased risk of suicide when compared with the employed ones.

Kim and Cho (2017) have found, in its turn, that lower levels of employment protection for regular contracts, rather than unemployment itself, had a

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consistently negative impact on suicide rates among people aged 25-34 years in 20 Organization for Economic Cooperation and Development (OECD) countries, in the period 1994–2010. The authors noted that it is possible to interpret the results in this way: after a country lowers the level of employment protection for regular contracts, the suicide rates increase. Furthermore, they have also concluded that economic factors, particularly the decrease in GDP per capita, showed to be a good predictor of increased suicide rates as well.

In this sense, some authors have already investigated if some form of welfare policy can alleviate any potentially adverse crisis' effect on mental health, and the mitigating effect does appear to exist. Stuckler et al. (2009) reviewed the mortality rates of 26 European Union (E.U.) countries during the economic crisis over three decades, between 1970 and 2007. They have noted that for each increased investment of 10 U.S. dollars per person in active labor market programs, there was a 0.038% lower effect of a 1% rise in unemployment on suicide rates in people younger than 65 years. Moreover, when the investment was higher than 190 US dollars per head per year (adjusted for purchasing power parity), rises in unemployment would have no adverse effect on suicide rates (p.321). The authors have also concluded that rapid and large rises in unemployment were associated with short-term rises in suicides in working-age men and women.

Two years later, Stuckler et al. (2011) offered a preliminary assessment based on mortality data from the period 2000–09, available for six countries in the pre-2004 E.U. and four countries in the post-2004 E.U. They have concluded that official unemployment in all countries did not increase until 2009, after the banking crisis, and job loss then increased rapidly, to about 35% above the 2007 level in both groups of countries. Besides, the steady downward trend in suicide rates in all countries before 2007 reversed with the 2008 increase of less than 1% in the new Member States, and by almost 7% in the old ones, with further increases in 2009 in both groups. Only Austria had fewer suicides (down 5%) in 2009 than in 2007, and countries facing the most severe crisis, such as Greece and Ireland, had higher suicides rises, 17% and 13%, respectively. According to the authors, their findings "also reveal the rapidity of the health consequences of financial crisis" (p.125).

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In this regard, Suhrcke and Stuckler (2012) have distinguished between physical and mental health consequences of the crisis, noting that "the main causes linked to significant short-term effects all reflect psychological problems, providing support to the notion that mental health is especially likely to be harmed during the course of a recession" (p.649). Looking at the effects of a 1% increase in unemployment rates compared to a higher than 3% increase in 26 E.U. countries, between 1971 and 2006, the authors have concluded that the size and sometimes the significance of the increase not only in suicide rates but also in homicides, alcohol poisoning, psychiatric disorders, liver cirrhosis and ulcers all tend to be greater in the context of "massive" increases in unemployment. Another conclusion was that the effects of a recession might even be positive, on average, in the short term at a national level, likely resulting from a reduction in road traffic fatalities outweighing increases in suicides. Nevertheless, Suhrcke and Stuckler noted that the health of population groups particularly affected in economic terms, namely through lay-offs, is likely to suffer in absolute or relative terms, in comparison with wealthier groups, potentially leading to widening health inequities (p.651).

Kawohl and Nordt (2020) have anticipated the effects of the currently expected rise in the unemployment rate on suicide rates, considering the expected number of job losses due to COVID-19 released by the International Labour Organization, which have predicted a decline of 24.7 million jobs as a high scenario, and 5.3 million jobs lost as a low scenario. The authors anticipate that in the high scenario, the worldwide unemployment rate would increase from 4.936% to 5.644%, which would be associated with an increase in suicides of about 9,570 per year. In the low scenario, unemployment would rise to 5.088%, associated with a rise of about 2,135 suicides per year.

As Carvalho (2015) notes, it is "a vicious cycle." People do not work because they do not have a job; they do not consume because there is no money, the same reason why they do not eat properly, nor have access to health care when ill; and because of being sick, they can hardly keep a job. "Hopelessness leads to addictive behaviors and addictions, as well as depressive and self-destructive feelings, leading many to the abyss – suicide" (Carvalho, 2015, p.23). The author also states that the delicate economic status of nations

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means that the media broadcast, in most of the television news, radio, newspapers and websites, many news about debt, unemployment rises and household indebtedness, “almost like macerating the same ideas, and thus anxiety is generalized to the population” (p.24). In the same sense, Reger et al. (2020) note that the 24/7 news coverage of the unprecedented events related to COVID-19 pandemic "could serve as an additional stressor, especially for individuals with preexisting mental health problems."

According to Thakur and Jain (2020), few cases have been reported around the world where people took their lives out of fear of getting COVID-19 infection, social stigma, isolation, depression, anxiety, emotional imbalance, economic shutdown, lack and/or improper knowledge, financial and future insecurities, related to COVID-19. However, the authors anticipate a rippling effect of the virus on worldwide suicide events, and they present seven representative cases showing psychological conditions and predictors leading to COVID-19 suicides: social isolation/distancing; worldwide lockdown creating economic recession; stress, anxiety, and pressure in medical healthcare professionals; and social boycott and discrimination. Among others, Thakur and Jain refer to the suicide of the finance minister of Germany's Hesse state, Thomas Schaefer (54-year-old economist), presenting as predictor his lack of ability to bear and cope with the stress about the economic fallout of COVID-19. Moreover, just to give another example, they also refer to the suicides of two nurses in Italy amid stress, anxiety, and pressure in medical healthcare professionals.

Some alerts for the near future

According to Gunnell et al. (2020), “suicide is likely to become a more pressing concern as the [COVID-19] pandemic spreads and has longer-term effects on the general population, the economy, and vulnerable groups” (p.468). The authors underline that “preventing suicide, therefore, needs urgent consideration," and the response must go beyond general mental health policies and practices (idem). In this sense, they recommend measures of two types of interventions. On the one hand, selective interventions to tackle mental illness and suicidal crisis, designed to individuals at higher risk of suicide. On the other hand, universal interventions designed to improve mental health and reduce suicide risk across the population, focusing on the following risk factors: financial stressors; domestic violence; alcohol consumption; isolation, entrapment, loneliness, and bereavement; access to lethal means; and irresponsible media reporting. Gunnel et al. point out that "mental health consequences are likely to be present for longer and peak later than the actual pandemic," noting, however, that "research evidence and the experience of national strategies provide a strong basis for suicide prevention" (p.470).

In a positive sense, as well, Reger et al. (2020) consider that there may be a silver lining to the current situation of coronavirus disease 2019, remembering that suicide rates have declined in the U.S. after national disasters, such as the terrorist attacks on September 11, 2001. They argue that one hypothesis is the so-called "pulling together effect," also noting that epidemics and pandemics may alter people's views on health and mortality, "making life more precious, death more fearsome, and suicide less likely."

Cheung et al. (2008) stress that "the increase in older adults suicide among the SARS period has shed light on the need for a more holistic approach in epidemic control," adding that "the need and the mental well-being for the vulnerable groups should be taken into consideration in formulating any epidemic control measures" (p.1237). Currently, in the middle of the COVID-19 pandemic, Thakur and Jain (2020) recommend the planning of socio-psychology needs and interventions for mental rehabilitation, suggesting the implementation of telephone counseling, along with 24x7 crisis response service for emotional, mental and behavioral support. In the same sense, Kawohl and Nordt (2020) underline the importance of hotlines and psychiatric services, to remain able to respond appropriately. They point out that mental health providers should raise awareness in politics and society that rising unemployment is associated with an increased number of suicides, arguing that "the downsizing of the economy and the focus of the medical system on the COVID-19 pandemic can lead to unintended long-term problems for a vulnerable group on the fringes of society" (Kawohl and Nordt, 2020, p.390).

A few years ago, WHO wrote, in 2011, that the effects of the economic crisis on mental health presented an opportunity to reinforce policies that would

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mitigate the impact of the recession on deaths and injuries arising from suicidal acts and alcohol use disorders. According to WHO, the economic downturn also strengthened powerful public health arguments for social protection, active labor market programs, family support, and debt relief, adding that mental health service provision needed to be strengthened by continued efforts to develop universal mental health care, supported by sound financial incentives (p.15).

As Antunes (2015) notes, the adverse effects of economic crises on populations are predictable and can be mitigated with appropriate measures. The author stresses that strong social protection systems enable societies to be better able to resist adversity, and programs to support low-income families, institutions capable of creating and promoting social networks, measures to combat over-indebtedness, reduce alcohol accessibility and proximity mental health centers can make a difference (p.274). Reeves et al. (2014) stress that “recessions will continue to hurt, but need not cause self-harm," and they refer three factors that may increase mental health resilience during economic shocks: access to secondary prevention, helping the newly unemployed to return to work and greater gender equality in the workplace. Regarding the first one, the authors point out that the majority of suicides occur among people with clinical depression, so effective treatment, such as antidepressants, may moderate the impact of economic shocks on suicide by controlling depression associated with financial uncertainty (p.2).

Christodoulou and Christodoulou (2013) also note that diagnosing depression and suicide potential is always important but acquires greater importance during periods of economic crisis, arguing that “mental illness prevention and mental health promotion should be integral parts of clinical management and service planning in times of financial crisis” (p.282). Furthermore, the authors argue that “welfare provision can limit psychiatric morbidity during periods of economic crisis and active labor market programs and family support programs have been found to be effective," adding that these measures should be culture-specific (idem). According to Nordt et al. (2015), the fact that the relative risk of suicide associated with unemployment remained quite similar between world regions and with time (they have analyzed public data from 2000 to 2011 from 63 countries), means that “there is

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a continuous need to focus on preventing suicides" and "these efforts are necessary and valuable not only in countries with high but also in those with low, unemployment rates" (p.244).

Concluding remarks

The COVID-19 impact on the populations' mental health worldwide is undoubtedly unknown, but past pandemics are described to have caused a rise in violent deaths, namely suicides, associated with the fear of contracting the disease, the infection itself and the suffering and grief for the loss of loved ones. The COVID-19 impact on the global economy is also unknown, but it is expected to cause a sharp global recession and significant rises in unemployment rates. The literature suggests that the quicker impact of recessions on populations' health is on mental rather than physical health, and tends to be greater in population groups particularly affected in economic terms. Past recessions have already shown us that suicide rates tend to reflect the increases in unemployment in very different countries.

As Reger et al. (2020) note, concerns about negative secondary outcomes of COVID-19 prevention efforts should not be taken to imply that public health actions, including "remarkable social distancing interventions" to reduce human contact and curb the spread of the virus, should not be taken. However, "implementation should include a comprehensive approach," considering multiple public health priorities, including suicide prevention, they say.

Fear and anxiety are intrinsic feelings to human beings and can be positive in some situations. However, depressions and suicide behaviors are not and are avoidable, and the recipe to mitigate them in times of pandemics and recessions seems to be already known: investment in mental healthcare, namely suicide prevention services, and on active employment policies.

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Right and yet wrong: A spatio-temporal evaluation of Germany's COVID-19 containment policy

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In order to get the COVID-19 pandemic under control, most governments around the globe have adopted some sort of containment policies. In the light of the enormous costs of these policies, in many countries highly controversial discussions on the adequacy of the chosen policies evolved. We contribute to this discussion by evaluating three waves of containment measures adopted by the German government. Based on a spatio-temporal endemic-epidemic model we show that in retrospective, only the first wave of containment measures clearly contributed to flattening the curve of new infections. However, a real-time analysis using the same empirical model reveals that based on the then available information, the adoption of additional containment measures was warranted. Moreover, our spatio-temporal analysis shows that a one-size-fits-all policy, as it was adopted in Germany on the early stages of the epidemic, is not optimal.

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1. Introduction

When SARS-CoV-2 (Severe Acute Respiratory Syndrome Coronavirus 2 - SARS-CoV-2), causing the respiratory disease COVID-19 (Coronavirus Disease 2019), spread first quickly within China (especially in the Hubei region) and then developed into a pandemic early 2020, the vast majority of affected countries adopted measures against the further (local) spread of virus. While a few countries at least temporarily considered a herd immunity strategy with only very mild containment measures, the majority of countries adopted strategies aiming at "flattening the curve". This strategy aims at slowing the spread of the epidemic so that the peak number of people requiring care at a time is reduced and the health care system does not exceed its capacity. However, governments differed substantially in the measures they adopted and how quickly they adopted them (Petherick et al. (2020)). The chosen containment measures range from public information campaigns, international travel restrictions, closings of educational institutions, workplaces, public transport and leisure and retail facilities, the cancellation of public and private events, restrictions on internal movement and obligations to wear face masks to stay-at-home requirements.

Especially in those countries which already passed the (preliminary) peak of new infections and in which the health systems did not reach their capacity limits, recently a debate on the adequacy of the chosen containment measures unfolded. As the (expected) costs of the adopted strategies are often enormous, this is not too surprising. The critics of containment policies typically argue that at least some of the chosen policies were unnecessary as finally the pandemic proved to be much less severe than the overly pessimistic prophecies have initially told. The supporters of restrictive containment policies argue that this point of view is the result of a self-defeating prophecy, as the final reason for the success of containment policies is the existence of "prophecies of doom".

Germany is a prominent example for these lively discussions. When Germany experienced strongly rising infection numbers in early March 2020, quickly spreading all over Germany, the Federal Government initiated three waves of containment measures.[1] Soon after new infections reached their peak and started decaying, a discussion on the adequacy of the German containment policy unfolded in both, the public and among scientists from vari-

[1] We describe these measures in detail in Section 4.2.

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ous disciplines. Since early April, numerous theoretical and empirical studies of the adequacy of the German containment measures evolved. Interestingly enough, they come to heavily differing results.[2]

This paper contributes to the literature by delivering new empirical evidence for the adequacy of various waves of containment measures adopted in Germany. Our empirical analysis is based on forecasts derived from an endemic-epidemic model which has proved to perform well in describing and forecasting other infectious diseases. As COVID-19 has a strong epidemic component, we employ a spatio-temporal model variant and conduct our analysis on the county-level. Doing so allows us constructing meaningful and consistent forecasts on various levels of spatial aggregation and to exploit all information in the available raw data (Giuliani et al. (2020)).

Our major finding is that the final judgment of the necessity of the adopted containment measures depends strongly on the available information set. When basing our analysis on all data which was available when this paper was written in the mid of June 2020, we find that only the first wave of containment measures clearly contributed to "flattening the curve". However, when we take a real-time perspective and use only data which was available when the decisions on the adoption of the containment measures were made, all three waves of containment measures appear to be justified, at least in principle. Our results also indicate that regionally differing containment policies are strongly superior in comparison to one-size-fits-all policies.

The paper is structured as follows. Section 2 discusses the related literature. Section 3 describes the employed data. Section 4 outlines the empirical strategy, explains the considered containment measures, introduces the employed empirical model and presents the main empirical results. Section 5 delivers the results for the real-time perspective. Section 6 discusses the adequacy of the containment measures on the disaggregated spatial level. Section 6 concludes.

2. Related Literature

Although the COVID-19 pandemic is still evolving in many parts of the world, there is already quite some literature which is concerned with evaluating different containment policies. In the following we briefly discuss this

[2] We review this literature briefly in Section 2 of this paper.

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literature, however, with a focus on Germany. Broadly, the literature can be divided into two strands: papers basing on calibrated theoretical models and econometric approaches.

Especially the model-based literature often bases upon the SIR model (Britton (2010)), which goes back to early work by Kermack et al. (1927). It assumes the population can be subdivided into at least three compartments: susceptible (S), infectious (I) and recovered individuals (I). Whenever central parameters such as the likelihood of susceptible individuals to become infectious, the time infectious individuals remain infectious and the time until recovery are known, the SIR model can be formulated as a system of differential equations and, after calibration, can be used for forecasting or simulation purposes.

The first strand of the literature employs calibrated (variants of) SIR models to study the effect of containment measures on the development of the COVID-19 epidemic.[3] To the best of our knowledge, the first study for Germany was delivered by an der Heiden and Buchholz (2020) and bases on an SEIR model, which extends the standard SIR model for a latent state of being exposed before becoming infectious. The model is mostly calibrated with data from China and concludes that without containment measures Germany would have reached quickly a critical level of infections exceeding the health system's capacity. The authors argue that under most scenarios a combination of various containment measures is necessary to prevent the health system from collapsing. A subsequent study by Donsimoni et al. (2020) relies on a very similar model, but calibrates it with more recent data from Germany. The authors show that public interventions can lead to more or less severe outcomes of the epidemic, depending on their timing and the employed outcome measures. Even the most recent calibration study for Germany by Dehning et al. (2020) relies on an SIR model. Here, the authors use Bayesian inference on Markov-Chain Monte-Carlo sampling to calibrate their model. The authors find that a model variant including three change points on March 6, March 15 and March 23 explain the data best and that even the third wave of containment measures was necessary to leave the path of exponential growth of new infections.

The second strand of the literature uses econometric methods to study

[3]See e.g. Maier and Brockmann (2020) for China or the multi-country-studies by Flaxman et al. (2020) and Gros et al. (2020).

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whether and how containment measures affected newly occurring infections, either on the country or the regional level. To the best of our knowledge, the earliest econometric study for Germany on the country-level was conducted by Hartl et al. (2020). The study is based on data collected by Johns-Hopkins-University and analyzes the effect of the policy package adopted on March 13, which included the decision to close educational institutions. Employing a simple linear trend model for the logarithm of confirmed cases the authors find a structural break on March 20. Assuming a time-lag of 7-8 days, they attribute the structural break to the measures adopted on March 13. Homburg (2020) follows a similar approach, based on the same (but more recent) data. He basically argues that the "lockdown" on March 23 and even the closure of educational institutions 7 days earlier was unnecessary as the the peak of new infections was already reached on March 29. Assuming a time-lag of the data of 17 days the new infections would already have started to decrease well before these measures were adopted by the German government.

Other econometric studies have exploited regional data to examine the effect of containment measures.[4] All regional studies for Germany base on data on new infections on the county level, published by the RKI. Mense and Michelsen (2020) cumulate the data on the week level and study the overall effectiveness of the German containment measures adopted in the 12th and 13th week 2020. In order to do so they regress new infections on past infections and a spatial lag of past infections within a two-way fixed effects panel setting. They find systematically lower coefficients for the spatial effect after the implementation of the containment measures and argue that, as a package, the adopted measures were effective. Glogowsky et al. (2020) employ an event-study framework for their empirical analysis. The authors find that the implemented containment measures reduced mobility and also significantly decreased new infections. Wieland (2020) employs the RKI data in daily frequency. However, before using them in his empirical approach he infers missing data points on the reference date from the available observations via auxiliary regressions and assumes an incubation time of 5 days. Based on the corrected data, he estimates logistic growth models to determine the local

[4]See e.g. the studies for China by Kraemer et al. (2020), for Spain by Orea and Álvarez (2020) and for the United States by Abouk and Heydari (2020), Chernozhukov et al. (2020) and Courtemanche et al. (2020). A multi-country study based on regional data was conducted by Hsiang et al. (2020).

infection points, e.g. the days when the growth rate of new infections started decreasing. For Germany as a whole he estimates the infection point to lie in between March 17 and March 20 and thus well before the third round of containment policies became effective. For as many as 255 out of 412 county observations, the infection point is estimated before March 23. Felbermayr et al. (2020) primarily aim at identifying the main "superspreader-event" which led to the subsequent spread of COVID-19 within Germany. In order to do so they conduct a (repeated) cross-section analysis of new infection counts using a negative binomial model and find a significant effect of the road distance to Ischgl, a skiing area in Austria which was heavily visited by German tourists. The authors interpret their finding that the distance-to-Ischgl-coefficient turned out to be significant even after the containment policies in Germany were adopted as indication that the containment policy was successful in limiting infections over county borders.

3. Data

The data we employ for our empirical analysis comes from the Robert Koch Institute (RKI). RKI is the German government's central scientific institution in the field of biomedicine. A major task of RKI is monitoring infectious diseases such as COVID-19. To fulfill this task, RKI collects data on all detected COVID-19 cases in Germany.

According to the German Infection Protection Act (Infektionsschutzgesetz, IfSG), physicians and laboratories detecting active COVID-19 cases have to report these cases within 24 hours to the local public health department (Gesundheitsamt).[5] COVID-19 cases meeting the definition of the RKI are transmitted electronically by the local health department to the state government which then forwards this information to the RKI at the latest on the next working day. Most of the involved health authorities transmit the data earlier and more frequently than required by law, usually daily and also at weekends. Nevertheless, there is typically a delay of several days in the transmission of cases. The data transmitted to RKI always contains information on gender and age (age groups) of the infected individuals, the

[5]Note that COVID-19 often occurs without any or with only mild symptoms (see e.g. Streeck et al. (2020)). Thus, the factual number of infections is likely larger than the one reported in the RKI data. However, as we explain later, underreported data is not a problem in our empirical approach.

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place of living (only county information) and the day, when the local public health department acquired knowledge on the case ("reporting date"). In roughly two-thirds of the cases the data also comes with information on the day, when the first symptoms occurred ("reference date"). Whenever the reference date is unknown, the reference date is set to the reporting date.

For our subsequent empirical analysis we need data on the date of infection. As this date is not included in the RKI data, we construct this information in a two-step procedure. In the first step we estimate the reference date for those observations in the RKI data, for which only the reporting date is available. In order to do so we employ those observations, for which both reporting and reference date are available. We then regress the difference between reference date and reporting date on age, gender and week-day and, in addition, use commune-fixed effects. We then use the estimated coefficients to impute the missing reference dates in the dataset. In the second step we calculate the most likely day of infection by assuming an incubation period of 5.8 days (see the meta-study by McAloon et al. (2020)).

In Figure 1 we show the development of new infections when referring to (i) the reference dates reported in the original RKI data, (ii) the corrected reference dates and (iii) the likely infection date. By construction, both types of corrections result in more cases earlier on the time axis. Obviously, the effect of the correction of missing reference dates is comparatively small until the mid of March 2020 and increases slightly in size over the rest of the sample period. In general, the correction for the incubation time has a much more pronounced effect on the resulting data of new infections.

4. Were the German Containment Measures Necessary?

4.1. Empirical Strategy

Our aim is to evaluate three waves of containment measures, initiated by the German Federal Government in March 2020. In order to judge whether these measures were necessary to reach the central goal of preventing a collapse of the health system we proceed in two steps: In a first step we define the three (groups of) containment measures we investigate in this paper; we thereby also have to define when exactly the measures became effective. This step is important, as Germany is a federal state where many of the containment measures become effective not before the referring local governments

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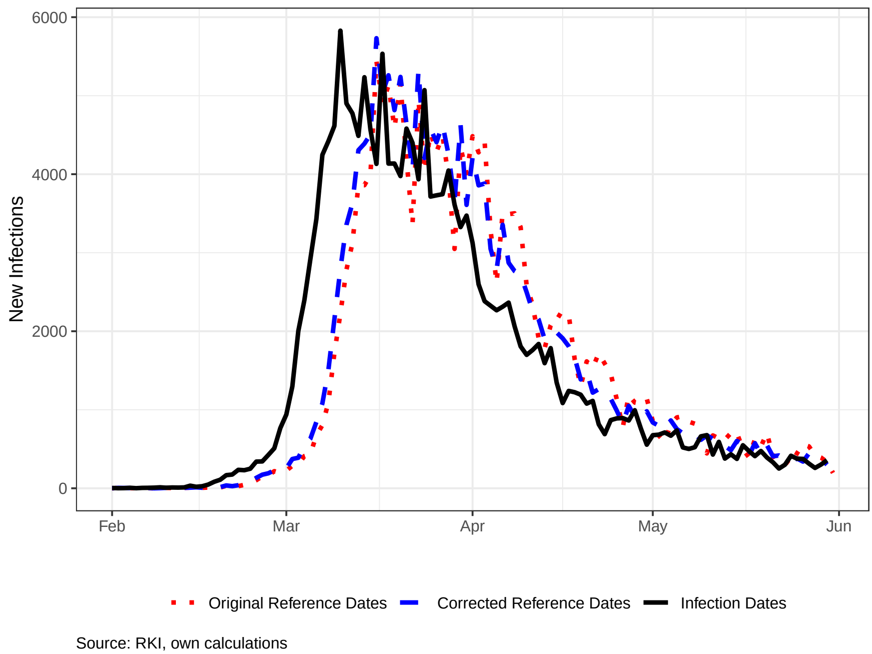

Figure 1: New Infections Based on Original Reference Dates, Corrected Reference Dates and Inferred Infection Dates

implemented them formally.[6] In the second step of our analysis we employ the data presented in Section 3 to estimate a spatio-temporal model using only data before a certain measure was adopted. We then use the estimated model to predict the likely development of new infections for the subsequent period. The likely effect of the containment measure is then the difference between factual new infections and the predicted values.

[6] As the three groups of measures we study in the following were discussed and approved in meetings of the federal government and the German state's prime ministers, it is justified to assume that the measures became effective at roughly the same point in time.

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4.2. Definition of Containment Measures

The first containment measure, we study in the following, is a ban on mass events. The ban was announced by German Minister of Health Jens Spahn who recommended to cancel all events with more than 1000 participants. While a number of events was already cancelled earlier (such as e.g. the international tourist fare ITB in Berlin on February 28 or the Leipzig book fare on March 4), the official recommendation was made on the afternoon of March 8. We assume that the ban on mass events became factually effective two days later, on March 10, when already numerous German states formally adopted the recommendation.

In the evening of March 16, a second round of containment measures was announced by Chancellor Angela Merkel. These containment measures included the closure of educational institutions (nursery schools, schools and universities), leisure facilities (e.g. gyms, playgrounds, bars and clubs) and retail facilities (with the exception of pharmacies, drugstores and groceries) as well as the introduction of national and international traveling restrictions. We assume that these measures became effective two days later, i.e. on March 18.

The third group of containment measures, we investigate in this paper, was announced late on March 22, again by Chancellor Merkel. The population was asked to minimize social contacts as much as possible ("social distancing"). Firms were advised to allow home-office wherever possible and to guarantee a minimization of social contact at the working place. While it was allowed to leave home for work, visiting the doctor, buying food or having a walk, a physical distance to non-family members of at least 1.5 meters had to be kept. Finally, even restaurants and hairdressers had to close. Again we assume that these measures became effective two days later, on March 24.

4.3. Prediction Model

The model we use to predict the onset of the COVID-19 epidemic in Germany is based on the earlier mentioned SIR model and focuses on describing the transmission from the state of susceptible individuals to infectious individuals, as reported in the earlier described RKI data. Our empirical implementation follows the basic idea of Held et al. (2005) to model our panel of areal count time series as Poisson branching process with immigration. In line with Meyer et al. (2017) we assume that the regional count of newly

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infected individuals $Y_{r,t}$ is determined by an endemic and two epidemic components.[7] More precisely, we assume that this process follows a negative binomial distribution (with overdispersion parameter $\psi > 0$) and has the conditional mean

$$\mu_{r,t}^{Y} = e_r \cdot \nu_t + \lambda_r \cdot \sum_{d=1}^{D} u_d \cdot Y_{r,t-d} + \phi_r \cdot \sum_{s \neq r} \sum_{d=1}^{D} w_{r,s} \cdot u_d \cdot Y_{s,t-d}. \quad (1)$$

The endemic component, i.e. the share of the population in region r at time t which is newly infected, regardless of a county's infection history and regardless of the infection histories of its neighbours, is modeled as

$$\ln(\nu_t) = \alpha_0 + \eta \cdot t + \gamma \cdot \sin(\omega \cdot t) + \delta \cdot cos(\omega \cdot t) \quad (2)$$

with α_0 being a constant, $\eta \cdot t$ being a time trend[8] and $\gamma \cdot \sin(\omega \cdot t) + \delta \cdot cos(\omega \cdot t)$ capturing possible seasonal variation of the endemic component as it is typical for many virus diseases. In order to receive the mean of the endemic component in region r, we further have to multiply ν_t by the size of the local population e_r.[9]

The epidemic components of the infection process consist of an autoregressive and a spatial part. The autoregressive epidemic component $\lambda_r \cdot \sum_{d=1}^{D} u_d \cdot Y_{r,t-d}$ accounts for the reproduction of COVID-19 within the same region. In line with Bracher and Held (2020a) we allow for more than one autoregressive lag (with the weighting factors u_d fulfilling $\sum_{d=1}^{D} u_d = 1$) to better capture the time-series properties of new infections. This component is modeled as

$$\ln(\lambda_r) = b_r, \quad (3)$$

with b_r being a region-specific random effect (with $b_r \sim N(0, \sigma_\lambda^2)$) that accounts for random differences between regions.

The spatial autoregressive component $\phi_r \cdot \sum_{s \neq r} \sum_{d=1}^{D} w_{r,s} \cdot u_d \cdot Y_{s,t-d}$ accounts for the transmission of COVID-19 between regions. The spatial

[7]This approach has recently also been used to model the spatio-temporal spread of the COVID-19 in Italian provinces, see Giuliani et al. (2020).

[8]The time trend also corrects for potential changes in the testing intensity.

[9]Note that in principle, e can vary over time; however, as local population counts are not available in high frequency on the county level, we employ the newest population counts, which relate to the end of 2019 and which are available in the RKI data.

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weights $w_{r,s}$ describe the flow of infections from region s to region r.[10] As for the autoregressive part, we allow for more than one spatial lag in our estimation approach. Similar as the autoregressive component we model the spatial component as

$$\ln(\phi_r) = c_r, \tag{4}$$

with c_r being a region-specific random effect (with $c_r \sim N(0, \sigma^2_\phi)$).

As our subsequent empirical analysis is partly based on relatively short panel data, we opt for two lags ($D = 2$) in the epidemic components of our model. We use exponentially decaying weights $u_d = \gamma \cdot (1-\gamma)^{d-1}$ with $\gamma = 0.6$.[11]

The described model can be estimated using penalized maximum likelihood procedures, as described in Paul and Held (2011) and Meyer and Held (2014). These techniques are implemented in the R package "surveillance"[12]. As we allow for more than one autoregressive term in our specification, we in addition use the R package "HHH4addon"[13] for the subsequent empirical analysis. Note that explicitly accounting for underreporting in the RKI data has little benefit in our application as we use the model primarily for forecasting and not for identifing parameters (see Bracher and Held (2020b)).

4.4. Empirical Results

We start out with a discussion of the results for the first wave of containment measures, announced on the afternoon of March 8, which are shown in the upper part of Figure 2. The black line illustrates new infection counts on the referring day. The forecast model is fitted over the period of February 1 to March 9. In the left part of the plot we show the fitted values of the model, disaggregated in the endemic, the autoregressive epidemic and the spatial epidemic part. We then use the model to predict the values of infections over

[10] The weights were derived from a row-normalized contiguity matrix of order one and type queen.

[11] In order to find the optimal value for γ we evaluated each estimated model for alternative values of γ ($\gamma \in \{0.6, 0.7, 0.8, 0.9\}$) using one-step-ahead in-sample forecasts and compared the models based on the logarithmic score, the ranked probability score and the Dawid-Sebastiani score (see e.g. Gneiting and Katzfuss (2014)). As the result of this procedure $\gamma = 0.6$ was chosen for all subsequent empirical specifications.

[12] Meyer et al. (2017).

[13] This package is available at https://github.com/jbracher/hhh4addon. See Bracher and Held (2020a) for an application to dengue fever in Puerto Rico and viral gastroenteritis in Berlin.

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the seven subsequent days[14] and show the resulting forecast interval.[15] It is easy to see that the model predicts strongly increasing new infections per day which almost double from around 4,600 to 8,600 new infections over the forecast horizon of one week. Over the same period, factual new infections slightly decreased and thus remained well below the projection. We take this as an indication that the first wave of containment measures contributed significantly to flattening the curve of new infections.

The results for the second wave of containment measures, announced on the evening of March 16, are shown in the middle part of Figure 2. The epidemic model was estimated for the period of February 1 to March 17. Again we use the estimated model to generate one-week-ahead forecasts[16] While the factual new infections are mostly below the mean values predicted by the epidemic model, the difference between the mean prediction and the realized new infections differ significantly only in the first few days. Thereafter, the 95-percent prediction interval includes the realized values. Moreover, the mean prediction has already a downward slope, indicating that even without the second wave of containment measures the number of new infections could be expected to decrease. While this does not imply that the second wave of containment measures was without effect, one might at least question whether they in fact were necessary to enforce decreasing new infections and to prevent the health system from collapsing. However, one should also take into account that the 95-percent prediction interval widens quickly and is partly consistent with rising new infections, so that we have to interpret this finding with some caution.

In the lower part of Figure 2 we show the results for the third wave of containment measures, which were announced on the evening of March 22. The epidemic forecast model was fitted over the period of February 1 to March 23. For the third wave of containment measures we do not observe a systematic difference between the predicted and the realized new infections. As the 95-percent prediction interval is strongly downward sloping, the empirical evidence points into the direction that the third wave of containment measures was not necessary to prevent a collapse of the health system.

[14]We refrain from further extending the forecast horizon as the next wave of containment measures was announced as early as on March 16.

[15]The forecast intervals were constructed via 10.000 Monte Carlo simulations.

[16]Again this is due to the fact that the third wave of containment measures was already announced a week later, on the evening of March 22.

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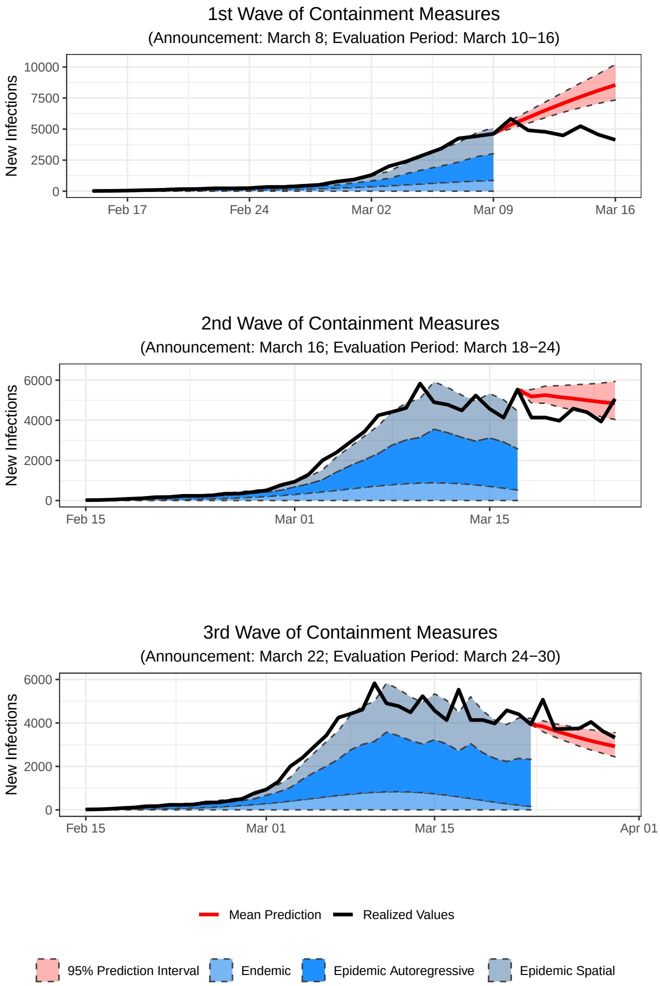

Figure 2: Evaluation of Alternative German Containment Measures

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Note that the reported empirical results do not imply, that the measures adopted in the second or third wave of containment measures such as closure of educational institutions or social distancing in general have no effect infections. In our empirical setting the effect of these measures are necessarily conditional on which measures were adopted before. Our conclusion is rather that given the already implemented ban on mass events (and the information campaigns which started even earlier), the second and especially the third wave of containment measures were not necessary to prevent a collapse of the German health system.

5. The Real-Time Perspective of the Acting Politicians

We conducted our previous analysis of the adequacy of the chosen containment policies in Germany on all data which was available in the mid of June. Doing so allowed us correcting the original data published by RKI to account for cases with missing reference date and the incubation period of 5.8 days. We also employed all data before the adoption of a certain containment measure to predict the likely future development of new infections. While doing so is adequate to judge the necessity of the containment measures in retrospect, parts of this information was unavailable to the acting politicians when they had to decide on the implementation of containment policies. Thus, while certain containment policies might be judged as unnecessary in retrospective, they might have looked reasonable at the time when they were adopted. In order to study this issue, we repeat our analysis under quasi real-time conditions, i.e. under the premise that only the data published on the day of announcement of a containment policy was available.

In the following we illustrate our procedure at the example of the first wave of containment measures, announced on March 8. We applied the same procedure to the later two waves of containment measures.

In the first step of our real-time analysis, we again infer the missing reference dates from those cases, where the difference between reporting and reference date is known via a regression model (as outlined in Section 3). However, we now use only data which was available on March 8, i.e. data with reporting dates until March 7. Moreover, we have to take into account that the most recent data is highly incomplete due to the fact that in most cases there is a delay between the reporting and the reference day. When we would use all observations until March 7 for the correction, the correction would be strongly biased towards too short corrections. In order to reduce

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this bias, we use only observations with reference dates before March 1 (e.g. 7 days earlier), as we then can expect to have at least 75 percent of all observations in our sample. After running the auxiliary regressions we use their results to infer the missing reference dates. We then correct for the mean incubation period of 5.8 days to end up with a corrected panel of infection counts.

In the second step we have to determine, which is the last reliable observation of new infection counts in our sample. As the mean incubation time amounts to 5.8 days and it takes another 7 days until at least 75 percent of all reference dates became part of the data, the last reliable observation of our newly constructed new infections variable is February 23. Thus, when estimating the parameters of our spatio-temporal model we use only infection data until February 23.

In the third and final step of our analysis we use the estimated model parameters to nowcast new infections until March 8. In the same manner we then construct projections for new infections over the subsequent week. We argue that this projection is describing what a well-informed politician should have expected for the near future.

Figure 3 shows the results we receive when applying this procedure to all three waves of containment measures.

In the upper part of Figure 3 we show the situation on March 8, when the German Minister of Health Jens Spahn announced the ban on mass events. The now- and forecasted subperiods are separated by dashed vertical lines. The model nowcasts slightly less than 500 new infections per day for March 8. According to the model's prediction, the new infection count is expected to double over the two subsequent weeks to almost 1000. The quickly widening and highly asymmetric 95-percent prediction interval[17] indicates a high degree of uncertainty on future infection counts and also includes exponential infection growth paths. Based on this projection, one might hardly classify the adoption of the first wave of containment measures as unnecessary.

In the middle part of Figure 3 we display the forecast derived from data available on March 15, when the second wave of containment measures was announced. Here, the model predicts exponential growth of new infections reaching values of more than 80.000 daily new infections on March 24. All

[17]Note that we skipped parts of the upper part of the prediction interval due to visibility of the mean prediciton.

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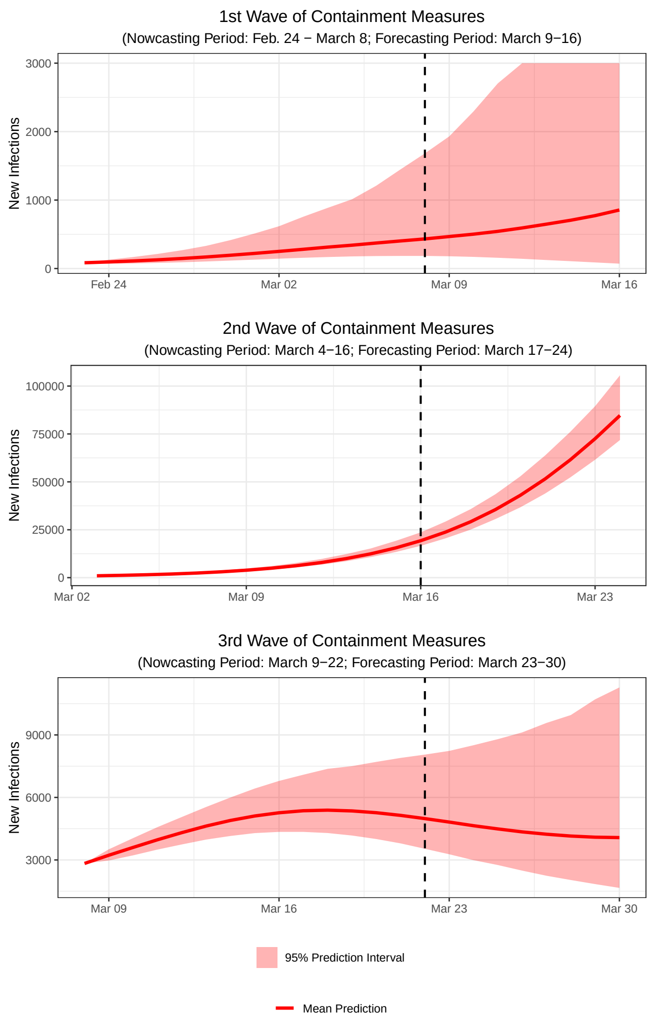

Figure 3: Real-time Forecasts without Further Containment Measures

infection paths consistent with the 95-percent prediction interval must be judged as a severe threat to the German health system. Thus, there is little doubt that based on the available information additional containment measures appeared as necessary to flatten the curve of new infections.

Finally, in the lower part of Figure 3, we show the model prediction for the third wave of containment measures, announced on March 22. Here, the situation is somewhat ambiguous. According to the mean nowcast for March 22, new infections are still on a comparatively high level, but already started to slightly decrease. A further slight decrease is expected over the period until March 30. One might argue that based on the information available on March 22, additional containment measures were unnecessary to reach the goal of preventing a collapse of the German health system. However, according to the mean model prediction the number of new infections remains on a comparatively high level. Moreover, the 95-percent prediction interval for the mean forecast turns out to be large and highly asymmetric towards higher new infections. Thus, there was still a significant probability of further rising new infections over the subsequent week(s). Thus, even when the mean forecast pointed already into the direction of a slight relaxation of the situation, a comparatively mild degree of risk aversion would render the decision to adopt further measures correct. One might therefore conclude that the decision to adopt further containment measures on March 22 was warranted, given the then available information.

6. Did One Size Fit All?

In the early phase of the COVID-19 epidemic, the German containment policies followed a one-size-fits-all strategy. All adopted measures were discussed on regular joint meetings of the federal and the state governments. On these meetings the involved politicians, after intense and sometimes controversial discussions, agreed on the measures to be adopted. After the announcement of the measures by the heads of the German federal coalition government, the state governments independently implemented these measures. Although there was a mild variety in the exact timing and even the implementation of the measures, at least throughout March 2020 it was the declared will of the acting politicians to realize a joint and highly coordinated

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containment policy.[18]

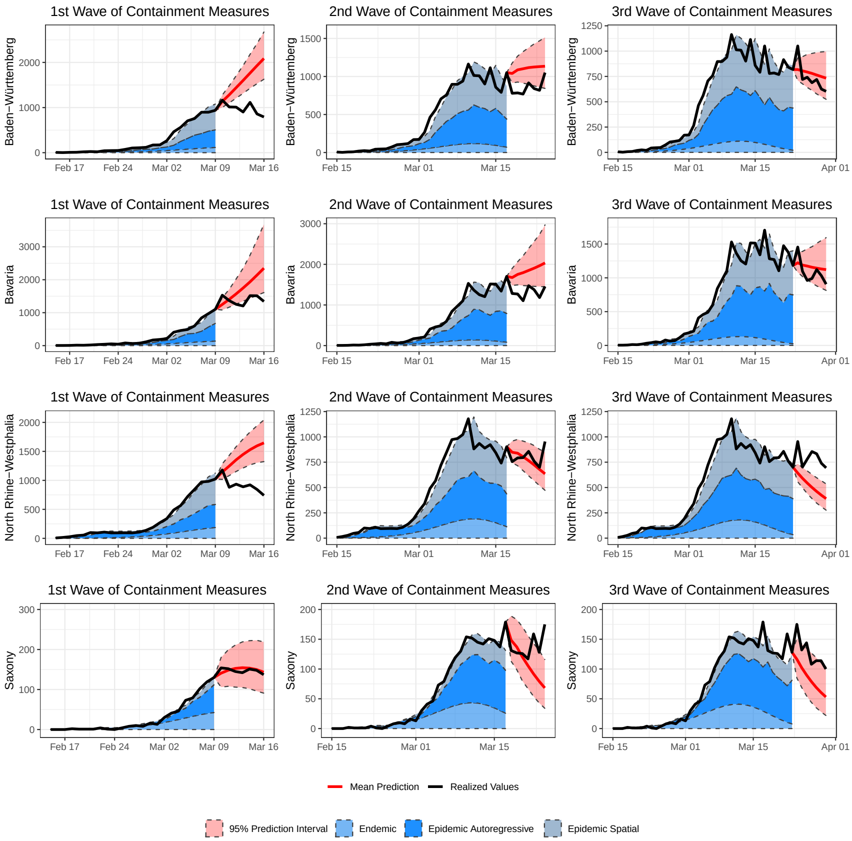

Figure 4: Adequacy of Alternative Containment Measures for Selected German States

While one might argue that the implementation of the same measures

[18]Note that this does not hold true for later implemented containment measures such as the obligation to wear face masks. Even the (much later) initiated relaxation of some of the containment measures differed enormously in both the time and the spatial dimension.

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in all parts of Germany might have at least initially contributed to a higher degree of acceptance of the containment policy in the population, one might question whether such a one-size-fits-all policy was the best choice for a country like Germany with many quite diverse regions and organized as a federal state. In order to shed some light on this question, we use the spatial dimension of the empirical model, we estimated in Section 4. As the model delivers results on the county-level, we can aggregate the results even on lower administrative levels such as the state level. In Figure 4 we show the results for four different states. We opted for the three most affected states Baden-Württemberg (first row), Bavaria (second row) and North Rhine-Westphalia (third row) as well as the most populated East German state Saxony (fourth row). In the columns we show the results for the three waves of containment measures.

While the first wave of containment measures turns out to be adequate for Baden-Württemberg, Bavaria and North Rhine-Westphalia, this hardly holds true for Saxony, where the mean forecast almost perfectly coincides with the factual development of new infections. Especially for the states with initially low infection counts in East and North Germany[19] already the first round of containment measures turns out to be somewhat questionable.

Similarly, even for the second wave of containment measures we find remarkable differences between the four states. While additional measures seemed to be adequate for Baden-Württemberg and Bavaria, for North Rhine-Westphalia the mean forecast was already indicating strongly decreasing new infections. The same applies to Saxony.

For the third wave of containment measures the mean predictions turn out to be downward sloping for all four states, thereby questioning the necessity of an additional round of containment measures. However, for Baden-Württemberg and Bavaria the 95-percent prediction interval includes also paths implying increasing new infections, rendering the decision to have an additional round of containment measures more rational than for North Rhine-Westphalia and Saxony. Interestingly enough, it was in fact Bavaria's prime minister Markus Söder who insisted on additional containment mea-

[19] An exception is Hamburg which had high infection counts in the early phase of the epidemic. This is mostly due to the fact that Hamburg is the only German state with skiing holidays in the first two weeks of March. At that time many tourists got infected in the skiing areas like Ischgl in Austria (see Felbermayr et al. (2020)) and then returned to their home regions.

sures whereas Armin Laschet, prime minister of North Rhine-Westphalia early advocated for milder containment policies and relaxations.

The disaggregated data also reveal that there is quite some variety in the relative importance of the endemic, the autoregressive epidemic and the spatial endemic components. As Figure 4 reveals, the spatial endemic component played only a minor role in the case of Saxony whereas spatial spillovers contributed much to the development of new infections in North Rhine-Westphalia. These regional differences might be taken as an indication that different sorts of containment measures are adequate in these regions.

7. Summary and Conclusions

When the COVID-19 epidemic reached Germany in the first quarter of 2020, the German government adopted various waves of country-wide containment measures. Employing a spatio-temporal endemic-epidemic model, which is estimated for reference-date- and incubation-time-corrected RKI data, we showed that the second and especially the third wave of containment measures was likely not necessary to prevent a collapse of the German health system. However, based on a quasi real-time analysis we also show that, based on the available information, the decisions to adopt additional measures can hardly be judged as wrong or even irrational. However, the depicted discrepancy between the ex-post and the ex-ante perspective indicates that the payoff of better and earlier available data on unfolding epidemics might be large, especially in the light of the enormous costs of many containment measures. Investments in the collection of reliable raw data in medical practices and laboratories and a quicker transmission of this data to the relevant policymakers and researchers might help to reduce the follow-up costs of unnecessary containment measures.

Our study also questions one-size-fits-all containment policies, as they were initially adopted in Germany and many other countries. While initially a common containment policy might be helpful in organizing the necessary public support as all citizens are exposed to the same measures, this comes at the price that the adopted measures might be too strict for less affected areas. In consequence, the total costs of the containment policy are unnecessarily large. A regional differentiation of containment policies, dependent on the local infection situation, seems to be preferable, at least in countries where the federal institutions are capable of conducting and supervising locally differing policies. The German states seem to have realized this in early

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May, when first Thuringia (May 4) and Bavaria (May 5) departed from the country-wide strategy and relaxed various measures. Only a few days later, on May 7, Chancellor Merkel announced the end of the regular coordination meetings and declared the states to be responsible for further containment strategies.

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Why can't everybody work remotely? Blame the robots

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A renewed interest in the ability to work remotely has arisen due to COVID-19. This paper seeks to understand the technological antecedents of that ability. We construct county-level measures of the ability to work at home (wah) using industry and occupation data from a variety of sources, and correlate these with county-level measures of automation and new-task implementation. Empirical results suggest that regions that faced automation produced job opportunities with lower wah, while regions that faced new innovation demonstrate no clear pattern. Finally we show that regions with low wah tend to employ lower-skilled immigrant populations, and have suffered higher unemployment due to COVID. Even as many technologies increase the ability of workers to work remotely, automating technologies tend to counter this, raising the potential for the need to shutdown certain industrial centers due to the COVID pandemic.

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1 Introduction

Economists around the world, currently forced to work at home, are grappling with the idea of many millions of laborers forced to do the same. An explosion of papers on the economic effects of laborers' ability to work at home (*wah*) has been one consequence. This paper takes a slightly different approach — here we seek to understand the technological antecedents of that ability. Different regions around the United States have experienced different kinds of technological changes, uniquely shaping industrial and occupational compositions across the country. The outbreak of COVID-19 has tested each region's ability to shelter in place while maintaining economic livelihoods. A local labor market's ability to remain productive in such an environment has been shaped to some extent by forces at work for decades.

We attempt a number of things in this paper. First, we describe a very simple theory of the economy with two types of technological changes, one related to automation and the other related to new task implementation. We show that automation tends to displace workers towards low-skilled service jobs, those which are consistent with a low *wah*-ability. New tasks on the other hand may or may not spur workers to join higher-end production work with higher *wah*-ability, depending on initial conditions.

We next develop county-level measures of the ability of workers to work from home. These are constructed from a variety of sources. Our measures show great variation in local labor markets across the country, and correlate imperfectly with existing work-at-home measures. We observe how two measures of technologies, one associated with automation and the other associated with new tasks, relate to our work-at-home measures. Consistent with the simple theory, automation produces lower *wah*, while new task implementation show no clear trends.

Finally, Basso et al. (2020) documents that automating technologies tend to be associated with low-skilled migration. This would suggest that these migrants would tend towards areas with relatively low *wah*, and thereby be more prone to job loss and/or infection upon the outbreak of COVID. We see that indeed, migrant groups that are first generation or that lack citizenship status tend more towards regions adopting automating technologies and less towards regions adopting new tasks. These are precisely those areas with low degress of *wah* ability. And we observe that regions with lower *wah* tended to suffer more unemployment in recent months due to the COVID epidemic.

All told, the polarizing effects of automating technologies, and the inequities that these create, have been further exacerbated by the low ability of laborers in these areas to work remotely once the

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pandemic hit. Furthermore, low-skilled migrant workers, having had found opportunities to work in places demanding their services, are now disproportionately affected by the COVID outbreak.

While continued social distancing may further exacerbate automation (Muro et al. 2020), here we show that places with low *wah* are precisely those areas that have *already* experienced a great deal of automation. Thus it could be that economic shutdowns caused by the low ability of workers to work remotely may not generate as much automation in the future. Indeed, we suggest that automation is not an inevitable or even advisable path to address economic production in the face of a pandemic — such a technological path can make the labor force even more vulnerable to health risks.

The rest of this paper proceeds as follows — section 2 discusses some literature, section 3 presents the simple model, section 4 describes how we construct measures of *wah* and technologies, section 5 presents some empirical findings, and section 6 concludes.

2 Literature

The COVID epidemic has produced a renewed interest in the types of jobs and workers that are able to work remotely if necessary. Bartik et al. (2020) pore over recent firm surveys and find a tremendous amount of variation across industries. Overall higher-skilled workers have a greater likelihood to be in an industry that can transition to remote work. Mongey and Weinberg (2020) echo this, but also highlight that workers with less of an ability to work at home are more likely to belong to a racial minority group, to come from the bottom half of the income distribution, and to be born outside of the United States.

Various demographic relationships have been pointed out. Women and lower-paid workers tend to work in jobs where face to face interactions are important (Avdiu and Nayyar 2020; Baker 2020; Dingel and Neiman 2020). Brynjolfsson et al. (2020), conducting their own survey, find that younger workers were more likely to switch to working remotely with the spread of COVID.

The renewed focus on remote work, spurred by a global pandemic, naturally spans the globe. Boeri et al. (2020) focuses on the number and types of jobs across Europe that could be done from home, also finding a great deal of variation. In Italy for example, where specific sectors were targeted for lockdown in March 2020, lockdowns tended to focus on those workers who could not work remotely yet were also from low-risk populations (Barbieri et al. 2020), perhaps causing needless economic hardship. In poorer countries, where the share of self-employed workers is high, urban laborers are less able to work from home than in richer countries (Gottlieb et al. 2020), though a great deal of heterogeneity exists across

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regions (Saltiel 2020).

The transition to remote work can also have various effects on productivity. For example Bloom et al. (2015) find evidence from call centre employees in China that teleworking enhanced the company's total factor productivity (TFP). On the other hand, face-to-face communication remains an important source of productivity and knowledge transmission for complex ideas. Battiston et al. (2017) for example suggests that teleworking is unsuitable for tasks requiring face-to-face communication. And Dutcher (2012) suggests that telecommuting may have a positive impact on the productivity of creative tasks but a negative impact on the productivity of "dull" tasks.

In this work we stress that the relationships between remote work and these demographic and productivity factors stem from technological changes that have been evolving for some time. Two types of changes bear mentioning. First, automating technologies have been adopted in various manufacturing industries for decades (Autor and Dorn 2013, Acemoglu and Restrepo 2020). These have been documented to polarize employment, and to attract migrant workers (Basso et al. 2020), potentially leading to changes in the types of workers and occupations that can work remotely. But how exactly? A polarized labor market can raise production in both manufacturing and services, and increase demand for both high- and low-skilled workers. The effects on the overall ability of workers to work remotely in a region with greater robot penetration thus remains unclear.

Second, new or breakthrough technologies (what Acemoglu et al. 2020 might label "radical" innovations) might raise the demand for highly skilled workers. Here again, however, the effects on remote work appear to be ambiguous. If these technologies involve complex ideas, face-to-face communication may become more important (Battiston et al. 2017). But if these technologies are skill-biased, they can attract workers in industries more naturally inclined to work at home (Bartik et al. 2020).

Overall then, how different technologies can influence the extent of *wah* in local labor markets must be to a large extent an empirical question. We next turn to a simple framework that yields plausible relationships between technological change and *wah*. We then test these relationships for the United States.

3 A Simple Framework

The following framework is a considerably simplified and amended version of Basso et al. (2020). Consider utility to be a function of the consumption of goods and services:

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$$U = \left(C_s^{\frac{\sigma-1}{\sigma}} + C_g^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}, \tag{1}$$

where C_s and C_g are per capita consumption of services and goods, respectively, and $\sigma \geq 0$ determines the elasticity of substitution between goods and services.

Workers can either work in providing services (producing Y_s) or in making goods (producing Y_g). We will presume that service workers have a lower ability to work at home than producers of goods.[1] Production of each are given by:

$$Y_s = C_s = L_s; \tag{2}$$

$$Y_g = C_g = \left(L_m^\gamma + L_h^\gamma\right)^{\alpha/\gamma} K^{1-\alpha}, \tag{3}$$

where L_s is labor in service occupations, K is capital, L_m is labor in goods production performing medium-skill level tasks, and L_h is labor in goods production performing high-level level tasks. $\gamma \leq 1$ dictates the elasticity of substitution between medium and high-level tasks, and $0 \leq \alpha \leq 1$ is the share of manufacturing income devoted to labor. We will assume here that K is exogenously given.[2]

All workers are paid their respective marginal products. Workers are indexed on a unitary number line by their ability, which equals η_i for worker i. We consider this as an endowment distributed in the population as described below. η_i takes on positive values ranging from 0 to ∞. This can be thought of as the ability to perform tasks in production relative to the ability to perform manual/service tasks; the latter is common to all workers and standardized to one. Workers choose whether to work in service production, or use their ability in manufacturing production. For those who choose production work, only those with a high enough η_i can perform high-level tasks.

Given these assumptions, there are two relevant ability thresholds for native workers. Let $\hat{\eta}$ be the ability level at which a worker would be indifferent between being either a low-skilled manual/service worker or a goods-producing worker. Anyone with skills lower than $\hat{\eta}$ works in a service occupation. On the other hand let $\bar{\eta} > \hat{\eta}$ be the minimum ability level at which workers can perform high-level

[1] This is naturally an extreme assumption, as many services in tech and business are both highly-skilled and are able to be performed remotely. The focus here however is on in-person service work. Higher-skilled service workers are assumed to work in goods production, where there is inherently a stronger ability to work from home. The data is able to make finer distinctions between these different types of service jobs.

[2] Autor and Dorn (203) and Basso et al. (2020) endogenize K by equating its marginal productivity with capital prices, but have these prices as exogenously determined. We do not lose any insights from outright manipulating K exogenously.

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tasks. For our exercises below we will endogenize $\hat{\eta}$ but not $\bar{\eta}$; the latter will be set as a proxy for the importance of "new tasks."

Further, let w_m be the per unit wage of human capital in middle-level tasks in production, and w_s the wage of service work. We assume that workers' ability is distributed as a negative exponential over the interval $[0, \infty]$. The density is given by $f(\eta) = e^{-\eta}$.

Since the planner's problem is the same as the decentralized equilibrium, we maximize (1) given the constraints mentioned above. Given values for technological proxies K and $\bar{\eta}$, an equilibrium for the economy is given by solving the following equations for w_m, w_s, $\bar{\eta}$, L_m, L_h, and L_s:

$$w_m = \frac{\partial Y_g}{\partial L_m}, \tag{4}$$

$$w_s = \left(\frac{L_s}{Y_g}\right)^{-1/\sigma}, \tag{5}$$

$$\hat{\eta} w_m = w_s, \tag{6}$$

$$L_m = (\hat{\eta} + 1)\, e^{-\hat{\eta}} - (\bar{\eta} + 1)\, e^{-\bar{\eta}}, \tag{7}$$

$$L_h = (\bar{\eta} + 1)\, e^{-\bar{\eta}}, \tag{8}$$

$$L_s = 1 - e^{-\hat{\eta}}. \tag{9}$$

We first look at "automation," which we model here as increases in capital in production.

Proposition 1. $\frac{\partial L_s}{\partial K} > 0 \;\forall\; \sigma > 0$.

Proof. Using (4), (5) and (6) we can write $\hat{\eta}$ as

$$\frac{w_s}{w_m} = \frac{(1/L_s)^{1/\sigma}\, Y_g^{1/\sigma}}{\alpha \left(L_m^\gamma + L_m^\gamma\right)^{(\alpha-\gamma)/\gamma} K^{1-\alpha} L_m^{\gamma-1}} = \frac{(1/L_s)^{1/\sigma} \left(L_m^\gamma + L_h^\gamma\right)^{\alpha/\gamma\sigma} K^{1-\alpha/\sigma}}{\alpha \left(L_m^\gamma + L_m^\gamma\right)^{(\alpha-\gamma)/\gamma} K^{1-\alpha} L_m^{\gamma-1}}$$

We are left with $K^{1/\sigma}$ in the numerator, and so the derivative of this expression with respect to K will be positive so long as $1/\sigma$ is positive. A rising $\bar{\eta}$ means a rising L_s by (9). □

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Remark. Increases in capital increases the productivity of goods production, raising the relative scarcity of services and thereby increasing their demand. This drives erstwhile production workers to join service occupations. Of course, this is a partial equilibrium result. Labor changes will then in turn change relative wages. In simulations we see that in general equilibrium capital growth results in an increase in L_s when $\sigma < 1$, that is, when goods and services are complementary.

This is precisely the channel on which Autor and Dorn (2013) focuses to generate low-end employment polarization (erstwhile routine workers becoming low-skilled service workers), keeping the number of high-skilled workers fixed. There is however another technological possibility highlighted by Acemoglu and Restrepo (2019) among others — the implementation of new tasks in production. Many of these tasks are designed to simplify or streamline production processes. These include "tasks related to programming, design, and maintenance of high tech equipment, such as software and app development, database design and analysis, and computer-security-related tasks, as well as tasks related to more specialized functions in existing occupations, including administrative assistants, analysts for loan applications, and medical equipment technicians" (Lin 2011). New tasks tend to be used by those with specialized skills and higher levels of education. These are also the areas that tend to be better equipped to handle remote work (Bartik et al. 2020). In this framework we model new tasks as a greater expansion of high-level tasks in production (i.e. a decrease in $\bar{\eta}$).

Proposition 2. $\frac{\partial L_s}{\partial \bar{\eta}} > 0 \ \forall \ \gamma < 1$ *given that initial values of* L_m *and* L_h *are "close enough" to each other.*

Proof. Further simplifying the wage ratio above, taking the derivative of this with respect to $\bar{\eta}$ and using the product rule gives us:

$$\frac{\partial \hat{\eta}}{\partial \bar{\eta}} = \frac{(1+\sigma)}{\sigma}\left(L_m^{\gamma} + L_h^{\gamma}\right)^{1/\sigma}\left(\gamma L_m^{\gamma-1}\left(\frac{\partial L_m}{\partial \bar{\eta}}\right) + \gamma L_h^{\gamma-1}\left(\frac{\partial L_h}{\partial \bar{\eta}}\right)\right) L_m^{1-\gamma} + \left[\left(L_m^{\gamma} + L_h^{\gamma}\right)^{1+\sigma/\sigma}(1-\gamma)L_m^{-\gamma}\right]\left(\frac{\partial L_m}{\partial \bar{\eta}}\right)$$

Using the fact that $\frac{\partial L_h}{\partial \bar{\eta}} = -\frac{\partial L_m}{\partial \bar{\eta}}$, we further simplify this to

$$\left[\frac{(1+\sigma)}{\sigma}\gamma\left(L_m^{\gamma-1} - L_h^{\gamma-1}\right)L_m + \left(L_m^{\gamma} + L_h^{\gamma}\right)(1-\gamma)\right]\left(L_m^{\gamma} + L_h^{\gamma}\right)^{1/\sigma} L_m^{-\gamma}\left(\frac{\partial L_m}{\partial \bar{\eta}}\right)$$

Thus we see that whatever value γ takes this expression is positive for values where L_m and L_h are not substantially different. ☐

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Remark. An expansion of high-level tasks raises the demand for mid-level tasks when these types of tasks are grossly complementary ($\gamma < 0$) and there are already a sufficient number of L_h workers. Because the two worker-types in production grossly complement each other, a large number of those performing high-end tasks can drive erstwhile service workers to join production jobs doing lower-end tasks. However, if L_m and L_h are grossly substitutable, or if there are large differences between them, then a rise in $\bar{\eta}$ will not generate a rise in manufacturing employment.

We numerically solve the model to demonstrate the above propositions. We are interested in how these two forms of technological changes influences workers' ability to work at home. If we make the extreme assumption that goods producers can perform all operations remotely, and that service workers can only work in person, then we can produce a "work at home" index (wah) that is simply the fraction of those who can work remotely. From the model this is given by

$$wah = \frac{e^{-\hat{\eta}}}{e^{-\hat{\eta}} + L_s}. \tag{10}$$

Simulations of the model demonstrate that, so long as goods and services in utility are grossly complementary, automation produces more employment in services. However, adoption of new tasks is not guaranteed to produce more employment in overall goods production. We can see this with numerical results in Table 1 — given our parameterization, we do not have an increase in goods employment with a rise of importance of high-level tasks. The reason is that in this case the productivity gain from more employment in high-skilled production drives greater demand for unskilled services. Given initial conditions, L_m and L_h do not complement each other enough to drive greater growth in mid-skilled employment.

Table 1: Numerical simulation

	wah	L_s	w_s	w_m
Baseline	0.327	0.638	0.188	0.185
K doubles	0.319	0.680	0.308	0.270
$\bar{\eta}$ halves	0.325	0.675	0.221	0.196

Parameter values for these numerical exercises are: $\sigma = 0.5$, $\gamma = 0.5$, $\alpha = 0.5$, $\bar{\eta} = 10$, $K = 0.1$.

Not modeled here, robot adoption can also raise unemployment, as discussed and analyzed extensively in Acemoglu and Restrepo (2020). It is unclear however if workers displaced by automation actually transition to low-skilled services. Rather displaced workers may simply exit the labor market. Basso et al. (2020) documents how automation creates low-skilled employment growth in part by attracting low-skilled immigrants. In this framework automation would attract an outsized group of foreign workers mainly in services unable to work remotely. Finally, this very simple model may miss some important features, such as the possibility that automation may raise the demand for skilled production workers. An empirical look is therefore needed.

4 Measures

The simple framework above provides some testable implications regarding the effects of technologies on the ability of workers to work remotely. To this end we construct indices of automation, new technologies, and the ability to work remotely for labor that vary across regions.

The proxy for automation commonly employed is an industry-specific measure of robot penetration. This is done in Acemoglu and Restrepo (2020). Robot measures are available for 23 industries which are then mapped to the full 61 industries. Acemoglu and Restrepo (2020) then map this industry-level measure to industry employment shares at the metropolitan statistical area level. Instead of this we map these industry-level measures to 1998 industry employment at the county level obtained from the Bureau of Labor Statistics.

$$Robots_c = \sum_i l_{ci}^{1998} APR_i \tag{11}$$

where l_{ci} is the employment share of industry i in county c, and APR_i is the adjusted penetration of robots in industry i. 1998 is the earliest year that the BLS reports industry composition using the modern system classification. This Bartik-style measure should allow us gage the extent of robot penetration in specific counties that is independent of other local economic changes occurring over the last couple of decades.

A common proxy for new technologies on the other hand is the number of novel tasks associated with production. The measure of new and emerging tasks by occupation comes from O*NET. We use the latest measure, capturing the emerging tasks for 2018. This measure is projected to industries using the employment distribution across occupations in the 1990 Census. Finally, we map these industry-level measures to 1998 industry employment weights in counties as before:

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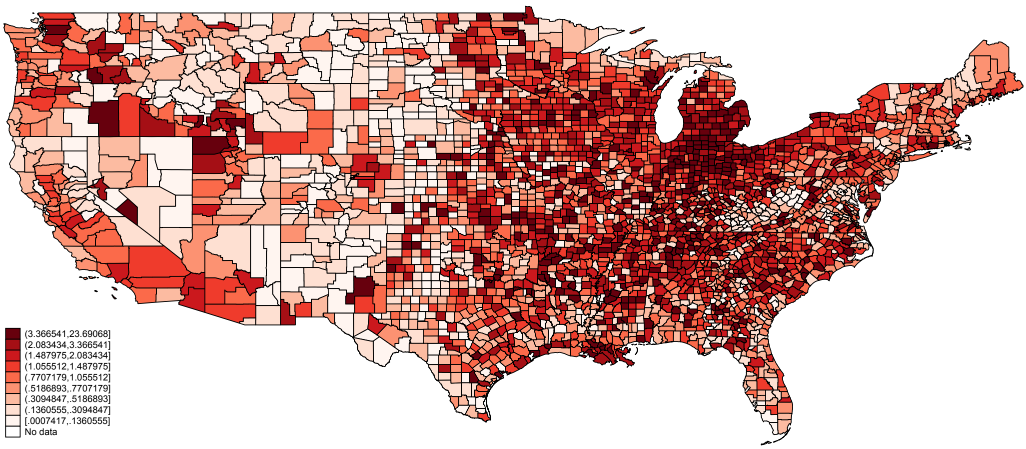
$$Newtasks_c = \sum_i l_{ci}^{1998} \#newtasks_i^{2018} \tag{12}$$

Figures 1 and 2 display the spatial variation of our measures given respectively by (11) and (12). While we do observe some overlap related to urban/production centers, there are also notable differences. We observe a great deal of robot penetration in Michigan and Ohio, as well as big swaths of the Midwest. New task implementation on the other hand is quite concentrated in the Northeast as well as the Southwest and portions of the Northwest.

Figure 1: Robots



We produce two alternative indices of a regional labor force's ability to work at home. The first exploits variations at the industry level. Industry-level measures of the ability to work at home are from del Rio-Chanona et al. (2020). The authors construct a Remote Labor Index for 277 industries. We then map this index to industrial employment across counties using 2018 employment shares provided by publicly available data from County Business Patterns.

We also construct an alternative "placebo" measure that weighs these industry-level values of remote work to county-level industrial employment in 1998. The purpose of this is to have a counter-factual measure that captures the extent of an area's ability to work at home if such remote techniques available today were available in 1998. Use of this measure will allow us to capture the *change* in industrial composition that results from technological developments. Thus we can ask more directly, did technology

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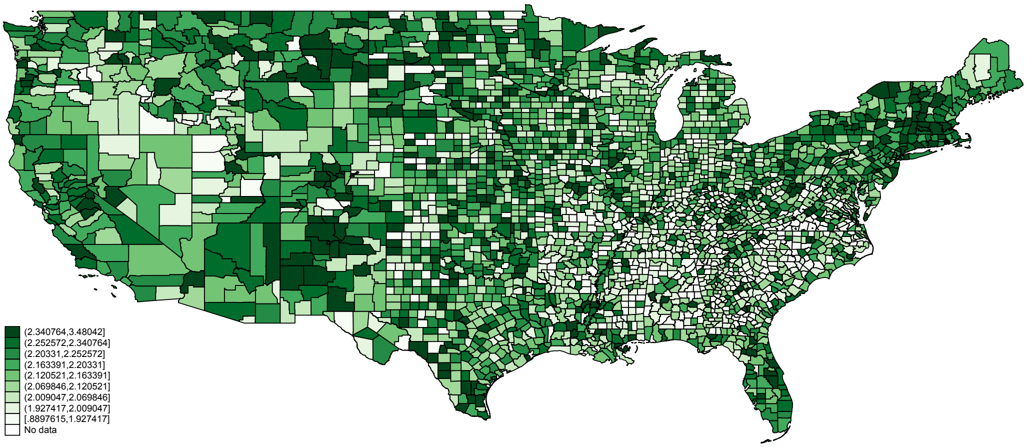
Figure 2: New Tasks



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change the local labor's ability to work from home?

The other measure constructed uses the fraction of workers who could work at home across occupations constructed by the BLS. To this we map current occupational employment at the MSA level available by Occupational Employment Statistics. This approach is similar to what is done in Dingle and Neiman (2020), which also focuses mainly on occupations.[3] The spatial variation of both measures is displayed in Figures 3 and 4. Each measure is scaled to range from 0 (no possibility of remote work for anybody) to 100 (all laborers can work remotely).

Distinguishing between industry-level variation in *wah* and occupation-level variation is not trivial. The correlation between the two indices is not particularly high ($r = 0.34$), suggesting a fair amount of difference between the two proxies on the ability of a community to work remotely. One possibility, missing from our simple theoretical framework, is that technologies can change the occupational structures of different industries in different ways. For example technological changes can adjust employment towards industries with greater inherent *wah*, such as those related to information technologies or professional services. But it can also change the composition of occupations within each industry, such as substituting away from or outsourcing software engineers and hiring more installation and repair personnel, potentially lowering the *wah* of the overall industry. On the other hand a worker who belongs to an occupation that may typically work from home may not so readily be able to if she is part of an

[3]The correlation between our wac_{occ} measure and the Dingle and Neiman (2020) measure is over 0.9.

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Figure 3: Work At Home Measure — weighted by industry composition

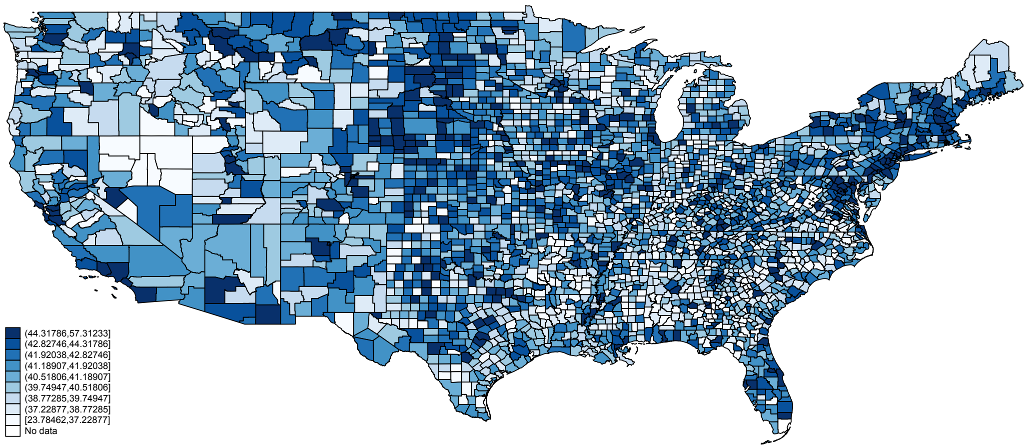
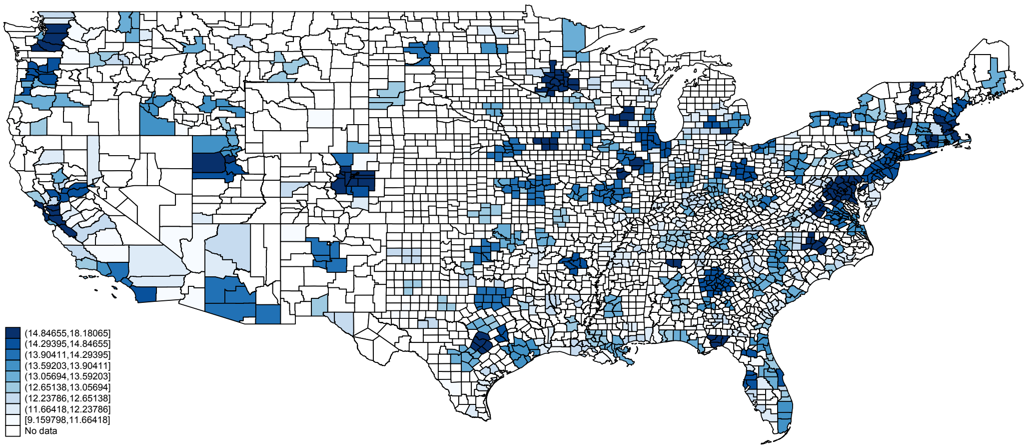


Figure 4: Work At Home Measure — weighted by occupational composition



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industry that typically cannot work from home — say for example a manager working on a construction site. For these reasons we look at two measures capturing different variations in the availability of remote work.

To get a better sense of these different measures, we rank order U.S. counties according to the relative strength of each measure. In table 2 we show the top 10 counties in each category, including regions that scored highly in one category but relatively lowly in the other. Not surprisingly, regions that neighbor major metropolitan centers such as Washington D.C., New York and San Francisco tend to score highly in both measures on the ability to work remotely. Counties listed in the lower-left box are those that score highly on the industry-level measure of *wac* but lowly on the occupation-level measure. These regions tend to have sizable manufacturing bases (which on average have moderate degrees of ability to work from home) but which may mainly employ those in primary production or installation/maintenance (with very low ability to work from home). On the other hand counties listed in the upper-right box are those that score highly on the occupation-level measure of *wac* but lowly on the industry-level measure. These may be areas that tend to employ many professionals and managers (high *wac*), but perhaps concentrated in transportation, utilities, or leisure and hospitality (low *wac*).

Finally, we include a range of covariates related to local demographic and education levels. These are measured at the commuting zone level for 2008.

5 Empirical Results

5.1 Working at Home and Technological Change

We first look at the relationships between our industry-level measure of *wac* and technologies. OLS estimates are displayed in Table 3. The first thing to note is that our county-level measures of robotization are negatively associated with our work-at-home measures. We also observe that populations with more Hispanic and black individuals tend to be in those regions with lower *wac*. This is generally consistent with other recent studies. We also see from specification 3 that the ability to work from home is positively associated with new task adoption.

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Table 2: Top 10 counties in each *wah* category

	High wah_{ind}	**Low** wah_{ind}
High wah_{occ}	Alexandria city, VA	Bastrop County, TX
	Arlington County, VA	Clear Creek County, CO
	District of Columbia	Fluvanna County, VA
	Fairfax County, VA	Gilpin County, CO
	Montgomery County, MD	Iowa County, WI
	St. Mary's County, MD	Jefferson County, WV
	San Francisco County, CA	Manassas Park city, VA
	Santa Clara County, CA	Nelson County, VA
	Stafford County, VA	Rappahannock County, VA
	Suffolk County, MA	Teller County, CO
Low wah_{occ}	Alexander County, NC	Crawford County, GA
	Bibb County, GA	Ector County, TX
	Elkhart County, IN	Jefferson County, TN
	Fond du Lac County, WI	Kalawao County, HI
	Hancock County, KY	Martin County, TX
	Murray County, GA	Maui County, HI
	Sheboygan County, WI	Midland County, TX
	Union County, SD	San Juan County, NM
	Whitfield County, GA	Sullivan County, IN
	Wyoming County, PA	Twiggs County, GA

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Table 3: Determinants of ability to work-at-home weighted by industrial employment

	(1) wah_{ind}	(2) wah_{ind}	(3) wah_{ind}	(4) wah_{ind}	(5) wah_{ind}
ln (Robots)	-1.436***	-1.290***	-1.056***	-0.124*	-0.144*
	(-12.24)	(-10.99)	(-8.72)	(-2.05)	(-2.26)
ln (New Tasks)	–	–	9.060***		-0.972
			(6.36)		(-1.36)
Population	–	1.972***	1.776**	0.132	0.147
		(3.45)	(3.10)	(0.70)	(0.79)
Age 26–35	–	-2.227	-0.374	2.072	1.964
		(-0.49)	(-0.08)	(0.90)	(0.86)
Age 36–45	–	-0.169	2.659	9.841**	9.517**
		(-0.03)	(0.50)	(3.25)	(3.13)
Age 46–55	–	14.42**	10.73	-10.73***	-10.37***
		(2.61)	(1.94)	(-3.60)	(-3.45)
Age 56–65	–	-30.60***	-25.46***	7.021*	6.687*
		(-4.89)	(-4.11)	(2.12)	(2.00)
Above 65	–	13.73**	11.38**	1.136	1.317
		(3.28)	(2.74)	(0.58)	(0.66)
Female	–	53.14***	49.38***	7.280	7.426
		(5.30)	(4.96)	(1.46)	(1.48)
Hispanic	–	-1.228*	-1.383**	-1.170**	-1.153**
		(-2.43)	(-2.71)	(-3.07)	(-3.00)
White	–	-2.734	-1.663	0.618	0.532
		(-1.95)	(-1.12)	(0.49)	(0.42)
Black	–	-7.901***	-6.120***	-0.0342	-0.170
		(-5.35)	(-3.92)	(-0.03)	(-0.13)
Asian	–	1.763	1.802	0.801	0.804
		(0.38)	(0.38)	(0.48)	(0.49)
High School	–	-4.194	-1.269	1.443	1.215
		(-0.70)	(-0.21)	(0.52)	(0.44)
Some College	–	-5.691	-2.935	2.043	1.856
		(-0.92)	(-0.48)	(0.75)	(0.69)
College	–	9.615	11.52	3.568	3.398
		(1.15)	(1.41)	(0.88)	(0.84)
$wah_{ind,1998}$	–	–	–	0.854***	0.859***
				(64.82)	(63.14)
R^2	0.06	0.16	0.18	0.77	0.77
N	3196	3165	3165	2992	2992

t statistics in parentheses. Excluded variables are Age 16–25, Other Race, and Masters Degree.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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Table 4: Determinants of ability to work-at-home weighted by occupational employment

	(1) wah_{occ}	(2) wah_{occ}	(3) wah_{occ}	(4) wah_{occ}	(5) wah_{occ}
ln(Robots)	-0.466***	-0.157**	-0.109*	-0.119*	-0.106*
	(-5.91)	(-2.62)	(-2.02)	(-2.18)	(-2.10)
ln(New Tasks)	–	–	1.635**	–	0.573
			(2.96)		(0.97)
Population	–	0.169	0.126	0.180	0.170
		(0.67)	(0.50)	(0.75)	(0.70)
Age 26–35	–	4.003	4.709	4.452	4.679
		(1.40)	(1.63)	(1.52)	(1.59)
Age 36–45	–	2.823	3.265	1.932	2.093
		(0.88)	(1.03)	(0.59)	(0.64)
Age 46–55	–	6.574	5.689	6.617	6.395
		(1.74)	(1.52)	(1.71)	(1.66)
Age 56–65	–	-7.395	-5.938	-5.349	-5.024
		(-1.94)	(-1.56)	(-1.39)	(-1.30)
Above 65	–	-5.479*	-6.328**	-6.315**	-6.521**
		(-2.56)	(-2.98)	(-2.95)	(-3.06)
Female	–	17.81*	17.55*	13.77	13.82
		(2.56)	(2.52)	(1.95)	(1.96)
Hispanic	–	-0.801**	-0.816**	-0.791**	-0.798**
		(-3.22)	(-3.29)	(-3.02)	(-3.05)
White	–	-1.098	-0.847	-2.373	-2.248
		(-0.61)	(-0.51)	(-1.35)	(-1.31)
Black	–	-1.775	-1.431	-2.766	-2.622
		(-0.97)	(-0.84)	(-1.56)	(-1.50)
Asian	–	-2.314	-2.145	-3.733	-3.612
		(-0.73)	(-0.69)	(-1.18)	(-1.14)
High School	–	-26.43***	-25.80***	-25.52***	-25.36***
		(-10.22)	(-9.98)	(-9.48)	(-9.39)
Some College	–	-24.78***	-24.27***	-24.40***	-24.26***
		(-9.20)	(-9.00)	(-8.74)	(-8.67)
College	–	-18.12***	-17.48***	-17.79***	-17.59***
		(-4.63)	(-4.46)	(-4.40)	(-4.34)
$wah_{ind,1998}$	–	–	–	0.445***	0.405***
				(4.59)	(3.94)
R^2	0.03	0.55	0.55	0.55	0.55
N	1190	1182	1182	1133	1133

t statistics in parentheses. Excluded variables are Age 16–25, Other Race, and Masters Degree.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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One might suggest a rather mechanical relationship here. Industries that have had greater robot penetration might be precisely those where it is not feasible to work remotely. This is not really accurate, as lower-skilled service industries have had low automation and are also those with low ability for remote work. Nevertheless, to address this we include in specifications 4 and 5 our work-at-home measure with the industry weights for 1998. Thus estimates for these specifications measure how factors have affected the *change* in industry composition over the last two decades. Here we see that while the estimates for new tasks are now negative and statistically insignificant, estimates for robot penetration remain negative and statistically robust (at 95% level). Magnitudes become smaller — a 1% increase in robotization is associated with over a tenth of a percent decline in jobs with remote work possibility. Overall, these results suggest that greater automation in a region lowers the ability of workers in that region to work remotely. On the other hand, while industries that had adopted new tasks tend to be those with greater *wah* capabilities, we see that new task implementation did not increase this capability further.

Table 4 looks at the same relationships but with our alternative *wac* measure using occupational weights. The associations between the ability to work remotely and different technology measures are consistent with those shown in Table 3. Robotization is consistently negatively related to the ability to work at home, including when we have our past industry weighted *wac* measure as an additional control. Magnitudes are also comparable — a 1% increase in robotization translates to one tenth of a percent fall in remote work. New task adoption on the other hand is positively associated with working at home only without this control. We also see that the ability to work remotely remains negatively associated with more Hispanic workers (but no longer black workers). This measure is also positively related to levels of education; this is also consistent with previous studies.

Table 5: Effects of industry-weighted *wah* on recent change in unemployment

	(1)	(2)	(3)	(4)	(5)	(6)
Estimation:	OLS	OLS	2SLS	2SLS	2SLS	2SLS
wah_{ind}	-0.0579*	-0.107***	-1.489***	-1.177***	-0.479***	-0.410***
	(-2.30)	(-4.41)	(-9.91)	(-8.22)	(-6.03)	(-4.43)
Population		4.025***		5.769***		4.506***
		(4.96)		(5.77)		(5.63)
Age 26–35		39.43***		35.84***		38.62***
		(6.17)		(4.53)		(6.04)
Age 36–45		67.65***		59.99***		65.09***
		(9.78)		(6.52)		(8.77)
Age 46–55		19.38**		33.69***		23.87**
		(2.61)		(3.54)		(3.12)
Age 56–65		8.883		-16.08		1.821
		(1.04)		(-1.37)		(0.19)
Above 65		-17.90**		-2.663		-13.61*
		(-2.62)		(-0.32)		(-2.07)
Female		84.92***		132.4***		97.92***
		(5.02)		(6.95)		(6.49)
Hispanic		-8.160***		-7.896***		-8.061***
		(-11.60)		(-8.94)		(-11.30)
White		10.08***		4.026		8.360***
		(5.74)		(1.46)		(3.81)
Black		0.0554		-10.74***		-2.981
		(0.03)		(-3.45)		(-1.23)
Asian		9.886*		10.35		10.14
		(2.20)		(1.42)		(1.72)
High School		-71.67***		-77.68***		-73.95***
		(-8.79)		(-7.96)		(-9.38)
Some College		-64.67***		-73.78***		-67.95***
		(-8.07)		(-7.50)		(-8.56)
College		-111.7***		-99.44***		-109.0***
		(-9.53)		(-7.08)		(-9.63)
R^2	0.01	0.201	0.54	0.711	0.752	0.811
ln(Robot) as instrument	–	–	yes	yes	yes	yes
ln(New tasks) as instrument	–	–	no	no	yes	yes
First-stage F-stat	–	–	195.25***	195.25***	202.69***	202.69***
N	3203	3167	3193	3163	3193	3163

Dependent variable is change in regional unemployment rate from April 2019 to April 2020.
t statistics in parentheses. Excluded variables are Age 16–25, Other Race, and Masters Degree.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In short, regions which have experienced an influx of robot employment have also raised employment in areas ill-suited for remote work. This would be consistent with automation fostering employment polarization at the lower end of the educational spectrum, as well as the influx of low-skilled migrant workers. Technologies which conceivably have helped organizations have their labor work remotely have in part been thrwarted by automating technologies, which raises employment in areas where working

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at home is not feasible.

5.2 Working at Home and Unemployment

Around the middle of March 2020 contact-intensive sectors such as restaurants and dentists' offices began shutting down due to the maintenance of social distancing. Several researchers have already provided estimates of the supply shock from COVID (Dingel and Neiman 2020, Hicks 2020, Koren and Peto 2020). We also know that workers who are less able to work remotely have suffered higher unemployment from COVID (Papanikolaou and Schmidt 2020). Given the analysis above however, we might wonder if *technology*-induced increases in contact-intensive jobs contributed to the unemployment spike witnessed in many regions due to the COVID pandemic.

OLS and 2SLS results from this thought experiment are presented in Tables 5 (for industry-weighted measures of *wah*) and 6 (for occupation-weighted measures of *wah*). The dependent variable here is the change in the regional unemployment rate from the end of April 2019 to the end of April 2020, as reported by the BLS. The year-on-year unemployment rate from March 2019 to March 2020 increased on average by 1.2 percent across counties. By contrast the rate from April 2019 to April 2020 rose an average of 8.8 percent.

In OLS estimates we see that our measures of working at home are negatively related to the rise in unemployment. Of course many features related to industrial composition may be at work here contributing to different employment responses due to the shutdown. To unpack this further, we run two-stage least squares regressions, where the first stage estimates *wac* using either *ln(robots)* as an instrument in a just-identified model, or both *ln(robots)* and *ln(new tasks)* in an over-identified model.[4] Magnitudes of the estimated effect rise dramatically in this case — depending on the specification, a one percent higher number of jobs which can be done remotely translates to between a one-half to one and one-half percent *smaller* spike in unemployment.[5]

We also perform the same exercise using our occupation-weighted measure, and show results in Table 6. Patterns here are similar — 2SLS estimates are larger than OLS estimates, and demonstrate a clear negative relationship between a technologically-induced ability to work remotely and the increase in

[4]Note that reduced form specifications show that both technology measures are positively associated with the year-on-year change in unemployment.

[5]Of course automation has widely been documented to create labor displacement and so can contribute to unemployment directly. However here we are looking only at the change in unemployment over the last few months, the bulk of which was due to the pandemic. County-level robot penetration as we measure it would arguably not directly impact this change.

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unemployment. No doubt many structural features of local economies had contributed to job losses with the COVID outbreak. Still, this provides some suggestive evidence that regions which experienced automation are also regions with the kinds of jobs most vulnerable to pandemics such as COVID.

Table 6: Effects of occupation-weighted *wah* on recent change in unemployment

	(1)	(2)	(3)	(4)	(5)	(6)
Estimation:	OLS	OLS	2SLS	2SLS	2SLS	2SLS
wah_{occ}	-0.471***	-0.675***	-4.035***	-10.56**	-1.312***	-2.667**
	(-5.78)	(-5.81)	(-5.19)	(-2.71)	(-3.48)	(-2.59)
Population	–	4.256***	–	5.610**	–	4.525***
		(5.46)		(2.67)		(5.68)
Age 26–35	–	2.711	–	42.90	–	10.99
		(0.26)		(1.26)		(0.89)
Age 36–45	–	49.59***	–	73.50*	–	54.31***
		(4.96)		(2.28)		(4.47)
Age 46–55	–	53.52***	–	117.4**	–	66.56***
		(4.14)		(2.64)		(4.25)
Age 56–65	–	9.210	–	-64.15	–	-5.428
		(0.63)		(-1.24)		(-0.30)
Above 65	–	-13.99	–	-61.24	–	-23.51*
		(-1.74)		(-1.88)		(-2.06)
Female	–	42.47	–	214.3*	–	76.82*
		(1.80)		(2.22)		(2.40)
Hispanic	–	-2.271*	–	-8.692*	–	-3.541*
		(-2.41)		(-2.13)		(-2.52)
White	–	14.42**	–	-0.867	–	11.30
		(2.62)		(-0.05)		(1.72)
Black	–	8.229	–	-12.73	–	4.004
		(1.46)		(-0.67)		(0.58)
Asian	–	21.76***	–	-5.241	–	16.24
		(3.40)		(-0.21)		(1.82)
High School	–	-39.92***	–	-305.3**	–	-94.09**
		(-4.18)		(-2.83)		(-3.18)
Some College	–	-27.31**	–	-277.0**	–	-78.35**
		(-3.06)		(-2.73)		(-2.80)
College	–	-74.01***	–	-253.2**	–	-111.0***
		(-5.33)		(-3.11)		(-4.53)
R^2	0.03	0.214	0.660	0.137	0.864	0.871
ln(Robot) as instrument	–	–	yes	yes	yes	yes
ln(New tasks) as instrument	–	–	no	no	yes	yes
First-stage F-stat	–	–	33.90***	33.90***	29.25***	29.25***
N	1191	1183	1188	1181	1188	1181

Dependent variable is change in regional unemployment rate from April 2019 to April 2020.
t statistics in parentheses. Excluded variables are Age 16–25, Other Race, and Masters Degree.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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5.3 Working at Home and Migrants

Finally, Basso et al. (2020) documents a positive relationship between automation and unskilled migrant labor. Here we explore how different migrant groups in various counties are associated with our alternative technological measures. Migrant characteristic data come from County Business Patterns. OLS estimates are provided in Table 7. Here we see that robotization is associated with regions with more non-citizen workers (relative to native and naturalized workers), with more Hispanic migrant workers (relative to European migrant workers), and with more first-generation migrant workers (relative to all migrant workers). However, when we include our measure of new tasks, our estimated effects from robotization weaken considerably. New task adoption on the other hand is strongly negatively related to these measures — these migrant groups tend to eschew regions where new tasks are being implemented.

Table 7: Effects of different technologies on immigrant characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent var:	Rel. no. non-citizens	Rel. no. non-citizens	Rel. no. Hispanic migrants	Rel. no. Hispanic migrants	Rel. no. 1st gen. migrants	Rel. no. 1st gen. migrants
ln(Robots)	0.000812^{*}	0.000476	0.0457^{*}	0.0164	0.000511^{**}	0.000386
	(2.13)	(1.20)	(2.02)	(0.70)	(2.58)	(1.86)
ln(New tasks)	–	-0.0102^{**}	–	-0.896^{***}		-0.00382^{*}
		(-2.70)		(-3.83)		(-1.97)
Female	0.0170	0.00846	2.956	2.209	-0.00160	-0.00479
	(0.54)	(0.26)	(1.53)	(1.13)	(-0.10)	(-0.29)
Population	-0.00385^{**}	-0.00339^{*}	-0.266^{**}	-0.227^{*}	-0.00141	-0.00124
	(-2.66)	(-2.37)	(-2.93)	(-2.55)	(-1.96)	(-1.72)
Age 26–35	0.0687^{***}	0.0627^{***}	5.677^{***}	5.158^{***}	0.0184	0.0161
	(3.69)	(3.36)	(5.05)	(4.56)	(1.85)	(1.62)
Age 36–45	-0.106^{***}	-0.109^{***}	-4.540^{***}	-4.797^{***}	-0.0466^{***}	-0.0477^{***}
	(-5.33)	(-5.51)	(-3.74)	(-4.02)	(-4.74)	(-4.85)
Age 46–55	-0.0455	-0.0393	-3.416^{*}	-2.877	-0.0358^{**}	-0.0335^{**}
	(-1.81)	(-1.55)	(-2.25)	(-1.88)	(-2.81)	(-2.61)
Age 56–65	0.0633^{*}	0.0532	5.349^{**}	4.461^{**}	0.0338^{*}	0.0300^{*}
	(2.35)	(1.95)	(3.21)	(2.65)	(2.38)	(2.09)
Above 65	0.105^{***}	0.111^{***}	5.988^{***}	6.487^{***}	0.0537^{***}	0.0559^{***}
	(6.74)	(7.03)	(6.33)	(6.80)	(6.32)	(6.53)
R^2	0.288	0.293	0.270	0.280	0.272	0.275
N	1182	1182	1182	1182	1182	1182

Rel. no. non-citizens is all non-citizens divided by all workers including naturalized citizens.
Rel. no. Hispanic migrants is all Hispanic migrants divided by all European migrants
Rel. no. 1st gen. migrants is all 1st generation migrants divided by all migrants
t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

How should we think about the relationship on the average ability to work at home and immigrant characteristics in local labor markets? Table 8 takes a look at this by regressing our occupation-weighted *wac* measure on these local immigrant characteristics. In short, the ability to work at home is less prevalent in areas with more non-citizens, with more minority-race migrants, with first generation migrants, and with more recent migrants.

Table 8: Immigrants/immigrant-characteristics and *wah*

Dep Var: wah_{occ} RH Var.	Est.	Dep Var: wah_{occ} RH Var.	Est.	Dep Var: wah_{occ} RH Var.	Est.	Dep Var: wah_{occ} RH Var.	Est.
Natives	.0028*** (20.18)	European	.190*** (72.74)	1st generation	-.006*** (-34.60)	Immigrated post-2010	-.056*** (-22.67)
Naturalized	-.0064*** (-3.60)	Latin American	-.0053*** (-53.27)	2nd generation	-.019*** (-19.31)	Immigrated 2000s	-.056*** (-42.53)
Non-citizens	-.0099*** (-17.59)	Asian	-.046*** (-46.71)	3rd generation	.005*** (50.10)	Immigrated 1990s	.174*** (25.82)
		Other	.041*** (16.19)			Immigrated 1980s	-.124*** (-20.95)
						Immigrated 1970s	.018* (2.09)
						Immigrated before 1970	.240*** (16.36)
R^2	.97		.99		.98		.99
N	1263		1263		1193		1263

t statistics in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Overall, we see that areas with employment geared more towards personal contact, shaped in part by automating technologies occurring these last few decades, are those areas employing historically disadvantaged migrant groups. This is part of the reason why such groups have been disproportionately affected by the COVID pandemic.

6 Conclusion

This paper has taken a brief look at technological factors that have shaped the ability of workers in local labor markets to work at home. In general, regions experiencing automation produces fewer opportunities for laborers to work remotely. Automation also tends to attract lower-skilled migrants, and tends to correlate with unemployment spikes due to COVID.

One lesson is that technological changes can shape production processes in radically different ways. While certain technologies related to IT and software development surely contributed to many higher-skilled workers now being able to work remotely, other technologies related to automating routine tasks may have had the opposite effect. The ability to work at home across various industries and occupations has in fact been fairly stable over the last few decades (Mas and Pallais 2020), suggesting regional changes in our ability to do so arise more from changes in industrial and occupational composition. Of course, continued social distancing will adversely impact different occupations very differently (Hicks et al. 2020), and therefore will affect different regions very differently. We have stressed that the extent to which pandemic-induced lockdowns affect economic activity and employment depend in large part on technologically-shaped industrial composition. Such factors are important when considering the asymmetric costs of lockdowns, and how leaders can best craft policies to address them.

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The COVID-19 shock and consumer credit: Evidence from credit card data[1]

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We use monthly credit card data from the Federal Reserve's Y-14M reports to study the early impact of the COVID-19 shock on the use and availability of consumer credit. First, we find that in counties severely affected by the pandemic, creditworthy borrowers reduce their credit card balances and credit card transactions, while the least creditworthy borrowers increase their outstanding balances. Second, while both local pandemic severity and non-pharmaceutical interventions (NPIs) have a significantly negative effect on credit use, the pandemic itself is the main driver. Third, we report a drastic reduction in credit card originations, which is more pronounced in counties affected by pandemic severity and in counties with more stringent NPIs. Finally, we find a reduction in the credit limits and an increase in the APR spreads of newly issued credit cards to the riskiest borrowers, which is consistent with a "flight-to-safety" response of banks to the COVID-19 shock.

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I. Introduction

The COVID-19 pandemic and the ensuing public health interventions have severely disrupted economic activity in the United States. There have been about 20 million job losses through the first week of April 2020 (Cajner et al., 2020b; Coibion et al., 2020b) and the U.S. real GDP has been forecasted to contract by 11 percent through 2020 (Baker et al., 2020b). This large economic shock has potentially important implications for the consumer credit market. Negative income shocks, rising unemployment, heightened economic uncertainty, and expected future wealth losses might trigger a reduction in consumption and therefore in the demand for consumer credit. At the same time, low-income and low-asset households may disproportionately rely on unsecured credit for consumption smoothing to offset unemployment-induced earnings losses (Sullivan, 2008; Herkenhoff, 2019). However, access to credit might be limited for these households if banks are reluctant to extend credit to risky borrowers in anticipation of rising default rates due to adverse economic conditions.

In this paper, we study the early impact of the COVID-19 shock on both the use and availability of credit in the U.S. consumer credit card market through March 2020. We investigate two potential, non-mutually exclusive, channels. First, changes in the use of credit could be driven by the effects of the pandemic itself, as measured by the local case severity across counties. If people are afraid to contract the virus, they go out less, shop less, and shelter at home—even in the absence of an official order. Accordingly, we use the term "pandemic itself" to refer not only to the direct health effects (people falling sick with the virus), but also to any voluntary changes in individual behavior in response to the outbreak ("fear of the virus"). Second, policy responses in the form of non-pharmaceutical interventions (NPIs) could also have adverse effects on local economic activity and might have similar, or potentially stronger, effects on the use of credit. Even more lenient NPIs, such as gathering size limitations or public venue closures, could disrupt local economies and therefore affect consumer credit markets. On the supply side, both the pandemic itself and the NPIs could have a negative effect on the availability of credit. If banks expect rising default rates, they might tighten lending standards, either by lowering credit limits or by raising interest rate spreads. These effects of the COVID-19 shock on the use and availability of consumer credit might be heterogeneous across borrower types. As low-income households (i.e., riskier borrowers) spend relatively more on non-discretionary expenses (such as food and utilities), it is potentially harder for them to reduce consumption and therefore their demand for consumer credit. At the same time, banks seeking to adjust their credit exposure in the wake of the COVID-19 crisis might disproportionately reduce their credit supply to riskier borrowers.

To investigate these questions, we construct a novel dataset from several sources. We obtain monthly account-level data on consumer credit card lending from the Federal Reserve's FR Y-14M reports, daily county-level data on the spread of the COVID-19 pandemic in the United States from the Johns Hopkins Coronavirus COVID-19 Global Cases Database, and daily county-level data on NPIs from the Coronavirus Intervention Dataset provided by Keystone Strategy. The granularity of the Y-14M dataset allows for a clean identification of the effects of the COVID-19 shock on credit market outcomes. The dataset contains, inter alia, account-level information on cardholders' ZIP codes and FICO scores, the card issuing bank, as well as the account origination and cycle-end dates. The latter two data items enable us to distinguish between cards that have a credit cycle end date (for existing cards) or are originated (for new cards) before March 15 and after March 15—that is, before and during the wide spread of the COVID-19 pandemic in the United States. Therefore, we can compare the credit market outcomes of borrowers in the *same county*, in the *same FICO bucket*, borrowing from the *same bank*, in a narrow time window around the COVID-19 outbreak. We measure changes in credit market outcomes by comparing February-to-March changes in outcomes between 2019 and 2020. By focusing on year-to-year changes in month-to-month changes, we control for both year and month fixed effects in our analysis.

Our main findings are as follows. First, we find a reduction in consumer credit use in response to the pandemic itself. Borrowers in severely affected counties exhibit, on average, a 5.4 percent reduction in balances and a 6.1 percent reduction in transactions compared to borrowers in unaffected counties. There is substantial heterogeneity in this response across borrower types. Reductions in both balances and transactions are driven by creditworthy borrowers, whereas we even find an increase in outstanding monthly balances for the riskiest borrowers in affected counties. Second, the effect of NPIs on the use of credit are qualitatively similar, though generally smaller in magnitude. Hence, we conclude that, at least in the early stages of the COVID-19 crisis, the pandemic itself was the main driver with regard to changes in consumer credit use. Third, we find a dramatic reduction in the origination of new credit cards in the second half of March. We find an overall

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drop of 48 percent in credit card originations, which is more pronounced in counties affected by the pandemic itself and in counties with more stringent NPIs. Fourth, while this collapse in credit card originations likely reflects both demand and supply effects, we provide evidence for a reduced availability of credit to riskier borrowers. We find a reduction in credit limits and an increase in APR spreads for new credit cards issued to the least creditworthy borrowers in affected counties.

Our findings that banks reduce credit availability to riskier borrowers following an adverse macroeconomic shock are consistent with both the theory (Bernanke, Gertler, and Gilchrist, 1996) and empirical evidence (Ramcharan et al., 2016; Benmelech et al., 2017; Di Maggio et al., 2017) of a "flight-to-safety" effect. Our findings on the use of credit warrant some more discussion. Both consumption theory (Carroll, 2001) and recent empirical evidence (Bunn et al., 2018; Fuster et al., 2018; Christelis et al., 2019) suggest that households with low levels of liquid wealth have a higher marginal propensity to consume (MPC) out of (negative) income shocks. By contrast, we find a stronger reduction in both credit card balances and transaction volumes for high-FICO borrowers. Since borrowers in higher FICO classes tend to have more liquid wealth than borrowers in lower FICO classes (Baker, 2018), consumption theory would predict a stronger negative response for low-FICO borrowers to the COVID-19 shock than for high-FICO borrowers. As we find the opposite effect, our results are not consistent with these explanations based on MPC heterogeneity. For this reason, we provide two alternative explanations for our findings. First, our results can be interpreted as a disruption of consumption patterns. The COVID-19 shock had heterogeneous effects on consumer spending across different categories of goods and services (Baker et al., 2020c). Discretionary spending categories (e.g., recreation, travel, and entertainment expenses) declined the most, while non-discretionary expenses (e.g., utilities, food, and childcare) declined only modestly (Coibion et al., 2020a).[1] As non-discretionary expenses likely make up a larger share of the consumption of less creditworthy borrowers, it is potentially harder for them to reduce spending and therefore their consumer credit use. Our findings corroborate evidence that a higher pre-shock spending share for "non-essential" goods and services is associated with a lager reduction in total spending in the wake of the COVID-19 pandemic (Andersen et al., 2020a). Second, our results

[1] We note that it is not always obvious which goods and services should be classified as "discretionary" and "non-discretionary". However, we adopt this terminology as well as the corresponding classification of expenditures from the existing literature (see e.g., Coibion et al. (2020a)). Some of the spending categories that are called discretionary are also substantially harder to consume during the pandemic.

are also consistent with recent findings on the heterogeneity in the consumer credit response to economic uncertainty shocks (Bloom, 2009). Di Maggio et al. (2017) find that local economic uncertainty is associated with an increase in the credit card balances of less creditworthy borrowers and with a decrease in the consumer credit demand of more creditworthy borrowers. The underlying mechanism is heterogeneity in the pecuniary costs of default between more and less creditworthy borrowers. Low-FICO borrowers with limited access to credit have a lower cost of default than high-FICO borrowers, which increases their incentives to engage in risk shifting (Guiso, Sapienza, and Zingales, 2013). Therefore, our empirical findings are also consistent with the interpretation of COVID-19 as an economic uncertainty shock (Baker et al., 2020b).

Our results must generally be interpreted as the effect on the consumer credit market during the early stages of the pandemic. By March 15 (our cut-off date for county-level affectedness), the overall effect of the COVID-19 pandemic was not yet as severe as in the following weeks. Importantly, while many counties had already imposed less stringent NPIs, such as large gathering bans and public venue closures, most had not yet imposed the most restrictive NPIs, such as stay-at-home orders and lockdowns. Hence, our results do not inform the discussion on more stringent public health interventions. At the same time, by mid-March the COVID-19 shock had already severely affected the U.S. economy. Stock markets had already dropped by 30 percent and jobless claims had already risen to all time highs through the end of March. Thus, focusing on a narrow time window between early and late March enables us to isolate the effect of the COVID-19 shock.

Our paper is related to several strands of literature. First, we add to the rapidly growing literature on the economic effects of the COVID-19 crisis. Previous papers have found that the pandemic strongly affected labor markets (Cajner et al., 2020b; Coibion et al., 2020b; Kahn et al., 2020), stock markets (Baker et al., 2020a), household expectations (Binder, 2020; Coibion et al., 2020a; Hanspal et al., 2020), economic uncertainty (Baker et al., 2020b), and overall economic activity (Lewis, Mertens, and Stock, 2020; Ludvigson, Ma, and Ng, 2020). More specifically, we contribute to the literature on the effects of the COVID-19 shock on consumer spending. Using transaction data from different sources, previous papers have studied the spending response in China (Chen, Qian, and Wen, 2020), Denmark (Andersen et al., 2020a), France (Bouniey, Camaraz, and Galbraith, 2020), Spain (Carvalho et al., 2020), the United Kingdom (Hacioglu, Känzig, and Surico, 2020; Chronopoulos, Lukas, and Wilson, 2020), and the United States (Baker et al., 2020c;

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Dunn et al., 2020). While most existing papers rely on data from individual banks or Fintech companies, our dataset encompasses the near-universe of accounts in the U.S. consumer credit card market. Moreover, our paper does not only investigate borrowers' spending response, but it also provides novel evidence on the availability of consumer credit in the wake of the COVID-19 shock. Finally, our paper is related to the empirical literature on households' consumption and debt response to negative income shocks. While most of this literature focuses on the marginal propensity to consume (MPC) out of positive income shocks, a number of recent papers investigate the asymmetric spending response to negative income shocks. Using survey data from the United Kingdom (Bunn et al., 2018), the United States (Fuster et al., 2018), and the Netherlands (Christelis et al., 2019), these papers find that households change their consumption significantly more in response to negative than in response to positive income shocks. Moreover, they provide evidence that households with low liquid wealth have a higher MPC out of negative income shocks, consistent with consumption theory (Deaton, 1991; Carroll, 2001; Kaplan, Violante, and Weidner, 2014). We contribute to this literature by investigating how the COVID-19 shock affects the credit use of different borrower categories.

The paper is structured as follows. Section II discusses our data sources and presents descriptive summary statistics. We discuss our methodology in Section III. Section IV presents the results for the use of credit, and Section V the results for the availability of credit. Robustness checks and further analyses are presented in Section VI. Section VII concludes.

II. Background and Data

A. The U.S. Consumer Credit Card Market

Credit cards are the largest unsecured consumer credit market in the United States. As of 2018, nearly 170 million Americans held (multiple) credit cards with a total outstanding balance of $900 billion and an average credit line of $8,200 per account. In the same year, consumers opened roughly 65 million new credit card accounts for a total credit line of $475 billon on new accounts (CFPB, 2019). While some consumers only use credit cards as payment instruments to facilitate small and medium consumption purchases, a significant share of consumers carry a balance from month to month and therefore use them as a source of credit. Moreover, consumers who pay off

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balances in full can benefit from the intra-month credit they provide.

We obtain data on consumer credit cards from Schedule D.1 of the Federal Reserve's FR Y-14M reports. The Y-14M data are collected by the Federal Reserve since June 2012 and require bank holding companies (BHCs) and intermediate holding companies (IHCs) of large foreign banking organizations with at least $100 billion in total consolidated assets to report detailed information on individual credit card accounts on a monthly basis. Our data contains information on 16 BHCs and IHCs, which cover a large portion of the market and account for 70 percent of outstanding balances on consumer credit cards as of year-end 2018 (CFPB, 2019). We study credit market outcome variables related to both the use and availability of credit. We therefore obtain monthly account-level data on cycle-end balances, transaction volumes, and utilization rates (use of credit), as well as data on credit limits and APRs (availability of credit).[2] We distinguish between existing credit cards and newly issued credit cards in our sample. In each month, existing cards are defined as cards which already existed in the previous month, while new cards are defined as accounts which have been originated in the given month.

To cleanly identify the effect of the COVID-19 shock, we further distinguish between existing cards with an account cycle-end date (and therefore a reporting date for information) early in the month (before the 15^{th}) and late in the month (after the 15^{th}). Similarly, for new cards, we distinguish between cards with an account origination date before and after the 15^{th}. While there were very few confirmed COVID-19 cases in the U.S. in early March, many counties were already severely affected by the pandemic by late March (as shown in Table II in Section II.B.1). Thus, differentiating between early- and late-month cards allows us to compare credit market outcomes in a narrow time window around the outbreak of the COVID-19 pandemic. Figure 1 shows the mobility patterns of individuals around the outbreak of the pandemic in the United States based on Google's COVID-19 community mobility reports (Google LLC, 2020). The figure illustrates that around mid-March people started to reduce their visits to places like restaurants, cafes, and shopping centers (Retail and Recreation in Panel A), transit stations (Panel B), their workplaces (Panel C), and instead started to stay home (Panel D). Thus, our March 15 cut-off date coincides with a significant change in individual behavior.

[2]We calculate APR spreads over the bank prime loan rate.

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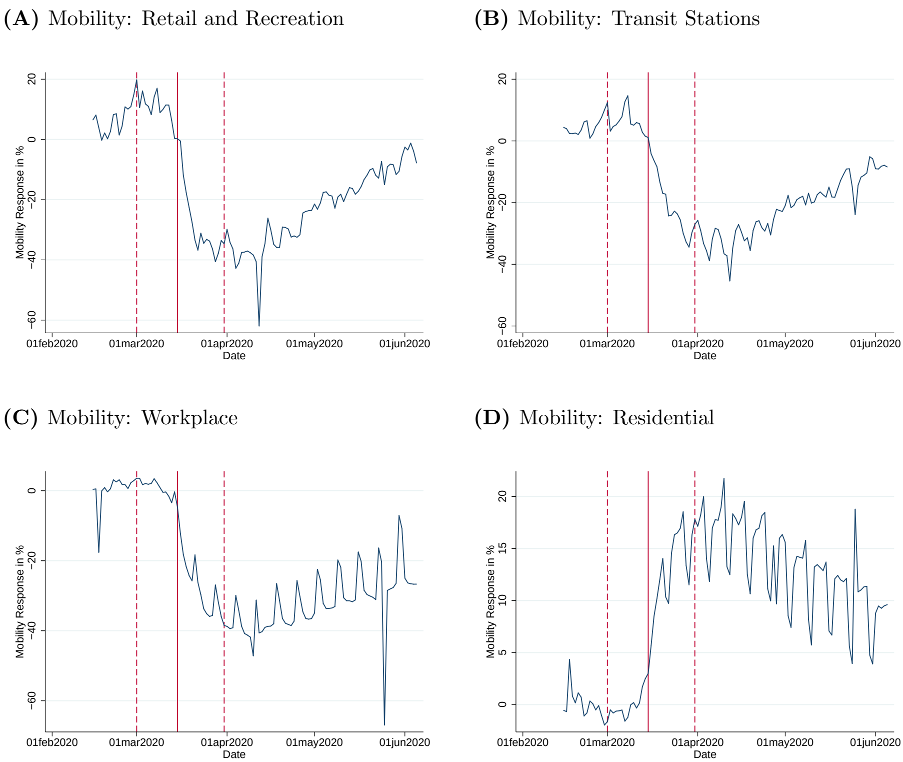

Figure 1. Mobility Response. This figure illustrates the mobility response of individuals around the outbreak of the COVID-19 pandemic based on Google's COVID-19 Community Mobility Reports (Google LLC, 2020) for the categories *Retail and Recreation*, *Transit Stations*, *Workplaces*, and *Residential*. The blue line in each panel shows the mean percent change in visits to places across counties. The solid vertical line marks March 15, our cut-off date for distinguishing between cards reporting information early and late in the month. The two dashed vertical lines mark March 1 and March 31. (Data Source: Google LLC (2020) and authors' own calculations.)

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We compare monthly changes (February to March) in credit market outcomes between 2019 and 2020. By focusing on year-to-year changes in month-to-month changes, we difference out both year and month fixed effects. Table I reports the summary statistics for the monthly changes in 2019 and 2020 for all variables, separately for cards with an account cycle-end date (for existing cards) or an account origination date (for new cards) before (Columns 1-4) and after (Columns 5-8) the 15^{th} of each month. Column 9 shows the differences in the changes between the two groups of cards. Panel A reports changes in outcomes associated with the use of credit for existing cards. Column 8 of Panel A shows that February to March balance growth rates are 3.60 percentage points (pp.) lower in 2020 than in 2019 for cards reporting information after the 15^{th} of each month, and that transaction growth rates are 8.95 pp. lower. As reported in Column 9 of Panel A, these annual differences in monthly changes are also significantly lower than those in Column 4 for cards reporting information before the 15^{th}. Moreover, we find significant but modest reductions in utilization rates.[3] Panel B reports the changes in credit market outcomes for newly issued cards. Columns 7 to 9 in the first row of Panel B document a dramatic collapse in the origination of new credit cards in the second half of March 2020. February to March origination growth rates were 48 percent lower in 2020 for cards originated after the 15^{th} of each month.[4] While we report the number of credit card originations under "availability of credit", these numbers likely reflect both demand and supply effects. The next two rows report the credit limits and APRs of newly issued cards, which are more closely linked to banks' credit supply. As can be seen in Column 9, we find that the credit limits of new cards increased and APRs remained unchanged. Thus, the aggregate time series evidence in Table I does not suggest that there was an overall reduction in the availability of consumer credit during our sample period. However, in Section V we provide evidence that these numbers likely reflect a reallocation of consumer credit from riskier to safer borrowers, consistent with a "flight-to-safety" mechanism.

[3]As we discuss in Section IV, the small magnitude of changes in utilization rates is due to the fact that balance reductions are driven by borrowers with high credit limits for which even large reductions in balances yield relatively small changes in utilization rates.

[4]As log differences are not a good approximation for large percentage changes, we calculate $e^{\frac{-65.39}{100}} - 1 = 48.00\%$.

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Table I
Summary Statistics: Changes in Credit Market Outcomes

This table reports summary statistics for the credit market outcome variables used in our paper. Panel A reports outcome variables related to the use of credit for existing cards, and Panel B outcome variables related to the availability of credit for new cards. Existing credit cards are defined as cards which existed in the previous month. New credit cards are defined as cards with an account origination date in the current month. We report 2019 and 2020 February to March monthly growth rates separately for cards issued before and after the 15^{th} of each month. The last column reports the differences in the differences in means. Log differences are indicated in percentage terms (multiplied by 100). All numbers are weighted by the number of credit cards as of 2020m1 per observational unit. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Before 15^{th}				After 15^{th}				
		Δ Feb-Mar				Δ Feb-Mar			
	Obs.	2019	2020	Δ Means	Obs.	2019	2020	Δ Means	ΔΔ Means
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Use of Credit (Existing Cards).									
Log Balances	196,888	−4.55	−4.23	0.32***	197,391	−7.03	−10.62	−3.60***	−3.92***
Log Transactions	196,891	−0.27	−5.82	−5.55***	197,397	2.38	−6.56	−8.95***	−3.39***
Utilization Rates	196,888	−0.82	−0.76	0.06***	197,391	−0.65	−0.67	−0.02	−0.09***
Panel B. Availability of Credit (New Cards).									
Log Number of Cards	59,947	−4.62	−3.44	1.18***	46,757	24.61	−39.60	−64.21***	−65.39***
Log Limits	58,593	−0.50	−2.81	−2.31***	45,659	−1.95	−0.73	1.22***	3.52***
APR Spreads	59,812	0.30	−0.25	−0.55***	46,565	0.27	−0.32	−0.59***	−0.04

B. The Geography of the COVID-19 Shock

B.1. The Spread of the COVID-19 Pandemic in the United States in March 2020

The first reported COVID-19 case in the United States occurred on January 22 in King County in Washington State. In the following weeks, the pandemic started to spread widely and rapidly across the United States. The COVID-19 outbreak was officially declared to be a pandemic by the WHO on March 11 (WHO, 2020) and the United States declared the outbreak to be a national emergency on March 13 (White House, 2020). While there were 30 confirmed cases by March 1, 2020, this number increased to 2,918 confirmed cases by March 15, and to 181,839 confirmed cases by March 31. There was considerable geographic heterogeneity in the speed and magnitude of the COVID-19 spread across regions in the U.S., with some counties facing the virus much earlier and much more severely than others.

We obtain daily county-level data on confirmed COVID-19 cases from the Johns Hopkins Coronavirus COVID-19 Global Cases GitHub repository (Dong, Du, and Gardner, 2020). For the United States, the database relies on the Centers for Disease Control and Prevention as well as on state-level health authorities. The database contains daily data on confirmed and death cases at the county level since January 22, 2020. Panel A of Table II provides county-level summary statistics on the spread of the COVID-19 pandemic across 3,131 counties in the United States in March 2020. The numbers illustrate both the rapid spread of the pandemic in March as well as the considerable cross-sectional heterogeneity across counties. While the average number of confirmed cases across counties was effectively zero on March 1, this number increased to 0.9 by March 15, and to 58 by March 31. We use the number of confirmed cases per 100,000 population as of March 15 to measure how severely a county was affected by the COVID-19 pandemic in March 2020.[5] By that date, there were already 377 affected counties with at least one confirmed case, 35 counties with more than 5 confirmed cases (per 100,000 population), and 12 severely affected counties with more than 15 confirmed cases (per 100,000 population) [6].

[5] It is reasonable to assume that people do not only react to COVID-19 cases in their own counties, but also to an outbreak of the pandemic in the surrounding area. In Section VI.A, we therefore provide a robustness check using an alternative measure of county-level case severity, which captures the severity of the COVID-19 outbreak in bordering counties. Our results are robust to this alternative specification.

[6] In the remainder of the paper, we adopt the following terminology: An *affected county* is a county with at least one confirmed case as of March 15, and a *severely affected county* is a county with at least 15 cases per 100,000 population as of March 15. For example, New York City had 16.5 and King County in Washington State had 17.2 cases per 100,000 population as of March 15.

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Table II
The COVID-19 Shock in the U.S. in March 2020

This table reports summary statistics related to the COVID-19 pandemic across 3,131 counties in the United States. Panel A reports summary statistics on the spread of confirmed COVID-19 cases and cases per 100,000 for the dates March 1, March 15, and March 31, 2020. These numbers are based on data from the the Johns Hopkins Coronavirus COVID-19 Global Cases database. Panel B reports summary statistics for the stringency of non-pharmaceutical interventions (NPIs) as of March 15, 2020. These numbers and indicators are based on daily county-level NPI data from Keystone Strategy. The construction of the indicator is described in Section II.B.2

	Mean	Med.	90%	95%	99%	Min	Max	SD
Panel A. The Spread of the COVID-19 Pandemic.								
Confirmed Cases:								
1-Mar	0.0	0	0	0	0	0	9	0.2
15-Mar	0.9	0	1	3	14	0	387	10.1
31-Mar	58.0	2	47	131	853	0	43 119	836.6
Confirmed Cases per 100,000:								
1-Mar	0.0	0.0	0.0	0.0	0.0	0.0	3.2	0.1
15-Mar	0.3	0.0	0.4	1.3	6.4	0.0	61.9	2.0
31-Mar	18.5	7.2	38.2	63.7	170.8	0.0	2647.4	66.4
Panel B. NPI Stringency (as of March 15).								
NPI Stringency Indicator	0.52	0	2	2	2	0	3	0.73
Closure of Public Venues	2.8 %					0	1	
Gathering Limitation: 0-10 People	0.0 %					0	0	
Gathering Limitation: 11-25 People	0.0 %					0	0	
Gathering Limitation: 26-100 People	10.3 %					0	1	
Gathering Limitation: 101-500 People	18.5 %					0	1	
Lockdown	0.0 %					0	0	
Non-Essential Services Closure	0.0 %					0	0	
Religious Gathering Bans	0.0 %					0	0	
Closure of Schools and Universities	13.6 %					0	1	
Shelter in Place	0.0 %					0	0	
Social Distancing	6.9 %					0	1	
Other	0.0 %					0	0	

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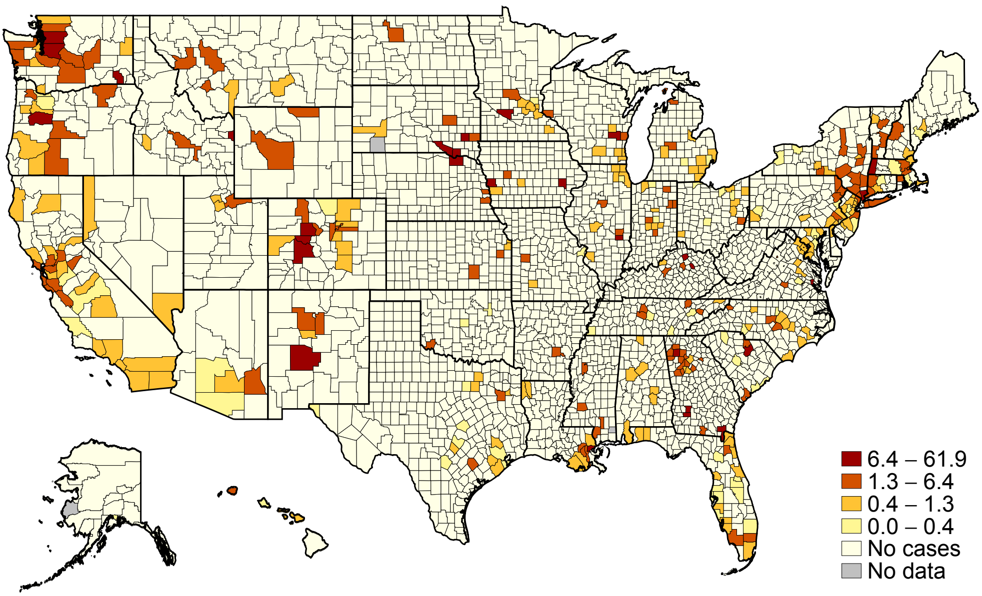

Figure 2. COVID-19 Cases per 100,000 as of March 15. This figure illustrates the number of COVID-19 cases per 100,000 population across US counties as of March 15, 2020. Cut-off values for the different colors were chosen to match the 50, 90, 95, and 99 percentile of COVID-19 cases per 100,000 in the data, respectively. (Data Source: Johns Hopkins Coronavirus COVID-19 Global Cases Database (Dong, Du, and Gardner, 2020) and and authors' own calculations.)

Thus, at our mid-month cut-off date, there is already a significant number of cases in many counties prior to possible credit market responses in the second half of the month. Figure 2 illustrates the considerable geographical heterogeneity at the county level in terms of COVID-19 case affectedness. While there are sizable clusters of affected counties in Washington State, New York State, and the Bay Area, which have received a lot of media attention, there are also strongly affected counties in other regions, such as Colorado, Georgia, and Florida. The map also illustrates that by mid-March many counties were not yet affected by the COVID-19 pandemic, with 2,754 out of 3,131 counties having reported zero confirmed cases as of March 15.

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B.2. Non-Pharmaceutical Interventions (NPIs)

Local economies are not only affected by the outbreak of the COVID-19 pandemic itself, but also by the ensuing policy responses in the form of NPIs. Many counties had already implemented social distancing measures by mid-March, such as bans on large gatherings and the closure of public venues, schools, and universities. As restrictive NPIs can have a strong negative effect on real economic activity and household spending behavior (Coibion, Gorodnichenko, and Weber, 2020b), they also have potentially severe consequences for consumer credit markets.

We obtain daily county-level data on NPIs from the Coronavirus Intervention Dataset made available by Keystone Strategy (Keystone, 2020). This dataset contains information on the start dates of 11 different NPIs for all 50 states and in detail for 350 counties: Non-essential services closure, shelter-in-place, closure of public venues, religious gathering bans, closure of schools and universities, social distancing, lockdowns, and gathering size limitations (0-10, 11-25, 26-100, and 101-500 people). We use these data to construct a simple county-level NPI stringency indicator as of March 15, 2020, by adding up the number of implemented NPIs within each county. Panel B of Table II provides summary statistics of this indicator and the individual NPIs.

While the majority of counties had not enacted any NPI measures in response to the COVID-19 pandemic as of mid-March, 1,209 counties had already imposed at least some restrictions; in particular gathering size limitations of more than 25 people and the closure of schools and universities. However, as of March 15, the most restrictive NPIs, such as shelter-in-place orders and lockdowns, were not yet implemented in any county. Our results so far, therefore do not inform the discussion on these most restrictive public health interventions. Figure 3 illustrates the geographical heterogeneity of our NPI stringency measure across U.S. counties. As the map shows, many counties inherited their NPI measures from state legislation and were therefore subject to a high degree of NPI stringency relative to their number of cases. The correlation coefficient between our measure of case severity (confirmed cases per 100,000) and our NPI stringency indicator is only 0.04. This allows us to disentangle the effect of the pandemic itself from the effect of NPIs.

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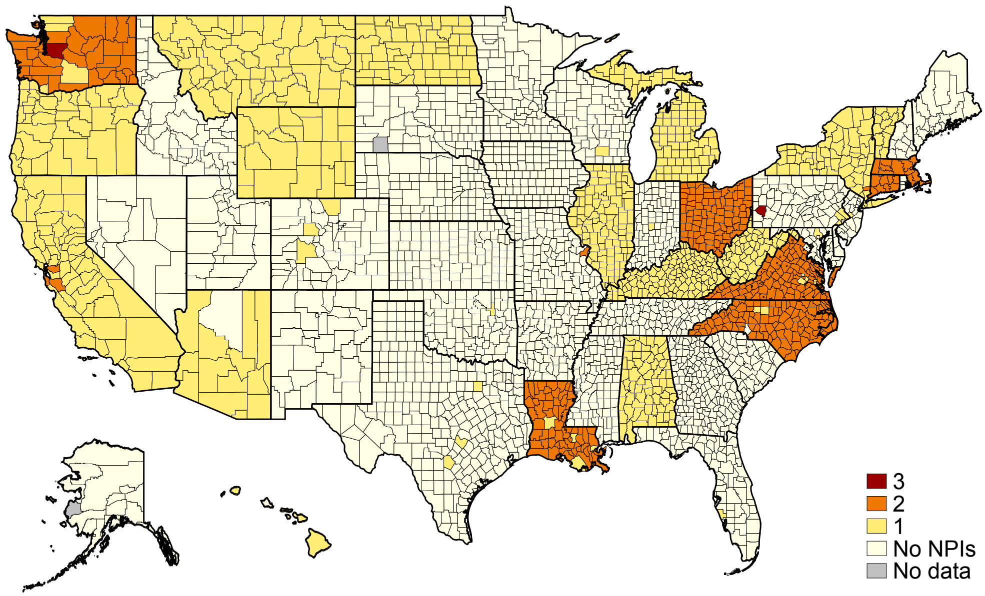

Figure 3. Non-Pharmaceutical Intervention Stringency (NPI) as of March 15. This figure illustrates the simple NPI stringency indicator as defined in SectionII.B across US counties as of March 15, 2020. (Data Source: Keystone Strategy Coronavirus Intervention Dataset (Keystone, 2020) and and authors' own calculations.)

C. Sample Construction

We collect monthly account-level data on consumer credit cards from Schedule D.1 of the Federal Reserve's FR Y-14M reports for January, February, and March in 2019 and 2020. We aggregate the data to the Month x County FIPS x Bank x FICO Bucket x *After15*-level, where *After15* is a dummy variable, which takes on the value of 1 if a card has an account cycle-end (for existing cards) or an account origination date (for new cards) after the 15^{th} of the month, and 0 otherwise.[7] We distinguish between five different FICO buckets: Below 580, 580-669, 670-739, 740-799, and 800-900. We focus on general purpose and private label (95% of all cards) unsecured (99%) consumer banks cards (93%) with a revolving feature (97%), for which the account is unclosed in the current month (88%). Furthermore, we exclude corporate cards (1%) and charged-off accounts (2%). This

[7]The Y-14M data are originally available at the 5-digit ZIP code level. We map ZIP codes into County FIPS codes using the mapping file provided by SimpleMaps (SimpleMaps, 2020).

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filtering process leaves us with a sample of about 400 million existing cards and 3 million new cards per month.

Next, we merge county-level data on COVID-19 cases from the Johns Hopkins Coronavirus COVID-19 Global Cases Database and county-level data on NPIs from the Coronavirus Intervention Dataset. Our final *existing cards data* is at the Month x County FIPS x Bank x FICO Bucket x *After15*-level and contains about 2.6 million observations, covering 3,131 counties, 16 banks, and 5 FICO buckets. Not all banks in our sample report card issuances in all months of our sample period (January, February, and March in 2019 and 2020), and we exclude these banks from our sample of newly issued cards. Our final *new cards data* contains about 1.04 million observations, covering 3,131 counties, 13 banks, and 5 FICO buckets.

III. Methodology

We study the effect of both the COVID-19 pandemic itself and NPIs on the use and availability of consumer credit by estimating the following regression specification:

$$y_{c,b,f,a} = \delta \times (\text{Affectedness}_c \times \text{After15}_a) + \gamma_c + \gamma_b + \gamma_f + \gamma_a + \varepsilon_{c,b,f,a} \tag{1}$$

where the outcome variable $y_{c,b,f,a}$ is defined as:

$$y_{c,b,f,a} = (Y_{c,b,f,a,2020,Mar} - Y_{c,b,f,a,2020,Feb}) - (Y_{c,b,f,a,2019,Mar} - Y_{c,b,f,a,2019,Feb}) \tag{2}$$

and thus as the year-to-year change in month-to-month changes in level outcomes Y (e.g log credit card balances), which allows us to difference out both year and month fixed effects. We compare changes in credit market outcomes for borrowers in county c, in FICO bucket f, borrowing from bank b. For our analysis of the use of credit, we focus on the sample of existing cards. In this analysis, index a refers to the dummy variable *After15*, which takes on the value of 1 if a card has an account cycle-end date after the 15^{th} of the month, and 0 otherwise; and our outcome variables of interest are changes in the logarithm of average credit card balances, in the logarithm of average credit card transactions (purchase volumes and cash advances), and in average credit card utilization rates. For our analysis of the availability of credit, we focus on the sample of newly

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issued cards, as it is likely easier for banks to tighten lending standards at the extensive margin (new cards) than at the intensive margin (existing cards). In this analysis, the dummy variable *After15* takes on the value of 1 if a card has an account origination date after the 15^{th} of the month, and 0 otherwise; and our outcome variables of interest are the changes in the logarithm of the number of credit card originations, in the logarithm of average credit limits, and in average APR spreads over the bank prime loan rate.

The variable $\textit{Affectedness}_c$ is either defined as $\textit{Case Severity}_c$ (as measured by cases per 100,000 population as of March 15) or as $\textit{NPI Stringency}_c$ (as measured by the NPI stringency indicator as defined in Section II.B.2). When including the former regressor, we estimate the effect of the pandemic itself and when including the latter regressor, we estimate the effect of NPIs on the use and availability of credit. Additionally, we include county fixed effects γ_c, bank fixed effects γ_b, FICO bucket fixed effects γ_f, and *After15* fixed effects γ_a. This specification allows us to compare changes in credit market outcomes for borrowers in the *same county*, in the *same FICO bucket*, borrowing from the *same bank*, in a narrow time window around the outbreak of the COVID-19 pandemic.

Changes in the use and availability of credit might differ substantially across different borrower types. The COVID-19 shock had heterogeneous effects on consumer spending across different categories of goods and services (Baker, Farrokhnia, Meyer, Pagel, and Yannelis, 2020c). Discretionary spending categories (e.g., recreation, travel, and entertainment expenses) declined the most, while non-discretionary expenses (e.g., utilities, food, and childcare) declined only modestly (Coibion, Gorodnichenko, and Weber, 2020a). As the latter category of expenses likely makes up a larger share of overall consumption for low-income households, they have potentially less leeway to reduce spending and therefore to reduce their use of consumer credit. Also changes in credit availability might differ substantially across different borrower types. Historically, during contractions, banks tend to reduce credit to the least creditworthy borrowers (Ramcharan et al., 2016; Benmelech et al., 2017; Di Maggio et al., 2017). To investigate this heterogeneity across borrower types, we estimate the following regression specification:

$$y_{c,b,f,a} = \sum_{f=1}^{F} (\delta_f \times \text{Affectedness}_c \times \text{After15}_a \times D_f) + \gamma_c + \gamma_b + \gamma_f + \gamma_a + \varepsilon_{c,b,f,a} \qquad (3)$$

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where we interact our regressor of interest $Affectedness_c \times After15_a$ with a battery of FICO bucket dummy variables D_f and where $f \in \{[0-579], [580-669], [670-739], [740-799], [800-900]\}$. We thus estimate a separate coefficient for each of our five FICO buckets.

Finally, we study whether the use and availability of credit during the early outbreak of the COVID-19 pandemic is more strongly affected by the pandemic itself or by the public health response in the form of NPIs. We include both regressors in a horse race regression and estimate the following regression specification:

$$y_{c,b,f,a} = \delta_1 \times (\text{Case Severity}_c \times \text{After15}_a) + \delta_2 \times (\text{NPI Stringency}_c \times \text{After15}_a) \\ + \gamma_c + \gamma_b + \gamma_f + \gamma_a + \varepsilon_{c,b,f,a} \quad (4)$$

The number of credit cards varies across the observational units in our sample (Month x County FIPS x Bank x FICO Bucket x After15). To ensure that our results are not driven by cells with few credit cards, we weight all regressions by the number of credit cards as of January 2020, the month immediately before our time period of interest.

IV. Use of Credit

A. *The Effect of the COVID-19 Pandemic on the Use of Credit*

Table III reports the estimation results of Equations (1) and (3) for the effect of the COVID-19 pandemic on changes in credit card balances, transaction volumes, and utilization rates. Column 1 reports the simple effect of county-level case severity on balances, and Column 2 the results interacted with the dummy variable *After15* for cards with an account cycle-end date after the 15^{th} of the month. The latter specification allows us to identify changes in credit market outcomes in a narrow time window around the outbreak of the COVID-19 pandemic between early and late March. Column 2 shows that a one-case increase per 100,000 population is associated with a 0.36 pp. reduction in credit card balance growth. The economic magnitude of this effect is substantial. It implies a 5.4 pp. reduction in the relative growth rates of balances between a severely affected county and a county with no cases as of March 15. As we are investigating a one-time change in growth rates, this result can also be interpreted as a 5.4 percent reduction in the level of credit card

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balances between a severely affected county and the counterfactual scenario where that county is unaffected.[8] Our results also imply large aggregate effects. In the average severely affected county, borrowers had $1.37 billion in total outstanding credit card balances as of end of February 2020. Our results thus imply a $73.7 million reduction in outstanding balances in the average severely affected county.

This overall response, however, masks important differences across borrower types. Column 3 reports the results for Equation (3), which separately estimates the effect for each of the five FICO buckets. The results show that the reduction in balances in response to the early COVID-19 outbreak is exclusively driven by the most creditworthy borrowers in the upper two FICO buckets (scores above 740). The coefficients imply a 8.8 percent reduction in balances for borrowers in the highest FICO class in severely affected counties relative to the counterfactual of having no cases. On the other end of the distribution, balances even increased by 2.4 percent between unaffected and severely affected counties for borrowers in the lowest FICO class (scores below 580). Figure 4 illustrates this heterogeneous response of credit card balances to the COVID-19 pandemic across different borrower types.

Columns 4 to 6 of Table III report the results for credit card transaction volumes. We find a similar overall effect. The coefficient in Column 4 implies a 6.1 percent reduction in monthly transactions between unaffected and severely affected counties. Again, our results also imply large aggregate effects. In the average severely affected county, the total transaction volume was $557 million in February 2020. A 6.1 percent reduction therefore implies a $34 million reduction in the aggregate credit card transaction volume in the average severely affected county. Also the reduction in transactions is driven by borrowers in the upper FICO classes. The estimated coefficients for the lower FICO classes are negative, but statistically insignificant and considerably smaller in magnitude. Figure 5 illustrates this heterogeneous response of credit card transactions to the COVID-19 pandemic across different borrower types.

[8] In Appendix VII, we explain how our differential growth results can be interpreted in terms of counterfactual level changes.

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Table III
The Effect of the COVID-19 Pandemic on the Use of Consumer Credit (Existing Cards)

This table presents the estimation results for the effect of COVID-19 case severity on the use of credit from Equations (1) and (3) in Section III. All outcome variables are defined as year-to-year changes (2019 to 2020) in month-to-month changes (February to March). The variable *Cases Severity*$_c$ measures county-level COVID-19 affectedness via the number of confirmed cases per 100,000 population as of March 15. *After15* is a dummy variable which takes on the value of 1 if a card has an account cycle-end date after the 15th of the month, and 0 otherwise. The variables $FICO_1$ through $FICO_5$ are indicator dummy variables for the five different FICO buckets: Below 580 ($FICO_1$), 580-669 ($FICO_2$), 670-739 ($FICO_3$), 740-799 ($FICO_4$), and 800-900 ($FICO_5$). All regressions are weighted by the number of credit cards as of 2020m1 per observational unit. Standard errors are clustered at the county level and Bank × FICO Bucket-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Log Balances			Log Transactions			Utilization Rates		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Case Severity$_c$	−0.15***			−0.31***			−0.01***		
	(0.04)			(0.05)			(0.00)		
Case Severity$_c$ × After15$_a$		−0.36***			−0.40***			−0.02***	
		(0.06)			(0.13)			(0.00)	
Case Severity$_c$ × After15$_a$ × FICO$_1$			0.18***			−0.12			0.00
			(0.06)			(0.15)			(0.01)
Case Severity$_c$ × After15$_a$ × FICO$_2$			0.16**			−0.30			0.01
			(0.07)			(0.19)			(0.01)
Case Severity$_c$ × After15$_a$ × FICO$_3$			−0.08			−0.37**			−0.01**
			(0.07)			(0.14)			(0.01)
Case Severity$_c$ × After15$_a$ × FICO$_4$			−0.45***			−0.45***			−0.03***
			(0.09)			(0.14)			(0.01)
Case Severity$_c$ × After15$_a$ × FICO$_5$			−0.60***			−0.43**			−0.02***
			(0.11)			(0.19)			(0.01)
County FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FICO FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
After15 FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.14	0.15	0.16	0.39	0.40	0.40	0.06	0.06	0.06
Observations	381,151	381,151	381,151	381,151	381,151	381,151	381,151	381,151	381,151

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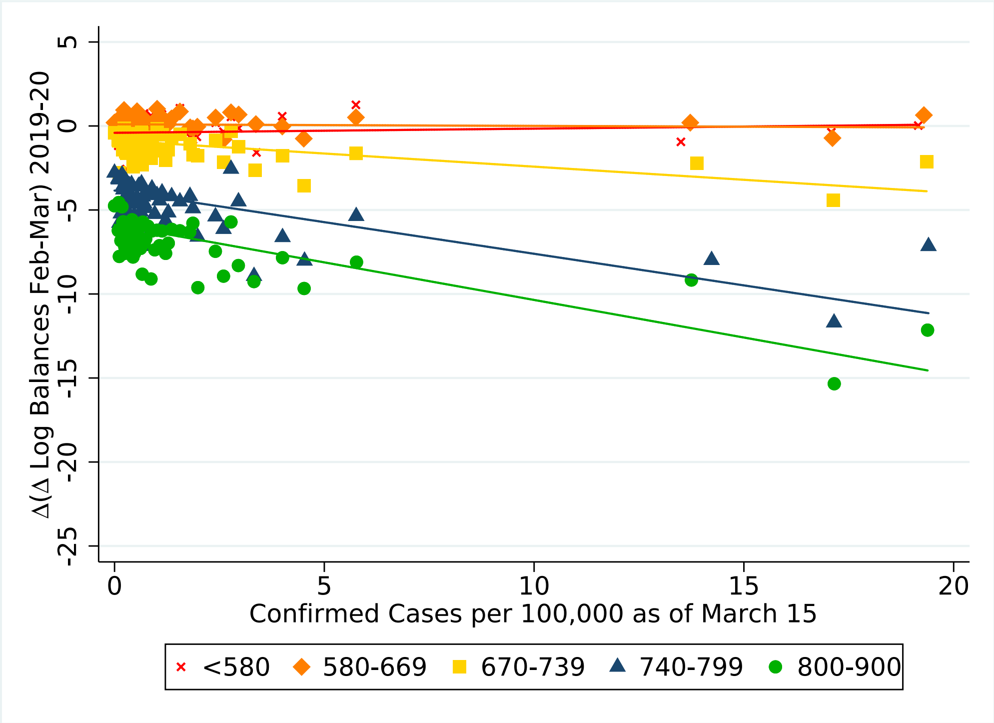

Figure 4. The Effect of Case Severity on Credit Card Balances Across FICO Classes. This figure illustrates the heterogeneous effect of COVID-19 case severity on credit card balances across different borrower classes. The graph is a binned scatter plot weighted by the number of cards as of 2020m1 per cluster. The different symbols mark the five different FICO buckets, and the corresponding weighted regression lines the relationship between county-level COVID-19 case severity and credit card balances within each FICO bucket. To maintain the confidentiality of the data, we create 100 equal-sized county bins sorted by their COVID-19 case severity as of March 15. (Data Source: Federal Reserve's Y-14M reports and authors' own calculations.)

Figure 6 shows a heatmap of the United States for changes in credit card transactions. The map illustrates that transaction volumes decreased the most in Washington State, California, the region around New York City, and Southern Florida, while transaction volumes remained largely constant in the Midwest. This pattern lines up with the geographical distribution of COVID-19 case severity shown in Figure 2.

Finally, Columns 7 to 9 of Table III report the results for credit card utilization rates, which are defined as an account's cycle-end balance relative to its credit limit. We do find a significant

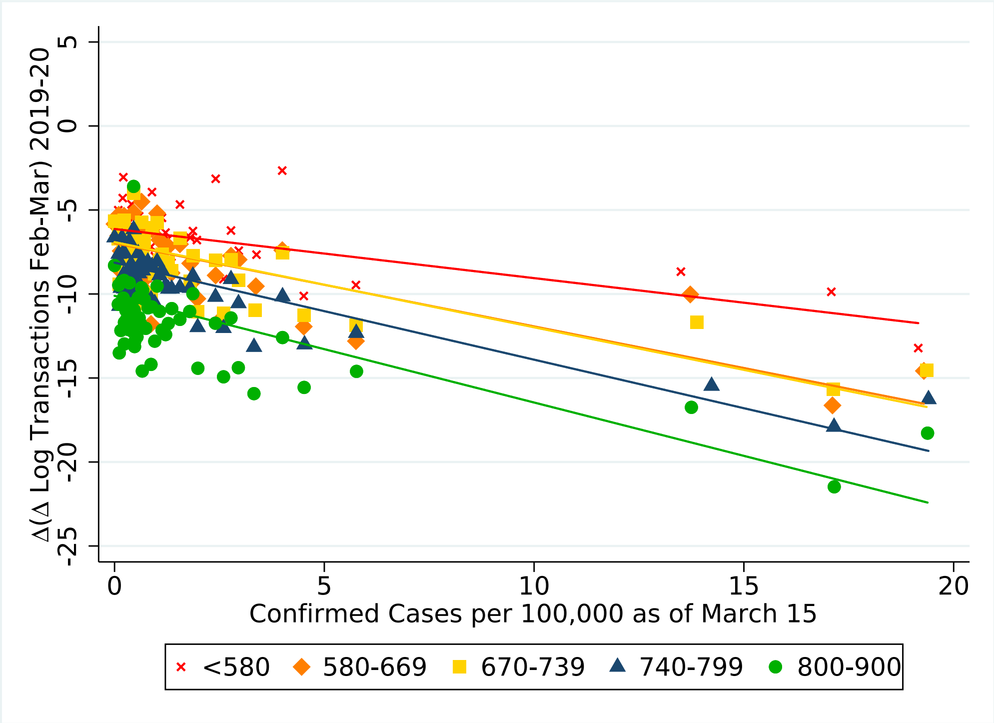

Figure 5. The Effect of Case Severity on Credit Card Transaction Volume Across FICO Classes. This figure illustrates the heterogeneous effect of COVID-19 case severity on credit card transactions across different borrower classes. The graph is a binned scatter plot weighted by the number of cards as of 2020m1 per cluster. The different symbols mark the five different FICO buckets, and the corresponding weighted regression lines the relationship between county-level COVID-19 case severity and credit card transactions within each FICO bucket. To maintain the confidentiality of the data, we create 100 equal-sized county bins sorted by their COVID-19 case severity as of March 15. (Data Source: Federal Reserve's Y-14M reports and authors' own calculations.)

negative effect of the COVID-19 pandemic on credit card utilization, again driven by creditworthy borrowers. The magnitudes of the coefficients are, however, modest. The difference in utilization rates between unaffected and severely affected counties is 0.2 pp. The reason why we find large effects on balances but small effects on utilization rates is that our results are driven by borrowers in the higher FICO classes, which generally have high credit limits and low utilization rates. Borrowers in the highest FICO class have an average utilization rate of 5.8 percent, while borrowers in the

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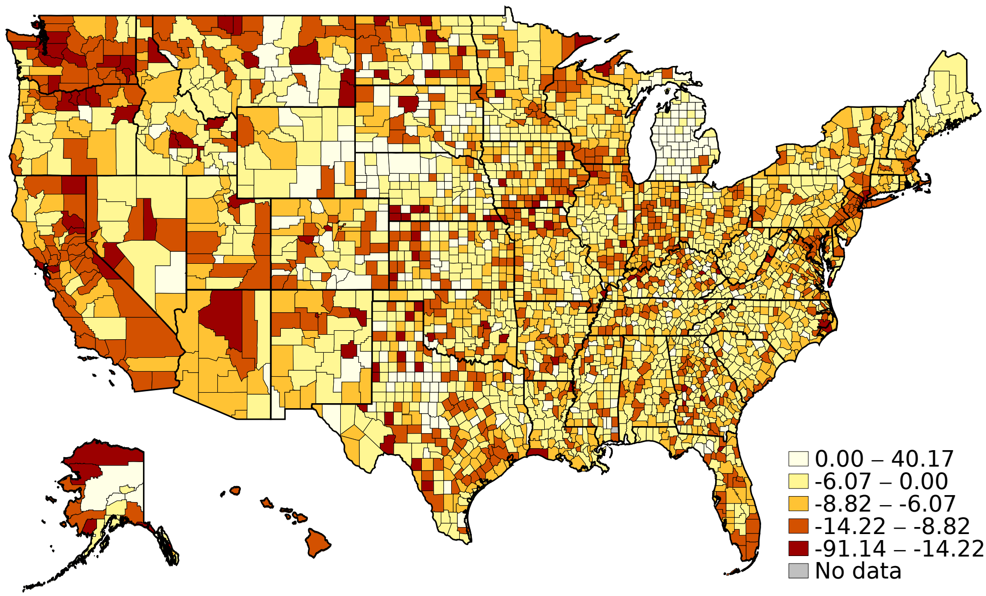

Figure 6. The Geographical Distribution of Changes in Credit Card Transaction Volumes. This figure illustrates changes in credit card transaction volumes across counties in the United States. The plotted variable is the year-to-year change (2019 to 2020) in month-to-month changes (February to March) in credit card transactions, our outcome variable in Columns (4) to (6) in Table III. Cut-off values for different colors were chosen to match the 5, 25, 50, and 90 percentile of the distribution, respectively. (Data Source: Federal Reserve's Y-14M reports and authors' own calculations.)

lowest FICO class have an average utilization rate of 78.9 percent (as of January 2020). Thus, for the most creditworthy borrowers even a large reduction in balances translates into only a relatively small reduction in utilization rates.

These results raise questions in the light of existing findings on consumers' response to negative economic shocks. Consumption theory (Carroll, 2001) predicts and recent empirical evidence (Bunn et al., 2018; Fuster et al., 2018; Christelis et al., 2019) finds a higher marginal propensity to consume (MPC) out of negative income shocks for households with low levels of liquid wealth. While we can neither directly observe income shocks nor cash-on-hand in our data, borrowers in lower FICO classes tend to have lower levels of liquid wealth (Baker, 2018). Moreover, there is evidence that COVID-19 income shocks disproportionately affected low-wage occupations (Cajner et al., 2020a)

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and hence borrowers in lower FICO classes.[9] Taken together, we would expect to see a stronger negative consumption response for low FICO classes. However, we find the opposite. While our results are at odds with classical explanations based on MPC heterogeneity, they are consistent with alternative explanations.

First, our results can be interpreted as a disruption of spending patterns. There is evidence that the COVID-19 shock had heterogeneous effects on consumer spending across different categories of goods and services in the U.S. (Baker et al., 2020c). Coibion et al. (2020a) find that discretionary spending categories (e.g., recreation, travel, and entertainment expenses) declined the most, while non-discretionary expenses (e.g., utilities, food, and childcare) declined only modestly. Similarly, JPMorgan (2020) reports that spending in non-essential categories (e.g., retail, restaurants, and entertainment) declined significantly more than spending for essential categories (e.g., drug stores, groceries, and utilities). As the latter category of expenses likely makes up a larger share of overall consumption for less creditworthy borrowers, they have potentially less leeway to reduce spending and therefore to reduce their use of consumer credit card debt. Thus, our findings are consistent with evidence that a higher pre-shock spending share for non-essential goods and services is associated with a lager reduction in total spending in the wake of the COVID-19 pandemic (Andersen et al., 2020a). In Section VI.B, we provide further evidence for this interpretation of our results.

Second, our results are also consistent with the interpretation of consumer credit responses to COVID-19 as an economic uncertainty shock (Baker et al., 2020b). Di Maggio et al. (2017) find that local economic uncertainty is associated with an increase in the credit card balances of less creditworthy borrowers and with a decrease in the consumer credit demand of more creditworthy borrowers. The underlying mechanism is heterogeneity in the pecuniary costs of default between more and less creditworthy borrowers. Low-FICO borrowers with limited access to credit have a lower cost of default than high-FICO borrowers, which increases their incentives to engage in risk shifting (Guiso, Sapienza, and Zingales, 2013). In contrast, high-FICO borrowers with a higher cost of default respond to increased uncertainty by targeting greater financial flexibility to protect their credit reputation and future credit access. While we do not provide direct evidence for this mechanism, our results are also consistent with this alternative explanation.

[9] In our dataset, the median annual income of borrowers (individual or household) across FICO buckets is \$53,999 (FICO below 580), \$69,454 (580-669), \$82,407 (670-739), \$90,243 (740-799), and \$104,516 (800-900).

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We next compare our results on the use of credit to other studies investigating the effect of the COVID-19 shock on consumer spending in the United States. While we report an implied 6.1 percent reduction in monthly credit card transactions, existing studies using daily transaction data find a reduction in overall spending ranging from 28 percent (Dunn, Hood, and Driessen, 2020), to 31 percent (Coibion, Gorodnichenko, and Weber, 2020a), to 40 percent (JPMorgan, 2020). Compared to these findings, our results are of modest magnitude and deserve further explanation. First, our coefficient of interest identifies the differential effect on credit card transactions between unaffected and severely affected counties. However, consumer spending also dropped significantly in counties without any confirmed cases. The (unreported) intercept of our transaction regression in Table III is -7.1, which can also be seen in Figure 5. Thus, while we find a stronger effect for counties with higher pandemic severity, the COVID-19 shock was also a national phenomenon with a severe economic impact in counties not directly affected by the outbreak (Kahn, Lange, and Wiczer, 2020). Second, our credit card data does not exclusively capture the period after the wide spread of the COVID-19 pandemic in the U.S. in the second half of March. We distinguish between existing cards with an account cycle-end date (and therefore a reporting date for information) early in the month (before the 15^{th}) and late in the month (after the 15^{th}). However, even for cards with an account cycle-end date after the 15^{th}, the data reflect information over the previous four weeks and therefore partially for a time horizon prior to the outbreak. This explains why our results are of smaller magnitude compared to papers which use daily transaction data.

B. The Effect of Non-Pharmaceutical Interventions (NPIs) on the Use of Credit

The use of consumer credit might not only be affected by the COVID-19 pandemic itself, but also by the policy response in the form of NPIs. Government-imposed social distancing measures, such as gathering bans and the closure of public venues, schools, and universities can have a significant effect on local economies and therefore on credit market outcomes. Table IV reports the estimation results of Equations (1) and (3) for the effect of NPIs (as measured by the NPI stringency indicator described in Section II.B.2) on our measures for the use of credit. Columns 1 to 3 of Table IV report the results for credit card balances. Similar to the effect of the pandemic itself, we find a significant negative effect of NPIs on balances, driven by borrowers in the highest FICO classes. The adoption of an additional NPI is overall associated with a 0.73 percent reduction in balances and with significantly lower credit card utilization. Unlike for the effect of the pandemic itself, we find, however, no significant effect of NPIs on credit card transactions (Columns 4 to 6) and again only a modest effect on credit card utilization (Columns 7 to 9).

C. The Pandemic or NPIs: What Matters More For the Use of Credit?

We next study whether the use of credit during the early outbreak of the COVID-19 pandemic is more strongly affected by the pandemic itself or by the policy response in the form of NPIs. Most counties which had enacted NPIs by mid-March had inherited them from state-wide legislation, even in the absence of confirmed cases. The correlation coefficient between our measure of case severity (confirmed cases per 100,000) and our NPI stringency indicator is only 0.04. This allows us to disentangle the effect of the pandemic itself from the effect of NPIs.

Table V presents the estimation results of Equation (4). To compare magnitudes across coefficients within each regression, we report standardized regression coefficients, which indicate how many standard deviations the dependent variable changes for a one-standard deviation change in the independent variable. The first column shows that both the pandemic itself and NPIs have a negative effect on balances. However, the magnitude of the standardized coefficient for case severity is three times as high as the corresponding coefficient for NPI stringency. The second column shows that the pandemic itself has a negative effect on credit card transactions, but that NPIs have not. Moreover, we find a four times stronger effect of the pandemic itself on credit card utilization rates as measured by the standardized regression coefficient.

These findings are consistent with existing evidence that the COVID-19 outbreak had strong and adverse economic effects, even in the absence of government-mandated restrictions (Andersen et al., 2020b; Aum et al., 2020; Kahn et al., 2020). We conclude that, at least in the early stages of the COVID-19 crisis, the local severity of the pandemic was the key driver for changes in households' use of credit, rather than government restrictions.

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Table IV
The Effect of NPIs on the Use of Consumer Credit (Existing Cards)

This table presents the estimation results for the effect of non-pharmaceutical interventions (NPIs) on the use of credit from Equations (1) and (3) in Section III. All outcome variables are defined as year-to-year changes (2019 to 2020) in month-to-month changes (February to March). The variable $NPI\ Stringency_c$ is the county-level NPI stringency indicator as defined in Section II.B.2. *After15* is a dummy variable which takes on the value of 1 if a card has an account cycle-end date after the 15^{th} of the month, and 0 otherwise. The variables $FICO_1$ through $FICO_5$ are indicator dummy variables for the five different FICO buckets: Below 580 ($FICO_1$), 580-669 ($FICO_2$), 670-739 ($FICO_3$), 740-799 ($FICO_4$), and 800-900 ($FICO_5$). All regressions are weighted by the number of credit cards as of 2020m1 per observational unit. Standard errors are clustered at the county level and Bank × FICO Bucket-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Log Balances			Log Transactions			Utilization Rates		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
NPI Stringency$_c$	-0.38^{***}			-0.49^{**}			-0.02^{***}		
	(0.13)			(0.22)			(0.01)		
NPI Stringency$_c$ × After15$_a$		-0.73^{***}			-0.44			-0.03^{**}	
		(0.25)			(0.36)			(0.01)	
NPI Stringency$_c$ × After15$_a$ × FICO$_1$			1.07^{***}			0.22			-0.03
			(0.26)			(0.60)			(0.04)
NPI Stringency$_c$ × After15$_a$ × FICO$_2$			1.09^{***}			0.06			0.02
			(0.25)			(0.82)			(0.03)
NPI Stringency$_c$ × After15$_a$ × FICO$_3$			0.39			-0.36			0.00
			(0.31)			(0.67)			(0.03)
NPI Stringency$_c$ × After15$_a$ × FICO$_4$			-1.19^{**}			-0.74			-0.07^{**}
			(0.47)			(0.70)			(0.03)
NPI Stringency$_c$ × After15$_a$ × FICO$_5$			-1.70^{***}			-0.48			-0.03
			(0.41)			(0.79)			(0.02)
County FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FICO FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
After15 FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.14	0.16	0.16	0.39	0.40	0.40	0.06	0.07	0.07
Observations	381,144	381,144	381,144	381,151	381,151	381,151	381,144	381,144	381,144

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Table V
Case Severity or NPIs: What Drives the Use of Credit?

This table presents the estimation results for the horse race regression in Equation (4) in Section III. All outcome variables are defined as year-to-year changes (2019 to 2020) in month-to-month changes (February to March). To compare magnitudes across coefficients within each regression, column 2,4, and 6 report the standardized coefficient, which indicates how many standard deviations the dependent variable changes for a one-standard deviation change in the independent variable. The variable $\textit{Cases Severity}_c$ measures county-level COVID-19 affectedness via the number of confirmed cases per 100,000 population as of March 15. The variable $\textit{NPI Stringency}_c$ is the county-level NPI stringency indicator as defined in Section II.B.2. *After15* is a dummy variable which takes on the value of 1 if a card has an account cycle-end date after the 15^{th} of the month, and 0 otherwise. All regressions are weighted by the number of credit cards as of 2020m1 per observational unit. Standard errors are clustered at the county level and Bank × FICO Bucket-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Log Balances		Log Transactions		Utilization Rates	
	(1)	(2)	(3)	(4)	(5)	(6)
Case Severity$_c$ × After15$_a$	-0.33^{***}	-0.08	-0.40^{**}	-0.06	-0.02^{***}	-0.04
	(0.05)		(0.12)		(0.00)	
NPI Stringency$_c$ × After15$_a$	-0.41^{***}	-0.03	-0.05	0.00	-0.01^{*}	-0.01
	(0.12)		(0.19)		(0.01)	
County FE	Yes		Yes		Yes	
Bank FE	Yes		Yes		Yes	
FICO FE	Yes		Yes		Yes	
After15 FE	Yes		Yes		Yes	
Adjusted R^2	0.16		0.41		0.07	
Observations	381,144		381,151		381,144	

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V. Availability of Credit

A. *The Effect of the COVID-19 Pandemic on the Availability of Credit*

Table VI reports the estimation results of Equations (1) and (3) for the effect of the COVID-19 pandemic on credit card originations, average credit limits, and average APR spreads of newly issued cards. Column 1 shows the simple effect of county-level case severity on credit card originations, which is significantly negative. A one case increase per 100,000 population is associated with a 0.94 percent reduction in credit card originations. This implies a 14 percent reduction in the issuances of new credit cards between an unaffected county and a severely affected county with 15 cases per 100,000 population. While we find no significant overall effect for the interaction with the dummy variable *After15*, we find significant effects for different FICO buckets. The number of credit card originations significantly decreases for borrowers both in the highest and in the lowest FICO class. For the most (least) creditworthy borrowers, the coefficients imply a 23.5 (37.9) percent reduction in originations in severely affected relative to unaffected counties.[10] This reduction in credit card originations at both ends of the FICO distribution could reflect both demand and supply effects respectively, similar to the mechanism in Agarwal et al. (2018). While low-FICO borrowers might have a high marginal propensity to borrow (MPB), consistent with our result for the use of credit, banks might have a low marginal propensity to lend (MPL) to these borrowers, consistent with a "flight-to-safety" effect. Thus, at the lower end of the FICO distribution, the reduction in credit card originations is likely primarily driven by supply effects. Conversely, while banks might have a high MPL to high-FICO borrowers, these borrowers might have a low MPB, again consistent with our use of credit results. Thus, at the higher end of the FICO distribution, the reduction in credit card originations is likely primarily driven by demand effects. Our further results provide corroborating evidence for this interpretation. Columns 4 to 6 of Table VI report the results for changes in the average credit limit of newly issued cards. While there are no significant overall effects, we find a reduction in the credit limits of newly issued cards to borrowers in the lowest FICO class. For these borrowers, a one case increase per 100,000 population is associated with a 0.32 percent reduction in the average credit limit.

[10]These very large magnitudes are in line with anecdotal evidence from the financial industry. As reported by the *American Banker*, "card originations were down 55% during the first two weeks of April compared with average February levels" (Wack, 2020).

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Table VI
The Effect of the COVID-19 Pandemic on the Availability of Consumer Credit (New Cards)

This table presents the estimation results for the effect of COVID-19 case severity on the availability of credit from Equations (1) and (3) in Section III. All outcome variables are defined as year-to-year changes (2019 to 2020) in month-to-month changes (February to March). The variable *Cases Severity*$_c$ measures county-level COVID-19 affectedness via the number of confirmed cases per 100,000 population as of March 15. *After15* is a dummy variable which takes on the value of 1 if a card has an account origination date after the 15th of the month, and 0 otherwise. The variables $FICO_1$ through $FICO_5$ are indicator dummy variables for the five different FICO buckets: Below 580 ($FICO_1$), 580-669 ($FICO_2$), 670-739 ($FICO_3$), 740-799 ($FICO_4$), and 800-900 ($FICO_5$). All regressions are weighted by the number of credit cards as of 2020m1 per observational unit. Standard errors are clustered at the county level and Bank × FICO Bucket-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Log No. Cards			Log Limits			APR Spreads		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Case Severity	−0.94***			−0.02			−0.03***		
	(0.17)			(0.07)			(0.01)		
Case Severity$_c$ × After15$_a$		−0.65			−0.06			−0.06***	
		(0.40)			(0.12)			(0.01)	
Case Severity$_c$ × After15$_a$ × FICO$_1$			−1.56***			−0.32**			0.12***
			(0.57)			(0.14)			(0.04)
Case Severity$_c$ × After15$_a$ × FICO$_2$			0.09			−0.08			0.04
			(0.57)			(0.41)			(0.04)
Case Severity$_c$ × After15$_a$ × FICO$_3$			0.46			−0.16			−0.06
			(0.52)			(0.27)			(0.05)
Case Severity$_c$ × After15$_a$ × FICO$_4$			−0.84			0.00			−0.08*
			(0.55)			(0.35)			(0.05)
Case Severity$_c$ × After15$_a$ × FICO$_5$			−2.53***			0.01			−0.11**
			(0.86)			(0.24)			(0.05)
County FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FICO FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
After15 FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.52	0.53	0.53	0.05	0.06	0.06	0.03	0.04	0.04
Observations	105,852	105,760	105,760	103,400	103,308	103,308	105,525	105,433	105,433

This implies a reduction of about 4.8 percent between an unaffected county and a severely affected county. Columns 7 to 9 of Table VI report the results for changes in the APR spreads of newly issued cards. We find a significantly negative overall effect which does not suggest that there was an overall reduction in the availability of consumer credit over our sample period. However, the effect on APR spreads is again not homogenous across borrowers types. As shown in Column 9, we find a substantial and large increase in the APR spreads for borrowers in the lowest FICO class and a reduction in APR spreads for borrowers in the highest FICO class. For the least creditworthy borrowers, the coefficient implies a 1.9 pp. increase in APR spreads in a severely affected county relative to the counterfactual of an unaffected county, while for the most creditworthy borrowers the coefficient implies a 1.7 pp. reduction. This heterogeneity in the availability of credit across borrower types suggests a "flight-to-safety" effect in consumer credit lending in response the COVID-19 shock. This is also consistent with banks' lending response to COVID-19 as an economic uncertainty shock (Di Maggio et al., 2017).

B. The Effect of Non-Pharmaceutical Interventions (NPIs) on the Availability of Credit

Table VII reports the estimation results of Equations (1) and (3) for the effect of NPIs on our measures for the availability of credit. As for the effect of the pandemic itself, we find very large effects of NPIs on credit card originations. The coefficient in Column 2 shows that the adoption of an additional NPI is associated with 3.9 percent reduction in originations. Again, these results are driven by borrowers in the lowest and in the highest FICO class. The coefficients in Column 3 imply that credit card originations decreased by 28.7 (40.6) percent for the least (most) creditworthy borrowers in counties with the highest values of NPI stringency (3) compared to counties without any NPIs.

Columns 4 to 6 of Table VII report the results for changes in the average credit limit of newly issued cards. We find again no overall effect and also no significant effects for individual FICO classes. Columns 7 to 9 of Table VI report the results for changes in the APR spreads of newly issued cards. Similar to the results in Table VI, we find an overall reduction in APR spreads for cards issued in counties with more stringent NPIs, but an increase in APR spreads for the least creditworthy borrowers. The results in Table VII are again consistent with a "flight-to-safety" effect in consumer credit lending in response to government-mandated restrictions.

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Table VII
The Effect of NPIs on the Availability of Consumer Credit (New Cards)

This table presents the estimation results for the effect of non-pharmaceutical interventions (NPIs) on the availability of credit from Equations (1) and (3) in Section III. All outcome variables are defined as year-to-year changes (2019 to 2020) in month-to-month changes (February to March). The variable *NPI Stringency*$_c$ is the county-level NPI stringency indicator as defined in Section II.B.2. *After15* is a dummy variable which takes on the value of 1 if a card has an account origination date after the 15th of the month, and 0 otherwise. The variables $FICO_1$ through $FICO_5$ are indicator dummy variables for the five different FICO buckets: Below 580 ($FICO_1$), 580-669 ($FICO_2$), 670-739 ($FICO_3$), 740-799 ($FICO_4$), and 800-900 ($FICO_5$). All regressions are weighted by the number of credit cards as of 2020m1 per observational unit. Standard errors are clustered at the county level and Bank × FICO Bucket-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Log No. Cards			Log Limits			APR Spreads		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
NPI Stringency$_c$	−2.16***			−0.32			−0.04*		
	(0.65)			(0.21)			(0.02)		
NPI Stringency$_c$ × After15$_a$		−3.90***			−0.49			−0.12***	
		(1.00)			(0.40)			(0.04)	
NPI Stringency$_c$ × After15$_a$ × FICO$_1$			−9.56***			−0.82			0.59***
			(2.35)			(0.82)			(0.20)
NPI Stringency$_c$ × After15$_a$ × FICO$_2$			−2.35			−0.44			0.34
			(3.18)			(1.12)			(0.22)
NPI Stringency$_c$ × After15$_a$ × FICO$_3$			2.39			−0.77			−0.25
			(3.02)			(1.01)			(0.30)
NPI Stringency$_c$ × After15$_a$ × FICO$_4$			−3.88			−0.29			−0.31
			(3.42)			(1.68)			(0.31)
NPI Stringency$_c$ × After15$_a$ × FICO$_5$			−13.52***			−0.36			−0.23
			(3.32)			(1.27)			(0.23)
County FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FICO FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
After15 FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.52	0.53	0.53	0.05	0.06	0.06	0.03	0.04	0.05
Observations	105,852	105,760	105,760	103,400	103,308	103,308	105,525	105,433	105,433

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C. The Pandemic or NPIs: What Matters More For the Availability of Credit?

Analogous to Section IV.C, we study whether the availability of credit during the early outbreak of the COVID-19 pandemic changed more strongly in counties affected by the pandemic itself or in counties with more stringent NPIs. We therefore estimate Equation (4) with changes in the availability of credit as the outcome variable.

Table VIII presents the estimation results for this horse race regression. To compare magnitudes across coefficients within each regression, we report standardized regression coefficients, which indicate how many standard deviations the dependent variable changes for a one-standard deviation change in the independent variable. The first two columns show that NPIs have a stronger negative effect on credit card originations than case severity, with the standardized coefficient being about four times larger. While we find no overall effects on credit limits, we observe a larger standardized effect on APR spreads for our case severity measure than for NPIs. Thus, while we find that the pandemic itself matters more for the use of credit than NPIs, our horse race results for the availability of credit are more ambiguous.

VI. Further Analysis and Robustness Checks

A. Robustness Check: Geographically Smoothed Measure of Case Severity

In our baseline analysis, we use the number of confirmed cases per 100,000 population to measure how severely a county was affected by the COVID-19 pandemic in March 2020. However, people might not only respond to COVID-19 cases in their own county but also to infections in the surrounding area. For example, Putnam County, NY had no confirmed cases in our data as of March 15, but it is surrounded by five strongly affected counties (among them the severely affected Westchester County). Thus, the perceived level of pandemic severity in Putnam County was likely higher than indicated by the zero confirmed cases in the county itself. We address this possible issue by constructing an alternative version of our case severity variable. Specifically, for each county we define the variable *Area Case Severity* as the number of confirmed cases in the county itself and all adjacent counties as of March 15 per 100,000 of the combined population in these counties.

From a statistical perspective, we use this variable to address potential measurement errors in

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Table VIII
Case Severity or NPIs: What Drives the Availability of Credit?

This table presents the estimation results for the horse race regression in (4) in Section III with changes in our measures of credit availability as the outcome variables. All outcome variables are defined as year-to-year changes (2019 to 2020) in month-to-month changes (February to March). To compare magnitudes across coefficients within each regression, column 2,4, and 6 report the standardized coefficient, which indicates how many standard deviations the dependent variable changes for a one-standard deviation change in the independent variable. The variable $Cases\ Severity_c$ measures county-level COVID-19 affectedness via the number of confirmed cases per 100,000 population as of March 15. The variable $NPI\ Stringency_c$ is the county-level NPI stringency indicator as defined in Section II.B.2. *After15* is a dummy variable which takes on the value of 1 if a card has an account origination date after the 15^{th} of the month, and 0 otherwise. All regressions are weighted by the number of credit cards as of 2020m1 per observational unit. Standard errors are clustered at the county level and Bank × FICO Bucket-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Log No. Cards		Log Limits		APR Spreads	
	(1)	(2)	(3)	(4)	(5)	(6)
Case Severity$_c$ × After15$_a$	−0.39	−0.01	−0.03	0.00	−0.05***	−0.03
	(0.45)		(0.11)		(0.01)	
NPI Stringency$_c$ × After15$_a$	−3.54***	−0.04	−0.46	−0.01	−0.07*	−0.01
	(0.91)		(0.37)		(0.04)	
County FE	Yes		Yes		Yes	
Bank FE	Yes		Yes		Yes	
FICO FE	Yes		Yes		Yes	
After15 FE	Yes		Yes		Yes	
Adjusted R^2	0.53		0.06		0.09	
Observations	105,760		103,308		105,433	

our baseline analysis. When we measure case severity, we are not so much interested in the direct health effects of the pandemic (i.e., people falling sick with the virus) but in the effects of the latent "fear of the virus" variable. Not taking into account the number of cases in the surrounding area might thus cause an attenuation bias in our estimated regression coefficients (Hausman, 2001), as the measurement errors are likely positively correlated with the latent variable. In geospatial settings similar to ours, various geographical smoothing techniques have been suggested to alleviate

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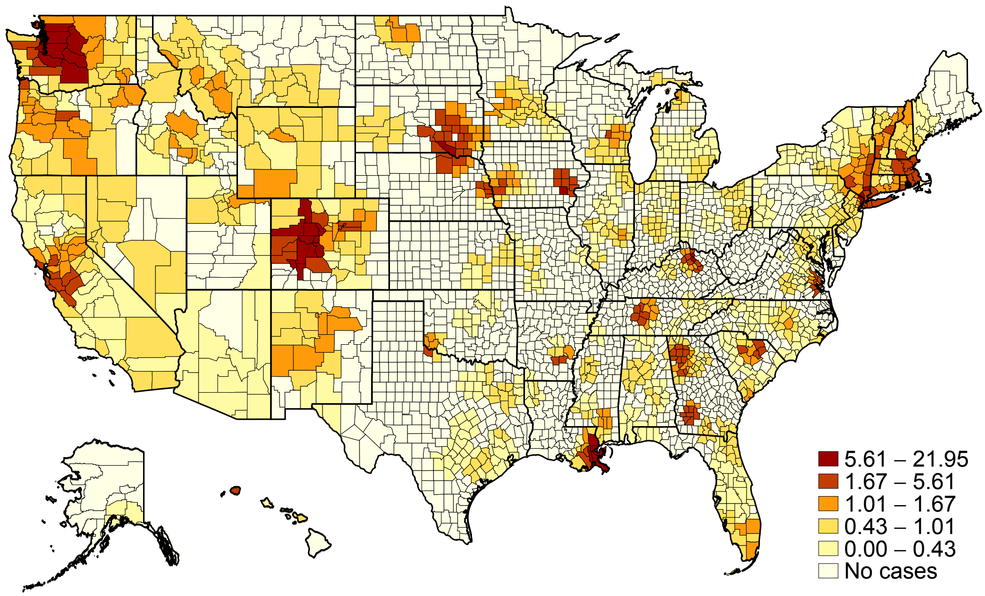

Figure 7. Area Case Severity. This figure illustrates the distribution of area case severity across U.S. counties as of March 15, 2020. We calculate a county's area case severity by summing up the number of all cases in the county itself and all bordering counties divided by the combined population in these counties. Cut-off values for the different colors were chosen to match the 50, 75, 90, 95, and 99 percentile of the distribution, respectively. (Data Source: Johns Hopkins Coronavirus COVID-19 Global Cases Database (Dong, Du, and Gardner, 2020) and authors' own calculations.)

this type of measurement error (see e.g., Gryparis et al. (2009) for a discussion). Accordingly, we use a simple geographical smoothing technique to construct our *Area Case Severity* measure to mitigate the potential attenuation bias. Figure 7 illustrates the geographical distribution of this alternative case severity measure across counties in the United States.

Table IX reports the estimation results of Equations (1) and (3) for our *Area Case Severity* measure. All of our results from Table III still hold and become even stronger. A one-case increase per 100,000 population in the surrounding area is associated with a 0.55 percent (previously 0.36 percent) reduction in credit card balances (Column 2) and with a 0.80 percent (previously 0.40 percent) reduction in transaction volumes (Column 5). Similarly, we find larger magnitudes for the effect on utilization rates and for the effects in individual FICO classes.

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Table IX
The Effect of Area Case Severity on the Use of Consumer Credit

This table presents the estimation results for the effect of area case severity in Section VI.A on the use of credit from Equations (1) and (3) in Section III. All outcome variables are defined as year-to-year changes (2019 to 2020) in month-to-month changes (February to March). The variable *Area Cases Severity*$_c$ is defined as in Section VI.A. *After15* is a dummy variable which takes on the value of 1 if a card has an account cycle-end date after the 15^{th} of the month, and 0 otherwise. The variables $FICO_1$ through $FICO_5$ are indicator dummy variables for the five different FICO buckets: Below 580 ($FICO_1$), 580-669 ($FICO_2$), 670-739 ($FICO_3$), 740-799 ($FICO_4$), and 800-900 ($FICO_5$). All regressions are weighted by the number of credit cards as of 2020m1 per observational unit. Standard errors are clustered at the county level and Bank × FICO Bucket-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Log Balances			Log Transactions			Utilization Rates		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Area Case Severity$_c$	−0.19***			−0.48***			0.00		
	(0.06)			(0.06)			(0.00)		
Area Case Severity$_c$ × After15$_a$		−0.55***			−0.80***			−0.02***	
		(0.08)			(0.11)			(0.01)	
Area Case Severity$_c$ × After15$_a$ × FICO$_1$			0.34***			−0.35			0.00
			(0.10)			(0.33)			(0.01)
Area Case Severity$_c$ × After15$_a$ × FICO$_2$			0.25***			−0.74**			0.01
			(0.10)			(0.32)			(0.01)
Area Case Severity$_c$ × After15$_a$ × FICO$_3$			−0.10			−0.76***			−0.01
			(0.11)			(0.21)			(0.01)
AreaCase Severity$_c$ × After15$_a$ × FICO$_4$			−0.70***			−0.85***			−0.04***
			(0.14)			(0.20)			(0.01)
Area Case Severity$_c$ × After15$_a$ × FICO$_5$			−0.97***			−0.82***			−0.03***
			(0.13)			(0.30)			(0.01)
County FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FICO FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
After15 FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.15	0.16	0.17	0.39	0.41	0.41	0.06	0.07	0.07
Observations	381,343	381,343	381,343	381,350	381,350	381,350	381,343	381,343	381,343

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These results suggest that our *Area Case Severity* measure alleviates a potential attenuation bias from the possible mis-measurement of the latent variable "fear of the virus" in our baseline analysis.

B. Use of Credit and Income Heterogeneity

Our findings in Section IV.A show that borrowers in the lower FICO classes reduce their credit card balances and transaction volumes less than borrowers in the higher FICO classes. In this section, we provide further evidence that these results are consistent with a disruption of consumers' spending patterns. Table X shows spending shares across different expenditure categories by income deciles from the September 2019 BLS Consumer Expenditure Survey. The last column illustrates that low income households (in the second decile) have a higher expenditure share for non-discretionary consumption categories and a lower expenditure share for discretionary categories than high income households (in the ninth decile).

Since the COVID-19 shock has a stronger affect on discretionary than on non-discretionary spending categories (Baker et al., 2020c; Coibion et al., 2020a; JPMorgan, 2020), low income borrowers likely have less leeway to reduce spending and therefore to reduce their use of consumer credit card debt. To directly test this hypothesis, we split our sample into five different income buckets: Below \$21,293; \$21,293-\$41,490; \$41,490-\$70,367; \$70,367-\$116,626; above \$116,626. The cut-off values are based on the income quintiles in the BLS Consumer Expenditure Survey. We then estimate the regression model in Equation (3) with income bucket dummy variables instead of FICO bucket dummy variables. Table XI reports the estimation results of this analysis. We find results of very similar magnitude compared to our baseline results in Table III. The reduction in credit card balances, transactions, and utilization rates is driven by borrowers in the highest income buckets. These findings are consistent with Chetty et al. (2020). Interestingly, we also find a decrease in outstanding credit card balances for borrowers in the lowest income buckets, while we find an increase for borrowers in the lowest FICO buckets. Overall, the combined findings in Table X and Table XI provide corroborating evidence that a higher pre-shock spending share for non-essential goods and services is associated with a lager reduction in total spending in the wake of the COVID-19 pandemic (Andersen et al., 2020a).

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Table X
Consumer Spending by Categories and Income Deciles

This table reports spending volumes and shares of total spending for selected expenditure categories by income decile from the September 2019 Consumer Expenditure Survey from the U.S. Bureau of Labor Statistics (BLS, 2019). We classify expenditure categories as *non-discretionary* or *discretionary* in line with the recent household consumption literature (Baker et al., 2020c; Coibion et al., 2020a). We report spending volumes (in \$1,000) and spending shares (as a fraction of total spending) for all ten income deciles. Total spending is defined as annual average expenditures. The last column reports the difference in spending shares between the second and the ninth income decile.

Decile	All	Lowest	2^{nd}	3^{rd}	4^{th}	5^{th}	6^{th}	7^{th}	8^{th}	9^{th}	Top	2^{nd}-9^{th}
In \$1,000												
Total Spending	61.2	25.3	27.5	37.2	42.8	49.2	54.2	64.0	74.2	95.1	142.6	
Non-Discretionary Expenses.												
Food At Home	4.5	2.7	2.7	3.7	3.6	3.9	4.3	4.9	5.3	6.0	7.7	
Share (in %)	7.3	10.6	10.0	9.8	8.4	7.9	7.9	7.7	7.1	6.3	5.4	3.7
Housing	20.1	9.9	11.2	13.8	14.8	17.1	18.6	21.0	23.5	28.4	42.6	
Share (in %)	32.8	39.0	40.9	37.2	34.6	34.8	34.3	32.7	31.6	29.9	29.9	11.0
Utilities and Fuels	4.0	2.1	2.7	3.2	3.6	3.9	4.0	4.5	4.8	5.4	6.3	
Share (in %)	6.6	8.4	9.8	8.6	8.4	7.9	7.4	7.0	6.5	5.7	4.4	4.1
Discretionary Expenses.												
Food Away	3.5	1.5	1.3	2.1	2.3	2.8	2.9	3.8	4.8	5.2	7.8	
Share (in %)	5.6	5.9	4.7	5.6	5.5	5.7	5.4	5.9	6.4	5.5	5.5	−0.7
Entertainment: Fees	0.8	0.3	0.2	0.2	0.3	0.4	0.5	0.7	0.9	1.3	2.8	
Share (in %)	1.3	1.2	0.6	0.6	0.7	0.8	1.0	1.1	1.2	1.3	2.0	−0.7
Transportation	9.8	3.5	4.0	6.2	7.4	7.9	9.4	10.3	12.3	16.4	20.4	
Share (in %)	15.9	13.8	14.4	16.6	17.2	16.0	17.3	16.1	16.5	17.3	14.3	−2.9

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Table XI
Use of Consumer Credit and Income Heterogeneity

This table presents the estimation results for the effect of COVID-19 case severity on the use of credit from Equations (1) and (3) in Section III. All outcome variables are defined as year-to-year changes (2019 to 2020) in month-to-month changes (February to March). The variable *Cases Severity*$_c$ measures county-level COVID-19 affectedness via the number of confirmed cases per 100,000 population as of March 15. *After15* is a dummy variable which takes on the value of 1 if a card has an account cycle-end date after the 15^{th} of the month, and 0 otherwise. The variables *Income*$_1$ through *Income*$_5$ are indicator dummy variables for the five different income buckets: Below $21,293 (*Inc.*$_1$); $21,293-41,490 (*Inc.*$_2$); $41,490-70,367 (*Inc.*$_3$); $70,367-116,626 (*Inc.*$_4$); and above $116,626 (*Inc.*$_5$). All regressions are weighted by the number of credit cards as of 2020m1 per observational unit. Standard errors are clustered at the county level and Bank × FICO Bucket-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Log Balances			Log Transactions			Utilization Rates		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Case Severity$_c$	−0.14***			−0.23***			−0.005***		
	(0.03)			(0.04)			(0.001)		
Case Severity$_c$ × After15$_a$		−0.33***			−0.34***			−0.015***	
		(0.06)			(0.12)			(0.004)	
Case Severity$_c$ × After15$_a$ × Income$_1$			−0.20			−0.19			−0.02*
			(0.13)			(0.16)			(0.01)
Case Severity$_c$ × After15$_a$ × Income$_2$			−0.20**			−0.29**			−0.01*
			(0.09)			(0.14)			(0.01)
Case Severity$_c$ × After15$_a$ × Income$_3$			−0.22***			−0.30**			−0.01
			(0.08)			(0.13)			(0.01)
Case Severity$_c$ × After15$_a$ × Income$_4$			−0.33***			−0.35**			−0.01*
			(0.11)			(0.16)			(0.01)
Case Severity$_c$ × After15$_a$ × Income$_5$			−0.53***			−0.44**			−0.03***
			(0.13)			(0.20)			(0.01)
County FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FICO FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
After15 FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.10	0.11	0.11	0.39	0.40	0.40	0.05	0.06	0.06
Observations	374,182	374,182	374,182	374,202	374,202	374,202	374,182	374,182	374,182

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VII. Conclusion

Geographic heterogeneity in the COVID-19 shock provides an opportunity to estimate its impact on the supply of and demand for consumer credit during the early stages of the pandemic through March 2020. Using comprehensive regulatory data on individual credit card accounts from the Federal Reserve's monthly Y-14M reports, we estimate the effect of the local severity of the outbreak and the effect of local policy responses in the form of NPIs. Exploiting the granularity of our dataset, we construct data of semi-monthly frequency at the County-Bank-FICO level, which allows an econometric specification that identifies the effects on credit use and availability even in the presence of County-Bank-FICO specific time trends.

In severely affected counties, we find a large reduction in the use of credit by more creditworthy borrowers and an increase in balances by less creditworthy borrowers. We find these effects when the impact of COVID-19 is measured by case severity or NPIs, but case severity appears to be the more powerful driver of changes in credit card use. The observed differential responses of high and low-FICO score households is inconsistent with consumption theory, which predicts a higher marginal propensity to consume (MPC) out of (negative) income shocks for households with low liquid wealth. Our findings are, however, consistent with alternative explanations. First, our results can be explained by the fact that more creditworthy borrowers have a higher expenditure share for goods and services that are harder to consume during the pandemic (e.g., restaurants, bars, and travel). Following this explanation, the COVID-19 shock constituted a disruption of hitherto existing spending patterns for creditworthy borrowers (Andersen et al., 2020a). Second, our results are also consistent with recent evidence on how consumer credit responds to economic uncertainty shocks. Low-FICO borrowers with limited access to credit have a lower pecuniary cost of default than high-FICO borrowers, which increases their incentives to engage in risk shifting. In contrast, high-FICO borrowers with a higher cost of default respond to increased uncertainty by targeting greater financial flexibility to protect their credit reputation and future credit access (Di Maggio et al., 2017).

Moreover, we find a large reduction in the origination of new credit cards for both high- and low-FICO borrowers, as well as a reduction in credit limits and an increase in APR spreads for new credit cards issued to the least creditworthy borrowers in affected counties. These findings are

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consistent with a "flight-to-safety" effect in the wake of a severely adverse macroeconomic shock. While low-FICO borrowers have a high marginal propensity to borrow (MPB), banks have a low marginal propensity to lend (MPL) to these borrowers. Conversely, while banks have a high MPL to high-FICO borrowers, these borrowers have a low MPB. Thus, the total effect on credit supply at the extensive margin (new credit card originations) likely reflects primarily supply effects at the bottom end of the FICO distribution and primarily demand effects for higher FICO classes (Agarwal et al., 2018).

We emphasize that these causal effects are measured only in the early stages of the pandemic, when most counties had not yet imposed the most stringent NPIs, such as stay-at-home orders and lockdowns and the wave of unemployment had yet to peak. We therefore caution against using these results to evaluate the efficacy and consequences of more restrictive policy measures. But our results also cast doubt that reopening the economy will yield a quick return to normal if the pandemic is not contained. We provide evidence that the fear of the virus yields strong negative effects on consumer credit demand, even in the absence of government-mandated restrictions.

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Appendix: Reconciliation of Growth and Level Effects

We refer to our estimates of the COVID-19's causal effect as both growth effects and level effects. Specifically, we interpret our findings as (i) the difference in the relative growth rates of affected versus unaffected counties, and (ii) the deviation of affected counties from their counterfactual levels. In this appendix, we show that the two interpretations are econometrically equivalent because the epidemic only affects the very last time period (i.e., March 2020) in our dataset.

Let us consider the following regression for the *level* of outcome variable Y:

$$Y_{c,b,f,a,y,m} = \delta \left(\text{Affectedness}_{c,y,m} \times \text{After15}_a\right) + \tilde{\gamma}_{c,y,m} + \tilde{\gamma}_{b,y,m} + \tilde{\gamma}_{f,y,m} + \tilde{\gamma}_{a,y,m} + \tilde{\varepsilon}_{c,b,f,a,y,m}, \quad \text{(A1)}$$

where the respective indexes c, b, f, a, y, m stand for counties, banks, FICO buckets, cycle-end/origination date buckets, years, and months. $\text{Affectedness}_{c,y,m}$ measures case severity in county c in year y in month m, and After15_a is an indicator dummy as defined in Section III. The specification in (A1) incorporates year-month constants $\gamma_{(\cdot),y,m}$, which control for level differences across counties, banks, FICO buckets, and cycle-end date/origination date buckets over time. We define variable y as the year-on-year difference in the February-to-March changes in variable Y between the years 2019 and 2020:

$$y_{c,b,f,a} \mathrel{\hat{=}} \left(Y_{c,b,f,a,2020,Mar} - Y_{c,b,f,a,2020,Feb}\right) - \left(Y_{c,b,f,a,2019,Mar} - Y_{c,b,f,a,2019,Feb}\right). \quad \text{(A2)}$$

Substituting (A1) into (A2) yields:

$$\begin{aligned}
y_{c,b,f,a} = \delta \Big[&(\text{Affectedness}_{c,2020,Mar} - \text{Affectedness}_{c,2020,Feb}) \\
&- (\text{Affectedness}_{c,2019,Mar} - \text{Affectedness}_{c,2020,Feb}) \Big] \times \text{After15}_a \\
&+ (\tilde{\gamma}_{c,2020,Mar} - \tilde{\gamma}_{c,2020,Feb}) - (\tilde{\gamma}_{c,2019,Mar} - \tilde{\gamma}_{c,2019,Feb}) \\
&+ (\tilde{\gamma}_{b,2020,Mar} - \tilde{\gamma}_{b,2020,Feb}) - (\tilde{\gamma}_{b,2019,Mar} - \tilde{\gamma}_{b,2019,Feb}) \\
&+ (\tilde{\gamma}_{f,2020,Mar} - \tilde{\gamma}_{f,2020,Feb}) - (\tilde{\gamma}_{f,2019,Mar} - \tilde{\gamma}_{f,2019,Feb}) \\
&+ (\tilde{\gamma}_{a,2020,Mar} - \tilde{\gamma}_{a,2020,Feb}) - (\tilde{\gamma}_{a,2019,Mar} - \tilde{\gamma}_{a,2019,Feb}) \\
&+ (\tilde{\varepsilon}_{c,b,f,a,2020,Mar} - \tilde{\varepsilon}_{c,b,f,a,2020,Feb}) - (\tilde{\varepsilon}_{c,b,f,a,2019,Mar} - \tilde{\varepsilon}_{c,b,f,a,2019,Feb}),
\end{aligned} \quad \text{(A3)}$$

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which we can simplify by noting that $\text{Affectedness}_{c,y,m} = 0$ for every county in year-month pairs before March 2020. Accordingly, we define $\text{Affectedness}_c \mathrel{\hat{=}} \text{Affectedness}_{c,2020,Mar}$, as well as $\gamma_{(\cdot)} \mathrel{\hat{=}} \left(\tilde{\gamma}_{(\cdot),2020,Mar} - \tilde{\gamma}_{(\cdot),2020,Feb}\right) - \left(\tilde{\gamma}_{(\cdot),2019,Mar} - \tilde{\gamma}_{(\cdot),2019,Feb}\right)$ and $\varepsilon_{c,b,f,a} \mathrel{\hat{=}} \left(\tilde{\varepsilon}_{c,b,f,a,2020,Mar} - \tilde{\varepsilon}_{c,b,f,a,2020,Feb}\right) - \left(\tilde{\varepsilon}_{c,b,f,a,2019,Mar} - \tilde{\varepsilon}_{c,b,f,a,2019,Feb}\right)$. After substitution, we get:

$$y_{c,b,f,a} = \delta \left(\text{Affectedness}_c \times \text{After15}_a\right) + \gamma_c + \gamma_b + \gamma_f + \gamma_a + \varepsilon_{c,b,f,a}, \tag{A4}$$

which is identical to our baseline regression specified in Equation (1) in Section III.

This shows that regression coefficient δ in our baseline specification captures the COVID-19's effect on the March 2020 *level* of Y, relative to its counterfactual value. Equivalently, coefficient δ measures the epidemic's effect on the 2020 February-to-March *change* of Y, relative to the change observed in 2019. Consequently, in the special case that outcome variable Y represents log levels, the two interpretations imply a percent effect on March 2020 levels or, equivalently, a percentage point effect on February-to-March growth rates in 2020.

A stylized numerical example

We demonstrate the equivalence of the level and growth interpretations of our analysis through an example, applying a simplified version of our identification strategy. Table A1 works through this stylized log credit card balances example, omitting the bank, FICO bucket, or cycle-end date dimensions, which are otherwise included in our baseline specification. In this example, our identification strategy, expressed in both (A1) and (A2), applies as follows. (i) The February-to-March percent growth rates of balances in unaffected and affected counties were -4.1% and -3.6% in 2019, respectively. That is, differences in factors unrelated from the COVID-19, balances in affected counties grew 50 basis points faster in the year before the epidemic. (ii) We impose our identification assumption that the observed pre-epidemic difference in balance growth rates between unaffected and affected counties remains constant over time (i.e., across years). (iii) The identification assumption implies that balances in affected counties would have grown 50 basis points faster also in 2020 if there had been no COVID-19. (iv) Therefore, observing a -5.1% percent growth rate of balances in unaffected counties, we infer that balances in affected counties would have grown by $(-5.1\% + 0.5\%) = -4.6\%$ *absent* the epidemic. Importantly, this counterfactual

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Table A1
Growth and Level Effects: Numerical Example

This table shows a stylized example of the changes in log cycle-end balances of credit cards to demonstrate the logic of our identification strategy. The figures represent log levels and log changes (growth) for February and March in the years 2019 and 2020, respectively. Boldface indicates the epidemic's estimated effect on the level and growth rate of balances in affected counties in 2020.

	Unaffected	**Affected**	Δ Growth (pp.)
Feb 2019	6.908	6.908	
Mar 2019	6.867	6.872	
Growth (%)	-4.1%	-3.6%	0.5pp
Feb 2020	7.003	7.003	
Mar 2020	6.952	**6.932**	
Growth (%)	-5.1%	-7.1%	-1.9pp
ΔGrowth (pp.)	-1.0pp	-3.5pp	**-2.5pp**

growth scenario would have implied a mean log balance level of $(7.003 - 0.046) = 6.957$ in affected counties in March 2020.

We can estimate the COVID-19's causal effect by comparing the actual observed balance levels and growth rates in affected counties to their respective counterfactual values. A comparison of the observed mean log balance level of 6.932 to the counterfactual log balance level of 6.957 implies that March 2020 balances in affected counties were 2.5 percent lower due to the COVID-19 shock. Equivalently, a comparison of the observed balance growth rate of -7.1% to the counterfactual balance growth rate of -4.6% implies that the February-to-March balance growth rate in 2020 was 250 basis points lower in affected counties due to the COVID-19 shock.