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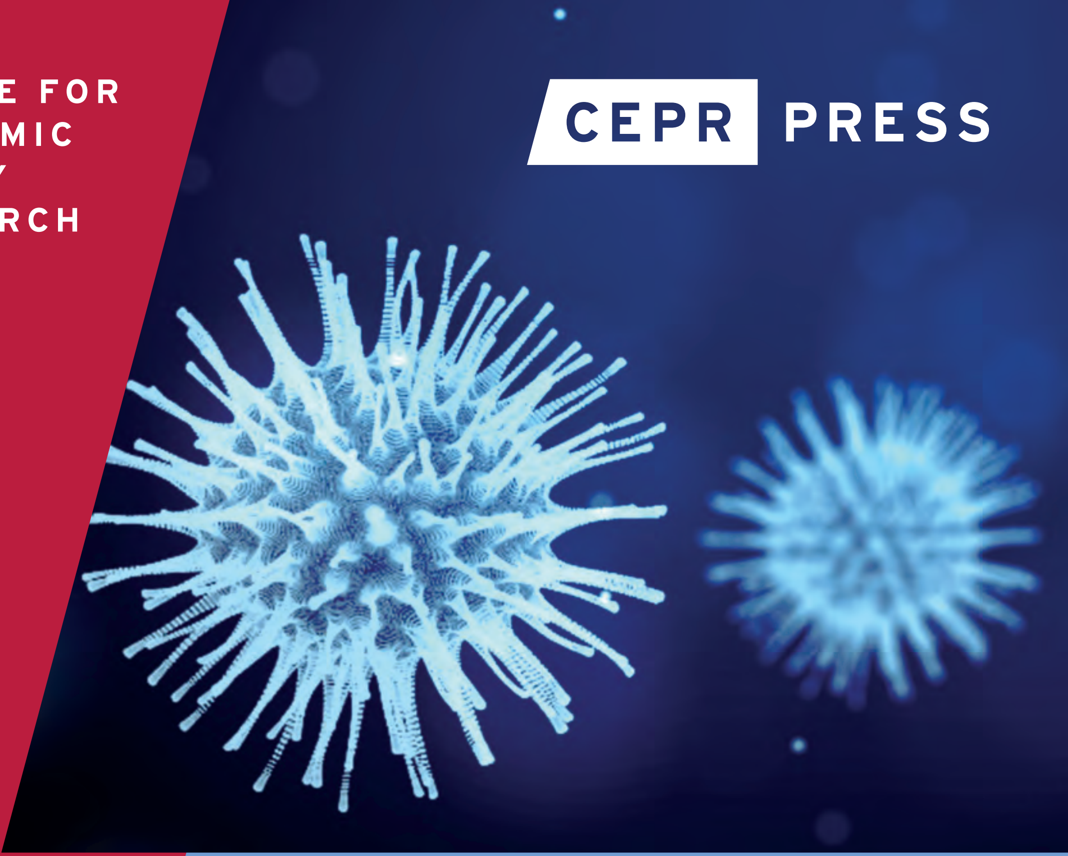

COVID ECONOMICS

VETTED AND REAL-TIME PAPERS

ISSUE 46

1 SEPTEMBER 2020

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Covid Economics
Vetted and Real-Time Papers

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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.

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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
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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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Japan's voluntary lockdown[1]

Tsutomu Watanabe[2] and Tomoyoshi Yabu[3]

Date submitted: 27 August 2020; Date accepted: 28 August 2020

Japan's government has taken a number of measures, including declaring a state of emergency, to combat the spread COVID-19. We examine the mechanisms through which the government's policies have led to changes in people's behavior. Using smartphone location data, we construct a daily prefecture-level stay-at-home measure to identify the following two effects: (1) the effect that citizens refrained from going out in line with the government's request, and (2) the effect that government announcements reinforced awareness with regard to the seriousness of the pandemic and people voluntarily refrained from going out. Our main findings are as follows. First, the declaration of the state of emergency reduced the number of people leaving their homes by 8.6% through the first channel, which is of the same order of magnitude as the estimate by Goolsbee and Syverson (2020) for lockdowns in the United States. Second, a 1% increase in new infections in a prefecture reduces people's outings in that prefecture by 0.026%. Third, the government's requests are responsible for about one quarter of the decrease in outings in Tokyo, while the remaining three quarters are the result of information updating on the part of citizens through government announcements and the daily release of the number of infections. Our results suggest that what is necessary to contain the spread of COVID-19 is not strong, legally binding measures but the provision of appropriate information that encourages people to change their behavior.

1 We would like to thank Kosuke Aoki, Gita Gopinath, Michihiro Kandori, Takayuki Mizuno, Makoto Nirei, Takaaki Ohnishi, Katsumi Tanabe, and Takashi Ui for helpful discussions and comments. We also thank Takayuki Mizuno for providing us with the data for the stay-at-home measure, and to Ayaka Nakahara for research assistance. This research forms part of the project on "Central Bank Communication Design" funded by the JSPS Grant-in-Aid for Scientific Research No. 18H05217. Tomoyoshi Yabu gratefully acknowledges financial support from the JSPS through the Grant-in-Aid for Scientific Research No. 20K01594.

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1 Introduction

In response to the spread of COVID-19, the Japanese government on February 27 issued a request to local governments such as prefectural governments to close schools. Subsequently, the Japanese government declared a state of emergency on April 7 for seven prefectures, including Tokyo, and on April 16 expanded the state of emergency to all 47 prefectures. Prime Minister Abe called on citizens to reduce social interaction by at least 70% and, if possible, by 80% by refraining from going out. In response to these government requests, people restrained from going out. For example, in March, the share of people in Tokyo leaving their homes was down by 18% compared to January before the spread of COVID-19, and by April 26, during the state of emergency, the share had dropped as much as 64%. As a result of people refraining from leaving their homes, the number of daily new infections in Tokyo fell from 209 at the peak to two on May 23, and the state of emergency was lifted on May 25.[1]

Unlike the lockdowns in China, the United States, and European countries such as Italy, restrictions during Japan's state of emergency had no legal binding force. There were no penalties such as fines or arrests for leaving the house during the state of emergency. The police did not warn anyone who was out on the streets. The situation in Japan was one of a "voluntary lockdown."[2] Looking at the "Government Response Stringency Index" – a composite measure of nine response indicators published by the University of Oxford's Blavatnik School of Government – shows that the value for Japan of 47.22 at the end of April during the state of emergency was considerably smaller than those for France (87.96), the United States (72.69), the United Kingdom (75.93), Germany (76.85), Italy (93.52), and Canada (72.69).[3] Instead, the value for Japan was essentially on the same level as that for Sweden (46.30). Looking at individual indicators, the status for "Restrictions on public gatherings" was "No restrictions" and that for "Closures of public transport" was "No measures," which is quite different

[1]However, as economic activity resumed after the state of emergency had been lifted, the number of new infections in Tokyo began to increase again in late June, surpassing the peak before the state of emergency.

[2]Regarding the difference between Japan and other countries, Gordon (2020) observes: "Many commentators both within Japan and around the world have emphasized the uniqueness of Japan's relatively soft "state of emergency," which, even though enacted by law, relies on requests and instructions rather than orders, fines, or arrests." Examples of overseas media reports highlighting that measures by the Japanese government were not legally binding include the following: "Japan's halfhearted coronavirus measures are working anyway" (Foreign Policy, May 14, 2020; online: `https://foreignpolicy.com/2020/05/14/japan-coronavirus-pandemic-lockdown-testing`); "Did Japan just beat the virus without lockdowns or mass testing?" (Bloomberg, May 22, 2020; online: `https://www.bloomberg.com/news/articles/2020-05-22/did-japan-just-beat-the-virus-without-lockdowns-or-mass-testing`); and "From near disaster to success story: How Japan has tackled coronavirus" (The Guardian, May 22, 2020; online: `https://www.theguardian.com/world/2020/may/22/from-near-disaster-to-success-story-how-japan-has-tackled-coronavirus`).

[3]`https://www.bsg.ox.ac.uk/research/research-projects/coronavirus-government-response-tracker`

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from other countries. Similarly, with regard to "Stay-at-home requirements," restrictions in Japan were weaker than in other countries: while in Japan people were "recommended" to stay at home, in the United States and various European countries they were "Required not to leave the house with exceptions."

The fact that the behavior of people in Japan changed even though the country only took measures that were not legally binding suggests that the (legally binding) lockdowns in other countries were not the only reason people changed their behavior. The aim of this study is to clarify the mechanisms by which the non-legally binding policies of the Japanese government led people to change their behavior such as refraining from going out.[4]

In this study, we focus on the following two channels through which the Japanese government's measures to prevent the spread of COVID-19 changed people's behavior. The first channel is the "intervention effect." This refers to changes in people's behavior as a result of obeying government orders and requests to refrain from leaving their homes. In the case of first Wuhan in China and then the United States and Europe, this took the form of severe lockdowns, in which the government used legal powers to deprive people of their freedom of movement. In Japan's case, the state of emergency was not legally binding and, if anything, was very limited, so that it is appropriate to regard it as a "request" from the government. On the other hand, the closure of schools by the Japanese government had a compulsory aspect.

The second channel is the "information effect." Generally, when a government implements a policy on something, it can be assumed that it makes its decisions based on various types of information gathered before reaching the decision. Therefore, government decisions provide the public with information about the current situation. This is the signaling effect of government measures. Applying this to measures such as the state of emergency declaration, it can be thought that the public obtained new information on the status of infections through the government's announcements. In developed countries, including Japan, details on infections are generally not disclosed to the public to protect the privacy of those infected. This means that governments have considerably more information than

[4] Although unrelated to the topic of this study, why Japan did not implement legally binding measures is also an important question. As highlighted by Kushner (2020), in the past, Japan used to give the police legal authority to enforce testing and quarantining in order to prevent the spread of cholera and leprosy. A very strict isolation policy was adopted, especially for leprosy patients. As pointed out by Kushner, however, both at the time and today these severe measures have been strongly criticized, and for this reason the Japanese government was unable to declare a state of emergency with legally enforceable restrictions on individual liberties, which would have been perceived as heavy-handed.

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is in the public domain (or at least this is what many people believe), so that government actions have a strong signaling effect.

To examine the role that these two channels played in affecting people's behavior, we use smartphone location data to construct a daily prefecture-level measure showing the degree to which people stayed at home (calculated as the percentage decline relative to January 2020, i.e., before the pandemic, in the number of people leaving their homes multiplied by the time outside the home). We then construct panel data to examine the effect of the two major government measures to contain infections – the declaration of the state of emergency and the closure of schools – on the stay-at-home measure. Importantly, in doing so, we distinguish between the intervention effect and the information effect. We do so by utilizing the fact that the timing of the start and the end of the state of emergency and of school closures differed across prefectures. For example, in Tokyo, a state of emergency was declared on April 7, but at that time no state of emergency had been declared in Tochigi prefecture, which is located 100km north of Tokyo, about an hour on the bullet train. In Tochigi prefecture, a state of emergency was declared on April 16. Therefore, there was no intervention effect in Tochigi prefecture from April 7 to 15. However, people in Tochigi prefecture were aware that a state of emergency had been declared in Tokyo, so there was an information effect, and the number of people leaving their homes decreased. On the other hand, people in Tokyo refrained from leaving their homes due to both factors, i.e., the intervention effect and the information effect. Therefore, assuming that people in Tokyo and Tochigi had the same information about infections and there was no difference in the information effect between the two prefectures, it is possible to extract the intervention effect by observing the difference in the stay-at-home measure of the two prefectures. As for the state of emergency, not only did the timing when it started differ across prefectures, but the lifting also was divided into three waves, so that the intervention effect can be identified by utilizing the difference in timing. Similarly, with regard to school closures, the date when school closures were lifted varies widely across prefectures, which can be used to identify the intervention effect.

The main findings of this study are as follows. First, the state of emergency had the effect of reducing outings by 20 percentage points compared to before the pandemic. Of these 20 percentage points, 7 percentage points were due to the intervention effect, while the remainder was due to the information effect. The estimated intervention effect is of the same order of magnitude as the estimate by Goolsbee and Syverson (2020) for lockdowns in the United States. On the other hand,

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school closures had the effect of reducing outings by 12 percentage points compared to before the pandemic. Of these 12 percentage points, 5 percentage points were due to the intervention effect. Second, an increase in the number of new infections within a particular prefecture had the effect that people in that prefecture reduced their outings: a 1% increase in the number of new infections in a prefecture reduced outings in that prefecture by 0.026%. People also reduced outings in response to the nationwide total of new infections. Third, if we decompose the reduction in outings in Tokyo into the intervention effect and the information effect, the contribution of the intervention effect is about one quarter, while the contribution of the information effect is three quarters, indicating that the dominant channel for changes in behavior was the information effect. Our findings show that people in Japan voluntarily reduced their outings based on various kinds information such as announcements of measures by the government and daily announcements of the number of infections. The results thus suggest that what is necessary for containing infections is not strong legally binding measures but the provision of appropriate information to encourage people to change their behavior.

There already is a considerable body of research on the economic impact of COVID-19, and the number of studies is increasing rapidly. Against this background, the present study is most closely related to the following three areas of research. The first is research on changes in behavior and the reduction in outings using smartphone location data. Examples of studies in this area include Alexander and Karger (2020), Barrios and Hochberg (2020), Couture et al. (2020), Chiou and Tucker (2020), and Gupta et al. (2020).

The second related area is studies on the impact of lockdown policies in the United States on people's movement and behavior. Examples include the studies by Forsythe et al. (2020), Rojas et al. (2020), Coibion et al. (2020), Goolsbee and Syverson (2020), Alexander and Karger (2020), and Gupta et al. (2020). The main interest of the present study is why the Japanese government's countermeasures against COVID-19 brought about changes in behavior even though they were not legally binding, and what is interesting in this regard is that a number of studies using U.S. data highlight that the lockdowns imposed by governments account for only a limited part of changes in people's behavior in the United States. For example, Goolsbee and Syverson (2020), using the fact that lockdowns were not implemented simultaneously across the entire United States but that the timing differed by state and county, compare counties that at a particular time were under lockdown and those that were not to examine whether there were significant differences in consumers' shopping behavior. They find that

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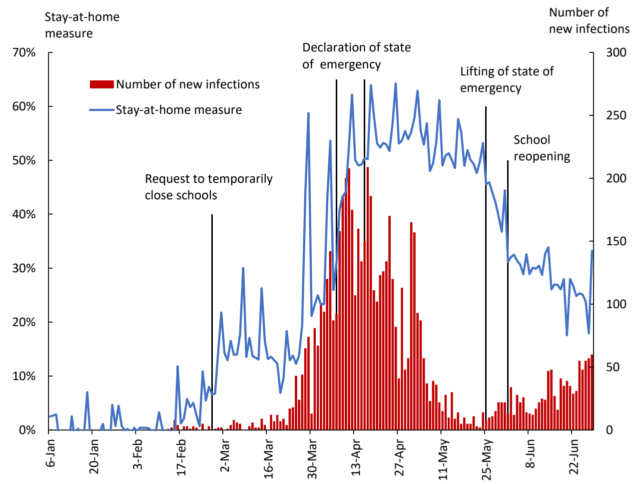
although differences between counties under lockdown and not under lockdown certainly existed, they were not that large. Meanwhile, Rojas et al. (2020) focus on the fact that the timing of school closures differed across states and examine whether there were differences in new claims for unemployment insurance between states that at a particular time were under lockdown and those that were not. They find that there were no statistically significant differences. That changes in American's behavior may not be due to (legally binding) lockdowns was also pointed out by Chetty et al. (2020). These studies highlight that changes in behavior in the United States were not the result of legally binding measures taken by the government but the result of U.S. citizens' voluntary response to infections. [5]

The third area to which our study is related is research on the effects of lockdown policies in Asia. Focusing on China and using mobile phone location data, Fang et al. (2020) use difference-in-differences estimation to examine how the movement of people changed following the lockdown in Wuhan. They found that, controlling for other factors, the effect of the lockdown itself was to reduce the inflow of people into Wuhan by 76%, the outflow by 56%, and the movement of people within Wuhan by 54%. Meanwhile, focusing on South Korea, Aum et al. (2020) compared the Daegu-Gyeongbuk area, where infections were especially prevalent, with other areas in South Korea using difference-in-differences estimation. They found that an increase in the number of infected persons per 1,000 population by one was associated with a reduction in employment by 2–3%. In a study on Japan, Cato et al. (2020) examined how households' expectations regarding the economic outlook differed immediately before and after the Governor of Tokyo held a press briefing on March 25 to explain the severity of infections. They show that, after hearing the press briefing, people's concerns about employment, prices, shortages of medical supplies and daily necessities, etc., increased.

The remainder of this study is organized as follows. Section 2 provides an overview of COVID infections in Japan. Sections 3 and 4 respectively describe the methods and data used in the empirical analysis. Next, Section 5 outlines the results of the empirical analysis. Finally, Section 6 presents the conclusion and discusses the policy implications of this study.

[5] A study using data for a country other than the United States is that by Sheridan et al. (2020), who compare Denmark, where the government used legal interventions on outings and economic activities to halt the spread of infections, and Sweden, where the government did not make such interventions. They find that the decline in economic activity, as observed in bank transaction data, was not significantly different and argue therefore government interventions were not a major cause of the economic contraction. Chudik et al. (2020) extend the standard susceptible-infected-recovered (SIR) model to incorporate compulsory and voluntary social distancing. Their simulation results show that compulsory social distancing is effective in flattening the epidemic curve, especially when it is targeted towards individuals most likely to spread the infection, while voluntary social distancing is relatively ineffective.

Figure 1: Stay-at-Home Measure and Number of New Infections, Tokyo



2 Outbreak of COVID-19 and Policy Responses in Japan

The first reported case of a COVID-19 infection in Japan – of a man who had traveled to Wuhan, China – was on January 16, 2020. Then, on February 5, 10 passengers of a cruise ship docked at Yokohama Port were confirmed to have caught the virus. The first death in Japan was reported on 10 February. Infections in Japan began to rise in earnest from the second half of February, and as of February 29, the cumulative number of infections had reached 242. Infections further accelerated in early March, so that by the end of the month the cumulative number of infections had reached 2,234. In response to the spread of infections, the government on February 27 requested elementary, junior high, and high schools nationwide to temporarily close, and on March 24 decided to postpone the Tokyo Olympic Games scheduled for the summer of 2020. Furthermore, on April 7, a state of emergency was declared for seven prefectures including Tokyo, and on April 16, this was expanded to all prefectures.

Figure 1 shows the number of daily new infections in Tokyo, represented by the orange bars. The number of new infections increased rapidly in late March, exceeding 100 on March 17 and exceeding

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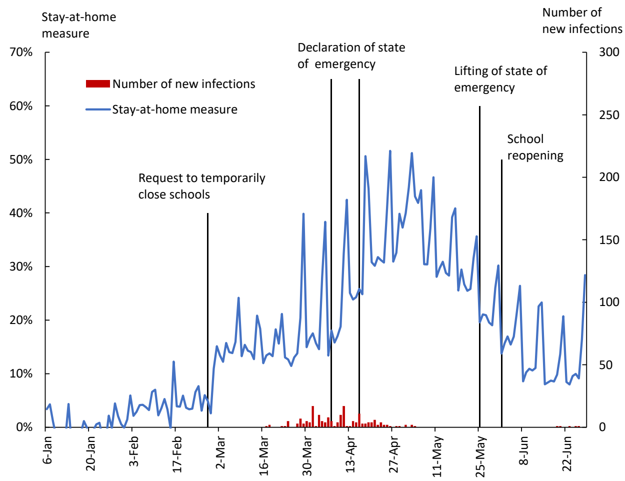
200 on April 10. With the declaration of the state of emergency, the number of new infections decreased and fell to almost zero in mid-May. However, the number of new infections started to increase again in the second half of May.

The blue line in Figure 1 is the stay-at-home measure created using mobile phone location data (details of how the measure is constructed are provided below). The line shows the extent to which Tokyo residents refrained from leaving their home compared to January 2020, before the pandemic. The figure indicates that as the number of new infections increased, people increasingly stayed at home. This shows that people updated their information on infections based on the number of new infections announced daily by the Tokyo governor and changed their behavior to avoid infection. Moreover, the stay-at-home measure jumped following the request for the temporary closure of schools on February 27 and the declaration of the state of emergency on April 7, showing that the government's measures changed people's behavior. Interestingly, the stay-at-home measure also increased on April 16, when the state of emergency was expanded to all prefectures. The state of emergency for Tokyo had already been declared on April 7, and the measure on April 16 should not have directly affected the residents of Tokyo. However, it is possible that although the measures targeted other prefectures, Tokyo residents obtained new information on the spread of infections from the government's announcement of the measure. Another point to note is that the stay-at-home measure remained at a high level of over 20% and thus higher than in January despite the fact that the state of emergency was lifted from late May to early June and schools were reopened. If the government's request to refrain from leaving home was the main reason for the change in people's behavior, the stay-at-home measure should have dropped to its original level as the request was lifted. That has not been the case, suggesting that a significant part of the change in people's behavior is voluntary.

While Tokyo is the prefecture with the highest number of infections in Japan, some prefectures have had zero or very few infections. Figure 2 shows an example of a prefecture with a small number of infections. Specifically, it shows Ibaraki prefecture, which is located northeast of Tokyo. By the end of June, the total number of infections in Ibaraki prefecture was 179, which is only 3% of the number for Tokyo. The stay-at-home measure for Ibaraki prefecture has shown a rise since the beginning of March, although not to the same extent as that for Tokyo. The stay-at-home measure jumped immediately after the request for schools to close on February 27 and the declaration of the state of emergency on April 16. This indicates that the government's request to refrain from leaving home had

Figure 2: Stay-at-Home Measure and Number of New Infections, Ibaraki



a certain effect even in areas with few infections such as Ibaraki prefecture. In addition, the stay-at-home measure for Ibaraki prefecture also shows a jump on April 7, when the state of emergency was declared for seven prefectures including Tokyo, but not Ibaraki itself. This suggests that residents of Ibaraki prefecture have been paying close attention to the situation in areas with high infections and have changed their behavior based on this.

3 Methodology

We construct and examine the stay-at-home measure for each of the 47 prefectures of Japan for the period from January 6 to June 28, 2020. Using this panel data, we identify the following.

First, we identify the "intervention effect" and the "information effect." Government policies such as the declaration of the state of emergency and school closures occurred at different times across prefectures, and by using these differences in timing, it is possible to determine which of the two effects was responsible for changes in people's behavior. For example, a state of emergency was declared in Tokyo on April 7, but at that time no state of emergency was declared for Tochigi prefecture, which is located 100km north of Tokyo, about an hour away on the bullet train. A state of emergency was

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declared in Tochigi on April 16. Therefore, there was no intervention effect in Tochigi prefecture from April 7 to 15. However, people in Tochigi prefecture were aware that the state of emergency had been declared in Tokyo, so there was an information effect, and the stay-at-home measure rose accordingly. On the other hand, people in Tokyo refrained from leaving home due to both the intervention and information effects, and the stay-at-home measure rose. Therefore, assuming that people in Tokyo and Tochigi had the same information about infections and there was no difference in the information effect between the two prefectures, the intervention effect can be extracted by observing the difference in the stay-at-home measure between the two prefectures. As for the declaration of the state of emergency, not only did the time when it was declared differ across prefectures, but the lifting also occurred in three waves, so that differences in the timing of the lifting of the state of emergency can also be used to identify the intervention effect. Similarly, with regard to school closures, the timing of when school closures were lifted varies widely across prefectures, and this can also be used to identify the intervention effect. Meanwhile, since all measures against COVID-19 are carried out at the prefectural level, there are few differences across smaller administrative units within the same prefecture such as municipalities.

The second identification we carry out is to examine whether changes in people's behavior depend on infections in their surroundings or in Japan as a whole. As seen in Figure 2, even in prefectures with a small number of new infections, people refrained from leaving home. This suggests that people may be making decisions about refraining from leaving home in response to the number of infections nationwide, not the number of infections in the prefecture. Since infections are concentrated in metropolitan areas such as Tokyo, the number of infections in such metropolitan prefectures and the number of infections nationwide are strongly correlated. However, in other prefectures, the number of infections in that prefecture and the nationwide number are only weakly correlated. Using this property, we can estimate to what extent the change in people's behavior is due to the number of infections within a prefecture or the number of infections nationwide.

The empirical approach used in this study is as follows. The stay-at-home measure at time t in prefecture i is denoted by y_{it}. The number of new infections at time t in prefecture i is denoted by $\tilde{x}_{it}$. The number of new infections nationwide is denoted by $\tilde{x}_t$. The distribution of the number of new infections is skewed to the right because the number of new infections is much larger in a small number of prefectures such as Tokyo than most other prefectures. While many existing studies use logarithms to cope with such highly skewed distributions, for some of the prefectures in Japan the number of new infections is zero on some days, so that we cannot take logarithms. Following Goolsbee and Syverson (2020), we transformed $\tilde{x}_{it}$ and $\tilde{x}_t$ using the inverse hyperbolic sine. Specifically, we define $x_{it} \equiv \ln(\tilde{x}_{it} + \sqrt{\tilde{x}_{it}^2 + 1})$ and $x_t \equiv \ln(\tilde{x}_t + \sqrt{\tilde{x}_t^2 + 1})$. The estimation equation used in this study is as follows:

$$
\begin{aligned}
y_{it} = \mu_i + & \underbrace{\alpha_0 D_{it}(\text{Emergency declaration}) + \beta_0 D_{it}(\text{School closure})}_{\text{Intervention effect}} \\
& + \underbrace{\sum\nolimits_k \alpha_k A_t(E_k) + \sum\nolimits_k \beta_k A_t(C_k) + \gamma_1 x_{it} + \gamma_2 x_t}_{\text{Information effect}} + \epsilon_{it}
\end{aligned}
\tag{1}
$$

where μ_i represents the effect unique to prefecture i. D_{it}(Emergency declaration) is a dummy variable that takes 1 when the state of emergency is active at time t in prefecture i, and 0 otherwise. Similarly, D_{it}(School closure) is a dummy variable that takes 1 when schools are closed at time t in prefecture i, and 0 otherwise. $A_t(E_k)$ represents the government's kth announcement regarding the state of emergency. It is a dummy variable that takes 1 in all prefectures after the kth announcement. Similarly, $A_t(C_k)$ is a dummy variable that represents the government's announcements with regard to school closures.

$\alpha_0 D_{it}$(Emergency declaration) and $\beta_0 D_{it}$(School closure) represent the intervention effect. On the other hand, since $\sum \alpha_k A_t(E_k)$ and $\sum \beta_k A_t(C_k)$ represent the effect of announcements with regard to the two policies on the stay-at-home measure, they represent the information effect. Moreover, the response of the stay-at-home measure to the number of new infections in the prefecture and in Japan overall can also be interpreted as representing the information effect.[6]

In Eq. (1), it was assumed that the source of people's information on infections was government policy announcements and the number of new infections. However, people may be able to obtain information about infections by other means. To take this into account, we also conduct estimations

[6]As explained earlier, the values of x_{it} and x_t are obtained by transforming the number of infections using the inverse hyperbolic sine. The coefficients on these two variables, γ_1 and γ_2 indicate that if the number of new infections in a prefecture increases by, for example, 1%, the stay-at-home measure increases by $0.01 \times \gamma_1$ percentage points. For more details on the inverse hyperbolic sine transformation, see Bellemare and Wichman (2020).

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using the following equation:

$$y_{it} = \mu_i + \underbrace{\alpha_0 D_{it}(\text{Emergency declaration}) + \beta_0 D_{it}(\text{School closure})}_{\text{Intervention effect}} + \underbrace{\lambda_t + \gamma_1 x_{it}}_{\text{Information effect}} + \epsilon_{it} \quad (2)$$

where λ_t is a time dummy. The specification of the intervention effect is the same as in Eq. (1). On the other hand, regarding the information effect, government policy announcements are expressed as a time fixed effect, since such announcements provide the same information to the residents of all prefectures. For the same reason, the number of new infections nationwide is also expressed as a time fixed effect. However, the time fixed effect differs from Eq. (1) in that it also contains information other than about infections.

Meanwhile, it should be noted that in Eq. (2), it is assumed that except for information on the number of new infections in the prefecture, the residents of all prefectures have the same information about infections, and the impact on their behavior is identical. In practice, however, the information that people have and how they react to information may differ from region to region. For example, the same government announcement may be perceived differently in areas with more infections than in areas with fewer infections. In Section 5, we divide Japan into seven regions and conduct estimations that take into account the possibility that the time fixed effect may differ across regions.

4 Data

4.1 The stay-at-home measure

For our location data, we use the "Mobile Spatial Statistics" provided by DoCoMo Insight Marketing.[7] The Mobile Spatial Statistics provide location records of about 78 million DoCoMo mobile phones at 10-minute intervals. Specifically, the mobile phone base stations in a particular area know which mobile phones are in the area. Based on this information, and dividing Japan into a mesh of 500m$\times$500m squares, DoCoMo compiles and publishes data on how many mobile phones are in a certain mesh element at a particular time (in 10-minute intervals), together with information on the age and sex of the owners of those mobile phones as well as the area where they live. However, mesh elements with a very small number of phones are excluded to protect individuals' privacy.

[7] For details, see `https://mobaku.jp/`.

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Using this data, we construct our stay-at-home measure in the following two steps. The first step consists of the detection of residential areas. For a certain mesh element, we count the average number of people in the time from midnight to 5am and take this as the nighttime population of that mesh element. Similarly, we count the number of people in the time from 9am to 5pm and take this as the daytime population of that mesh element. An area can then be regarded as a commercial area if the daytime population is greater than the nighttime population and as a residential area if the daytime population is smaller than the nighttime population. Specifically, we define a mesh element as a residential area if it satisfies daytime population/nighttime population < 0.8 as of January 2020, i.e., before the pandemic. The estimation results presented below were qualitatively the same when we set the threshold to 0.9 or 0.7.

The second step is the calculation of the ratio of those leaving their homes. For mesh elements that in the first step were identified as residential areas, we calculate the number of people leaving home by counting the nighttime population and daytime population on a certain day and subtracting the daytime population from the nighttime population. Next, for each prefecture, we calculate the number of people leaving their homes each day by aggregating the number of people that have left their homes in each mesh element. For example, in Tokyo, as of January (i.e., before COVID), residential areas had a nighttime population of approximately 5.3 million, while the daytime population on weekdays was approximately 3.6 million, so that the number of persons leaving their homes was approximately 1.7 million.

Finally, we take the number of persons leaving their homes in January 2020 (January 6 to 31), i.e., before the outbreak of COVID-19, as the number of persons leaving their homes during normal times, and then calculate for each prefecture and day the percentage difference from the number of people leaving their homes during normal times. We use the deviation rate multiplied by -1 as the stay-at-home measure.[8]

4.2 Number of new infections

The central government and prefectures announce the number of new infections daily. The date of infection is the day when a doctor confirms that a person's polymerase chain reaction (PCR) test

[8]See Mizuno et al. (2020) for details of the calculation procedure for the stay-at-home measure.

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was positive (test result date).[9] We use figures from the database constructed and published by JAG Japan Co., Ltd.[10] The number of new infections varies greatly depending on the day of the week. In the analysis here, we assume that people make their decision on whether to leave their homes or not based on the trend in new infections over the preceding week, and we therefore use the moving average over the preceding week including the day in question.

4.3 Government's measures against the spread of COVID-19

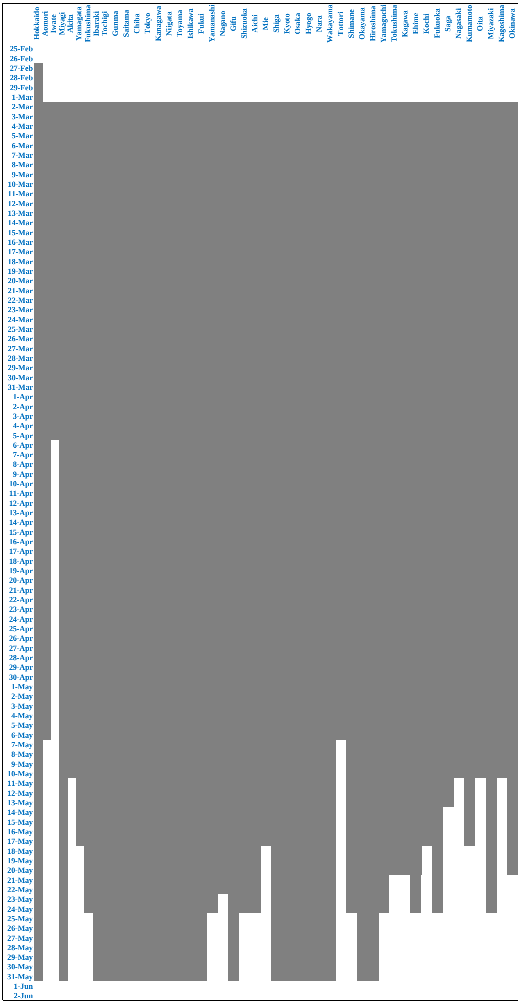
School closures On February 27, the government requested all elementary schools, junior high schools, high schools, and special needs schools to be closed from March 2 onwards. In response to this, all prefectures except Hokkaido closed schools from March 2.[11] We constructed the following two dummy variables for school closures. *School closure* is a dummy variable that takes 1 during the period schools were closed in a particular prefecture, and 0 otherwise. Specifically, except for Hokkaido, the dummy takes 1 from March 2, the day that schools were closed, until the date on which schools were opened again in a particular prefecture. For Hokkaido, the dummy takes a value of 1 from February 27, the day on which schools were closed in that prefecture. The date of the reopening of schools varies widely across prefectures: the earliest date was April 6, while the latest date was June 1. The start and end dates for school closures for each prefecture are shown in Figure 3. Note that if a prefecture closed schools again within a short time of reopening them, we do not regard this as reopening. The second dummy variable, *School Closure Announcement*, represents the announcement of the government's request for schools to close and is set to 1 for all prefectures except Hokkaido from the day after February 27, when the government made the request. For Hokkaido, the dummy is set to 1 from the day following the announcement for Hokkaido on February 26. Meanwhile, the reopening of schools was decided at the prefecture level. Therefore, while the prefectures made announcements, the central government did not, so that we do not use dummies representing the announcement of the end of school closures.

[9] There are two possible ways to define the date of infection: the date a person actually contracted the virus, and the date infection with the virus is confirmed. However, since the infected person may not necessarily know for certain when and where they contracted the virus, the date of infection is often impossible to determine. On the other hand, the date of a test result is clearly recorded. Since the number of infections reported in newspapers and on TV is based on the date of the test result, it is likely that people's decision-making on whether to leave their home is affected by the number of infections based on the date of the test result.

[10] See `https://gis.jag-japan.com/covid19jp`. Persons for which the prefecture of residence is unknown or who reside abroad are excluded from the sample.

[11] For details, see `https://www.mext.go.jp/content/20200304-mxt_kouhou02-000004520_1.pdf` (in Japanese).

Figure 3: Start and End of School Closures



State of emergency The government declared a state of emergency for seven prefectures (Saitama, Chiba, Tokyo, Kanagawa, Osaka, Hyogo, Fukuoka) on April 7, and expanded the state of emergency to

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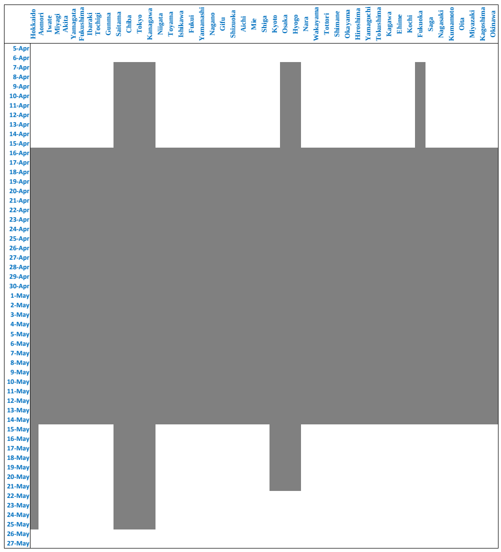
Figure 4: Start and End of State of Emergency



all prefectures on April 16.[12] The state of emergency was lifted in 39 prefectures with few infections on May 14, in three more prefectures on May 21, and finally in the remaining five prefectures including Tokyo on May 25. We also construct two types of dummies for the state of emergency. The first, *State of Emergency*, is a dummy that takes 1 when the state of emergency is active in a particular prefecture, and 0 otherwise. For example, for Tokyo, the *State of Emergency* dummy is set to 1 from April 8, the day after a state of emergency was declared for Tokyo, to May 25, when it was lifted. The start and end dates of the state of emergency for each prefecture are shown in Figure 4. The second type of dummies, *State of Emergency Announcement*, represents the government's announcements regarding the start and end of the state of emergency, and takes 1 for all prefectures from the day after the announcement. Specifically, there were two announcements regarding the start of the state of emergency (April 7 and 16) and three announcements regarding the end of the state of emergency (May 14, May 21, May 25), and we construct five dummy variables, i.e., one for each date.

[12]Details of which activities were restricted differed by prefecture. For details, see `https://www3.nhk.or.jp/news/special/coronavirus/tokyo/` (in Japanese).

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4.4 Other factors affecting whether people left their homes

To take other factors into account, a rain dummy (*Rain*) and a dummy for weekends and public holidays (*Weekend/Holiday*) are used. The rain dummy takes 1 if the amount of precipitation in the prefectural capital was greater than 0, and takes 0 otherwise. Precipitation data were obtained from the Japan Meteorological Agency website. The *Weekend/Holiday* dummy takes 1 for Saturdays, Sundays, and public holidays, and 0 otherwise.

5 Results

5.1 Baseline Regressions

We used the stay-at-home measure, expressed in percent, as the dependent variable and conducted the estimations using a fixed effects regression model. The results are presented in Table 1. In specification (1), the estimation was performed using the *School Closure* dummy and the *State of Emergency* dummy as explanatory variables, and adding the *Rain* and *Weekend/Holiday* dummies as other explanatory variables. The estimation results indicate that both school closures and the state of emergency had a significant effect, with the former raising the stay-at-home measure by 11 percentage points and the latter raising it by 16 percentage points.

As highlighted by Coibion et al. (2020) and others, the increase in the number of infections not only triggered government responses, such as the declaration of a state of emergency in Japan's case, it also had the effect of increasing people's fear of infection and led them to voluntarily refrain from leaving their homes. The increase in the stay-at-home measure in specification (1) thus may be due to an increase in the number of infections rather than government measures. Therefore, in specification (2), we added the number of new infections within the prefecture and the number of new infections nationwide as explanatory variables. Adding those variables reduces the coefficients on the *School Closure* and *State of Emergency* dummies from specification (1). However, the coefficients on all dummy variables remain significantly different from zero, indicating that the school closures and the state of emergency still had a statistically significant effect on the stay-at-home measure even after controlling for the number of infections.

Next, in specification (3), in order to control for changes in the stay-at-home measure due to the announcement effect of school closures and the state of emergency, we added a dummy for the

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Table 1: Baseline Results

	(1)	(2)	(3)	(4)	(5)
School Closure (SC)	11.238***	4.200***	8.265***	7.210***	4.873***
	(0.346)	(0.349)	(0.380)	(0.773)	(0.816)
State of Emergency (SE)	15.886***	12.091***	8.235***	7.923***	7.082***
	(0.695)	(0.453)	(0.835)	(0.807)	(0.920)
SC Announcement (Feb. 27)			0.203		
			(0.373)		
SE Announcement (Start, Apr. 7)			2.841***		
			(0.542)		
SE Announcement (Start, Apr. 16)			4.594***		
			(0.672)		
SE Announcement (End, May 14)			5.155***		
			(0.950)		
SE Announcement (End, May 21)			-0.304		
			(0.368)		
SE Announcement (End, May 25)			-4.423***		
			(0.538)		
No. of New Infections Within Prefecture		1.344***	2.382***	2.831***	2.199***
		(0.547)	(0.473)	(0.486)	(0.328)
No. of New Infections Nationwide		2.367***	0.845***		
		(0.100)	(0.111)		
Rain	1.904***	1.924***	1.815***	1.506***	0.765***
	(0.315)	(0.204)	(0.151)	(0.189)	(0.185)
Weekend/Holiday	7.034***	7.083***	7.150***		
	(0.245)	(0.241)	(0.232)		
Obs.	8225	8225	8225	8225	8225
Adjusted R^2	0.694	0.799	0.859	0.937	0.964
FEs	Prefecture	Prefecture	Prefecture	Prefecture Day	Prefecture Day×Region

Notes: Figures in parentheses are cluster-robust standard errors. *, **, and *** denote statistical significance at the 10%, 5% and 1% level, respectively. For the number of new infections within prefectures and nationwide, the inverse hyperbolic sine transforms ($\text{arcsinh}(x) = \ln(x + \sqrt{x^2+1})$) were used.

announcement of school closures by the government on February 27, while for the state of emergency we added the two dummies for the announcement of the start of the state of emergency and the three dummies for the announcement of the lifting of the state of emergency. For instance, the *State of Emergency Announcement* (*Start, Apr. 7*) dummy takes 1 for all prefectures from April 8, the day after the announcement, and if the coefficient is positive, this indicates that the state of emergency

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announcement on April 7 raised the stay-at-home measure regardless of whether the state of emergency applied to a particular prefecture.

Looking at the effects of the closure of schools based on the results for specification (3), the coefficient on the *School Closure* dummy is around 8.3 and significantly different from zero. However, the coefficient on the *School Closure Announcement* dummy is small and not significantly different from zero. This shows that there was a large intervention effect that exceeded the information effect. Next, looking at the effect of the state of emergency declaration, the coefficient on the *State of Emergency* dummy is 8.2, showing that the intervention effect of the state of emergency had more or less the same size as the closure of schools. Moreover, the coefficients on the two dummies for the announcement of the start of the state of emergency are both positive and significant, and the effect was to increase the stay-at-home measure by 7.4 percentage points in total. On the other hand, the sum of all coefficients for the announcement of the lifting of the state of emergency is close to zero. The above results show that the declaration of the state of emergency had the effect of raising the stay-at-home measure through both the intervention effect and the information effect.

Next, specification (4) includes time fixed effects. Time fixed effects capture factors that have the same effect for all prefectures. They capture the effect that people refrain from leaving their homes in response to various other types of information about the pandemic, not only of government announcements about school closures and the state of emergency. The coefficients on both the *School Closure* dummy and the *State of Emergency* dummy are somewhat smaller than in specification (3) but remain statistically significant.

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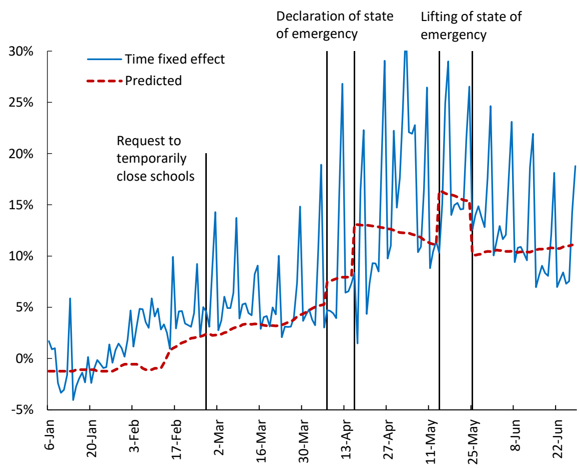
Figure 5: Estimates of Time Fixed Effects



The blue line in Figure 5 shows the coefficients on the time dummies obtained in this estimation. The time fixed effects show two jumps in April. The first jump corresponds to the declaration of the state of emergency for seven prefectures on April 7, while the second one corresponds to the expansion of the state of emergency to all prefectures on April 16. The dotted red line in Figure 5 shows the predicted values obtained by regressing the time fixed effects estimated in specification (4) on the number of infections nationwide, the declaration of the state of emergency (April 7 and 16), the lifting of the state of emergency (May 14 and May 25), and the *Weekend/Holiday* dummy.[13] Looking at the results, although the predicted value captures changes in the time fixed effect, and much of the change in the time fixed effect is explained by variables such as the announcements regarding the state of emergency, developments in the predicted value during some periods differ from the actual time fixed effect. For instance, in early February, there is no change in the dotted red line despite the increase in the time fixed effect. The latter likely reflects the fact that the Diamond Princess, a cruise ship with confirmed cases of coronavirus on board called at Yokohama Port on February 3 and was put under quarantine on February 5, which was widely reported in the media and gained public

[13]Since in specification (3) the coefficients on the *School Closure Announcement* (*Feb. 27*) and the May 21 announcement of the lifting of the state of emergency were insignificant, they are excluded from the estimation here. We also set the coefficient on the *Weekend/Holiday* dummy to zero.

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attention. It is possible that this news led people in all prefectures to refrain from leaving their homes.

In specifications (3) and (4), we assumed that residents in all prefectures reacted to government announcements in the same manner. However, people's reaction may differ depending on where they live. Therefore, in specification (5), we divide Japan into seven regions (Tohoku, Kanto, Kinki, Chubu, Chugoku, Shikoku, and Kyushu) and, by specifying the time fixed effect as Day FE×Region, allow for the possibility that the time fixed effect may differ across regions. Looking at the estimation results, the coefficient on *School Closure* is 4.9, while the coefficient on *State of Emergency* is 7.1, which shows that the coefficient on *School Closure* has dropped significantly. However, the coefficients for both *School Closure* and *State of Emergency* continue to be positive and significant.

Let us compare the results of specification (5) with previous studies on the United States. Goolsbee and Syverson (2020) found that a shelter-in-place (S-I-P) order in a certain county reduced the number of customers visiting retail stores in that county by 7.6%. This is the intervention effect of S-I-P orders in the United States. To compare this with our results, we convert our estimate as follows. As the stay-at-home measure immediately before the state of emergency was declared was 0.17, the level of outings, relative in January 2020 before the outbreak of the pandemic, was 0.83. The coefficient on the *State of Emergency* dummy in specification (5) of 7.1 means that the level of outings dropped to 0.76 due to the intervention effect of the emergency declaration (i.e., 0.83-0.07=0.76), and the rate of change in outings is -8.6%. Thus, although the figures are not directly comparable, since Goolsbee and Syverson (2020) measure the reduction in outings using the number of customers visiting stores, the order of magnitude of their result and ours – a decline of 7.6% due to S-I-P orders in the United Sates and of 8.6% due to the declaration of the state of emergency in Japan – is quite similar. Interestingly, therefore, the intervention by the Japanese government in the form of a "request" and the legally binding lockdowns in the United States had more or less the same intervention effect.

Next, let us compare the coefficient on new infections within the prefecture in specification (5) with results for the United States. The coefficient on the number of new infections within the prefecture is 2.2, and, based on the level of the stay-at-home measure just before the state of emergency was declared, an increase in the number of new infections in the prefecture by 1% led to a reduction in outings of 0.026%.[14] On the other hand, Goolsbee and Syverson (2020) showed that an increase in

[14]The level of outings immediately before the state of emergency was declared was 0.83. The coefficient on new infections within the prefecture in specification (5) of 2.2 means that the level of outings dropped by 0.00022 in response to a 1% increase in the number of new infections, implying that the rate of change in outings is -0.026%.

the cumulative number of deaths from COVID-19 by 1% reduced the number of consumer visits by 0.03%. Although the figures cannot be directly compared, since one refers to the number of infections while the other refers to the number of deaths, they suggest that people in Japan and the United States changed their behavior more or less the same way in response to information on the spread of COVID infections.

How should the results of positive and statistically significant coefficients on the *School Closure* and *State of Emergency* dummies be interpreted? Regarding school closures, Prime Minister Abe proposed that schools be closed and requested local governments such as prefectures and municipalities, which have jurisdiction over schools, to do so. Local governments accepted this request, and four days after Prime Minister Abe's proposal, schools actually closed. This sequence of events suggests that Prime Minister Abe's request had compelling force on local governments. Presumably, following the decisions by local governments, students (and their parents) started to refrain from leaving their homes. The above-mentioned results that school closures had an intervention effect support this.

Turning to the state of emergency, in contrast with the lockdowns in China, the United States, and Europe, Japan's state of emergency was not legally binding. There is no rational reason for people to follow "requests and instructions" without penalties such as fines or arrests, and another explanation why the declaration of the state of emergency had the effect it did is needed. One possibility is that the government's request triggered a change in strategic relationships among companies. For example, a major issue when a firm considers whether to shorten working hours or switch to working from home is how other firms that it does business with react. If business partners do not shorten working hours or allow their employees to work from home, it is not desirable for the firm to switch on its own. And if all firms think like this, no firm will switch. However, if a firm's business partners switch to shorter working hours or to working from home, it is desirable for the firm to also switch. If this virtuous cycle is created, all firms will shorten their business hours and/or switch to working from home and, as a result, people will refrain from going out. It is possible that the government's "request" triggered a change in expectations about how other firms will respond, which may have led to coordinated restraint from going out.[15]

[15] Another possibility is that the government's request triggered an increase in social pressure to conform with restraint from going out. This is symbolized, for example, by the emergence of a "self-restraint police" ("virus vigilantes") that look for and criticize people who are out and about. See the following articles on such virus vigilantes: https://www.washingtonpost.com/world/asia_pacific/in-japan-busy-pachinko-gambling-parlors-defy-virus-vigilantes-and-countrys-light-touch-lockdown/2020/05/14/8ffee74e-9447-11ea-87a3-22d324235636_story.html

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Table 2: Estimation Distinguishing between Weekdays and Weekends/Holidays

	(1)	(2)	(3)	(4)
School Closure	10.040***	5.558***	13.553***	3.302***
	(0.293)	(0.936)	(0.557)	(0.700)
State of Emergency	13.621***	7.085***	18.616***	6.887***
	(0.869)	(0.970)	(0.580)	(0.768)
No. of New Infections Within Prefecture		2.178***		2.290***
		(0.369)		(0.318)
Rain	0.863***	0.722***	3.186***	1.039***
	(0.295)	(0.193)	(0.475)	(0.277)
Obs.	5499	5499	2726	2726
Adjusted R^2	0.682	0.951	0.679	0.973
Sample	Weekdays	Weekdays	Weekends and Holidays	Weekends and Holidays
FEs	Prefecture	Prefecture Day×Region	Prefecture	Prefecture Day×Region

Notes: Figures in parentheses are cluster-robust standard errors. *, **, and *** denote statistical significance at the 10%, 5% and 1% level, respectively. For the number of new infections within prefectures, the inverse hyperbolic sine transforms ($\text{arcsinh}(x) = \ln(x + \sqrt{x^2 + 1})$) were used.

5.2 Separate estimations for weekdays and weekends/holidays

The estimations in Table 1 took into account that the constant for weekends/holidays and weekdays may differ by including a *Weekend/Holiday* dummy. However, it is possible that not only the constant but also the coefficients on the independent variables may differ between weekends/holidays and weekdays. Whether people leave their homes on weekdays to a large extent depends on how their workplace or school responds to government requests. On the other hand, with regard to going out on weekends/holidays, it is individuals who make the decision, and hence the response depends on how individuals react to requests from the government. By comparing the degree of restraint from going out on weekdays and weekends/holidays, we can further investigate the reasons for the decline in outings.

Columns (1) and (2) in Table 2 show the estimation results using weekday observations only, while columns (3) and (4) show those using weekend/holiday observations only. Columns (1) and (3) correspond to specification (1) in Table 1, while columns (2) and (4) correspond to specification

https://www.japantimes.co.jp/news/2020/05/13/national/coronavirus-vigilantes-japan/.

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(5) in Table 1. Comparing the results in columns (1) and (3) shows, firstly, that the *School Closure* dummy is positive and significant not only for weekdays but also for weekends/holidays. Refraining from going out on weekends should be interpreted as voluntary, since school closures did not order people to refrain from going out on weekends/holidays. Second, the coefficients on the *School Closure* and *State of Emergency* dummies are both larger for weekends. Eichenbaum et al. (2020) point out that the pandemic affects the behavior of both workers and consumers. That is, workers hesitate to come into contact with others at the workplace and hesitate to go to the workplace, while consumers refrain from consuming because they worry about contact with others at the place of consumption. The estimation results here thus can be interpreted as suggesting that restraint from going out on weekends/holidays was larger because the change in behavior of consumers was greater than that of workers.

Like specification (1) in Table 1, on which they are based, specifications (1) and (3) in Table 2 do not distinguish between the intervention effect and the information effect, so that the coefficients on the *School Closure* and *State of Emergency* dummies contain both effects. On the other hand, specifications (2) and (4), in order to control for other factors affecting the stay-at-home measure, also contain the number of new infections in the prefecture and time dummies. The time fixed effects are specified such that they may differ across the seven regions.

Looking at the estimation results for specifications (2) and (4) in Table 2, we find, firstly, that the coefficients on the *School Closure* and *State of Emergency* dummies are larger for weekdays. It is not surprising that the intervention effect of school closures is greater on weekdays. On the other hand, with regard to the *State of Emergency* dummy, the greater intervention effect for weekdays suggests that workers responded more actively than consumers to the government's requests. This can be interpreted as resulting from firms actively promoting the shortening of business hours and the shift to work from home in response to the government's requests. Second, compared with specifications (1) and (3), the relative sizes of the *School Closure* and *State of Emergency* dummies for weekdays and weekends/holidays are reversed. This suggests that the information effect, in contrast with the intervention effect, is larger for weekends/holidays than weekdays. This can be interpreted as indicating that the information effect is driven by consumers, not by firms or workers.

5.3 Weighted least squares regression

The stay-at-home measure is created from smartphone data for each prefecture; however, since the number of smartphones differs across prefectures, the accuracy of the stay-at-home measure also differs across prefectures . To take this into account, in Table 3, we used weighted least-squares estimation, using the number of smartphones in the residential areas of each prefecture as weights.

Looking at the estimation results, the coefficients on the *School Closure* and *State of Emergency* dummies are very similar to those in Table 1. Specifically, the state of emergency had the effect of reducing outings by 20 percentage points compared to before the pandemic. Of these 20 percentage points, 7 percentage points were due to the intervention effect, while the remainder was due to the information effect. On the other hand, school closures had the effect of reducing outings by 12 percentage points compared to before the pandemic. Of these 12 percentage points, 5 percentage points were due to the intervention effect. Turning to the coefficients on the number of new infections, these are somewhat larger than in Table 1. A 1% increase in the number of new infections in a prefecture reduced outings in that prefecture by 0.022 percentage points.

Next, Figure 6 presents a decomposition of changes in the stay-at-home measure for Tokyo using the estimation results from specification (5) in Table 3. During the period examined here, the stay-at-home measure for Tokyo peaked at 55% on May 1, and dividing the increase from January into the contribution of the various components shows that the intervention effect of school closures contributed 5 percentage points and the intervention effect of the state of emergency contributed 7 percentage points, for a combined intervention effect of 12 percentage points. On the other hand, the contribution of the number of new infections within Tokyo prefecture was 12 percentage points, while the contribution of the time fixed effect was 30 percentage points, for a combined information effect of 42 percentage points. Thus, the intervention effect contributed about a quarter and the information effect about three quarters to the reduction in outings, indicating that the dominant channel for changes in behavior was the information effect.

6 Summary and Policy Implications

Recent theoretical research on COVID-19 suggests that people have an incentive to refrain from going out to reduce their risk of becoming infected. However, once individuals are infected and are

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Table 3: Weighted Least Squares Regressions

	(1)	(2)	(3)	(4)	(5)
School Closure (SC)	12.119***	3.413***	9.325***	8.398***	4.797***
	(0.269)	(0.529)	(0.523)	(1.169)	(1.002)
State of Emergency (SE)	20.181***	14.970***	8.339***	8.155***	7.045***
	(1.471)	(0.701)	(0.700)	(0.743)	(0.983)
SC Announcement (Feb. 27)			0.141		
			(0.527)		
SE Announcement (Start, Apr. 7)			4.413***		
			(0.774)		
SE Announcement (Start, Apr. 16)			6.482***		
			(0.466)		
SE Announcement (End, May 14)			3.489***		
			(0.887)		
SE Announcement (End, May 21)			0.146		
			(0.518)		
SE Announcement (End, May 25)			-2.943***		
			(0.896)		
No. of New Infections Within Prefecture		1.808*	2.960***	3.255***	2.234***
		(0.919)	(0.604)	(0.558)	(0.361)
No. of New Infections Nationwide		2.644***	0.468**		
		(0.245)	(0.216)		
Rain	3.408***	2.944***	2.638***	2.307***	0.873***
	(0.561)	(0.309)	(0.238)	(0.375)	(0.235)
Weekend/Holiday	6.143***	6.175***	6.292***		
	(0.378)	(0.369)	(0.360)		
Obs.	8225	8225	8225	8225	8225
Adjusted R^2	0.708	0.819	0.886	0.942	0.977
FEs	Prefecture	Prefecture	Prefecture	Prefecture Day	Prefecture Day×Region
Weights	No. of smartphone users in each prefecture as of Jan 2020				

Notes: Figures in parentheses are cluster-robust standard errors. *, **, and *** denote statistical significance at the 10%, 5% and 1% level, respectively. For the number of new infections within prefectures and nationwide, the inverse hyperbolic sine transforms ($\text{arcsinh}(x) = \ln(x + \sqrt{x^2 + 1})$) were used.

asymptomatic or have only minor symptoms, they have little incentive to avoid infecting others and hence to refrain from going out, so that they unwittingly may infect others. Given this externality, the health damage to society as a whole is magnified and can lead to socially undesirable consequences (see, for example, Eichenbaum et al., 2020). An important implication of this theoretical prediction is

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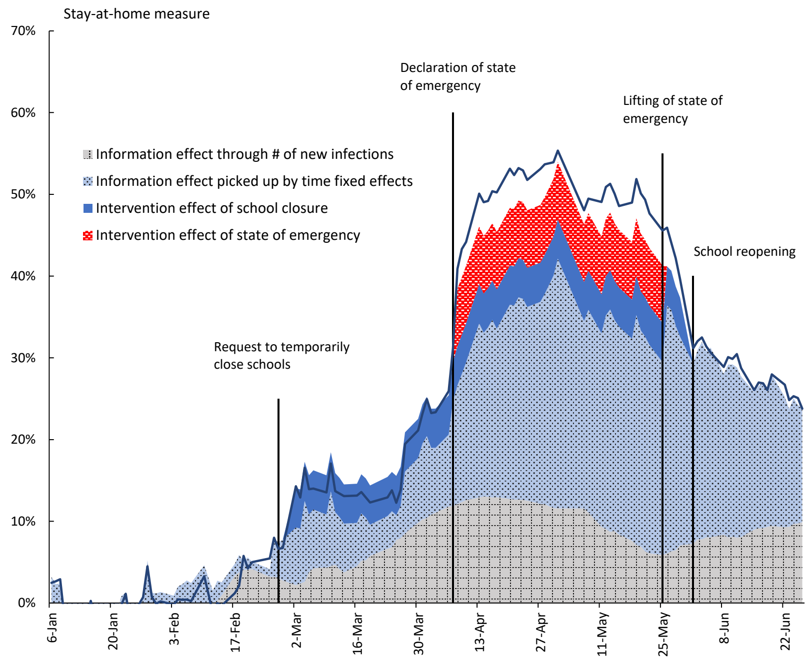
Figure 6: Decomposition of Changes in the Stay-at-Home Measure for Tokyo



the need for government intervention. In other words, from the perspective of preventing the spread of infections, it is justified that the government deprives people of the freedom to leave their homes.

In China, the United States, and Europe, legally binding interventions such as lockdowns have been used to prevent people from leaving their homes. On the other hand, in Japan, such intervention took the form of a government "request" calling on people to refrain from going out. At the time, there were many in Japan who were concerned that such a "request" would not have a sufficient effect. However, the analysis in this study has shown that the Japanese government's declaration of a state of emergency to a certain extent was successful in changing people's behavior. Specifically, in prefectures where a state of emergency was declared, outings (Number of persons going out×Time spent out) fell by 8.6%. According to Goolsbee and Syverson (2020), in the United States, the number of customers visiting retail stores decreased by 7.6% in counties that had imposed a lockdown. Thus,

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interestingly, the effects of government intervention were similar in Japan and the United States.

What does this finding mean? First, both the compulsory lockdown in the United States and the voluntary lockdown in Japan had a substantial impact on people's mobility. In both countries, the movement of people decreased substantially compared to normal times. However, the lockdowns were responsible only for part for this reduction. The remainder of the reduction was due to the fact that these measures increased people's awareness of the seriousness of the pandemic, for example through government announcements and the release of the number of infections and deaths; in other words, the remainder was due to the information accompanying these measures, which led people to voluntarily refrain from going out. Thus, the lesson of the experience both in Japan and the United States is that in order to contain infections, it is necessary to provide people with correct information in a timely manner and to encourage voluntary changes in behavior.

Second, if government intervention is needed, what is preferable: a compulsory lockdown or voluntary lockdown? The advantage of a compulsory lockdown is that its effects can be predicted to some extent, and uniform changes in behavior can be expected for a wide range of people (though not all). On the other hand, from citizens' point of view, a compulsory lockdown imposes severe restrictions on their personal freedom: to avoid being penalized with a fine or arrest, people have to respond in a uniform manner regardless of their individual circumstances. By contrast, in a voluntary lockdown, there is room for each citizen to decide whether or not to comply with the request, based on their own personal circumstances. On the other hand, though, the effect is uncertain. Whether a similar request for voluntary lockdown would be effective in Japan in the future or in other countries such as the United States or in Europe is unclear. While this study suggests that the government's request may have provided the impetus for cooperation to avoid the spread of COVID-19 infections, further analysis is needed.

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The impact of COVID-19 lockdowns and expanded social assistance on inequality, poverty and mobility in Argentina, Brazil, Colombia and Mexico[1]

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We use microsimulation to estimate the distributional consequences of covid-19-induced lockdown policies in Argentina, Brazil, Colombia and Mexico. Our estimates of the poverty consequences are worse than many others' projections because we do not assume that the income losses are proportionally equal across the income distribution. We also simulate the effects of most of the expanded social assistance governments have introduced in response to the crisis. This has a large offsetting effect in Brazil and Argentina, much less in Colombia. In Mexico, there has been no such expansion. Contrary to prior expectations, we find that the worst effects are not on the poorest, but those (roughly) in the middle of the ex ante income distribution. In Brazil we find that poverty among the afrodescendants and indigenous populations increases by more than for whites, but the offsetting effects of expanded social assistance also are larger for the former. In Mexico, the crisis induces significantly less poverty among the indigenous population than it does for the

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nonindigenous one. In all countries the increase in poverty induced by the lockdown is similar for male- and female-headed households but the offsetting effect of expanded social assistance is greater for female-headed households.

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1. Introduction

The recent COVID-19 pandemic has come at overwhelming health and economic costs to Latin America. In August, Brazil, Mexico Peru, Colombia and Chile are among the top ten countries in terms of infections; Peru, Chile and Brazil are among the top ten in terms of deaths per hundred thousand inhabitants.[1] To contain the spread of the virus, governments implemented lockdown policies of various degrees.[2] Inevitably, these measures caused a sharp reduction of activity, a fall in employment and income, and a rise in poverty and inequality.[3] In this paper we analyze the impact of lockdown policies on poverty, inequality, and income mobility (before and after the shock) in the four largest countries in Latin America: Argentina, Brazil, Colombia and Mexico.[4] In addition to lockdowns to control infection rates, governments have introduced new or expanded social assistance measures to varying degrees. We assess the extent to which these measures offset the negative effects of the lockdowns.

We obtain our estimates by simulating potential income losses at the household level using microdata from household surveys. The simulations first identify individuals whose income is "at risk" because they work in sectors in which the lockdowns have reduced or eliminated activity. We aggregate this at-risk income to the household level and then simulate actual losses using a range of two key parameters: the share of households with at-risk income that actually lose income and, of those who lose income, the share of at-risk income lost. We allow both parameters to range from zero to one-hundred percent, yielding an equally wide range of possible outcomes. To narrow our focus to reasonable possibilities, we choose a combination of the two key parameters that yields a decline in per capita income that comes closest to the IMF World Economic Outlook forecast from June 2020.[5] Even here, there are multiple possibilities of which we present two extremes, one in which a smaller proportion of households lose a large share of their income and another in which a larger number of households lose less income. Other cases are available in the appendix.

To complete the analysis, we construct a simulated income distribution that incorporates the losses we estimate and compare it with the *ex ante* distribution. We also simulate a third distribution that incorporates the effects of the lockdown plus any new compensatory social assistance measures each

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[1] https://coronavirus.jhu.edu/data/mortality Also, projections by the Institute of Health Metrics and Evaluation released on June 25, 2020 estimated that by October 1, total deaths due to Covid-19 in LAC would reach near 440,000. http://www.healthdata.org/news-release/correction-new-ihme-covid-19-model-forecasts-latin-american-caribbean-nations-will-see

[2] For a description of lockdowns by country see, for example, Pages et al. (2020).

[3] According to IMF (2020) and ECLAC (2020), the region's GDP could contract in 2020 by 9.4 and 9.1 percent, respectively.

[4] Note that mobility here refers to *ex ante/ex post* comparisons and not to mobility over time or intergenerational mobility.

[5] We use the IMF predictions for 2020 adjusted to per capita growth rates using data on population growth for latest year available. Then, following the method suggested by Ravallion (2003) and applied by Lackner et al. (2020), we assume a "pass-through" of GDP growth to household (gross) income growth of 0.85.

government has taken. In addition to comparing standard distributional statistics for each income distribution, we find it especially useful to examine income losses conditional on one's position in the *ex ante* distribution.[6]

In addition to the obvious observation that the impact of the crisis is huge by any standard, our approach yields four important conclusions. First, increases in poverty are worse than if we had assumed that each household's income declines by an equal proportion as many other studies do.[7] This is a convenient assumption for the rapid analysis the crisis demands, and a necessary one for those working only with macroeconomic data, but it is inaccurate. Second, contrary to many people's priors, the non-anonymous growth incidence curves show that the losses are greatest in the middle (roughly) of the *ex ante* distribution rather than among the poorest. This is because the social assistance policies put in place in most Latin American countries over the past 25 years (Stampini and Tornarolli, 2012) put a "floor" under the incomes of the poorest. Third, the governments that have introduced substantial expansions of existing social assistance or entirely new programs (Argentina and Brazil) have been able to offset a significant share of the poverty caused by the crisis.

Fourth, in Brazil we find that poverty among afrodescendants and indigenous populations increases by more than for whites, but the offsetting effects of expanded social assistance also are larger for the former. In Mexico, the crisis induces significantly less poverty among the indigenous population. In all countries the increase in poverty induced by the lockdown is similar for male- and female-headed households but the offsetting effect of expanded social assistance is greater for female-headed households.

This paper makes several contributions. First, most existing exercises that predict the impact of Covid-19 on poverty assume that income losses are proportional across the income distribution.[8] Based on existing information, however, the distribution of income is changing—and changing fast--during the lockdowns. In particular, "real time" telephone surveys seem to show that it is the poorer and informal sector workers who lose employment and income in larger proportion due to the "Covid-19 effect."[9] Our use of microsimulation allows us to relax the equal loss assumption and so incorporate distributional changes in the analysis. In particular, we use techniques analogous to non-anonymous growth incidence curves to describe income losses across the *ex ante* income distribution. Second, ours is the first work to describe the distributional consequences of the expanded social assistance governments have implemented in response to the crisis and the extent to which that assistance offsets the crisis' effect on poverty. Some countries have expanded social assistance considerably in response

[6] This is analogous to the non-anonymous growth incidence curves in Bourguignon (2011), albeit here describing a contraction.
[7] For example, CONEVAL (2020) (for Mexico), Gerszon Mahler et al. (2020), Sumner, Hoy and Ortiz-Juarez (2020), Valensisi (2020) and World Bank (2020a). Decerf et al. (2020) focus on a different question but they also assume no change in the distribution of income.
[8] ECLAC (2020), Universidad de los Andes (2020) (for Colombia) and Vos, Martin, and Laborde (2020) are exceptions.
[9] See, for example, Bottan, Vera-Cossio, and Hoffman, 2020; Brussevich, Dabla-Norris, and Khalid, 2020; Busso et al., 2020; INEGI, 2020; Universidad Iberoamericana, 2020).

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to the crisis, so ignoring this exaggerates the recent increases in poverty. Third, we estimate the impact of lockdowns and social assistance by race and ethnicity, and gender.

Our exercise has some important caveats. The microsimulations do not take into account behavioral responses or general equilibrium effects, so they yield first-order effects only. The depth and duration of the crisis is still uncertain and the economies could end up contracting by more (or less) than the IMF June 2020 projections. Our results depend on the specific assumptions we make about income sources that are "at risk" (which we detail in Table A2 in appendix) and the extent to which losses are concentrated or dispersed across households.[10]

2. Data and Methodology

For our simulations, we use the most recent household survey available in each country: Argentina: Encuesta Permanente de Hogares (EPH, 2019), Brazil: Pesquisa Nacional por Amostra de Domicilios Continua (PNAD, 2019), Colombia: Gran Encuesta Integrada de Hogares (GEIH, 2019), Mexico: Encuesta Nacional de Ingresos y Gastos de los Hogares (ENIGH, 2018). The household surveys for Brazil, Colombia and Mexico are representative at the national level. In Argentina the survey covers urban areas only that represent around 62 percent of the population. (For simplicity, in this paper we will refer to "Argentina" except in tables and figures where we shall add "urban").

We obtain our estimates by simulating potential income losses at the household level using microdata from household surveys. The simulations first identify individuals whose income is "at risk" because they work in sectors in which the lockdowns have reduced or eliminated activity (see more details below). We aggregate this at-risk income to the household level and then simulate potential losses using a range of two key parameters: the share of households with at-risk income that actually lose income and, of those who lose income, the share of at-risk income lost. Households who actually lose income (from the set of households with at-risk income) are randomly selected. We allow both parameters to range from zero to one-hundred percent (in 10 percent intervals), yielding a ten-by-ten matrix of possible income losses.[11]

We use gross income per capita as the welfare indicator. Gross income is defined as labor income plus rents, private transfers, pensions, and government cash transfers before any direct taxes. To maintain comparability across countries, we exclude own-consumption and the rental value of owner-occupied housing[12,13] We update gross incomes for Mexico to 2019 by the rate of growth of GDP per capita for

[10] A minor caveat is that our simulation of social assistance programs includes most but not all of the emergency programs implemented.

[11] The results for this ten-by-ten matrix by country are shown in Table 2 in the next section.

[12] This may result in some discrepancies with poverty estimates published in national and international databases such as the World Bank's PovcalNet.

[13] For Mexico and Colombia we do have information on these two incomes. Including own-consumption has little effect on the results as this is a small amount even for the poorest. What effect there is, however, is concentrated among poorest. The rental value of OOH reduces the share of at-risk income roughly equally across the income distribution for both countries.

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2019 multiplied by a so-called pass through of 0.85.[14] Also, for Mexico, we update gross incomes to take into account the significant reforms introduced to the cash transfers system in 2019.[15]

We base our determination of at-risk income on the economic sectors in which one works. We assume that income derived from work in sectors that are "essential" is not at risk, while any income earned in "nonessential" sectors is at risk. For Argentina and Colombia, the lockdown measures stated explicitly which sectors are essential. For Brazil and Mexico we use the ILO definition of essential sectors.[16]

At the household level, the at-risk incomes also include rental incomes and incomes of informal street vendors (regardless of the sector in which they work). For all other employment, we do not distinguish between formal and informal jobs. We assume that incomes from cash transfers programs, social security pensions, public employment and private transfers (e.g., remittances) are not affected by the lockdowns. Finally, we do not consider the income of white collar workers who are CEO's, managers and researchers with internet access at home to be at-risk even if they work in nonessential sectors.[17]

Our initial results for income losses provide a wide range of possibilities, too many to consider for further simulations. To narrow our focus, we choose outcomes that yield an overall loss of income per capita similar to the declines in GDP estimated in the June, 2020 World Economic Outlook predictions of the IMF.[18] In particular, we choose two scenarios that produce the income declines described: one in which a smaller proportion of households lose relatively large amounts of at-risk income; another in which many households lose a relatively smaller amount. We call these "concentrated losses" and "dispersed losses," respectively. We will say more on this below.

In addition to examining the *ex ante* and post-lockdown income distributions, we construct a third distribution that simulates most of the additional policies each government has put in place to cushion the impact of the crisis, including both expansions of existing social assistance and introduction of new programs. This yields a post-lockdown, post-policy response distribution. Table 1 gives a brief description of each government's policy responses that we can incorporate in our simulations of

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[14] The use of a pass through to convert GDP changes into changes in household disposable incomes was proposed by Ravallion (2003) and is applied by Lakner et al. (2020).
[15] The reforms are briefly described in Lustig and Scott (2019); details on how this update was carried out are available upon request.
[16] Decree 297/2020 (Argentina), Decree 457 of March 22nd of 2020 (Colombia), and ILO Monitor: COVID-19 and the world of work (Brazil and Mexico). Table A2 in the appendix shows the distribution of employment between at-risk and not-at-risk by sector.
[17] In the case of Argentina, the household survey does not allow us to identify internet access at home for white-collar workers.
[18] We use the IMF predictions for 2020 adjusted to per capita growth rates using data on population growth for latest year available. Then, following the method suggested by Ravallion (2003) and applied by Lackner et al. (2020), we assume a "pass-through" of GDP growth to household (gross) income growth of 0.85.

emergency social assistance programs.[19] Note that Mexico has provided no additional social assistance in the wake of the crisis.[20]

Table 1. Covid-19 New and Expanded Social Assistance Included in Simulations

Country	Program	Target population of new programs	Number of transfers	Amount of the transfers		Transfer as % of poverty line		Total beneficiaries (administrative data)	Fiscal cost in % of GDP
				LCU	USD	National	$5.50 PPP		
Argentina	AUH / AUE	-	1	ARG$3,100	US$46	34.7	77.5	4.3 million people	0.06%
	Ingreso Familiar de Emergencia*	Vulnerable, Informal workers	3	ARG$10,000	US$148	111.9	249.8	9 million people	1.14%
Brazil	Auxílio Emergencial*	Vulnerable, Informal workers	5	R$600	US$107	120.2	138.4	53 million people	1.95%
Colombia	Familias en Acción	-	3	COL$145,000	US$38	58.7	52.5	2.6 million households	0.10%
	Jóvenes en Acción	-	3	COL$356,000	US$92	144.1	128.9	204 thousand people	0.02%
	Colombia Mayor	-	3	COL$160,000	US$42	64.8	57.9	1.7 million people	0.07%
	Ingreso solidario*	Vulnerable, Informal workers	3	COL$160,000	US$42	64.8	57.9	3 million households	0.13%
	Bogotá solidaria*	Vulnerable, Informal workers	3	COL$233,000	US$60	94.3	84.4	300 thousand households	0.01%
Mexico	No additional social assistance								

Notes: * refers to new social assistance programs that were introduced in the first months of lockdowns. For a more detailed description (and sources) see Table A1 in appendix. Amount of the transfer in (local/USD) prices of May 2020. The number of beneficiaries in the simulations do not necessarily correspond exactly to those shown above because in Argentina the simulations apply to urban areas only. The numerator of the fiscal cost is obtained by multiplying the size of the transfers by the number of times it was given and the number of beneficiaries; the denominator equals GDP per IMF projections for 2020 (IMF, 2020).

3. Results

Composition of Pre-Crisis Income Across the Income Distribution

Figure 1 shows the composition of income by centile of the pre-crisis income (per capita) distribution across five categories: cash transfers, social security pensions, government salaries, other incomes not-at-risk, and incomes at-risk. There are two results to note. First, the share of income that is not at risk – everything but the dark blue area – is not equal across the income distribution as many studies assume, nor is it uniformly decreasing in income as it would be if the poorest were most at risk. Rather,

[19] We do not include the employment support programs. Their impact is implicit in the projected aggregate contraction in the sense that the income of the beneficiary households of these programs is not at risk. In order to estimate the benefit of this policy, proper pre-policy counterfactuals need to be generated,

[20] Mexico neither expanded nor introduced new safety nets. There were really only two mitigation policies and neither involves an additional transfer: beneficiaries of the noncontributory pensions and scholarships were given two months in advance (with total payments for the year unchanged, at least for now) and access to "credito a la palabra" (a loan without any guarantees) to mainly small and medium enterprises (which could become a transfer in retrospect if they are not paid back).

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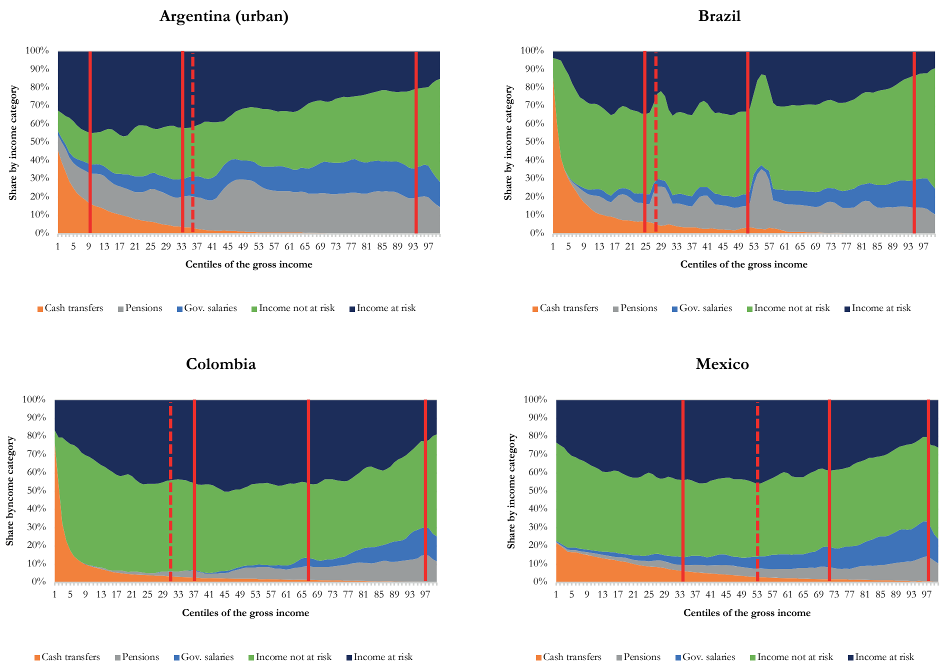
it is U-shaped with the greatest risk in the middle of the income distribution rather than either extreme. The very poorest households have an income floor (albeit low) that protects an important share of their income. Second, while the richest households also have relatively low income at risk, in Colombia, Mexico, and perhaps Brazil, this is due to their receipt of social security pensions and employment in the public sector, not labor income from essential sectors or white-collar jobs that permit remote work.[21]

Figure 1. Composition of Household Gross Income



Notes: The dashed line is the national poverty line and the bold lines are—from left to right-- the $5.50 (moderate poor), $11.50 (lower-middle class) and $57.60 (middle class) per day international lines (in 2011 PPP), respectively.
Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

Cells in Table 2 show the range of possible households' gross income losses (as a proportion of *ex ante* gross income) for each country as we vary both the probability that a household loses at-risk income (down the rows) and the share of that at-risk income it loses (across the columns).[22] For example, in the 10 percent-10 percent cell of this matrix we show the fall in income in percent

[21] Here we should note that the Argentina survey is urban only which may explain its variance from the other countries.
[22] See more details in the methodology section.

corresponding to the case in which 10 percent of the households (with at risk income) lose 10 percent of their income each (and so on). The possible losses are very wide indeed, ranging from near zero to 25-over 30 percent of pre-crisis income (depending on the country). To make the rest of the analysis manageable, we narrow our focus to outcomes that have income losses similar to the IMF's June 2020 World Economic Outlook projections for the decline in GDP per capita for each country, highlighted in yellow in Table 2. These form an "iso-loss" curve that runs through each table (see highlighted cells).[23] In particular, we choose the two such results closest to the corners of the table where either the smallest proportion of households lose much income (upper right) –"concentrated losses" scenario-- or the largest proportion of households lose smaller amounts of income (lower left) – "dispersed losses" scenario.

Table 2. Income Losses Matrix (as % of total household income)

Panel (a) Argentina (urban)

% of income lost / % households losing income	10%	20%	30%	40%	50%	60%	70%	80%	90%	100%
10%	0.3	0.5	0.8	1.1	1.3	1.6	1.9	2.1	2.4	2.7
20%	0.5	1.0	1.6	2.1	2.6	3.1	3.6	4.2	4.7	5.2
30%	0.8	1.6	2.4	3.2	4.0	4.8	5.6	6.4	7.2	8.0
40%	1.0	2.1	3.1	4.2	5.2	6.3	7.3	8.3	9.4	10.4
50%	1.3	2.6	3.9	5.2	6.5	7.8	9.1	10.4	11.7	13.0
60%	1.6	3.1	4.7	6.2	7.8	9.3	10.9	12.4	14.0	15.5
70%	1.8	3.7	5.5	7.3	9.2	11.0	12.8	14.7	16.5	18.3
80%	2.1	4.2	6.3	8.4	10.5	12.6	14.7	16.8	18.9	21.0
90%	2.4	4.8	7.1	9.5	11.9	14.3	16.7	19.0	21.4	23.8
100%	2.6	5.3	7.9	10.6	13.2	15.9	18.5	21.1	23.8	26.4

Panel (b) Brazil

% of income lost / % households losing income	10%	20%	30%	40%	50%	60%	70%	80%	90%	100%
10%	0.3	0.5	0.8	1.1	1.4	1.6	1.9	2.2	2.5	2.7
20%	0.5	1.1	1.6	2.2	2.7	3.3	3.8	4.4	4.9	5.4
30%	0.8	1.6	2.5	3.3	4.1	4.9	5.7	6.5	7.4	8.2
40%	1.1	2.2	3.3	4.4	5.5	6.6	7.6	8.7	9.8	10.9
50%	1.4	2.7	4.1	5.4	6.8	8.2	9.5	10.9	12.3	13.6
60%	1.6	3.3	4.9	6.5	8.1	9.8	11.4	13.0	14.6	16.3
70%	1.9	3.8	5.7	7.6	9.5	11.4	13.3	15.2	17.1	19.0
80%	2.2	4.3	6.5	8.7	10.8	13.0	15.2	17.3	19.5	21.6
90%	2.4	4.9	7.3	9.7	12.1	14.6	17.0	19.4	21.8	24.3
100%	2.7	5.4	8.1	10.8	13.5	16.2	18.9	21.6	24.3	27.0

23 We have also checked how poverty and inequality post-Covid vary as we move along the diagonal of the matrix shown in Table 2. The results can be seen for poverty in Figure A1 and for inequality in Figure A2 in the appendix. One can observe how the indicators accelerate in the negative direction (more poverty and more inequality, that is) as the order of magnitude of the two parameters is above 50-50 percent.

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Panel (c) Colombia

% of income lost / % households losing income	10%	20%	30%	40%	50%	60%	70%	80%	90%	100%
10%	0.3	0.7	1.0	1.4	1.7	2.0	2.4	2.7	3.1	3.4
20%	0.7	1.3	2.0	2.6	3.3	3.9	4.6	5.2	5.9	6.6
30%	1.0	2.0	2.9	3.9	4.9	5.9	6.9	7.8	8.8	9.8
40%	1.3	2.6	4.0	5.3	6.6	7.9	9.2	10.5	11.9	13.2
50%	1.7	3.3	5.0	6.6	8.3	9.9	11.6	13.3	14.9	16.6
60%	2.0	4.0	6.0	8.0	10.0	12.0	13.9	15.9	17.9	19.9
70%	2.3	4.7	7.0	9.4	11.7	14.0	16.4	18.7	21.1	23.4
80%	2.7	5.3	8.0	10.7	13.4	16.0	18.7	21.4	24.0	26.7
90%	3.0	6.0	9.0	12.0	15.0	18.0	21.1	24.1	27.1	30.1
100%	3.4	6.8	10.2	13.6	17.0	20.4	23.8	27.2	30.6	34.0

Panel (d) Mexico

% of income lost / % households losing income	10%	20%	30%	40%	50%	60%	70%	80%	90%	100%
10%	0.3	0.6	0.9	1.2	1.6	1.9	2.2	2.5	2.8	3.1
20%	0.6	1.3	1.9	2.5	3.1	3.8	4.4	5.0	5.7	6.3
30%	1.0	1.9	2.9	3.8	4.8	5.7	6.7	7.6	8.6	9.5
40%	1.3	2.5	3.8	5.1	6.4	7.6	8.9	10.2	11.4	12.7
50%	1.6	3.2	4.8	6.4	8.0	9.6	11.2	12.8	14.3	15.9
60%	2.0	3.9	5.9	7.8	9.8	11.7	13.7	15.6	17.6	19.6
70%	2.3	4.6	7.0	9.3	11.6	13.9	16.3	18.6	20.9	23.2
80%	2.6	5.3	7.9	10.6	13.2	15.8	18.5	21.1	23.8	26.4
90%	3.0	5.9	8.9	11.8	14.8	17.7	20.7	23.6	26.6	29.6
100%	3.3	6.6	9.9	13.1	16.4	19.7	23.0	26.3	29.6	32.8

Notes: Cells in yellow correspond to losses similar to the loss projections by IMF (2020); cells in dark yellow correspond to the "concentrated losses" and "dispersed losses" scenarios described in the text.

Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

Impact on poverty and inequality

For poverty, we estimate the incidence of poverty using two poverty thresholds: the national poverty lines and the US$5.50 a day international poverty line (in 2011 purchasing power parity).[24,25] We use the Gini coefficient to measure the impact on inequality.

Table 3 shows the change in poverty from *ex ante* income to post-lockdown income and from *ex ante* income to post-lockdown, post-policy response income. It is not surprising that the increases in poverty due to the lockdowns are very large for all countries, poverty lines, and scenarios (column 4). At national poverty lines, the results are quite similar across scenarios, suggesting that our results are robust to any particular pair of loss probability and loss share chosen from Table 2 so long as they produce a national decline in income per capita similar to the IMF's projections for GDP. At the $5.50

24 The national poverty line in 2011 PPP a day is equivalent to $12.3 in Argentina, $6.3 in Brazil, $4.9 in Colombia, and $7.8 in Mexico.

25 For Argentina, the conversion to 2011 PPP uses Buenos Aires city's CPI because the one produced by the National Statistics Institute (INDEC) went through a series of methodological changes that weakened its credibility. See, for example, Cavallo (2013).

poverty line, poverty increases do differ somewhat across scenarios for Argentina and Brazil, but remain large. Note also that at the $5.50 line the poverty increases are considerably smaller in the "dispersed losses" scenario than the "concentrated losses" scenario for Argentina and Brazil.[26]

Column 7 of Table 3 gives a second key set of results: in Argentina and Brazil, where governments have committed significant resources to new or expanded social assistance, those policies have offset a considerable amount of the lockdown-induced increase in poverty. Indeed, in Brazil the offset is almost complete at the national poverty line and more than complete at the $5.50 line. In Colombia and Mexico, where governments have dedicated much less (or nothing) to new social assistance spending, the effect is much smaller or nil.

Table 3. Incidence of Poverty

Panel (a) "Concentrated losses"

Country	*Ex ante*	*Ex post*	Change	New poor (in millions)	*Ex post* + Social Assistance	Change	New poor (in millions)
Panel (a) Headcount (National Poverty Line)							
Argentina (urban)	35.5	42.8	7.2	2.0	40.3	4.8	1.4
Brazil	28.2	34.6	6.4	13.5	31.5	3.4	7.1
Colombia	31.8	37.9	6.1	3.0	37.3	5.6	2.7
Mexico	53.8	60.1	6.4	8.0			
Panel (b) Headcount ($5.5 PPP Poverty Line)							
Argentina (urban)	10.9	19.1	8.2	2.3	16.8	5.9	1.7
Brazil	25.4	32.0	6.6	13.9	27.9	2.5	5.3
Colombia	37.6	43.6	6.0	2.9	43.0	5.4	2.7
Mexico	34.9	43.8	9.0	11.2			

Panel (b) "Dispersed losses"

Country	*Ex ante*	*Ex post*	Change	New poor (in millions)	*Ex post* + Social Assistance	Change	New poor (in millions)
Panel (a) Headcount (National Poverty Line)							
Argentina (urban)	35.5	43.1	7.6	2.1	40.7	5.1	1.4
Brazil	28.2	33.6	5.4	11.4	29.5	1.3	2.8
Colombia	31.8	36.9	5.2	2.5	36.4	4.6	2.3
Mexico	53.8	60.8	7.0	8.8			
Panel (b) Headcount ($5.5 PPP Poverty Line)							
Argentina (urban)	10.9	15.8	4.9	1.4	12.8	1.9	0.5
Brazil	25.4	29.8	4.5	9.3	25.2	-0.2	-0.4
Colombia	37.6	42.6	5.1	2.5	42.1	4.5	2.2
Mexico	34.9	42.9	8.1	10.1			

Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

[26] As a check on the importance of the assumptions we have made about which income is at-risk, we repeated our analysis assuming that *all* income (except for income from cash transfers, pensions and government salaries) is at-risk. We find the results to be broadly similar, though the increases in poverty and inequality are slightly less when we restrict our attention to outcomes with income losses similar in scale to the IMF's predictions for declines in GDP.

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Table 4 gives similar results for inequality. Column 4 has the difference between *ex ante* and post-lockdown income. Under the "concentrated losses" scenario, the increase in inequality is large in all countries, but it is less so in the "dispersed losses" scenario. In the former, a smaller proportion of households are losing almost all their at-risk income which shifts them far to the lower end of the income distribution, necessarily increasing inequality almost regardless of where they started. In the latter, each losing household's loss is smaller and so less likely to move a large number of households to the low end of the distribution.[27] As with poverty, the new social assistance measures implemented in Argentina and Brazil succeed in reducing or eliminating the lockdown-induced increase in inequality.

Table 4. Gini Coefficient

Panel (a) "Concentrated losses"

Country	*Ex ante*	*Ex post*	Change	*Ex post* + Social Assistance	Change
Argentina (urban)	0.444	0.486	0.042	0.469	0.025
Brazil	0.554	0.591	0.037	0.565	0.011
Colombia	0.550	0.578	0.028	0.574	0.024
Mexico	0.464	0.503	0.039		

Panel (b) "Dispersed losses"

Country	*Ex ante*	*Ex post*	Change	*Ex post* + Social Assistance	Change
Argentina (urban)	0.444	0.467	0.022	0.451	0.007
Brazil	0.554	0.570	0.016	0.545	-0.009
Colombia	0.550	0.564	0.014	0.560	0.010
Mexico	0.464	0.479	0.015		

Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

Differential Impact by Ethnicity and Gender

Table 5 presents results for the change in poverty across distributions by race in Brazil and ethnicity in Mexico.[28] In Brazil, the impact of the lockdown (column 4) on afrodescendants and indigenous populations is more severe for both poverty lines and both scenarios. At the same time, the newly introduced social assistance offsets more of the poverty increase for afrodescendants and indigenous populations, leaving the overall increase in poverty roughly the

[27] As expected, the Gini coefficient is in all cases lower for the "dispersed losses" than for the "concentrated losses" scenarios, but this is not true for the headcount because it depends on the density of the population around the poverty line. The squared poverty gap (FGT(2))—which is distribution-sensitive among the poor--follows the same pattern as the Gini coefficient (see Table A5 in appendix).

[28] These distinctions are not possible in the data from Colombia and Argentina.

same for both groups. In Mexico, the impact of the lockdown is much less for indigenous people than for whites.[29]

Table 5. Headcount Estimates by Race of the Household Head

Panel (a) "Concentrated losses"

	White					Afrodescendants and indigenous				
Country	*Ex ante*	*Ex post*	Change	*Ex post* + Social Assistance	Change	*Ex ante*	*Ex post*	Change	*Ex post* + Social Assistance	Change
Panel (a) Headcount (National Poverty Line)										
Brazil	27.2	33.5	6.3	30.6	3.4	35.2	42.5	7.2	38.5	3.2
Mexico	51.7	58.3	6.6			77.2	80.9	3.6		
Panel (b) Headcount ($5.5 PPP Poverty Line)										
Brazil	24.6	31.0	6.4	27.1	2.5	31.1	38.8	7.7	33.9	2.8
Mexico	32.1	41.4	9.3			66.0	71.4	5.4		

Panel (b) "Dispersed losses"

	White					Afrodescendants and indigenous				
Country	*Ex ante*	*Ex post*	Change	*Ex post* + Social Assistance	Change	*Ex ante*	*Ex post*	Change	*Ex post* + Social Assistance	Change
Panel (a) Headcount (National Poverty Line)										
Brazil	27.2	32.5	5.3	28.5	1.3	35.2	42.1	6.8	36.7	1.4
Mexico	51.7	58.9	7.2			77.2	81.7	4.5		
Panel (b) Headcount ($5.5 PPP Poverty Line)										
Brazil	24.6	28.9	4.3	24.3	-0.2	31.1	37.1	6.0	31.5	0.3
Mexico	32.1	40.4	8.3			66.0	71.7	5.7		

Notes: In Brazil, the afrodescendants and indigenous populations category includes individuals who self-reported as "black" and "indigenous." In Mexico, the category only includes those individuals who responded that they speak an indigenous language.

Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

Table 6 presents results for male- and female-headed households. The increase in poverty caused by the lockdowns (column 4) is broadly similar. Only in Argentina is the difference greater than one percentage point, and that only in the "concentrated losses" scenario. In Argentina and Brazil, the poverty increases from the *ex ante* to the post-lockdown, post-policy response distribution is less in female-headed households, indicating that the new social assistance introduced in response to the crisis has favored female-headed households. This reflects the emphasis these countries have placed on targeting them.

[29] In Brazil, the afrodescendants and indigenous populations category includes individuals who self-reported as "black" and "indigenous." In Mexico, the indigenous population is identified by those individuals who responded that they speak an indigenous language.

Table 6. Headcount Estimates by Sex of the Household Head

Panel (a) "Concentrated losses"

	Men					Women				
Country	*Ex ante*	*Ex post*	Change	*Ex post* + Social Assistance	Change	*Ex ante*	*Ex post*	Change	*Ex post* + Social Assistance	Change
Panel (a) Headcount (National Poverty Line)										
Argentina (urban)	33.6	41.0	7.4	39.0	5.4	38.7	45.0	6.3	42.5	3.8
Brazil	25.2	31.8	6.5	29.3	4.1	31.4	37.6	6.2	34.0	2.6
Colombia	30.1	35.9	5.9	35.5	5.5	34.9	41.5	6.6	40.6	5.8
Mexico	54.1	60.6	6.5			52.7	58.7	6.0		
Panel (b) Headcount ($5.5 PPP Poverty Line)										
Argentina (urban)	9.2	18.6	9.4	16.4	7.2	13.7	20.6	7.0	17.6	3.9
Brazil	22.7	29.3	6.6	26.2	3.4	28.2	34.8	6.5	29.8	1.5
Colombia	37.0	42.7	5.7	42.2	5.2	38.7	45.2	6.5	44.6	5.9
Mexico	35.6	44.8	9.2			32.6	40.9	8.3		

Panel (b) "Dispersed losses"

	Men					Women				
Country	*Ex ante*	*Ex post*	Change	*Ex post* + Social Assistance	Change	*Ex ante*	*Ex post*	Change	*Ex post* + Social Assistance	Change
Panel (a) Headcount (National Poverty Line)										
Argentina (urban)	33.6	41.5	7.9	39.0	5.4	38.7	46.0	7.3	43.3	4.6
Brazil	25.2	30.3	5.1	27.0	1.7	31.4	37.2	5.8	32.3	0.9
Colombia	30.1	35.3	5.2	34.7	4.7	34.9	40.1	5.2	39.4	4.5
Mexico	54.1	61.3	7.2			52.7	59.2	6.5		
Panel (b) Headcount ($5.5 PPP Poverty Line)										
Argentina (urban)	9.2	14.1	4.9	11.2	2.0	13.7	18.7	5.0	15.3	1.7
Brazil	22.7	26.8	4.1	23.1	0.3	28.2	33.2	4.9	27.5	-0.7
Colombia	37.0	41.8	4.9	41.3	4.4	38.7	44.1	5.4	43.5	4.8
Mexico	35.6	43.7	8.1			32.6	40.5	7.9		

Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

Coverage of Social Assistance

Figure 2 shows the coverage of social assistance transfers that existed before the crisis (on the left of each figure) and those measures plus any new or expanded social assistance implemented in response (on the right) for those who were poor before the crisis (in gray) and for those who became poor after the lockdown (in red). In both the "concentrated losses" and "dispersed losses" scenarios the two countries that have expanded social assistance significantly in response to the crisis show impressive increases in coverage for both the *ex ante* poor and the new poor which helps to explain their success at offsetting the poverty increase that lockdowns caused. It is also interesting that in these two countries the coverage of the *ex ante* social assistance measures is much higher for the *ex ante* poor, as it should be, but the difference narrows once the new policies are added to the mix, also as it should. These results suggest good targeting of both *ex ante* and new social assistance measures in Argentina and Brazil. Colombia also has a substantial increase in coverage for both the *ex ante* and the new poor, despite the limited budget it has dedicated to new social assistance measures.

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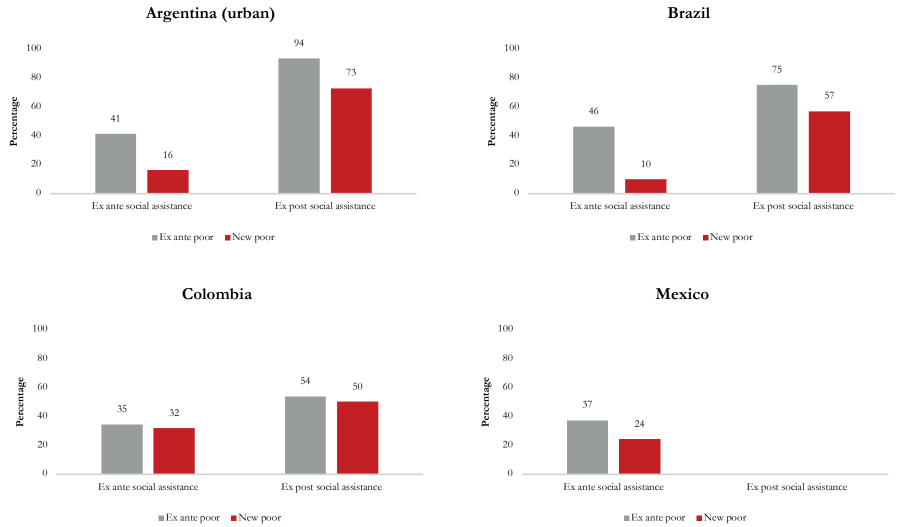
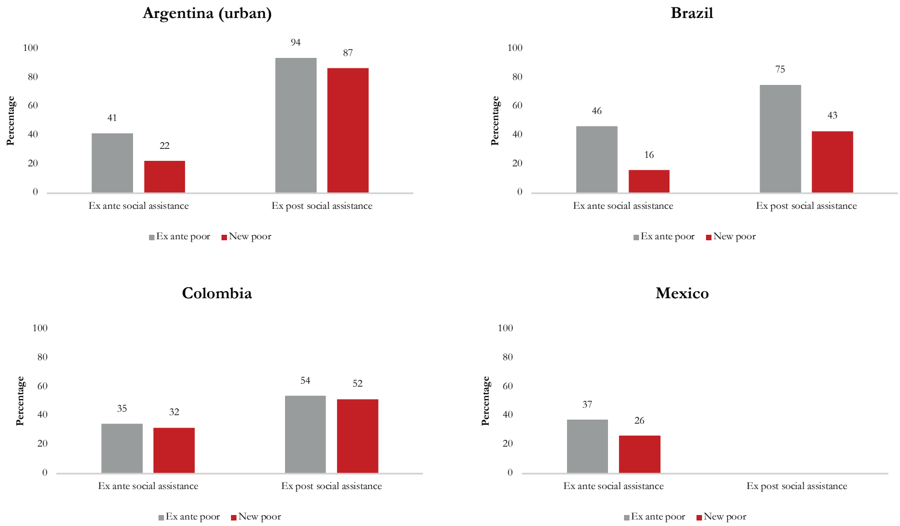
Figure 2. Coverage of Existing and New or Expanded Social Assistance

Panel (a) "Concentrated losses"



Panel (b) "Dispersed losses"



Notes: Poverty measured using the national poverty line.

Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

Impact on income mobility

The poverty and inequality comparisons above are anonymous. By (re-)ranking households from poorest to richest in each distribution, they do not consider the income trajectories of individual households. But those income trajectories are of considerable interest when income losses (or gains) differ, perhaps greatly, among households as they do here. To describe those trajectories, we use two non-anonymous distributional comparisons: non-anonymous growth incidence curves (GIC)—in this case, "contraction incidence curves"-- and mobility across broad income classes with income transition matrices.[30] These income classes are: extreme poor -- less than \$3.20 per day; moderate poor -- between \$3.20 and \$5.50 per day; lower-middle class -- between \$5.50 and \$11.50 per day; middle class -- between \$11.50 and \$57.60 per day; and rich -- more than \$57.60 per day.[31]

Figure 3 shows the change in income at each percentile of the *ex ante* income distribution.[32] Households across the entire income distribution are worse off on average (regardless of the scenario) after the lockdowns, which is not surprising, but the losses tend to be higher for the middle deciles rather than the poorest, which perhaps is surprising. The latter reflects the fact that poorer households have a cushion given by the existing social assistance programs (the yellow "band" in Figure 1); it also reflects the fact that three types of income are both not at risk and concentrated at the top end of the *ex ante* income distribution: social security pensions, salaries earned in the public sector, and labor earnings of white collar workers who are CEO's, managers and researchers with internet access at home. The dotted lines show the GIC after considering the effect of the expanded social assistance. As expected, social assistance cuts the losses and, indeed, increases the income of poor households by significantly more in Argentina and Brazil where the mitigation policies have been much more

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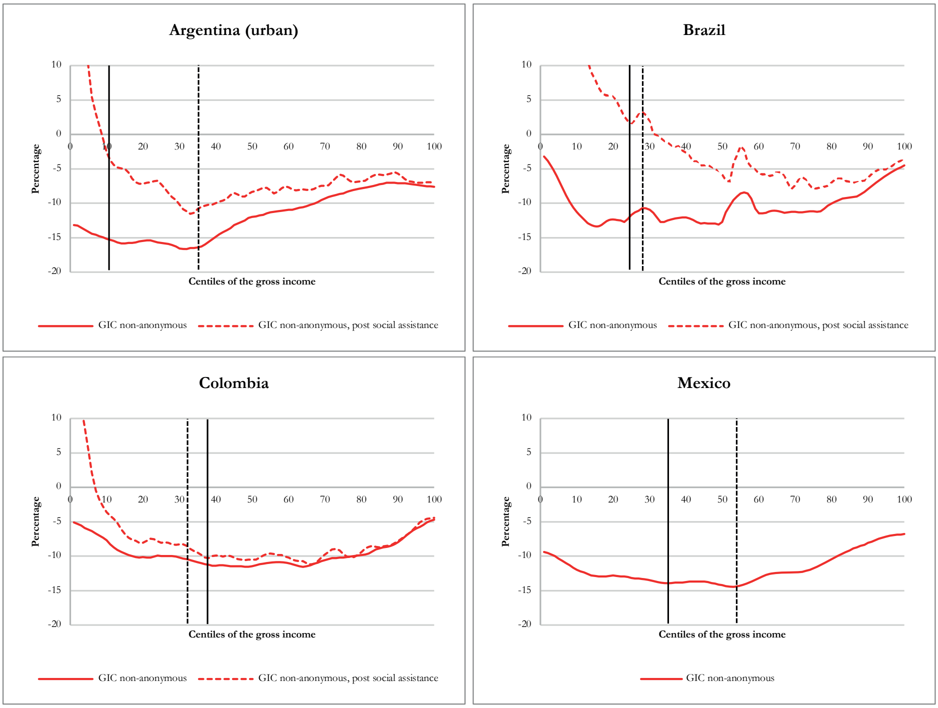
[30] Bourguignon (2011) discusses the theoretical and practical differences between the standard anonymous comparisons and non-anonymous methods, including the ones we use here.

[31] All cut-off values are in 2011 purchasing power parity (PPP) dollars. The default cut-off values \$3.20 and \$5.50 correspond to the income-category-specific poverty lines suggested in Jolliffe & Prydz (2016). The US\$3.20 and US\$5.50 PPP per day poverty lines are commonly used as extreme and moderate poverty lines for Latin America and roughly correspond to the median official extreme and moderate poverty lines in those countries. The \$11.50 and \$57.60 cutoffs correspond to cutoffs for the vulnerable and middle-class populations suggested for the 2005-era PPP conversion factors by López-Calva and Ortiz-Juarez (2014); \$11.50 and \$57.60 represent a United States CPI-inflation adjustment of the 2005-era \$10 and \$50 cutoffs. The US\$10 PPP per day line is the upper bound of those vulnerable to falling into poverty (and thus the lower bound of the middle class) in three Latin American countries, calculated by López-Calva and Ortiz-Juarez (2014). Ferreira and others (2013) find that an income of around US\$10 PPP also represents the income at which individuals in various Latin American countries tend to self-identify as belonging to the middle class and consider this a further justification for using it as the lower bound of the middle class. The US\$10 PPP per day line was also used as the lower bound of the middle class in Latin America in Birdsall (2010) and in developing countries in all regions of the world in Kharas (2010). The US\$50 PPP per day line is the upper bound of the middle class proposed by Ferreira and others (2013).

[32] In other words, each point on the curves shows the loss for the households that are, *ex ante*, in the shown centile in the x-axis. The y-axis shows the average change in per capita income. For example, the households in the first centile in Argentina could potentially lose about 13 percent of their pre-Covid per capita income before the expanded social assistance; that loss becomes a gain of roughly 30 percent once we consider expanded social assistance.

ambitious.[33] In all three countries that have new social assistance transfers those transfers favor the *ex ante* poor.

Figure 3. Non-anonymous Growth Incidence Curves
Panel (a) "Concentrated losses"



[33] Figure A3 in the appendix shows both the anonymous and non-anonymous GICs. The anonymous GIC tend to be upward sloping (except for the very poorest) and lie below the nonanonymous ones. In fact, the decline of incomes at the bottom before the expanded social assistance is much larger especially for the "concentrated scenario" because some of the households that were not among the poorest *ex ante* end up with almost zero income.

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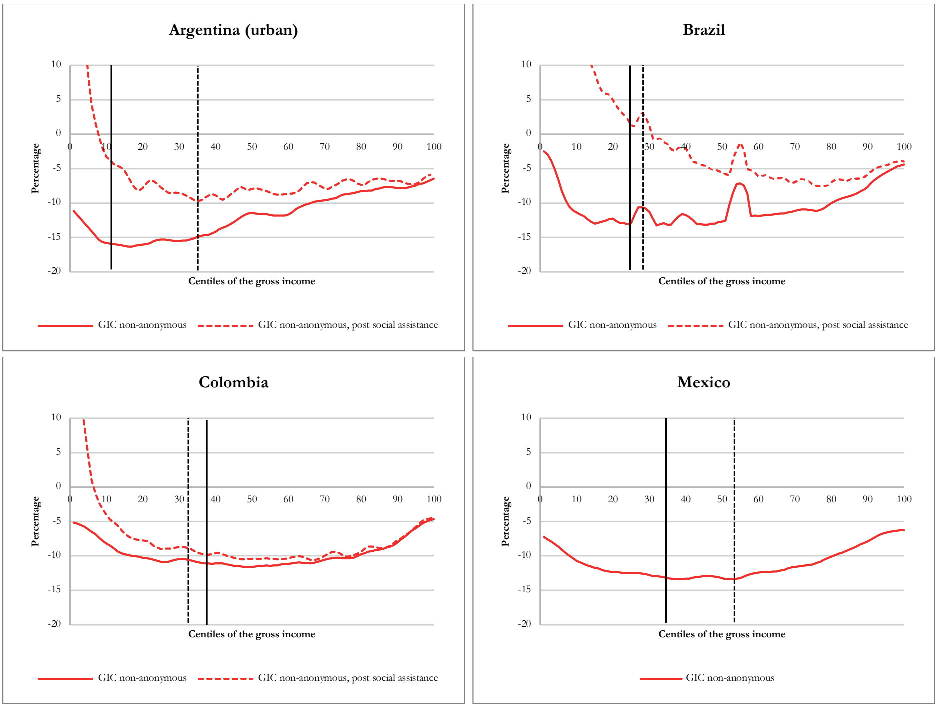
Panel (b) "Dispersed losses"



Notes: The dashed line is the national poverty line and the bold line is the $5.50 (moderate poor) per day international line (in 2011 PPP). Poverty lines based on the *ex ante* distribution of income. Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

Table 7 shows the downward mobility of the lower middle class and middle class caused by the crisis. Large shares of the *ex ante* poor fall into extreme poverty in both scenarios, and large shares of the *ex ante* lower-middle class fall into poverty. In the "concentrated losses" scenario, even some previously middle class households fall into poverty, though this does not occur in the "dispersed losses" scenario as the losses to any individual household are smaller and thus not sufficient to drive a previously middle-class household into poverty. The difference across scenarios in the impact of the newly introduced social assistance is much more striking in Argentina and Brazil, the two countries with substantial new programs. In the "concentrated losses" scenario, the new transfers offer only very small reductions in those who fall into extreme poverty or poverty. In this scenario, households are losing substantial amounts of income which the new social assistance is too small to replace. In the "dispersed losses" scenario, though, each losing household loses less income and the new transfers more often are sufficient to offset those losses and thus prevent households from falling into (extreme) poverty.[34]

Table 7. Inter-Income Group Mobility

Country	Without social assistance			With social assistance		
	% of moderate poor who fall to extreme poor	% of the lower-middle class who fall to poor	% of the middle class who fall to poor	% of moderate poor who fall to extreme poor	% of the lower-middle class who fall to poor	% of the middle class who fall to poor
Panel (a) "Concentrated losses"						
Argentina (urban)	22.6	20.8	6.2	19.2	19.0	5.8
Brazil	16.2	14.8	6.0	13.9	12.9	5.4
Colombia	14.4	15.2	4.9	14.0	15.0	4.9
Mexico	20.1	20.3	4.8			
Panel (b) "Dispersed losses"						
Argentina (urban)	27.5	21.9	0.0	7.8	13.0	0.0
Brazil	24.1	16.8	0.0	7.5	8.0	0.0
Colombia	20.0	17.0	0.0	18.3	16.2	0.0
Mexico	22.1	21.2	0.0			

Note: Income groups in terms of 2011 PPP are: moderate poor: between \$3.20 and \$5.50 per day; lower-middle class: between \$5.50 and \$11.50 per day; and middle class: between \$11.50 and \$57.60 per day.

Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

Comparison to Other Studies

We have not found studies with non-anonymous analysis of income losses (income transitions or losses across the income distribution). Nor have we found any work on the distributional consequences of expanded social assistance. There is, however, a growing literature on the poverty impact of the crisis. Several studies assume that losses are proportional across the income distribution. For example, see Gerszon Mahler et al. (2020), Sumner, Hoy and Ortiz-Juarez (2020) and Valensisi (2020). Valensisi (2020) and Gerszon Mahler et al. (2020) do not present results for individual countries so we do not include them here.

Sumner, Hoy and Ortiz-Juarez (2020) use the World Bank's Povcal.Net platform and generate new poverty estimates by assuming that the poverty line increases by the same amount as their assumed contractions in income. For an aggregate contraction of 10 percent (which is the closest to our scenarios) and the \$5.50 poverty line, their estimates predict an increase in the number of poor for Latin America and the Caribbean (LAC) of 23.5 million. For the four countries included here the increase is 15.8 million. In our analysis, the rise in the number of poor for these four countries before social assistance is 23.3 ("dispersed losses") and 30.4 ("concentrated losses") million. Their estimates

[34] The full set of income transition matrices can be found in the appendix (Tables A6 and A7).

are lower than ours because their study assumes proportional contractions across the income distribution.[35]

In ECLAC (United Nations Economic Commission for Latin America and the Caribbean, July 2020) incomes do not contract proportionally. For the four countries included here, ECLAC projects an increase of 31.2 million poor people using national poverty lines. Our estimate is an increase of between 24.9 ("dispersed losses") and 26.5 ("concentrated losses") millions, considerably lower. This is due to the fact that our estimates take into account the cushion that existing social assistance programs provide to the poorest while ECLAC's projections use labor income before transfers. A further difference in our results is that we take account of the offsetting effects of expanded social assistance. Do so reduces our estimates to an increase of between 15.3 (dispersed losses) and 19.1 (concentrated losses) poor people.

4. Conclusions

To contain the spread of the novel coronavirus, governments implemented lockdown policies of various degrees that, together with the global crisis, inevitably caused a sharp reduction in activity, a fall in employment and income, and a rise in poverty and inequality. Our microsimulations show that increases in poverty are worse than if we had assumed that each household's income declines by an equal proportion as many other studies of the crisis do. Contrary to prior expectations, we find that the worst effects are not on the poorest, but those (roughly) in the middle of the *ex ante* income distribution. We also find that the expanded social assistance governments have introduced in response to the crisis have a large offsetting effect in Brazil and Argentina, much less in Colombia (and nil in Mexico, a country where no expansion of social assistance took place). In Brazil, the lockdowns caused larger increases in poverty in the afrodescendant and indigenous populations than for others. But we also find that in Brazil the offsetting effects of expanded social assistance are larger for households whose head is afrodescendant or indigenous. In all countries the increase in poverty induced by the lockdown is similar for male- and female-headed households but the offsetting effect of expanded social assistance is greater for female-headed households.

It should be noted that our simulations do not include all the mitigation measures that have been implemented by governments. However, it covers the most important ones. Still, the impact of expanded social assistance on poverty, inequality and income mobility shown here is probably a lower bound. On the other hand, the decline in economic activity and employment may be higher than what

[35] As a check on the importance of including the change in the distribution of income on our poverty estimates, we repeated our analysis assuming that everybody's income declines by the same per capita fall projected by the IMF for each country. In this case, the increase in the number of poor in the four countries taken together would equal 12.5 million (using the $5.50 poverty line). This has to be compared with our distribution-sensitive simulations. As shown in Table 3, the increase in number of poor from the lockdowns is estimated to be between 23.3 ("dispersed losses) and 30.4 ("concentrated losses") million individuals. When we compare our results for the case without changes in the distribution to Sumner, Hoy and Ortiz-Juarez (2020), there is a difference of roughly three million more new poor in their estimates compared to ours: 12.5 m (ours) vs. 15.8 (theirs). We think that our estimates are lower because the IMF projections indicate lower than 10 percent contractions for Argentina, Brazil and Colombia.

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the IMF (2020) forecasted. Thus, the impact of the coronavirus crisis on poverty and inequality may also be higher than our results show.

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Appendix

Table A1. Description of Existing and New Social Assistance Programs by Country

ARGENTINA
INCREASED *Asignación Universal por Hijo* is a conditional cash transfer program for children and adolescents (younger than 18 years old) living in poverty or vulnerability situation. The program includes conditions related to health and education obligations. The beneficiaries are individuals and a household can receive of up to 5 allowances. During March 2020, the federal government has announced a unique increase of $3,100 ARS. The program represents around 35% and 77% of the national and $5.5 PPP per day poverty lines, respectively. https://www.unicef.org/argentina/media/4186/file/Universal%20Child%20Allowance%20(AUH).pdf
INCREASED (not included in simulation) *Pensión Universal para el Adulto Mayor* is an unconditional cash transfer aid to elderly than 65 years old that are not receiving any pension from the contributory system. The beneficiaries are individuals and there are no restrictions in the number of allowances per household. During March 2020, the federal government has announced a unique increase of $3,000 ARS. The program represents around 34% and 75% of the national and $5.5 PPP per day poverty line respectively. https://www.anses.gob.ar/pension-universal-para-el-adulto-mayor
NEW *Ingreso Familiar de Emergencia* is an unconditional transitory cash transfer aid to informal and vulnerable workers between 18 and 65 years old during the COVID-19 pandemic. The beneficiaries are individuals and only one allowance could be received per household. It is a monthly payment of $10,000 and it was delivered to beneficiaries during May, June and July. The program represents around 112% and 250% of the national and $5.5 PPP per day poverty line respectively. https://www.anses.gob.ar/ingreso-familiar-de-emergencia
BRAZIL
INCREASED NUMBER OF BENEFICIARIES (not included in simulation) *Bolsa Família* is a conditional cash transfer program for families living in poverty. The program includes variable benefits depending on the characteristic of the household. The beneficiaries are individuals and there are no restrictions in the number of allowances per household. The program includes conditions related to health and education obligations. https://publications.iadb.org/publications/english/document/How-Does-Bolsa-Familia-Work-Best-Practices-in-the-Implementation-of-Conditional-Cash-Transfer-Programs-in-Latin-America-and-the-Caribbean.pdf
NEW Auxílio Emergencial is a financial benefit for informal workers, individual microentrepreneurs, self-employed and unemployed and its purpose is to provide emergency protection in the period of coping with the crisis caused by the COVID 19 pandemic. The beneficiaries are individuals and there are no restrictions in the number of allowances per household. During April 2020, the federal government has announced the program that consists in five payments of $600 R. The program represents around 120% and 138% of the national and $5.5 PPP per day poverty line respectively. https://auxilio.caixa.gov.br/#/inicio

CEPR PRESS

NEW *Beneficio Emergencial de Manutenção do Emprego e Renda* is a program that helps companies and employees agree a proportional reduction of working hours and wages; or the temporary suspension of the employment contract. In compensation to that reduction, the government will cover the original wage of the worker. The beneficiaries are individuals and there are no restrictions in the number of allowances per household. During April 2020, the federal government has announced the program. The program represents around 160% and 184% of the national and $5.5 PPP per day poverty line respectively.https://servicos.mte.gov.br/bem/
COLOMBIA
INCREASED *Familias en Acción* is a conditional cash transfer program for children and adolescents (younger than 18 years old) living under food insecurity conditions. The beneficiaries are individuals and a household can receive of up to 3 allowances. The program includes conditions related to health and education obligations. The program represents around 59% and 53% of the national and $5.5 PPP per day poverty lines, respectively. https://plataformacelac.org/programa/481#:~:text=Familias%20en%20Acci%C3%B3n%20es%20un,permanencia%20en%20el%20sistema%20escolar.
INCREASED *Jóvenes en Acción* is a conditional cash transfer program for young adults (between 16 to 24 years old) facing economic difficulties to continue or finish their studies. The program includes conditions related to eligibility criteria on other programs such as Familias en Accion, Red de la superacion de la pobreza extrema, etc. The beneficiaries are individuals and there are no restrictions in the number of allowances per household. During August 2020, the federal government increased the number of beneficiaries in 140 thousand. The program represents around 144% and 129% of the national and $5.5 PPP per day poverty lines, respectively. https://prosperidadsocial.gov.co/sgpp/transferencias/jovenes-en-accion/cupos/
INCREASED *Colombia Mayor* is an unconditional cash transfer program that aims to increase protection for older adults who do not have a pension, or live in extreme poverty or indigence, through the delivery of a monthly economic subsidy. The beneficiaries are individuals and there are no restrictions in the number of allowances per household. During June 2020, the federal government has announced a payment increase of $160,000 Col for three months. The program represents around 65% and 58% of the national and $5.5 PPP per day poverty lines, respectively. https://www.fondodesolidaridadpensional.gov.co/fondo-de-solidaridad/que-es-el-fondo-de-solidaridad-pensional/programas/programa-colombia-mayor.html#:~:text=El%20Programa%20de%20Protecci%C3%B3n%20Social,de%20un%20subsidio%20econ%C3%B3mico%20mensual.
NEW *Ingreso solidario* is an unconditional cash transfer program that aims to mitigate the situation of households facing economic difficulties due to COVID-19 crisis. The beneficiaries of Ingreso Solidario are not obligated to any condition but they must not being receiving any other social programs. The beneficiaries are households and only one allowance per household is permitted. The program represents around 65% and 58% of the national and $5.5 PPP per day poverty line respectively. https://ingresosolidario.dnp.gov.co/

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NEW *Bogotá solidaria* is a unconditional cash transfer program (from Mayor's Office of Bogotá) that aims help vulnerable or poor families in the city of Bogotá so that they have a basic income during the COVID-19 quarantine. The beneficiaries of Bogotá Solidario must not have any intra-household violence record. The beneficiaries are households and only one allowance per household is permitted. During March 2020, the federal government has announced the program $233,000 Col for five months. The program represents around 94% and 84% of the national and $5.5 PPP per day poverty line respectively. https://rentabasicabogota.gov.co/
NEW (not included in simulation) *Subsidio de Nómina* is a subsidy received by the employers as a contribution of up to 40% of the value of the current legal monthly minimum wage for each employee. The beneficiaries are the formal workers and there are no restrictions in the number of allowances per household. The maximum value of the program represents approximately 142% and 127% of the national and $5.5 PPP per day poverty line respectively. https://www.grupobancolombia.com/personas/alivios-financieros/subsidio-pago-nomina#:~:text=Consiste%20en%20un%20subsidio%20creado,julio%20y%20agosto%20de%202020.

Table A2. Employment by sector
Panel (a) Argentina (urban)

Sector	Not at risk	At risk	Total
Agriculture	65,109	0	65,109
Mining	36,897	12,281	49,178
Manufacturing	736,190	663,709	1,399,899
Electricity, gas and water supply	52,041	37,702	89,743
Construction	119,479	984,050	1,103,529
Retail and wholesale	593,180	1,584,484	2,177,664
Accommodation and food service	112,358	344,128	456,486
Transport	150,331	490,213	640,544
Information and communication	86,118	170,555	256,673
Financial services	178,675	88,681	267,356
Real estate	36,809	30,604	67,413
Professional activities	695,307	251,581	946,888
Public administration	1,016,020	0	1,016,020
Education	1,012,903	0	1,012,903
Health	793,233	0	793,233
Other sectors	349,785	1,404,260	1,754,045
Total	6,034,435	6,062,248	12,096,683
%	49.9%	50.1%	

Panel (b) Brazil

Sector	Not at risk	At risk	Total
Agriculture	8,636,764	0	8,636,764
Mining	384,819	28,358	413,177
Manufacturing	3,996,924	6,910,053	10,906,977
Electricity, gas and water supply	744,746	153,773	898,519
Construction	321,999	6,493,117	6,815,116
Retail and wholesale	8,352,357	9,543,628	17,895,985
Accommodation and food service	385,260	5,236,263	5,621,523
Transport	2,641,323	2,194,322	4,835,645
Information and communication	1,241,353	102,909	1,344,262
Financial services	1,103,351	168,406	1,271,757
Real estate	70,257	476,066	546,323
Professional activities	4,062,780	3,481,562	7,544,342
Public administration	5,111,266	0	5,111,266
Education	6,588,520	0	6,588,520
Health	4,747,906	0	4,747,906
Other sectors	698,142	10,602,821	11,300,963
Total	49,087,767	45,391,278	94,479,045
%	52.0%	48.0%	

Panel (c) Colombia

Sector	Not at risk	At risk	Total
Agriculture	3,515,167	0	3,515,167
Mining	195,612	1,222	196,834
Manufacturing	1,450,032	1,089,303	2,539,335
Electricity, gas and water supply	113,037	38,081	151,118
Construction	120,927	1,392,706	1,513,633
Retail and wholesale	1,632,476	2,815,331	4,447,807
Accommodation and food service	26,771	1,492,637	1,519,408
Transport	518,790	946,252	1,465,042
Information and communication	213,505	46,873	260,378
Financial services	305,304	26,567	331,871
Real estate	40,836	311,224	352,060
Professional activities	792,673	554,786	1,347,459
Public administration	711,302	0	711,302
Education	959,010	0	959,010
Health	956,935	0	956,935
Other sectors	205,906	1,688,689	1,894,595
Total	11,758,283	10,403,671	22,161,954
%	53.1%	46.9%	

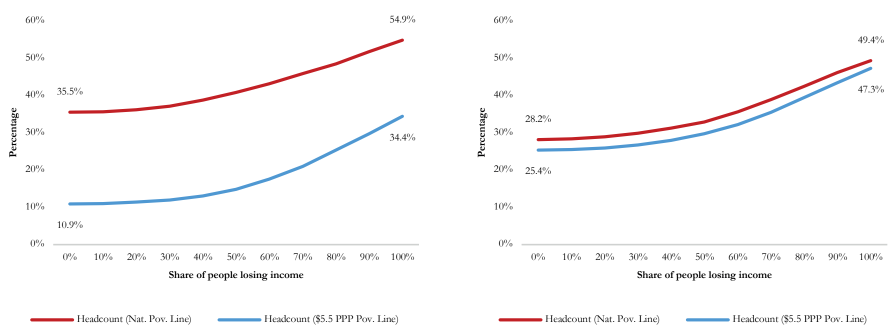
Panel (d) Mexico

Sector	Not at risk	At risk	Total
Agriculture	8,953,313	0	8,953,313
Mining	198,514	0	198,514
Manufacturing	4,098,366	5,470,030	9,568,396
Electricity, gas and water supply	220,675	655	221,330
Construction	348,183	4,477,639	4,825,822
Retail and wholesale	5,893,101	5,145,482	11,038,583
Accommodation and food service	181,228	4,754,290	4,935,518
Transport	813,780	1,628,415	2,442,195
Information and communication	470,479	0	470,479
Financial services	558,741	557	559,298
Real estate	377,231	108	377,339
Professional activities	1,351,674	31,126	1,382,800
Public administration	2,172,350	0	2,172,350
Education	2,818,952	0	2,818,952
Health	1,670,654	0	1,670,654
Other sectors	6,208,673	5,566,657	11,775,330
Total	36,335,914	27,074,959	63,410,873
%	57.3%	42.7%	

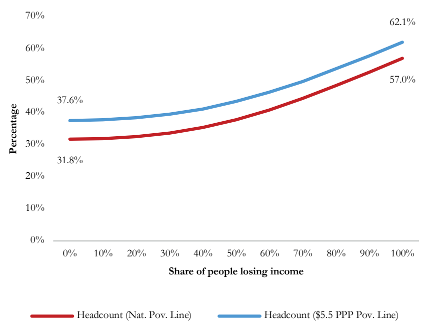
Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

Figure A1. Sensitivity analysis of poverty

Panel (a) Argentina (urban) **Panel (b) Brazil**



Panel (c) Colombia

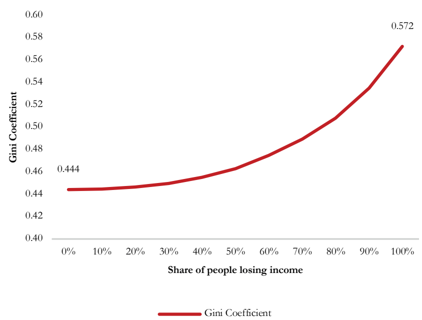


Panel (d) Mexico

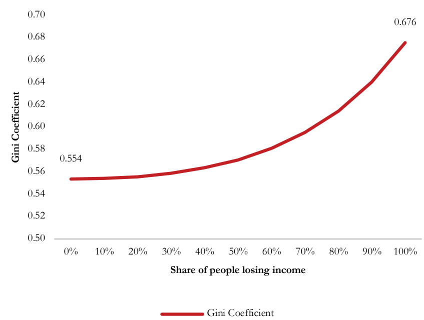
Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

Figure A2. Sensitivity analysis of inequality

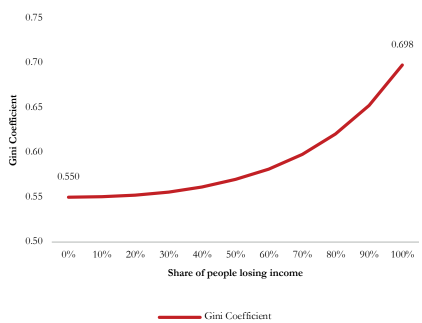
Panel (a) Argentina (urban)



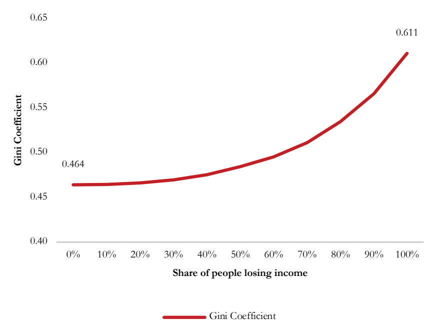
Panel (b) Brazil



Panel (c) Colombia



Panel (d) Mexico



Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

Table A3. Incidence of Poverty for All Scenarios

Country	Scenario	*Ex ante*	*Ex post*	Change	New poor (in millions)	*Ex post* + Social Assistance	Change	New poor (in millions)
Panel (a) Headcount (National Poverty Line)								
Argentina (urban)	40% lose 90%	35.5	42.8	7.2	2.0	40.3	4.8	1.4
Argentina (urban)	50% lose 70%	35.5	42.9	7.4	2.1	40.3	4.7	1.3
Argentina (urban)	60% lose 60%	35.5	43.1	7.6	2.1	40.6	5.1	1.4
Argentina (urban)	70% lose 50%	35.5	43.0	7.4	2.1	40.7	5.2	1.5
Argentina (urban)	90% lose 40%	35.5	43.1	7.6	2.1	40.7	5.1	1.4
Brazil	30% lose 100%	28.2	34.6	6.4	13.5	31.5	3.4	7.1
Brazil	50% lose 60%	28.2	34.5	6.3	13.2	30.6	2.4	5.1
Brazil	60% lose 50%	28.2	33.7	5.6	11.6	30.1	1.9	4.0
Brazil	100% lose 30%	28.2	33.6	5.4	11.4	29.5	1.3	2.8
Colombia	30% lose 80%	31.8	37.9	6.1	3.0	37.3	5.6	2.7
Colombia	40% lose 60%	31.8	37.8	6.1	3.0	37.1	5.4	2.6
Colombia	60% lose 40%	31.8	37.4	5.7	2.8	36.8	5.1	2.5
Colombia	80% lose 30%	31.8	36.9	5.2	2.5	36.4	4.6	2.3
Mexico	40% lose 80%	53.8	60.1	6.4	8.0			
Mexico	60% lose 50%	53.8	60.4	6.7	8.3			
Mexico	100% lose 30%	53.8	60.8	7.0	8.8			
Panel (b) Headcount ($5.5 PPP Poverty Line)								
Argentina (urban)	40% lose 90%	10.9	19.1	8.2	2.3	16.8	5.9	1.7
Argentina (urban)	50% lose 70%	10.9	18.0	7.1	2.0	14.9	4.0	1.1
Argentina (urban)	60% lose 60%	10.9	17.5	6.6	1.9	13.9	3.0	0.9
Argentina (urban)	70% lose 50%	10.9	16.5	5.6	1.6	13.0	2.1	0.6
Argentina (urban)	90% lose 40%	10.9	15.8	4.9	1.4	12.8	1.9	0.5
Brazil	30% lose 100%	25.4	32.0	6.6	13.9	27.9	2.5	5.3
Brazil	50% lose 60%	25.4	31.1	5.8	12.0	26.4	1.1	2.3
Brazil	60% lose 50%	25.4	30.6	5.2	11.0	25.9	0.5	1.1
Brazil	100% lose 30%	25.4	29.8	4.5	9.3	25.2	-0.2	-0.4
Colombia	30% lose 80%	37.6	43.6	6.0	2.9	43.0	5.4	2.7
Colombia	40% lose 60%	37.6	43.5	6.0	2.9	42.9	5.4	2.6
Colombia	60% lose 40%	37.6	43.2	5.6	2.7	42.6	5.0	2.5
Colombia	80% lose 30%	37.6	42.6	5.1	2.5	42.1	4.5	2.2
Mexico	40% lose 80%	34.9	43.8	9.0	11.2			
Mexico	60% lose 50%	34.9	43.4	8.5	10.7			
Mexico	100% lose 30%	34.9	42.9	8.1	10.1			

Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

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Table A4. Gini Coefficient for All Scenarios

Country	Scenario	*Ex ante*	*Ex post*	Change	*Ex post* + Social Assistance	Change
Panel (c) Gini Coefficient						
Argentina (urban)	40% lose 90%	0.444	0.486	0.042	0.469	0.025
Argentina (urban)	50% lose 70%	0.444	0.477	0.033	0.460	0.016
Argentina (urban)	60% lose 60%	0.444	0.475	0.030	0.457	0.013
Argentina (urban)	70% lose 50%	0.444	0.470	0.026	0.453	0.009
Argentina (urban)	90% lose 40%	0.444	0.467	0.022	0.451	0.007
Brazil	30% lose 100%	0.554	0.591	0.037	0.565	0.011
Brazil	50% lose 60%	0.554	0.577	0.023	0.551	-0.002
Brazil	60% lose 50%	0.554	0.574	0.020	0.549	-0.005
Brazil	100% lose 30%	0.554	0.570	0.016	0.545	-0.009
Colombia	30% lose 80%	0.550	0.578	0.028	0.574	0.024
Colombia	40% lose 60%	0.550	0.572	0.022	0.568	0.018
Colombia	60% lose 40%	0.550	0.566	0.016	0.563	0.012
Colombia	80% lose 30%	0.550	0.564	0.014	0.560	0.010
Mexico	40% lose 80%	0.464	0.503	0.039		
Mexico	60% lose 50%	0.464	0.487	0.023		
Mexico	100% lose 30%	0.464	0.479	0.015		

Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

Table A5. Squared Poverty Gap
Panel (a) "Concentrated losses"

Country	*Ex ante*	*Ex post*	Change	*Ex post* + Social Assistance	Change
Panel (a) Squared Poverty Gap (National Poverty Line)					
Argentina (urban)	7.8	13.9	6.1	11.6	3.8
Brazil	9.0	15.1	6.1	10.9	1.9
Colombia	8.9	12.6	3.7	12.0	3.0
Mexico	10.7	16.7	6.0		
Panel (b) Squared Poverty Gap ($5.5 PPP Poverty Line)					
Argentina (urban)	2.2	6.1	3.9	3.9	1.8
Brazil	7.7	13.7	6.0	9.6	1.9
Colombia	11.1	15.0	3.8	14.4	3.2
Mexico	6.0	11.0	5.0		

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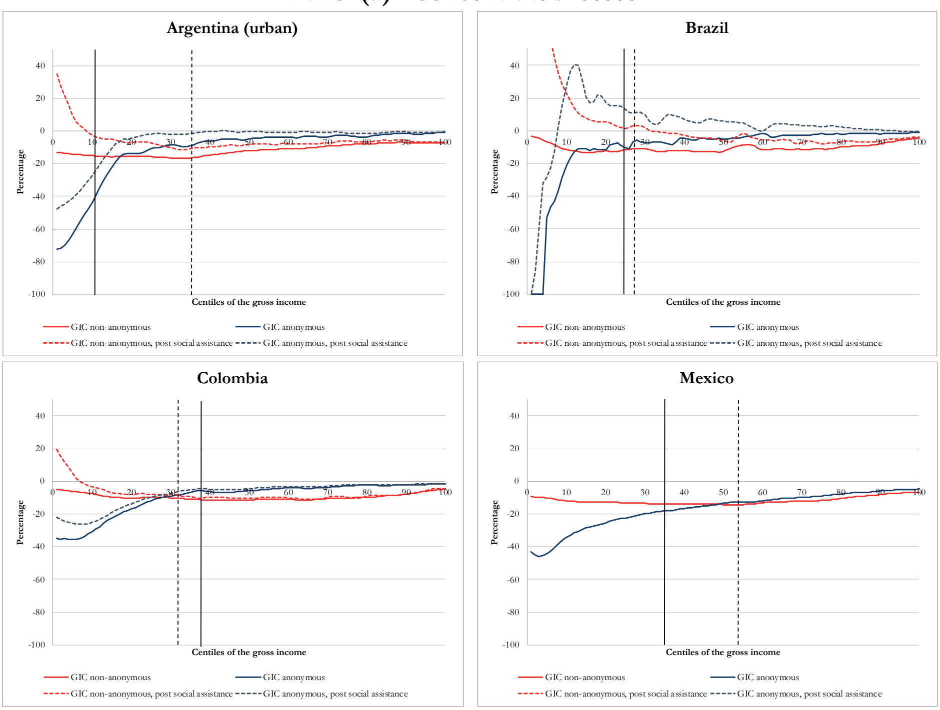
Panel (b) "Dispersed losses"

Country	*Ex ante*	*Ex post*	Change	*Ex post* + Social Assistance	Change
Panel (a) Squared Poverty Gap (National Poverty Line)					
Argentina (urban)	7.8	10.8	3.0	8.7	1.0
Brazil	9.0	10.5	1.6	7.4	-1.6
Colombia	8.9	10.4	1.5	9.9	1.0
Mexico	10.7	14.0	3.3		
Panel (b) Squared Poverty Gap ($5.5 PPP Poverty Line)					
Argentina (urban)	2.2	3.0	0.8	1.8	-0.4
Brazil	7.7	9.0	1.3	6.0	-1.7
Colombia	11.1	12.8	1.7	12.3	1.1
Mexico	6.0	8.0	2.0		

Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

Figure A3. Anonymous and Non-anonymous Growth Incidence Curves

Panel (a) "Concentrated losses"



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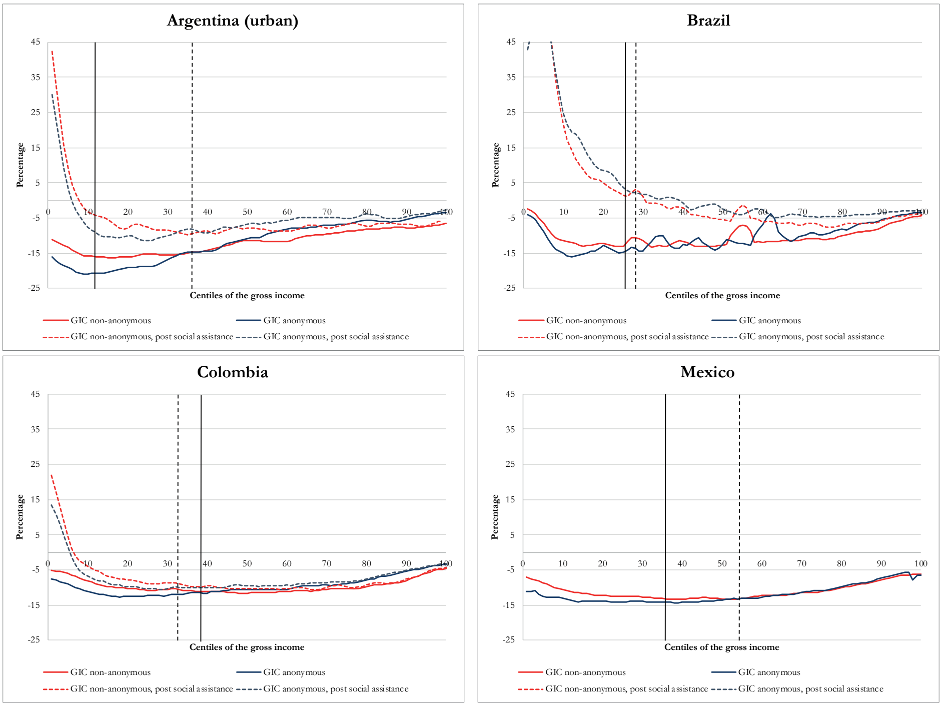
Panel (b) "Dispersed losses"



Notes: The dashed line is the national poverty line and the bold line is the $5.50 (moderate poor) per day international line (in 2011 PPP). Poverty lines based on the *ex ante* distribution of income. Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

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Table A6. Transition Matrices, "Concentrated losses"

Panel (a) Argentina (urban)

Income group		Post: Extreme poor y < 3.20	Post: Moderate poor 3.20 <= y < 5.50	Post: Lower-Middle Class 5.50 <= y < 11.50	Post: Middle Class 11.50 <= y < 57.60	Post: Rich 57.60 <= y	% Population
Pre	y < 3.20	78.4%	21.5%	0.1%	0.0%	0.0%	100.0%
	3.20 <= y < 5.50	19.2%	53.8%	26.9%	0.0%	0.0%	100.0%
	5.50 <= y < 11.50	11.6%	7.3%	72.9%	8.1%	0.0%	100.0%
	11.50 <= y < 57.60	3.3%	2.5%	5.1%	88.9%	0.2%	100.0%
	57.60 <= y	0.0%	0.0%	1.8%	10.6%	87.6%	100.0%
Change wrt. the same group		131.9%	11.4%	-4.6%	-7.1%	-10.6%	

Panel (b) Brazil

Income group		Post: Extreme poor y < 3.20	Post: Moderate poor 3.20 <= y < 5.50	Post: Lower-Middle Class 5.50 <= y < 11.50	Post: Middle Class 11.50 <= y < 57.60	Post: Rich 57.60 <= y	% Population
Pre	y < 3.20	71.4%	25.9%	2.7%	0.1%	0.0%	100.0%
	3.20 <= y < 5.50	13.0%	55.1%	31.8%	0.1%	0.0%	100.0%
	5.50 <= y < 11.50	8.1%	4.2%	76.8%	10.9%	0.0%	100.0%
	11.50 <= y < 57.60	4.2%	1.1%	5.1%	89.4%	0.2%	100.0%
	57.60 <= y	1.2%	0.0%	0.2%	4.5%	94.1%	100.0%
Change wrt. the same group		13.5%	-2.9%	0.7%	-3.3%	-4.4%	

Panel (c) Colombia

Income group		Post: Extreme poor y < 3.20	Post: Moderate poor 3.20 <= y < 5.50	Post: Lower-Middle Class 5.50 <= y < 11.50	Post: Middle Class 11.50 <= y < 57.60	Post: Rich 57.60 <= y	% Population
Pre	y < 3.20	97.2%	2.8%	0.0%	0.0%	0.0%	100.0%
	3.20 <= y < 5.50	14.0%	83.3%	2.7%	0.0%	0.0%	100.0%
	5.50 <= y < 11.50	10.5%	4.4%	84.5%	0.5%	0.0%	100.0%
	11.50 <= y < 57.60	2.0%	2.9%	7.0%	88.1%	0.0%	100.0%
	57.60 <= y	0.0%	0.0%	0.0%	6.6%	93.4%	100.0%
Change wrt. the same group		28.1%	-1.1%	-6.8%	-10.8%	-6.6%	

Panel (d) Mexico

Income group		Post: Extreme poor y < 3.20	Post: Moderate poor 3.20 <= y < 5.50	Post: Lower-Middle Class 5.50 <= y < 11.50	Post: Middle Class 11.50 <= y < 57.60	Post: Rich 57.60 <= y	% Population
Pre	y < 3.20	100.0%	0.0%	0.0%	0.0%	0.0%	100.0%
	3.20 <= y < 5.50	20.1%	79.9%	0.0%	0.0%	0.0%	100.0%
	5.50 <= y < 11.50	11.6%	8.7%	79.7%	0.0%	0.0%	100.0%
	11.50 <= y < 57.60	1.6%	3.2%	8.2%	86.9%	0.0%	100.0%
	57.60 <= y	0.0%	0.0%	0.0%	8.5%	91.5%	100.0%
Change wrt. the same group		73.4%	-1.6%	-14.6%	-12.7%	-8.5%	

Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

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Table A7. Transition Matrices, "Dispersed losses"

Panel (a) Argentina (urban)

Income group		Post: Extreme poor y < 3.20	Post: Moderate poor 3.20 <= y < 5.50	Post: Lower-Middle Class 5.50 <= y < 11.50	Post: Middle Class 11.50 <= y < 57.60	Post: Rich 57.60 <= y	% Population
Pre	y < 3.20	81.6%	18.4%	0.0%	0.0%	0.0%	100.0%
	3.20 <= y < 5.50	7.8%	76.9%	15.2%	0.0%	0.0%	100.0%
	5.50 <= y < 11.50	0.0%	13.0%	82.1%	4.9%	0.0%	100.0%
	11.50 <= y < 57.60	0.0%	0.0%	9.6%	90.3%	0.2%	100.0%
	57.60 <= y	0.0%	0.0%	0.0%	15.5%	84.5%	100.0%
Change wrt. the same group		-4.3%	29.3%	12.3%	-6.4%	-13.7%	

Panel (b) Brazil

Income group		Post: Extreme poor y < 3.20	Post: Moderate poor 3.20 <= y < 5.50	Post: Lower-Middle Class 5.50 <= y < 11.50	Post: Middle Class 11.50 <= y < 57.60	Post: Rich 57.60 <= y	% Population
Pre	y < 3.20	73.0%	24.9%	2.1%	0.1%	0.0%	100.0%
	3.20 <= y < 5.50	6.5%	69.3%	24.1%	0.1%	0.0%	100.0%
	5.50 <= y < 11.50	0.0%	7.1%	87.0%	5.9%	0.0%	100.0%
	11.50 <= y < 57.60	0.0%	0.0%	7.8%	92.1%	0.1%	100.0%
	57.60 <= y	0.0%	0.0%	0.0%	6.7%	93.3%	100.0%
Change wrt. the same group		-21.2%	12.6%	11.6%	-3.4%	-5.8%	

Panel (c) Colombia

Income group		Post: Extreme poor y < 3.20	Post: Moderate poor 3.20 <= y < 5.50	Post: Lower-Middle Class 5.50 <= y < 11.50	Post: Middle Class 11.50 <= y < 57.60	Post: Rich 57.60 <= y	% Population
Pre	y < 3.20	97.9%	2.1%	0.0%	0.0%	0.0%	100.0%
	3.20 <= y < 5.50	18.3%	80.0%	1.6%	0.0%	0.0%	100.0%
	5.50 <= y < 11.50	0.0%	16.2%	83.5%	0.3%	0.0%	100.0%
	11.50 <= y < 57.60	0.0%	0.0%	13.5%	86.5%	0.0%	100.0%
	57.60 <= y	0.0%	0.0%	0.0%	7.8%	92.2%	100.0%
Change wrt. the same group		13.9%	9.9%	-1.9%	-12.5%	-7.8%	

Panel (d) Mexico

Income group		Post: Extreme poor y < 3.20	Post: Moderate poor 3.20 <= y < 5.50	Post: Lower-Middle Class 5.50 <= y < 11.50	Post: Middle Class 11.50 <= y < 57.60	Post: Rich 57.60 <= y	% Population
Pre	y < 3.20	100.0%	0.0%	0.0%	0.0%	0.0%	100.0%
	3.20 <= y < 5.50	22.1%	77.9%	0.0%	0.0%	0.0%	100.0%
	5.50 <= y < 11.50	0.0%	21.2%	78.8%	0.0%	0.0%	100.0%
	11.50 <= y < 57.60	0.0%	0.0%	16.2%	83.8%	0.0%	100.0%
	57.60 <= y	0.0%	0.0%	0.0%	11.8%	88.2%	100.0%
Change wrt. the same group		38.7%	14.2%	-10.1%	-15.7%	-11.8%	

Source: Authors' calculations based on ENIGH (2018), EPH (2019), GEIH (2019), PNADC (2019).

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Sharing when stranger equals danger: Ride-sharing during the Covid-19 pandemic[1]

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Using data collected from one of the most popular ridesharing platforms, we illustrate how mobility has changed after the exit from the Covid-19 induced confinement. We measure the impact of the Covid-19 outbreak on the level of mobility and the price of ridesharing. Finally, we show that the pandemic has exacerbated ethnic discrimination. Our results suggest that a decision-maker encouraging the use of ridesharing during the pandemic should account for the impact of the perceived health risks on ridesharing prices and should find ways to ensure fair access.

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1 Motivation

Pandemic notwithstanding, equitable and cost-effective transportation systems are indispensable to the functioning of modern economies. Meanwhile, in recent months, public-health experts have repeatedly called to avoid crowded and enclosed spaces, both typically associated with public transportation.[1] In fact, Harris (2020) argues that New York's subway was "a major disseminator if not the principal transmission vehicle" in the city's Covid-19 outbreak.[2]

In France, the government encourages ridesharing as the country emerges from the Covid-19 induced lock-downs. Ridesharing, contrasted with traditional means of public transportation, involves a lower number of people traveling together and therefore, might be a way to keep the transmission rates of the virus at low levels, without restricting mobility. It is unclear whether the government's oral encouragement is sufficient to convince people to get into a stranger's car, especially during a pandemic. Here, we look into what happens to the most popular ridesharing service in France as the country lifts shelter-at-home orders.

The ridesharing platform that we study, called BlaBlaCar, is a marketplace for city-to-city rides, where drivers are generally non-professionals. The platform matches a driver, who decides the route and the price, with passengers. In this way, the service allows the drivers to cover part of their costs and provides cheap transportation for passengers. The flexibility in terms of destinations, departure times, and prices might turn out to be of particular value in the uncertain times of re-opening. In this paper, we use observational data to highlight some developments on the platform over the last two months.

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[1] See https://www.cdc.gov/coronavirus/2019-ncov/prevent-getting-sick/prevention.html

[2] The final jury on the role of public transportation in the spread of Covid-19 is still out. For example, a recent study by Santé Publique France (2020) shows that none out of 150 Covid-19 clusters identified in France between the 9th of May and 3rd of June were related to public transportation. Furthermore, methodological concerns over the results of Harris (2020) were raised (https://pedestrianobservations.com/2020/04/15/the-subway-is-probably-not-why-new-york-is-a-disaster-zone and https://marketurbanism.com/2020/04/19/automobiles-seeded-the-massive-coronavirus-epidemic-in-new-york-city).

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Figure 1: French minister of ecological transition and solidarity encouraging use of ridesharing.

Source: Twitter Le Point account

First, we show rapid growth in the number of trips after the shelter-at-home orders were lifted. This increase happened mostly in regions less affected by the Covid-19 outbreak, in terms of cases, hospitalizations, and deaths. Second, we document a substantial dispersion in prices set by drivers. We also provide evidence suggesting a price premium associated with traveling in this period. The prices are particularly high in the regions most affected by the Covid-19 outbreak. Such an increase in prices could result from a higher perception of health risks associated with traveling with a stranger. We check the robustness of this result by exploring the errors of daily reports in Covid-19 cases and by excluding regions following different re-opening protocol.

Finally, studying reviews left by past passengers we show the ethnic composition of cars and how it changed during the pandemic. In particular, we first, document an increase in the number of passengers from ethnic minorities, and second, we show some evidence suggesting that drivers from the ethnic majority that review travel requests manually are less likely to accept a minority passenger during the Covid-19 outbreak than before it.[3] To obtain this result we exploit the fact that for a large subset of drivers in our data we observe

[3]McLaren (2020) provides evidence of a strong positive correlation between total Covid-19 cases and the share of a minority population in a county. Interestingly, the difference can be explained by the use of public transport.

all the reviews, both written by passengers traveling with these drivers before the Covid-19 outbreak and during it.

This paper relates to a quickly growing economics literature on the Covid-19 outbreak; Brodeur et al. (2020) provides an overview of this literature. Several papers focused on the inequality of the impact of the pandemic across racial or ethnic lines. In a related paper, Bartos et al. (2020), using a representative sample from the Czech Republic, shows a magnified hostility against foreigners, especially towards those from Asia. At the same time, they show no change in attitude towards domestic out-groups (migrants and minorities).

The rest of the article is organized as follows: in section two, we discuss the timing of lock-down orders and the re-opening in France. We present data that we use to measure the geographical differences in the severity of the Covid-19 outbreak. Section three contains further details on BlaBlaCar, discussion of the data collection process, and finally, we present some descriptive statistics. In section four, we present the main descriptive evidence on the number of trips offered and the prices. Section five focuses on ethnic discrimination, and finally, section six concludes.

2 Covid-19 outbreak, lock-down, and re-opening in France

France introduced a shelter-at-home order on the 17th of March. Mobility was restricted to essential travels and public transportation was substantially limited. These measures were heavily policed. The first stage of re-opening started on the 11th of May; the restriction to essential travels was lifted. However, the non-essential trips were limited to 100 kilometers from the address of residence. The country was divided into *green* and *red* zones. The *red* zone consisted of departments that were most heavily impacted by Covid-19 and some of the measures were enforced longer.[4] Finally, on the 2nd of June, the limit to 100 kilometers was lifted.

Figure 2 shows the change in mobility trends in places related to public transport hubs such as subway, bus, and train stations from Google's Covid-19 Community Mobility report. The three vertical lines are on the: 17th of March (lock-down), 11th of May (1st stage opening), and 2nd of June (2nd stage opening). From this figure, we can see a sharp drop in mobility following the introduction of the shelter-at-home measures and a gradual increase as the country started the re-opening.

[4]Department (from French département) is the administrative unit of France. It is the middle of the three levels of the regional government, between the regions and the communes.

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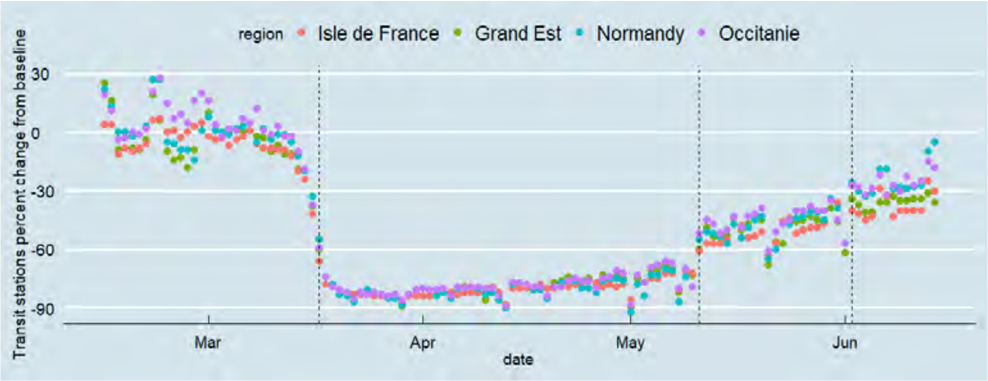
Figure 2: Mobility in France during Covid19 outbreak



Note: Change in mobility in public transport hubs from baseline in selected regions of France. Vertical lines from left: 17th of March (lock-down), 11th of May (1st stage opening), and 2nd of June (2nd stage opening). Source: https://www.google.com/covid19/mobility/

Grand Est and *Ile de France* are regions most impacted by the Covid-19 outbreak, while *Normandy* and *Occitanie* were amongst the less influenced regions. We can see towards the end of the graph (late May and June) that regions less affected were approaching the baseline faster than *Grand Est* and *Ile de France*.

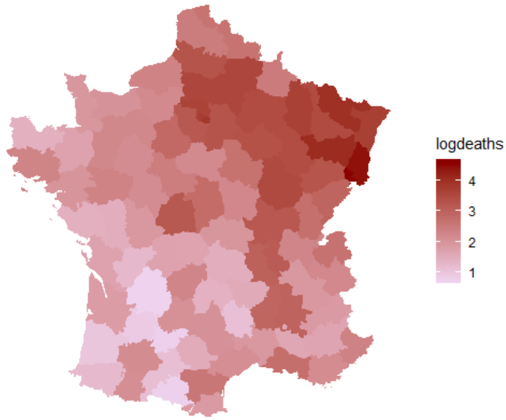
Covid-19 outbreak and related measures: We use four variables, at the daily frequency, to proxy the severity of the Covid-19 outbreak per department. First, we have the number of people that tested positive for the virus per thousand people in the department - (*positive*). We also include the number of tests carried out in the department (*tested*). Second, we use *hosp* and *deaths* which are the number of hospitalizations per thousand inhabitants in the department and the number of deaths per thousand. The source of this data are daily reports available at *https://www.data.gouv.fr/*.

In all regressions that follow we use sums of these measures per department (sums across time, ie, day); thus, we exploit the differences across departments.[5] Figure 3 presents the geographical variation in the number of Covid-19 related deaths in France.

Additionally, we define a binary variable *red zone*, taking the value of one when the observed trip originates from a city in a department designated as heavily impacted by the

[5]These data have also an interesting time dimension, which shows the Covid-19 outbreak in France in time, which we discuss in Appendix A.

Figure 3: Covid-19 outbreak in France



Note: Logarithm of the number of deaths per 100 thousand per department. Source https://www.data.gouv.fr/

Covid-19 outbreak and zero otherwise.

All measures defined above and used in the subsequent analysis refer to the city of departure. Finally, to control for the heterogeneity across departments, we include the share of population above 60 and 75 years per department, and the total population; these variables come from INSEE - the national statistics bureau of France.

3 BlaBlaCar and the data collection

BlaBlaCar is the global leader of city-to-city ridesharing; it is particularly popular in France, where it was established. BlaBlaCar drivers are mostly non-professionals that are looking for a way to cover some costs of day-to-day commutes or longer trips. On BlaBlaCar, the driver sets the price; the passenger observes available drivers and sends a booking request. Some drivers are using automatic acceptance, others reserve the option to manually review the requests.

BlaBlaCar operates at the long tail of transportation; it is most popular on routes where public transportation is of low quality or at times of the day when it is hard to find a ride. The French government has been actively promoting ridesharing. In France, there are over

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2000 zones for ridesharing (*aires de covoiturage*), which are spots close to key transportation hubs and highway entrances that are dedicated drop-off and pick-up locations.

We have started collecting data from BlaBlaCar on the 8th of May, that is during the lock-down in France, and three days before the first stage of reopening and continued until the 12th of July. It's worth pointing out that until the 26th of June BlaBlaCar has restricted the number of seats a driver can offer to one.

The main source of our data is the BlaBlaCar website and it has been collected using the BlaBlaCar's API. Some additional information that is specific to drivers and not available on the API has been collected using a web-scraper. Each day we were looking for all trips that depart from all cities that have a *Hôtel de préfecture*, which means that they can be considered as a capital of the administrative area called department, or in its vicinity. There are in total 102 departments; we have not collected any trips in oversea departments, and we have collected but did not include in the present analysis trips departing from Corsica (there were 76 of them during the sample period).[6]

Table 1 provides summary statistics of main variables.

Table 1: Summary statistics of selected variables

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
price	239,934	16.153	12.040	1.115	6.691	22.304	221.928
distance	239,934	269.328	226.425	0	103	380	1,567
price per km	239,923	0.115	0.302	0.001	0.034	0.124	58.549
average rating	52,375	4.409	1.105	0.000	4.500	4.800	5.000
number of ratings	52,375	42.420	62.029	0.000	7.000	51.000	778.000
red zone	239,934	0.265	0.441	0	0	1	1
hosp	239,934	34.185	27.274	2.852	14.678	50.302	140.141
deaths	239,934	15.766	15.525	0.222	6.102	21.012	101.031
share 60+	239,934	0.284	0.045	0.097	0.253	0.322	0.393
share 75+	239,934	0.103	0.020	0.021	0.089	0.116	0.149
positive	239,934	0.956	1.056	0.002	0.146	1.458	3.790
tested	239,934	0.013	0.013	0.00004	0.004	0.017	0.066
rating	1,889,892	4.653	0.602	1	4	5	5
reviewer minority	1,889,892	0.089	0.284	0	0	0	1
reviewer male	1,889,892	0.489	0.500	0	0	1	1
driver minority	52,375	0.096	0.294	0	0	0	1
driver male	52,375	0.698	0.459	0	0	1	1

[6]The oversea departments are: Guadeloupe, Martinique, the Guianas, Réunion, and Mayotte.

In the data collected from BlaBlaCar's API, we observe the price set by the driver and the commission added by BlaBlaCar. In Table 1 and in the subsequent analysis, we use the price paid by the passenger, which is the price that includes added commission. *Distance* is measured in kilometers (km). *Number of ratings* is the number of reviews (stars from 1 to 5) available on profiles of drivers. *Average rating* is the average of these ratings. Car categories are assigned by BlaBlaCar and include *Berline*, for example Ford Fiesta, Renault Clio, *Van*, e.g., Peugeot 5008, Ford C-MAX, *Break* e.g., Peugeot 508, Audi A3, *Tourism* e.g. Ford Focus, Citroen Picasso, and the last category is *Cabriolet*. We also observe whether a driver decided to signal that they allow smoking and pets and whether the booking is automatic or the driver reviews the requests.

Finally, we used the names available on drivers' profiles to establish their gender and whether the name is either of Arabic or African origin suggesting that the driver is of an ethnic minority. Additionally, we used the names associated with reviews left by the past passengers (both before the pandemic and during) to establish the gender and the ethnicity of passengers. In this way, we also have some information about the driver-passenger pairs before and during the pandemic.

There is a number of missing observations because our data collection routine collects different variables on a different speed. We have collected data on all the trips available from the API; however, drivers' characteristics that were not available from the API (for example ratings and reviews) and were scrapped from the website, are available only for some drivers.

4 Descriptive evidence

In this section, we explore the variation across departments in the severity of the Covid-19 outbreak to study its impact on the number of trips offered on BlaBlaCar and on the prices for these trips.

4.1 Rapid increase in the number of trips post-reopening

With shelter-at-home orders in place and mobility reduced to essential workers, the activity on the BlaBlaCar platform was minimal. As the country started to ease these restrictions, the number of drivers offering trips on BlaBlaCar started to slowly take off. Figure 4 shows the total number of trips per day in the sample period. The two vertical lines indicate the key moments of the de-confinement.

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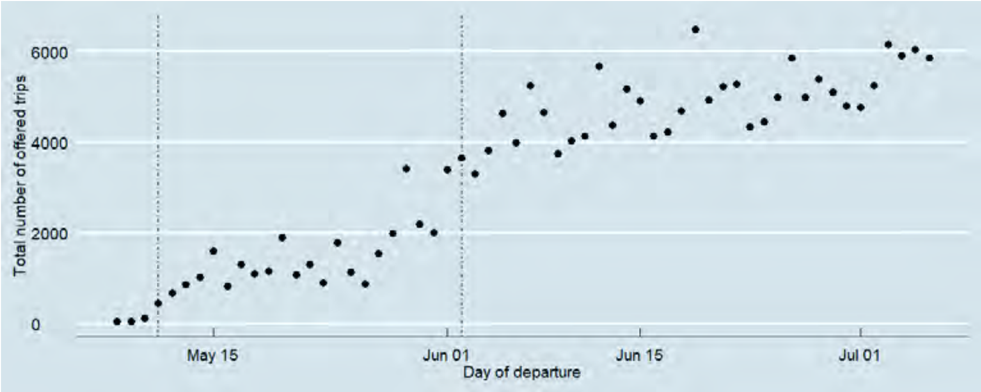
Figure 4: Growth in the number of trips



Note: First vertical line 11th of May is the first stage of easing of restrictions, the second one is the 2nd of June

First, some restrictions were lifted on the 11th of May. Prior to this date, only essential trips were allowed and this has been strictly enforced. After the 11th of May, the non-essential trips up to 100 kilometers from the place of residence were permitted. Next, the line on the 2nd of June shows the second stage of easing of restrictions when traveling in the country became mostly unrestricted. During the period between the 11th of May and the 2nd of June, some departments were indicated as *red*, referring to their higher level of virus circulation. Restrictions in these areas were stricter and enforced more severely.

From Figure 4, we observe that the number trips increased very quickly from the total of 36 trips per day on the 8th of May to around 5000 trips daily in the second part of June and beginning of July.[7]

In the second step, we explore the geographical variation in this growth. To do so, we sum the number of trips by the department and the day of departure and regress these sums on different measures of severity of the virus outbreak. We use four measures: *positive* for the number of people tested positive per thousand, *hosp*, for the number of hospitalization per thousand, *deaths* for the number of deaths per thousand, and *red zone* which is the binary variable taking the value one when the ride departs from a department in the red zone and zero otherwise. The results are presented in Table 2. All regressions have day fixed effects. The dependent variable is in logarithm and we use the OLS estimator. Standard errors are

[7] The dates of opening were announced well in advance.

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clustered at the department level.

Table 2: The severity of the virus' impact determines the number of trips

	Dependent variable:			
	log total trips per department			
	(1)	(2)	(3)	(4)
positive	-0.14^{**} (0.07)			
hosp		-0.01^{**} (0.002)		
deaths			-0.01^{**} (0.004)	
red zone				-0.44^{***} (0.15)
total pop	0.001^{***} (0.0001)	0.001^{***} (0.0001)	0.001^{***} (0.0001)	0.001^{***} (0.0001)
Constant	-0.13 (0.24)	-0.08 (0.24)	-0.11 (0.24)	-0.16 (0.24)
Observations	5,602	5,602	5,602	5,602
R^2	0.57	0.57	0.57	0.59
Adjusted R^2	0.57	0.57	0.56	0.58
Residual Std. Error (df = 5535)	0.77	0.77	0.77	0.76
F Statistic (df = 66; 5535)	112.10^{***}	113.24^{***}	111.15^{***}	118.29^{***}
Note:				*p<0.1; **p<0.05; ***p<0.01

Note that all our measures of the severity of the Covid-19 outbreak are related to a lower number of trips departing from that department, even after controlling for the total population. The magnitude of this effect is quite substantial. An increase from the 25th percentile to the 75th percentile in the number of positive tests is associated with a decrease in the number of trips by 22%, as indicated by the first regression. Using the regression in column two, hospitalization per thousand, we find that the change from the 25th percentile in the number of hospitalizations per thousand to the 75th percentile is correlated with a 21% decrease in the number of trips departing from the city.

4.2 Prices during the pandemic

BlaBlaCar drivers set prices themselves; they receive a recommendation from BlaBlaCar that the price should be 0.062 EUR per km. At this price, a driver should be able (for an average car) to roughly cover the costs of the trip by selling two seats. However, this suggestion is not prominently displayed and generally, most of the drivers deviate from it.

There are several reasons why prices in the period of a pandemic might differ from "normal" times, for example:

1. Low demand due to the introduced measures: during a substantial part of the period of the analysis, the non-essential movement of people was restricted to less than 100 km.

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2. Low supply of transportation alternatives: in the period of the study, severe restrictions in public transportation were in place, and the number of drivers on the BlaBlaCar was much lower than during the pre-pandemic period. Moreover, drivers were allowed to sell only one seat in their cars to promote social distancing.

3. Health risks associated with sharing a ride: higher risk as perceived by passengers can result in a decrease in the demand for seats. On the other hand, drivers might find it more costly (in terms of expected health costs) to have someone in the car, thus they might set a higher price. We conducted a simple text analysis of the comments associated with the listings and we find that 20% of drivers mention "masks" in their description. This suggests that drivers consider health hazards.

The direction of the change in prices, as compared to pre-pandemic levels would indicate which of the above-mentioned effects dominate. Unfortunately, we do not have access to comparable prices from before the Covid-19 outbreak. We will, however, use the geographical variation in the severity of Covid-19 measures to investigate, which of these forces are likely to prevail. Additionally, we provide some comparison with Lambin and Palikot (2019) (LP herein), which has a large dataset encompassing listings posted in 2017 and 2018.

Figure 5 presents a distribution of prices per kilometer, the green line is the mean. We provide two points of reference: the red vertical line is the price suggested by BlaBlaCar, and the orange line is a mean price from LP. The mean price per kilometer is substantially higher both than the price recommended by the BlaBlaCar and the price documented in LP. This suggests that the low supply and health risk as perceived by the drivers are the dominant factors.

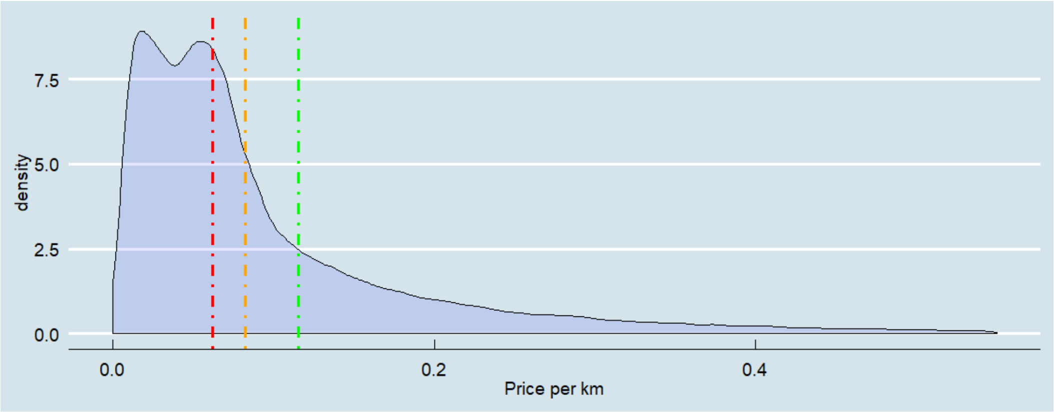
However, a closer inspection of this distribution shows that there is substantial dispersion in prices. The price on the 25th percentile is 0.03 EUR per km while the price on the 75th percentile is 0.12 EUR per km. Furthermore, the median price is 0.065; thus, most of the drivers set prices that are around the price suggested by the BlaBlaCar. The Gini coefficient of prices in our dataset is 0.46, while in the LP it is 0.11.[8] A substantially higher Gini coefficient provides further evidence of the high price dispersion. Finally, the coefficient of

[8]The Gini coefficient is defined as

$$G = \frac{\sum_{i=1}^{n}\sum_{j=1}^{n}|x_i - x_j|}{2n^2\bar{x}},$$

where x are observed prices n is the number of observations and $\bar{x}$ is the mean price. To calculate the Gini coefficient and the coefficient of variation we have used only a subset of our prices associated with trips that depart or arrive to Paris in order to make the sample more comparable to the LP sample. Including the entire dataset does not alter the conclusion.

Figure 5: Distribution of prices per km



Note: Red vertical line - price recommended by BlaBlaCar; orange vertical line - mean price in LP; green vertical line - mean in the current sample

variation (the ratio of the standard deviation to mean) is significantly higher too: in the current dataset, it amounts to 1.11, while in LP to 0.83. Thus, it has increased by 34%. We interpret this as a 34% increase in perceived risk or in the uncertainty of offering the service, for which drivers require compensation, so they set higher prices.

To shed some light on the factors that contribute to the differences in prices, we provide regressions controlling for the severity of the Covid-19 outbreak in the department from which the ride departs. Results are in Table 3. Here we also include controls for the driver level of reputation (the number of ratings and the average rating); additionally, we include whether the driver has a manual acceptance of booking requests, i.e. if she reserves the right to accept or reject the passenger, driver's gender, smoking and pet policy, and the category of the car. Each of the regressions control for day fixed effects. We use the OLS estimator and cluster the standard errors at the level of department.[9]

We observe that all measures of the impact of the virus are associated with higher prices per kilometer. The impact is highly statistically significant. Furthermore, the economic significance of it is also substantial. Around the mean, an increase in the number of positive cases per thousand by one unit is associated with an increase in prices by 6.5% while, an increase in the rate of hospitalization by a unit leads to prices higher by 0.25%.

[9]In Appendix B, we show results of similar regressions that include only controls collected with BlaBlaCar API, which results in a substantially increased sample.

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Table 3: The impact of the Covid-19 circulation on the price per kilometer

	Dependent variable:			
	log(price km)			
	(1)	(2)	(3)	(4)
distance	-0.003^{***} (0.00004)	-0.003^{***} (0.00005)	-0.003^{***} (0.00004)	-0.003^{***} (0.00005)
positive	0.065^{***} (0.012)			
hosp		0.003^{***} (0.001)		
deaths			0.003^{***} (0.001)	
red zone				0.067^{**} (0.031)
total pop				0.0001^{***} (0.00003)
manual booking	0.192^{***} (0.013)	0.192^{***} (0.013)	0.193^{***} (0.013)	0.198^{***} (0.012)
driver male	0.162^{***} (0.013)	0.162^{***} (0.012)	0.165^{***} (0.012)	0.163^{***} (0.012)
average rating	0.024^{***} (0.007)	0.023^{***} (0.007)	0.023^{***} (0.007)	0.024^{***} (0.007)
number ratings	-0.0004^{***} (0.0001)	-0.0004^{***} (0.0001)	-0.0004^{***} (0.0001)	-0.0004^{***} (0.0001)
smoking	0.082^{***} (0.010)	0.085^{***} (0.011)	0.084^{***} (0.011)	0.083^{***} (0.010)
pets	-0.019 (0.013)	-0.021^{*} (0.013)	-0.022^{*} (0.013)	-0.019 (0.012)
Constant	-1.631^{***} (0.134)	-1.672^{***} (0.147)	-1.614^{***} (0.141)	-1.753^{***} (0.161)
Car Category	x	x	x	x
Day FE	x	x	x	x
Observations	37,873	37,873	37,873	37,873
R^2	0.456	0.456	0.454	0.457
Adjusted R^2	0.455	0.455	0.453	0.456
Residual Std. Error	0.759 (df = 37796)	0.759 (df = 37796)	0.760 (df = 37796)	0.759 (df = 37795)
F Statistic	417.403^{***} (df = 76; 37796)	417.131^{***} (df = 76; 37796)	414.329^{***} (df = 76; 37796)	413.186^{***} (df = 77; 37795)
Note:				*p<0.1; **p<0.05; ***p<0.01

4.2.1 Robustness

Our findings are based on observational data, which does not allow us to claim with full confidence a causal nature of the identified patterns. However, to reinforce the conclusion that the increase in perceived health risk results in an increase in prices, we provide two robustness checks. First, we exploit misreports in daily counts of Covid-19 cases as a source of exogenous variation in the perception of such risk. Second, we exclude *red* regions, due to differences in the enforcement of shelter-at-home orders.

Misreports of daily Covid-19 counts: Daily reports of the Covid-19 cases exhibit sudden jumps; we show this in Figure 6. These day to day jumps, or discontinuities, might be due to several reasons such as, for example, mistakes in earlier reporting or a large batch of tests carried out on a given day. It is, however, unlikely that they are due to a sudden change in the number of infections (especially the negative shocks).

One might worry that our claim of the risk premium is due to confounders. That is, there might be omitted (from regressions) region-specific characteristics that are both correlated with the number of Covid-19 cases and the prices. Thus, the positive relationship that we showcase is spurious. If there are, indeed, important variables that we have omitted from our analysis, they are probably unrelated to the errors in reporting of Covid-19 cases. Hence,

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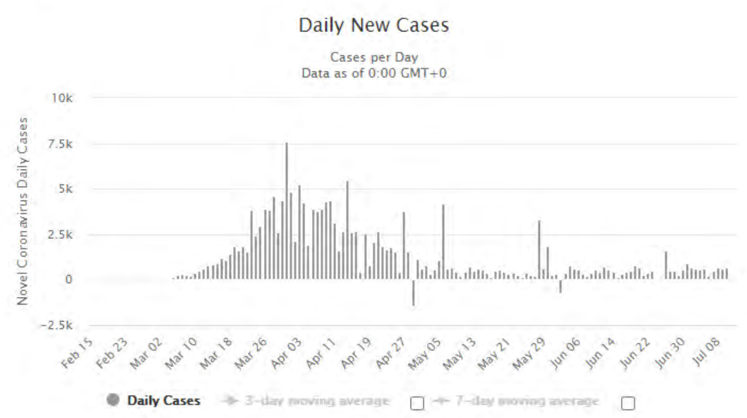
Figure 6: Daily reports of Covid-19 cases in France



Source: https://www.worldometers.info/coronavirus/country/france/

if drivers and passengers use the daily reports in the number of Covid-19 cases, to evaluate the risk of traveling by ride-sharing, we can exploit the errors in these reports as a source of exogenous variation.

In the time-window of our dataset, there are four shocks: three of them are positive (28th and 30th of May, and 26th of June) and one is negative (2nd of June). Drivers' perception of the health hazard just before the positive (negative) jump should be lower (higher) than just after it. Indeed, the prices in our dataset seem to react to such discrete changes. The prices one day before the positive jump are 6% lower than one day after it. The negative shock has the opposite impact on the perception of risk. So when we include both types of shocks, we compare the prices after the negative shock and before the positive one, with prices before the negative shock and after the positive one. Such comparison leads to a change in prices by 3.4%.

To control for potential changes in drivers' characteristics before and after such shocks, region-specific effects, and the time trend, we carry out regression analysis. To do so, we use the sub-sample that includes only trips that depart one day before and one day after these discontinuities. Results are in Table 4. In the second regression, we control for time trend and in the third one, additionally, for department fixed effects. We are interested in the variable *shock*, which takes the value of 1 after positive shocks (31st of May and 27th of June) and before the negative one (1st of June), and zero otherwise (27th of May, 3rd of June, and 25th of June).[10] As before, standard errors are clustered at the department level.

Table 4: The impact of the Covid-19 reporting shocks on the price per kilometer

	Dependent variable:		
	log(price km)		
	(1)	(2)	(3)
shock	0.080*** (0.028)	0.084*** (0.029)	0.084*** (0.029)
distance	−0.003*** (0.0001)	−0.003*** (0.0001)	−0.003*** (0.0001)
manual booking	0.212*** (0.023)	0.211*** (0.023)	0.199*** (0.022)
driver male	0.197*** (0.021)	0.200*** (0.021)	0.193*** (0.022)
average rating	0.008 (0.013)	0.007 (0.013)	0.007 (0.013)
number ratings	−0.0001 (0.0002)	−0.0001 (0.0002)	0.00003 (0.0002)
smoking	0.062*** (0.019)	0.063*** (0.019)	0.065*** (0.019)
pets	−0.024 (0.023)	−0.022 (0.023)	−0.024 (0.024)
Constant	−2.291*** (0.079)	−48.165 (32.095)	−53.793* (31.669)
Car Category	x	x	x
Time Trend		x	x
Department FE			x
Observations	6,120	6,120	6,120
R^2	0.485	0.485	0.511
Adjusted R^2	0.483	0.484	0.503
Residual Std. Error	0.757 (df = 6105)	0.757 (df = 6104)	0.743 (df = 6014)
F Statistic	409.915*** (df = 14; 6105)	383.100*** (df = 15; 6104)	59.870*** (df = 105; 6014)
Note:			*p<0.1; **p<0.05; ***p<0.01

The increase in price following a sudden change in the number of reported cases is robust to changes in drivers' characteristics, time-trend, and the region fixed effects. To calculate the magnitude we use Kennedy transformation (Kennedy, 1981)

$$\frac{\exp(\hat{\delta})}{exp(0.5 \times \hat{V}(\hat{\delta}))} - 1$$

,where $\hat{\delta}$ is the estimated coefficients and $\hat{V}(\hat{\delta})$ is the variance of it. In this way, we find that the impact of the dummy *shock* is 8.7% (we use the coefficient form the last column). The estimated coefficient does not take into account, the fact that the included shocks are of different magnitude. Nevertheless, we conclude from this that an increase in perception of health risk translates into higher prices.

[10] We have excluded the 29th of June because it's between two positive shocks.

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Excluding *red* regions: In the next step, we exclude regions marked as *red*. Indeed, since the level of public transportation restrictions, and the enforcement of the ban on non-essential trips over 100km, might have been stricter in these departments, the prices there would be further increased due to no health hazard risks. The results are in Table 5.

We find that even excluding the regions which were hit the hardest and just exploring the differences in the level of virus circulation within the *green* departments, we find that there is a higher level of prices per kilometer when there are more cases, hospitalizations, and deaths related to Covid-19.

Table 5: The impact of the Covid-19 circulation on the price per kilometer excluding red regions

	Dependent variable:		
	log(price per km)		
	(1)	(2)	(3)
distance	−0.003*** (0.0001)	−0.003*** (0.0001)	−0.003*** (0.0001)
positive	0.039*** (0.012)		
hosp		0.002** (0.001)	
deaths			0.005* (0.003)
manual booking	0.204*** (0.014)	0.205*** (0.014)	0.205*** (0.014)
driver male	0.170*** (0.013)	0.169*** (0.013)	0.169*** (0.013)
average rating	0.019** (0.009)	0.019** (0.009)	0.019** (0.009)
number of ratings	−0.001*** (0.0001)	−0.001*** (0.0001)	−0.001*** (0.0001)
smoking	0.073*** (0.012)	0.074*** (0.012)	0.074*** (0.012)
pets	−0.026* (0.015)	−0.027* (0.014)	−0.026* (0.014)
Constant	−1.759*** (0.047)	−1.886*** (0.074)	−1.865*** (0.075)
Car Category	x	x	x
Day FE	x	x	x
Observations	28,161	28,161	28,161
R^2	0.463	0.463	0.463
Adjusted R^2	0.461	0.462	0.461
Residual Std. Error (df = 28086)	0.750	0.750	0.750
F Statistic (df = 74; 28086)	327.015***	327.147***	326.852***
Note:			*p<0.1; **p<0.05; ***p<0.01

To conclude, at this stage, we cannot completely isolate the impact of the increased health hazard on the prices. The three factors mentioned earlier (the demand decrease, the negative shock to supply of other means of transportation, and health risk) are likely all at play.[11] Nevertheless, we can conclude that the health factor plays an important role: higher prices in regions that are more affected by the virus (even in the restricted sample), the

[11] In Appendix C, we try to disentangle the supply and demand effects by studying the number of sold seats.

frequent mentioning of the need to have masks in the shared car, and the impact of daily reports suggest that drivers are taking into account the health risk factor.

5 Impact of the pandemic on ethnic discrimination

In our sample, there are only drivers that used the platform at least once after the 8th of May. However, most of them used the platform before as well; the average number of ratings is 43. Thus, for each driver in our sample, we have reviews that she obtained before and during the pandemic.[12] Furthermore, we also know the names of reviewers which allows us to establish the ethnicity and gender of the passengers. In total, we have 278 thousands of reviews left during the pandemic and 2.35 millions of reviews left before it.[13]

We observe that the share of passengers that are of ethnic minority increases substantially during the pandemic, specifically from 9% to 14%. We document this increase in Figure 7, which shows the share of minority passengers per day, starting from January 2019. We see that the share of minority passengers was rather stable before the 17th of March, during the lock-down the share of minority reviewers was high and volatile (there is also much fewer observations in this period - 30 thousand), after the shelter-at-home orders were lifted the share of minority passengers stayed at a level higher than pre-pandemic.

Furthermore, we observe that the increase in the number of minority passengers occurs both in cars with minority and nonminority drivers. A nonminority driver before the pandemic had on average 8% of minority passengers, and 13% during the pandemic, while minority drivers increased their share of minority passengers from 16% to 23%.[14]

Again, several factors could contribute to a higher share of minority passengers during the lock-down: for example, minorities might be more likely to be essential workers, thus during the lock-down they had to commute to work.[15]

Note that it is also important to recognize that passengers before and during the pandemic have access to a different set of drivers. For example, if, during the pandemic, we would observe a higher share of ethnic minority drivers, a passenger who prefers a majority driver

[12] In this section we use the 17th of March as the start date of the pandemic, which was the day in which lock-down has been implemented.

[13] Prior literature used names to establish ethnicity; see for example Rubinstein and Brenner (2014); Laouénan and Rathelot (2020). For the purpose of this section, we assume that the propensity to leave a review is the same during the pandemic and before. Note, that generally, a very high share of passengers leave reviews on BlaBlaCar.

[14] Such horizontal sorting is partly due to the route or the day of the trip. The means computed above do not control for such factors.

[15] Borjas and Cassidy (2020) and Coven and Gupta (2020) provide evidence of this from the United States.

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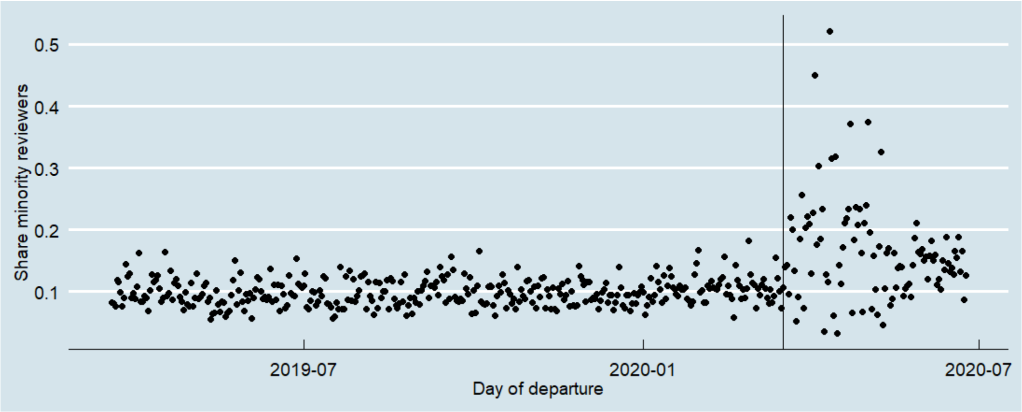
Figure 7: Share of minority passengers per day



Note: The share of reviews written by minority passengers per day. Vertical line March 17.

might find it harder to book a trip during the pandemic then before it. In the current version we do not control for the selection into the sample on ethnicity (and gender) of drivers, but one should recognize that they play a role. (However, we observe that the shares of female and minority drivers are roughly similar to those provided by LP).

Prior literature has shown a positive relationship between greater exposure to health threats and negative attitudes towards out-group members. O'Shea et al. (2020) shows that in US higher exposure to infectious diseases, exacerbates racial prejudice. Do we observe that an increased health hazard, as perceived by the drivers, makes them less eager to accept a minority passenger?

Recall that, on BlaBlaCar, drivers might decide to reserve the right to review a booking request of a passenger - to study her profile for example. An interesting point of comparison is whether drivers that review requests are less likely to take a minority passenger on board during the pandemic. To test for this we carried out two regressions; results are displayed in Table 6.

Column one presents the results of an OLS estimator, while column two is a within panel estimator, where we control for driver fixed effects. The dependent variable takes the value one when a passenger is a minority and zero otherwise; Covid-19 takes the value one for reviews left after the 17th of March and zero otherwise. We are mostly interested in the interaction : *driver nonminority * Covid19 * manual.*

We find that, first, during the Covid-19 there is an indeed higher chance of a passenger

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Table 6: Share of minority passengers across minority and non-minority drivers

	Dependent variable:	
	passenger minority	
	OLS	*panel linear*
	(1)	(2)
driver nonminority	-0.083^{***} (0.001)	
number ratings	0.0001^{***} (0.00000)	
Covid-19	0.071^{***} (0.004)	0.059^{***} (0.004)
manual	-0.022^{***} (0.001)	-0.001 (0.006)
Covid-19*driver nonminority	-0.019^{***} (0.004)	-0.014^{***} (0.005)
Covid-19*manual	0.014^{**} (0.006)	0.007 (0.006)
driver nonminority*manual	0.015^{***} (0.001)	0.001 (0.006)
driver nonminority* Covid-19 * manual	-0.011^{*} (0.006)	-0.016^{**} (0.007)
Constant	0.161^{***} (0.001)	
Driver FE		x
Observations	1,955,469	1,955,469
R^2	0.009	0.001
Adjusted R^2	0.009	-0.013
Residual Std. Error	0.286 (df = 1955460)	
F Statistic	$2{,}142.231^{***}$ (df = 8; 1955460)	238.664^{***} (df = 6; 1929823)
Note:		*p<0.1; **p<0.05; ***p<0.01

being a minority. Second, non-minority drivers are less likely to have a minority passenger on board, and it is even less so during the pandemic. Finally, the coefficient of the interaction: *driver nonminority * Covid19 * manual* has a negative sign. Thus, non-minority drivers that use manual acceptance are even less likely to accept a minority passenger during the pandemic.[16]

6 Conclusion

Our results suggest that a decision maker encouraging the use of ride-sharing during or right after the Covid-19 outbreak should take into account the likely increase in prices, particularly in the regions which are already severely affected by the pandemic, and exacerbated ethnic/racial inequality of access.

In our view, the price premium might be in part due to an increase in the perception of health hazards associated with traveling with a stranger in the car. We provide evidence suggesting that even daily reports of the number of cases shape drivers' perception of such

[16] We have also carried out an analogous analysis concerning male versus female passengers. We do not find any substantial differences due to the pandemic.

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risks. This result highlights the importance of the provision of accurate and up-to-date information.

Design and comparison of policies that might ensure fair access to the service are out of the scope of this article. There is, however, a rich literature studying discrimination in online markets. For example in the context of ride-sharing, Ge et al. (2016) provides a comparison of different platforms, which suggests that removing names from trip booking has the potential to alleviate the immediate problem. Lambin and Palikot (2019) develops a structural model which also shows that the removal of names can mitigate ethnic discrimination.

Finally, we would like to reiterate that the results obtained in this article are based on observational data and we cannot have full confidence in their causal interpretation. Although we have provided some robustness checks and controlled for a number of other potential effects, further work is needed to fully understand the impact of an increase in risk perception, due to a pandemic, on prices, and on access to the service of (ethnic) minorities.

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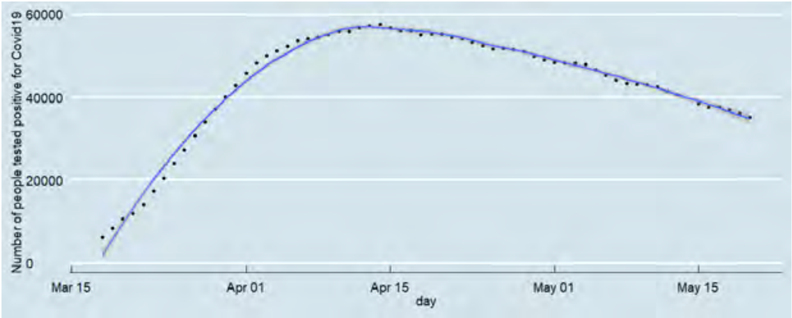
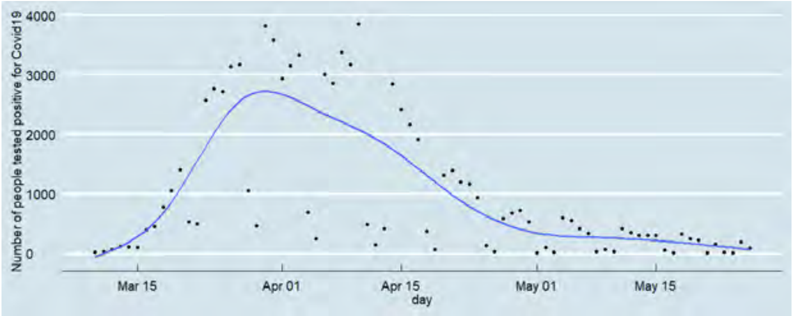
Figure 8: Covid-19 outbreak in time



Note: Sum of people hospitalized. Source https://www.data.gouv.fr/

Figure 9: Covid-19 outbreak in time



Note: The number of people tested positive per day. Source https://www.data.gouv.fr/

A Timing of Covid-19 pandemic

In Figure 3 we have presented the geographical variation in the Covid-19 outbreak. However, these data have also an interesting time dimension, which shows the timing of the pandemic. In Figures 8 and 9, we show daily sums of the number of hospitalizations and positive test.

The two measures are closely related. It is also worth noticing that the shelter-at-home orders were introduced just before the height of the pandemic and lasted throughout the peak of it.

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B Price regressions with larger sample

In the analysis of section 4.2, we have included driver characteristics collected using a web-scrapper. This has allowed us to account for the impact on prices of these characteristics, but at the same time substantially reduced the sample size.

Table 7: Impact of Covid-19 outbreak on prices

	Dependent variable:			
	log(price per km)			
	(1)	(2)	(3)	(4)
distance	−0.003*** (0.00004)	−0.003*** (0.00004)	−0.003*** (0.00004)	−0.003*** (0.00004)
positive	0.068*** (0.016)			
hosp		0.003*** (0.001)		
deaths			0.004*** (0.001)	
red zone				0.088*** (0.031)
total pop				0.0001*** (0.00004)
booking manual	0.184*** (0.007)	0.184*** (0.007)	0.184*** (0.007)	0.192*** (0.007)
Constant	−1.540*** (0.153)	−1.604*** (0.117)	−1.526*** (0.122)	−1.707*** (0.134)
Day FE	x	x	x	x
Observations	239,923	239,923	239,923	239,923
R^2	0.465	0.467	0.464	0.470
Adjusted R^2	0.465	0.467	0.464	0.470
Residual Std. Error	0.763 (df = 239807)	0.761 (df = 239807)	0.763 (df = 239807)	0.759 (df = 239806)
F Statistic	1,811.323*** (df = 115; 239807)	1,826.963*** (df = 115; 239807)	1,806.699*** (df = 115; 239807)	1,831.579*** (df = 116; 239806)
Note:				*p<0.1; **p<0.05; ***p<0.01

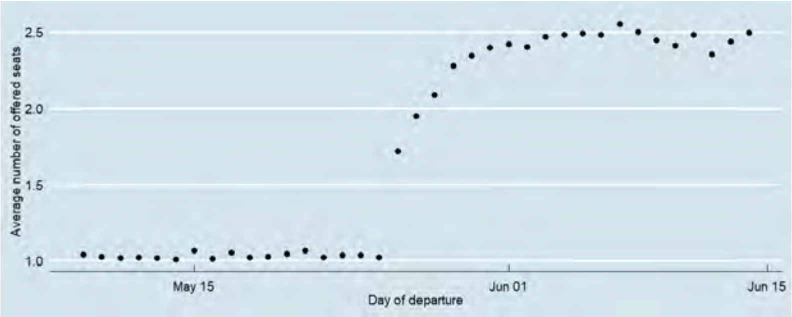
In Table 7, we show the results of regressions with prices as the dependent variable and Covid-19 outbreak measures as explanatory variables. The sample size is increased from 37 thousands to 239 thousands. The conclusion and even the magnitude of the effect are similar to the baseline specification: prices are higher in the departments with more Covid-19 cases.

C Impact of Covid-19 on the number of sold seats

During the height of the Covid-19 pandemic BlaBlaCar has restricted the number of seats a driver can offer on the platform to one. During the de-confinement, starting from the 26th of May, this restriction has been lifted. Figure 10 documents it, by showing the average per day of the number of seats offered by drivers.

Using the part of the dataset after the 26th of May we can study the determinants of the number of sold seats per driver. The variables of key interests include price, measures of the Covid-19 outbreak, and driver characteristics. We are interested in the elasticity of passengers' to changes in price and the impact of the virus. Note, that the availability of other means of transportation might differ depending on the severity of the outbreak in a given department.

Figure 10: Average number of offered seats



Note: Average number of offered seats over the data period; on the 26th of May the constraint to maximum one seat has been lifted.

To address the endogeneity of prices and quantity we introduce instrumental variables. We use sums of characteristics of other drivers (driver male, average rating, number of ratings) and the number of drivers departing from the same city on the same day, and the number of listings on the same route. Such measure of distance in characteristics space influence the markups that drivers can achieve but should not influence the utility of a passenger Berry et al. (1995)).

Note, that we neither control for the number of seats a driver has offered nor for difference in access to other forms of transportation across days and departments. Results are available in Table 8.

The first column in Table 8, shows the first stage regression results. We conclude that the instruments we proposed are statistically significant. Regressions (2) to (4) show that passengers' have negative elasticity to changes in prices. Coefficients associated with the number of the Covid-19 cases and hospitalizations are positive and significant, while the number of deaths is statistically insignificant. Therefore, controlling for the differences in prices, drivers in departments impacted stronger by the Covid-19 are selling more seats.

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Table 8: Sold seats as a function of driver characteristics and Covid-19 oubtreak

	Dependent variable:			
	price	sold seats		
	OLS	*instrumental variable*		
	(1)	(2)	(3)	(4)
price		−0.031*** (0.008)	−0.020*** (0.008)	−0.018*** (0.007)
distance	0.005*** (0.0003)	0.0002*** (0.00005)	0.0001*** (0.00005)	0.0001*** (0.00004)
positive		0.065*** (0.011)		
hosp			0.001*** (0.0004)	
deaths				0.001 (0.001)
driver male	2.177*** (0.143)	0.133*** (0.023)	0.112*** (0.022)	0.111*** (0.021)
manual booking	2.654*** (0.140)	−0.067*** (0.026)	−0.094*** (0.025)	−0.099*** (0.023)
average rating	−0.147* (0.079)	0.104*** (0.009)	0.105*** (0.008)	0.105*** (0.008)
number of ratings	−0.011*** (0.001)	0.001*** (0.0001)	0.001*** (0.0001)	0.001*** (0.0001)
smoking	0.994*** (0.153)	0.079*** (0.018)	0.068*** (0.018)	0.065*** (0.017)
pets	−0.598*** (0.144)	0.010 (0.016)	0.015 (0.016)	0.016 (0.015)
instrument 1	0.307*** (0.035)			
instrument 2	−0.041** (0.019)			
instrument 3	−0.001** (0.0003)			
instrument 4	−0.299** (0.116)			
instrument 5	0.011*** (0.001)			
Constant	10.892*** (0.579)	0.396*** (0.101)	0.289*** (0.093)	0.295*** (0.091)
Car Category	x	x	x	x
Day FE	x	x	x	x
Observations	25,587	25,587	25,587	25,587
R^2	0.082	−0.197	−0.093	−0.076
Adjusted R^2	0.079	−0.201	−0.097	−0.080
Residual Std. Error	10.645 (df = 25500)	1.152 (df = 25503)	1.100 (df = 25503)	1.092 (df = 25503)
F Statistic	26.566*** (df = 86; 25500)			

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

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How has labor demand been affected by the COVID-19 pandemic? Evidence from job ads in Mexico[1]

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There is a concern among social scientists and policymakers that the COVID-19 crisis might permanently change the nature of work. We study how labor demand in Mexico has been affected during the pandemic by web scraping job ads from a leading job search website. As in the U.S., the number of vacancies in Mexico declined sharply during the lockdown (38 percent). In April there was a change in the composition of labor demand, and wages dropped across the board. By May, however, the wage distribution and the distribution of job ads by occupation returned to their pre-pandemic levels. Overall, there was a slight decline in specific requirements (gender and age), no change in required experience, and a temporary increase in demand for low-skilled workers. Contrary to expectations, opportunities for telecommuting diminished during the pandemic. Using a simple Oaxaca-Blinder decomposition, we find that the variation in the average advertised wage in April is explained more by a higher proportion of low-wage occupations than by a reduction in the wages paid for particular occupations. In sum, we find no evidence of a significant or permanent change in labor demand during the pandemic in Mexico.

1 We are grateful for research assistance from Alejandro Uribe. The content of this study does not necessarily reflect the official opinion of the Banco de Mexico or its Board. Responsibility for the information, errors, omissions, and views expressed lies entirely with the authors. Declaration of interests: none.
2 Banco de Mexico, on leave from El Colegio de Mexico.
3 Banco de Mexico.
4 Banco de Mexico.

1. Introduction

COVID-19 has substantially affected the economy in a very short period. Labor markets have been disrupted to an unprecedented extent. More than 20 million jobs were lost in the United States just in April 2020, a tripling of the unemployment rate in that month. In Mexico, the economically active population lost 12 million workers between March and April 2020, a decline of 21 percent. These dramatic changes may have effects on the labor market that are yet to be understood, especially on the behavior of potential employers, and there is a valid and growing concern about whether this shock will permanently affect the labor market. In this paper we analyze this question using data from job ads in Mexico before and during the pandemic.

It is possible that social distancing measures might permanently change the nature of work through a reallocation of jobs and changes in occupational structures (Baldwin, 2020). Delivery-oriented firms and essential sectors are growing, whereas sectors considered non-essential, such as tourism and transportation, are shrinking (Barrero, Bloom, & Davis, 2020). The future of work may be based on maintaining social distance, and it may depend on technology to communicate with and evaluate employees (Stahl, 2020). However, some authors have found that workers expect current work patterns to prevail (Von Gaudecker et al. 2020). It is therefore of the utmost importance to determine whether this crisis will cause a permanent adjustment to labor demand.

Traditional household surveys and administrative data provide ample information on workers' characteristics that can help us understand the effects of the crisis on labor supply. However, there is relatively little information about its effects on labor demand. Specifically, we know little about what types of jobs are still available or what new jobs are most in demand.

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An important source of data regarding labor demand is online job search websites. Web scraping and data science techniques make it possible to analyze the information included in these sites: types of vacancies, wages offered, job characteristics, and qualifications sought can be extracted and disentangled with the use of text analysis. Previous studies have analyzed the job search process (Faberman & Kudlyak, 2016) and the matching process between job seekers and vacancies (Choi, Banfi, & Villena-Roldan, 2019; Kuhn & Shen, 2013a; Marinescu & Rathelot, 2018). Others have focused on labor demand and hiring preferences, including the demand for specific skills and characteristics (Kuhn & Shen, 2015). There have also been studies focused on wages, which have found that the specification of compensation is strategic (Brenčič, 2012), and that wages are related to the wording used in job titles (Marinescu & Wolthoff, 2020). Other studies have analyzed how ads incorporate stereotypes of gender (Arceo-Gómez, Campos-Vázquez, Badillo-Salas, & López-Araiza, 2020) and have revealed the preferences of employers according to the characteristics of the job ads posted (Chowdhury et al. 2018; Kuhn & Shen, 2013b).

Online job boards have also allowed us to understand the labor market during crisis periods. Brown and Matsa (2016) study the matching process between job seekers and vacancies during the Great Recession in the U.S. They find that distressed firms attract fewer candidates and lower-quality applicants than non-distressed firms. Other studies focus on how skill requirements vary in times of economic crisis (Hershbein & Kahn, 2018; Modestino, Shoag, & Ballance, 2019), and find that skill requirements such as minimum education or experience increase when economic conditions worsen.

Forsythe, Kahn, Lange, and Wiczer (2020) analyze job search websites during the COVID crisis in the U.S. They have two key findings. First, job websites replicated the timing of unemployment insurance claims during the early stages of the COVID crisis,

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meaning that the information they contain is relevant at the macroeconomic level. Second, they find that employment vacancies declined by 30 percent in March 2020, and that all types of vacancies were affected, regardless of industry or occupation. For live tracking of the evolution of job postings, see Chetty et al. (2020).

Our paper contributes to this literature in several ways. First, there is scant evidence on how labor demand adjusts to a crisis in a developing country. Second, the crisis of the Great Confinement differs from the Great Recession. There is currently a debate about whether this crisis might permanently change the nature of work and labor demand, for example by favoring occupations with more options for telecommuting instead of face-to-face interaction. We provide some evidence against this possibility, at least in the early stages of the crisis. Third, we analyze the wage and occupational distribution of new job postings, as this distribution provides further evidence of possible changes in the labor market.

The COVID-19 pandemic has severely affected the labor market in Mexico in a very short time, mostly as a result of the measures to contain the virus. The first case in Mexico was diagnosed in late February. On March 14, the government suspended non-essential activities and rescheduled large public events. On March 23, the government announced a voluntary lockdown. These measures had an immediate impact on the labor market: in the last two weeks of March, Mexico lost more than 346,800 formal employees,[1] and the situation worsened in April, with a loss of more than 550,000 formal workers. The most affected sectors were personal services and construction, and more than 80 percent of those who lost their employment were in the lower part of the wage distribution (with earnings less than twice the minimum wage).

[1] https://elpais.com/economia/2020-04-08/mexico-pierde-en-dos-semanas-el-empleo-creado-en-2019-en-un-ambiente-de-tension-entre-el-gobierno-y-los-empresarios.html

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We obtained our data by web scraping the job ads from a leading job search website in Mexico.[2] We downloaded ads daily, beginning in January 2020. The period we analyze here is from January to July 2020. A typical ad includes the job title, the state in which the job is located, compensation, and the text of the ad. Companies use the text to describe the educational and skill requirements of the job, as well as some demographic requirements (age or sex). In total, we analyzed 254,605 ads.

The main results are as follows. We are not able to identify a fundamental change in the nature of work, only a decrease in the number of available jobs, which is expected given the lower level of economic activity. As in the U.S., the number of vacancies declined sharply with the beginning of mobility restrictions: there was a decline of 38 percent in the number of job ads between February and May 2020. Although there was a recovery in June and July, the number of job ads in those months was still 13 percent lower than in February. In April there was a temporary increase in demand in low-wage occupations and a decrease in high-wage occupations. That month, wages also dropped across the board. However, by May they had returned to their pre-pandemic levels. Using a simple Oaxaca-Blinder decomposition, we find that the decline in the average wage offered in April was mainly due to a larger share of low-wage occupations, rather than a change in the wages offered in given occupations. Finally, one might have expected that companies would maintain or increase labor demand for telecommuting jobs, but the data clearly rejects that option. The decline in this type of vacancy was more than proportional: their share of the total number of job ads fell from 51 percent in January-February to 45 percent in July. In sum, we do not find evidence of a permanent or significant change in the nature of work during this crisis in Mexico.

[2] For confidentiality reasons we do not reveal the website's name. However, it is among the top-5 job search websites in Mexico. See https://onaliat.mx/blog/index.php/las-5-mejores-plataformas-buscar-trabajo/.

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2. Dataset

We construct the dataset using information from a leading Mexican online job search website. We web scrape the site daily from January to July 2020, for a total of 254,605 advertisements. The raw dataset contains the posting date, the job title, the description, the compensation offered, the location, and the economic sector of the hiring company. We also build other variables based on the textual description, including age, sex, education, and skills required. Finally, we classify the possibility of telecommuting, based on the work of Monroy-Gómez-Franco (2020).

To construct the wage variable, we use text analysis of the information in the ad; where a range is specified, we take the mean value. We manually classify the job description according to the SINCO 2011 sector and subsector classifications of the Mexican National Institute of Statistics and Geography (INEGI) and the Mexican Ministry of Labor and Social Welfare (STPS). The advantages of these classifications are that they were especially designed for the Mexican labor market, and that they can easily be translated to other international classifications, such as the ISCO-08. This last point is important in following the methodology of Monroy-Gómez-Franco (2020) to classify telecommuting availability.

Other variables are obtained from terms regularly used in the job descriptions. The main objective of these categories is to understand the requirements for the jobs advertised. We classify these variables into three categories: sociodemographic variables, variables related to qualifications and benefits, and pandemic-related variables. Sociodemographic variables are characteristics such as gender, age, location, marital status, and education. Variables related to qualifications and benefits are based on the inclusion of words such as *commitment*, *punctual*, *honest*, *attentive*, *teamwork*, *helpful*, *courteous*, *control*, *initiative*, *motivation*, *pressure*, *proactive*, *responsible*, *enthusiasm*,

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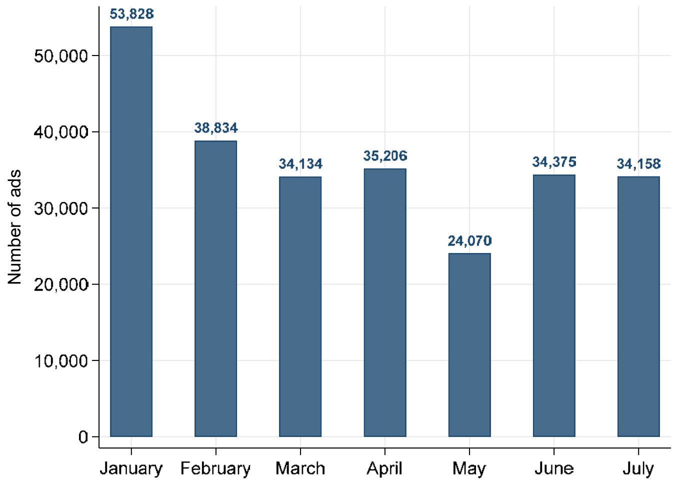
leadership, *requests photograph*, *specifies appearance*, *English*, *common computer software*, *sales*, *customer*, *follow-up*, *availability*, *travel*, *driver's license*, *growth*, *development*, *training*, *bonus, benefits*, *and insurance*. Finally, the last category of variables is based on the inclusion of words related to the COVID-19 pandemic, such as *COVID*, *telecommuting*, *on-site work*, *distance*, and *health*.

3. Evolution and Distribution of Job Ads

Figure 1 shows the number of job ads in the sample. Usually the largest number of jobs are posted in January, advertisements decrease in February, and then they stabilize in subsequent months. From February to April there was a decline of 10 percent in the number of job ads, and from February to May the decline reached 38 percent. These reductions are larger than those found by Chetty et al. (2020) and Forsythe, Kahn, Lange, and Wiczer (2020) for the U.S. Although there is a recovery, the number of job ads in July is still 13 percent lower than in February.

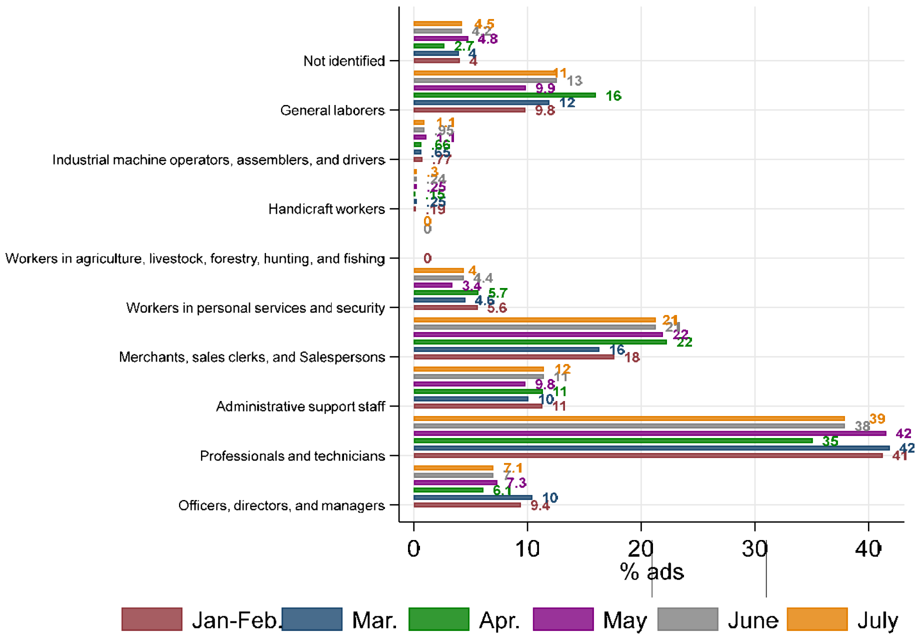
Figure 1. Number of jobs advertised by month, Jan.-July 2020



Note: Authors' calculations.

Figure 2 shows the distribution of job ads by occupation, codified at the one-digit level using the SINCO 2011 classification. Internet job searches in Mexico are biased toward higher-paying occupations. The 2019 Labor Force Survey indicates an average monthly wage of MXN $6,600, while the average in the sample for January and February is MXN $13,445. The occupational category with the highest demand (41 percent of the total ads in January-February) is professionals and technicians.

Figure 2. Distribution of job ads by occupation



Notes: Authors' calculations. Occupations are defined by SINCO 2011 classification at the one-digit level: officers, directors, and managers (SINCO 2011=1); professionals and technicians (SINCO 2011=2); administrative support staff (SINCO 2011=3); merchants, sales clerks, and salespersons (SINCO 2011=4); workers in personal services and security (SINCO 2011=5); workers in agriculture, livestock, forestry, hunting, and fishing (SINCO 2011=6); handicraft workers (SINCO 2011=7); industrial machine operators, assemblers, and drivers (SINCO2011=8); and general laborers (SINCO 2011=9).

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The figure shows the temporary shift in labor demand towards certain occupations. There is a pronounced spike in two occupations between January-February and April: merchants and sales personnel, and general laborers. Although voluntary confinement began in mid-March, there is no evidence of a clear shift in occupational targeting. Only during April does there appear to be a certain shift, but the distribution in June-July is very similar to that of January-February, except for an increase in the share of merchants and sales personnel, and small reductions in the demand for officers, directors, and managers, as well as for professionals and technicians.

Figure 3 shows changes in the frequency (relative to the total number of words) of the most common words in job titles. These 25 words represent 30 percent of the total number of words in job titles (not including stop words). The bars show the frequency of these words for each month, with the values for January-February shown on the right in brown and those for April in green. The figure shows that jobs related to sales, support, and account executives are in greatest demand. By April, there was a substantial increase in the frequency of job titles with the words *support*, *salesperson*, *general* (usually referring to general laborers), *technician*, *helper*, *cashier*, *driver*, *security*, *physician*, and *operator*. Many of these words returned to their January-February values by June-July, except for *physician,* which continued to increase, and for *general* and *cashier*, that remain slightly above their January-February levels.

Intuitively, based on Figure 3, it seems that there was a decline in the demand in April for the best paid managerial and professional jobs, but increased demand for low-paid general labor, operations, and support jobs, and in general a return to the January-February levels in May.

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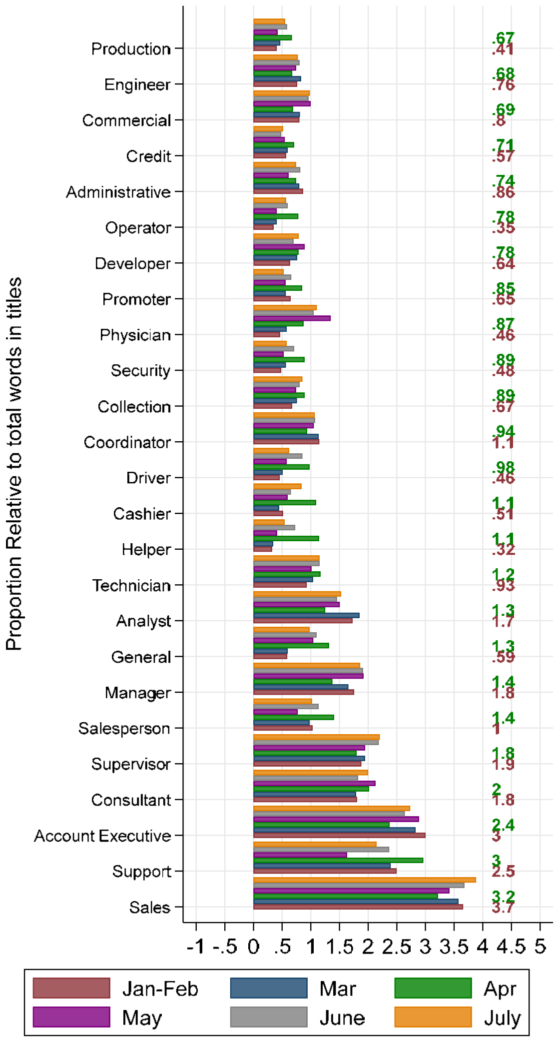
Figure 3. Most repeated words in job titles



Notes: Authors' calculations. The table shows the most frequent words relative to the total number of words in job titles, omitting stop words. To calculate the proportions, we grouped together words with different endings for gender (e.g., *vendedor*/*vendedora*) and number (i.e., plural vs. singular). The figures on the right indicate the proportion in January-February (brown) and in April (green).

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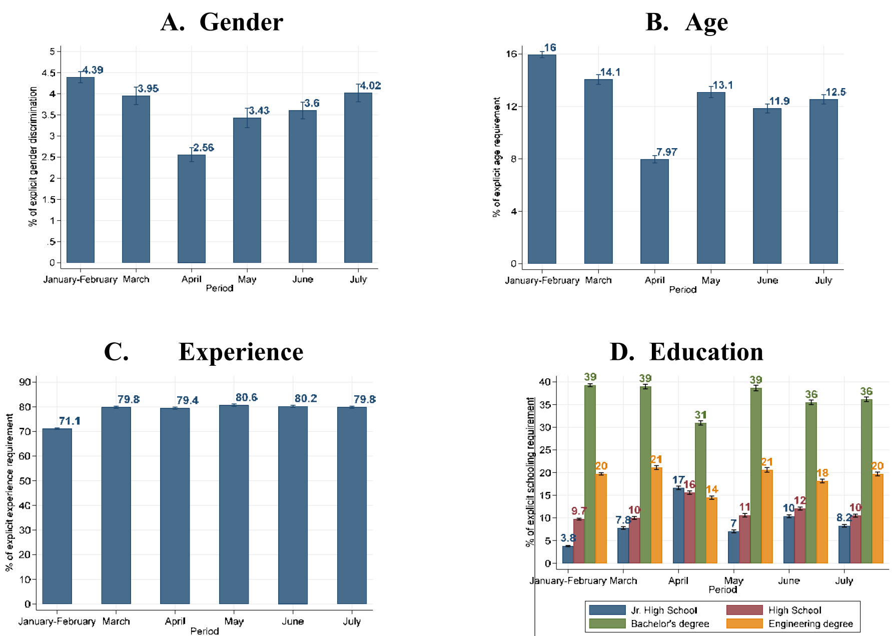
4. Change in Job Requirements During the Pandemic

If labor demand is shifting rapidly, and there is a greater need to hire people, we might expect to see a decline in specific job requirements. Alternatively, if the labor market tightens, we might expect to see an increase in certain requirements, such as experience or education. We calculate the proportion of job ads that specify gender, age, experience, or educational requirements (Figure 4).

Figure 4. Proportion of job ads that include specific requirements



Notes: Authors' calculations. Range plot shows the IC at 95 percent.

Panel A shows that there was a temporary reduction in ads specifying gender. From January to March, about 4 percent of job ads specified a gender, but in April this proportion fell to 2.5 percent. However, by July it had already returned to 4 percent. Panel B shows that there was a similar temporary decline in ads specifying age from March to April (from 14 to 8 percent), and then a rebound from May to July (from 8 to

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approximately 12.5 percent). These two panels combined suggest that labor demand changed significantly during April but that it returned to normal very quickly. The only change in requirements that seems to be more lasting is an increase in the demand for experience, which increased in March and has remained higher than its pre-pandemic level (panel C). This result is consistent with previous findings about stricter labor demand requirements in the context of an economic crisis (Hershbein & Kahn, 2018; Modestino, Shoag, & Ballance, 2019). Finally, the proportion of ads specifying educational requirements (panel D) shifted slightly in April but rapidly returned to its previous level: requests for bachelor's and engineering degrees declined in April while those for junior high and high school diplomas increased.

To better understand the requirements for different jobs, we analyze the use of selected words referring to skills, applicant characteristics, and criteria that could be related to the pandemic. Table 1 shows the percentage of ads that contain a word at least once, for the full sample and for specific occupations such as professionals, merchants, sales personnel, and general laborers, the occupations that have shown the largest change in demand during the pandemic.

Interestingly, characteristics like teamwork and commitment were in greater demand in April. The proportion of ads specifying teamwork increased from 15 percent in January-February to 31 percent in April but returned to the former value in May; in June and July, this specification increased again, to 22 percent. Ads specifying commitment increased from 5 percent in January-February to 13 percent in April; the proportion specifying this characteristic has remained higher than before the pandemic. Other requirements related to social interaction, such as being attentive, having a good appearance, including a photograph in the resume, and knowing English, fell during the first stage of the pandemic but soon returned to their previous levels, suggesting that there was no major shock for face-to-face jobs.

The specification of benefits also changed in April, with a decrease in ads using words such as *training*, *base wage*, and *commission*, but with an increase in the use of the word *benefits*. This could be related to the reduction in the average wage in April, with monetary or non-monetary benefits compensating for lower wages. However, since May these benefits-related words have slowly returned to their pre-pandemic levels.

Additionally, we explore some words related to the pandemic like *work at home*, *on-site*, *health*, *digital*, and *COVID*. Overall, we conclude that there are no large changes in the use of these words during the period studied. Using text analysis, we codify whether a job ad refers to *work at home*. There is an increase in the number of job ads using this term, particularly for professionals and technicians as well as for merchants, salesclerks, and salespersons, but the proportion of ads that use it is nonetheless relatively small (3 percent). There is also a small increase in the number of job ads using the term *work on site*. Thus, it seems that there has not been a substantial change in the option to work at home.

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Table 1. Specific requirements in job posts by occupation.

		All posts				Professional and Technicians				Merchants, Salesclerks & Salespersons				General laborers			
		Jan-Feb	April	May	June-July	Jan-Feb	April	May	June-July	Jan-Feb	April	May	June-July	Jan-Feb	April	May	June-July
Qualifications and Benefits	Observations	92,662	35,206	24,070	68,533	38,205	12,286	9999	26,423	16,333	7834	5274	14,486	9093	5640	2372	8192
	Commitment	5	13	6	8	4	9	5	6	4	16	7	9	8	19	8	14
	Punctual	6	6	5	6	6	5	5	6	6	6	5	5	6	9	5	8
	Honest	1	1	1	1	1	1	1	1	1	1	1	1	2	2	1	1
	Attentive	23	16	22	20	23	18	21	20	33	23	31	30	18	10	17	14
	Teamwork	15	31	17	22	14	25	15	19	11	31	14	21	18	42	22	31
	Helpful	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	Courteous	1	0	1	1	1	0	0	1	0	0	0	1	1	0	1	0
	Control	18	16	21	20	21	19	22	23	9	9	12	11	12	7	12	12
	Initiative	3	2	3	3	3	2	3	3	3	2	2	3	2	1	2	2
	Pressure	9	5	8	8	10	6	9	9	7	4	7	7	7	3	6	5
	Proactive	8	5	8	7	8	5	7	7	8	6	10	9	5	2	5	4
	Responsible	11	8	12	11	11	8	11	11	10	7	12	9	10	6	12	8
	Motivated	38	30	43	44	33	24	29	32	77	56	85	84	36	37	47	36
	Leadership	7	9	8	9	7	10	8	9	7	8	8	9	6	7	8	7
	Requests photograph	4	2	3	3	3	2	3	3	5	2	3	4	2	1	2	1
	Specifies good appearance	11	6	10	10	9	5	7	7	18	9	16	16	7	2	6	5
	English	18	12	18	15	25	20	25	22	11	4	8	7	7	2	7	5
	Common computer software	12	7	12	11	15	11	15	14	7	3	6	6	5	2	4	4
	Sales	34	29	34	34	22	18	18	19	78	61	78	75	40	25	38	33
	Customer	41	44	42	42	35	37	34	34	64	68	66	68	44	42	45	43
	Follow-up	20	14	19	19	19	15	18	19	26	15	22	23	17	10	18	14
	Availability	3	2	3	3	3	2	3	2	4	2	3	3	2	2	3	2

	Travel	6	7	7	7	6	7	6	7	8	7	7	9	5	7	8	7
	Growth	19	12	18	16	19	13	15	14	27	16	26	24	20	8	17	13
	Development	3	3	4	3	4	3	4	3	4	4	6	4	3	2	3	2
	Training	19	14	21	17	18	14	17	15	31	20	35	28	21	10	19	15
	Bonus	18	20	20	18	16	17	14	13	31	29	37	30	23	23	25	22
	Benefits	56	73	62	65	57	67	58	60	60	75	65	70	54	84	69	74
	Insurance	5	4	5	4	5	4	5	4	8	4	6	5	4	2	4	3
	Commissions	15	10	15	13	6	4	4	4	46	25	45	38	27	12	21	18
	Base Wage	29	20	30	27	28	24	28	26	46	23	43	38	32	16	30	23
Pandemic-related words	Work at home	1	2	3	3	1	2	4	4	1	2	4	3	0	0	1	0
	Work on site	1	1	2	2	1	1	1	1	2	1	3	2	1	0	1	0
	Work on site (in job title)	1	1	2	2	1	1	1	1	2	1	3	2	1	0	1	0
	Health	3	4	5	5	4	6	8	8	4	2	3	3	2	2	3	2
	Distance	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	Digital	3	3	5	4	4	4	6	5	3	2	5	4	1	0	1	1
	COVID	0	0	1	1	0	1	1	2	0	0	0	1	0	0	1	0

Note: Authors' calculations. See table S1 for words in Spanish. Each row indicates the percentage of ads that include the word in the first column at least once.

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5. Telecommuting

As noted, the COVID-19 crisis might change the nature of work by increasing the possibility of telecommuting. To account for this mechanism, we analyze the possibility for different occupations. Following Monroy-Gómez-Franco (2020), we match the SINCO 2011 classification to a pivotal classification (ISCO-08) and we then use the methodology of Dingel and Neiman (2020) to determine whether a particular job can be performed remotely. This methodology relies on the work context (such as email use and type of work) and the general activities involved in a job (such as physical activity, manipulation of objects, machines, or equipment, or personal contact with customers or the public). Like Monroy-Gómez-Franco (2020), we classify sales personnel in stores as not adaptable to telecommuting (ISCO-08=5521), because most Mexican stores do not have delivery services or computer systems. However, a disadvantage of this methodology is that the concept applies by its nature to occupations rather than individual jobs, and there may be heterogeneity within occupations in the demand for telecommuting.

Figure 5 shows the proportion of adaptability to telecommuting for different occupations. Anecdotal evidence indicates that there may be increased demand in adaptable occupations during the pandemic (Oppenheimer, 2020; Stahl, 2020). Baldwin (2020) goes even further by suggesting that the pandemic will increase demand for telemigrants, that is, workers working from abroad in countries with lower wages. However, compared to the first two months of 2020, the demand for such jobs in Mexico decreased by 11 percentage points in April and by 6 percentage points in May. This decrease is related to the increase in demand in April for workers in support activities, such as cashiers and drivers, and the decrease in professional activities adaptable to telecommuting. June and July show a similar proportion of telecommuting as in May. In

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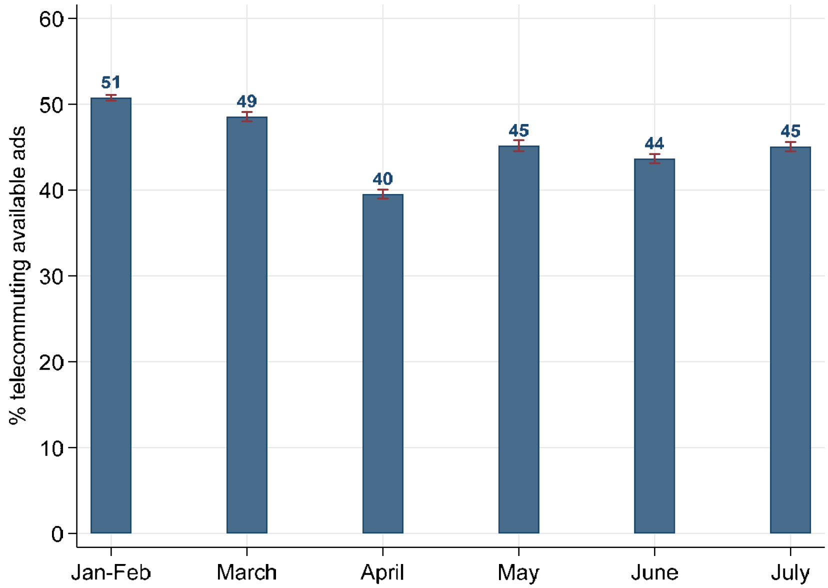
sum, there is thus far no empirical evidence that the nature of work is changing through an increase in demand for telecommuting.

Figure 6. Adaptability to telecommuting



Notes: Authors' calculations. Adaptability to telecommuting was calculated using the methodology of Monroy-Gómez-Franco (2020), including translation of the SINCO 2011 classification to O*NET to classify occupations, as in Dingel and Neiman (2020).

6. Change in Wages During the Pandemic

Table 2 presents the distribution of average monthly wages offered in job ads by month. The average monthly wage fell in April but increased in May. In April there was not only a drop in average wage, but also a shift in the overall distribution, as seen in the wages across different percentiles. This drop in wages is related to the type of jobs in demand. April saw a decrease in demand in the best-paid occupations, such as professionals and technicians, and an increase in demand for low-paid workers in support positions. The increase in wages observed in May is related to the increase in demand in higher-paid occupations. In June and July, the distribution of wages returned to the January-February level.

Table 2. Distribution of Average Monthly Wages, by Month

Period	Mean Wage (MXN)	10th Percentile	50th Percentile	90th Percentile	No. of Obs.
January	13,169 (13,067-13271)	4,697 (4,697-4,697)	9,394 (9,056-9,394)	26,304 (25,834-26,774)	53,495
February	13,827 (13,703-13952)	4,912 (4,725-5,146)	9,823 (9,823-10,151)	28,066 (27,599-28,066)	38,501
March	14,184 (14,049-14319)	4,680 (4,586-4,680)	10,296 (10,296-10,296)	28,080 (28,080-28,548)	33,908
April	12,332 (12,203-12460)	3,971 (3,971-4,078)	7,633 (7,565-7,943)	26,003 (26,003-26,476)	35,013
May	15,349 (15,179-15519)	4,710 (4,710-4,737)	11,303 (11,303-11,303)	30,614 (3,0614-32027)	23,958
June	13,965 (13,830-14,100)	4,684 (4,684-4,684)	9,603 (9,368-9,837)	28105 (28,105-28,112)	34,269
July	14,377 (14,233-14,520)	4,667 (4,667-4,667)	10,267 (10267-10,267)	28,466 (28,000-29,400)	34,088

Note: Authors' calculations. Wages in pesos (MXN) of 2018m7. 95% CI in parentheses, calculated using the binomial method.

To analyze the change across the full distribution we plot wage growth with respect to January-February by decile and month for the period April-July. Figure 7 shows the deciles for each month and the difference in average wage by decile. As seen in Table 2, the wage patterns are very different during April and May. In April, there is a decline in the average wage for each decile with respect to January-February: the wage distribution shifted to the left. By May, however, the pattern is reversed: there is wage growth in all deciles except the second one. The numbers for June and July reveal moderate increases in offered wages along most of the distribution. This figure is

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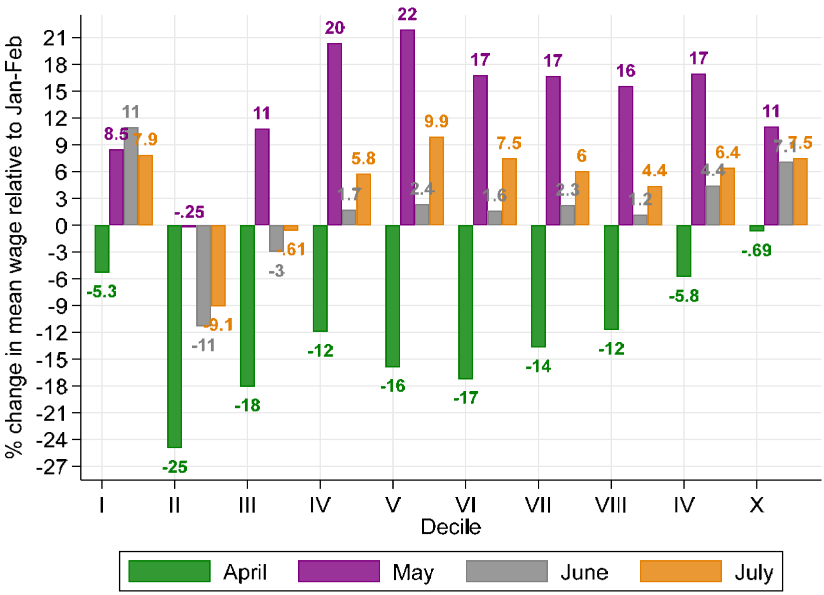
consistent with a temporary and significant change in labor demand during April that is quickly reversed in subsequent months.

Figure 7. Change in wages by decile with respect to January-February



Notes: Authors' calculations. Deciles are calculated for each month and then the average monthly wage is calculated within each decile.

Finally, we implement a simple Oaxaca-Blinder decomposition to investigate what is behind the abrupt change in wages during April and May. The Oaxaca-Blinder decomposition helps to understand whether the change in average wage is due to a shift in the proportion of occupations in demand or to a change in wages within occupations. This decomposition consists in estimating two OLS regressions, one for the period January-February and the other for each month. The independent variables are occupation dummies. If coefficients are stable across months, the Explained component should not change that much. On the other hand, if the Unexplained component grows, it means the premium for each occupation is gaining in relevance.

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Table 3 presents the results for the Oaxaca-Blinder decomposition with respect to January and February. The first rows indicate the average (log) wage of each month. April was the month most affected. The gap was 0.04 in March, but by April it decreased to -0.13 (log points). However, by June and July it had largely recovered. The Explained component row indicates the share of the total differential explained by the characteristics. This explained component proportion is very stable with the exception only of July, when it grows. This implies that the variation in wages observed throughout the period is mainly due to a change in demand for occupations and not a change in wages within occupations. For the specific case of the significant reduction in the average advertised wage in April, this result implies that it is due to a higher proportion of job ads for low-wage occupations than by a reduction in the wages paid for given occupations.

Table 3. Oaxaca-Blinder Decomposition

	March	**April**	**May**	**June**	**July**
Differential					
Current month	9.29	9.11	9.36	9.26	9.29
	[.002]	[.0022]	[.0023]	[.002]	[.0019]
January-February	9.24	9.24	9.24	9.24	9.24
	[.0011]	[.0011]	[.0011]	[.0011]	[.0011]
Difference current month vs. Jan-Feb	0.04	-0.13	0.12	0.02	0.05
	[.0022]	[.0024]	[.0026]	[.0023]	[.0022]
Decomposition					
% Explained	0.31	0.29	0.32	0.30	0.46
% Unexplained	0.69	0.71	0.68	0.70	0.54
Observations in current month	32,566	34,074	22,810	32,812	32,561
Observations in Jan-Feb	88,267	88,267	88,267	88,267	88,267
Total number of observations	120,833	122,341	111,077	121,079	120,828

Notes: Authors' calculations. Standard errors in brackets.

7. Conclusions

The COVID-19 crisis has had a major impact on the economy and the labor market. As a result, there are valid concerns that this crisis might permanently change the nature of work. Using job ads from a leading job search website in Mexico, we show that there is no evidence thus far to support this concern. We download 254,605 ads from January to July 2020 and analyze the jobs advertised and the posted wages, and we use text analysis to analyze the skills and personal characteristics sought.

We find that there is a decline in the number of job advertisements, but that there is no structural change in labor demand. As in the U.S. (Chetty et al. 2020; Forsythe, Kahn, Lange, & Wiczer 2020), the number of vacancies declined sharply in the early stages of the pandemic (around 38 percent) due to the implementation of mobility restrictions. By July, the number of vacancies had partially recovered, and it was only 13 percent less than at the beginning of the year. Most importantly, however, the structure of labor demand changed only temporarily: there was greater demand for low-wage occupations and workers with low educational levels in April, but from May to July demand was back to pre-pandemic levels. The possibilities offered for telecommuting did not increase in this period. The skills and personal characteristics sought did not fundamentally change during the period.

Future research should continue to monitor the behavior of labor demand. Qualitative and case studies are also necessary to investigate changes in the nature of work within occupations and within firms. It is plausible that job ads and aggregate data on employment do not account for small changes in the employer-employee relationship. These small changes might be the causal mechanism that explains structural changes in the nature of work that might occur in the future.

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Supplementary Material

Table S1: Translation of Words in Table 2

	Words English	Spanish
Qualifications and Benefits	Commitment	Compromiso
	Punctual	Puntual
	Honest	Honesto
	Attentive	Atento
	Teamwork	Trabajo en Equipo
	Helpful	Servicial
	Courteous	Amable
	Control	Control
	Initiative	Iniciativa
	Pressure	Presión
	Proactive	Proactivo
	Responsible	Responsable
	Motivated	Motivado
	Leadership	Liderazgo
	Requests photograph	Foto
	Specifies good appearance	Presentación
	English	Inglés
	Common computer software	Software
	Sales	Ventas
	Customer	Cliente
	Follow-up	Seguimiento
	Availability	Disponibilidad de tiempo
	Travel	Viajar
	Growth	Crecimiento
	Development	Desarrollo
	Training	Capacitación
	Bonus	Bono
	Benefits	Prestaciones
	Insurance	Seguro
	Commissions	Comisiones
	Base wage	Salario Base
Pandemic-related words	Work at home	Trabajo en Casa
	Work on site	Trabajo Presencial
	Work on site (in job title)	Trabajo Presencial (en el título del empleo)
	Health	Salud
	Distance	Distancia
	Digital	Digital
	COVID	Covid

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Table S2. Occupations translation in Figure 4

English	Spanish
Officers, directors and managers	Funcionarios, directores y jefes
Professionals and technicians	Profesionistas y técnicos
Administrative support staff	Trabajadores auxiliares en actividades administrativas
Merchants, salesclerks, and salespersons	Comerciantes, empleados en ventas y agentes de ventas
Workers in personal services and security	Trabajadores en servicios personales y vigilancia
Workers in agriculture, livestock, forestry, hunting, fishing	Trabajadores en actividades agrícolas, ganaderas, forestales, caza y pesca
Handicraft workers	Trabajadores artesanales
Industrial machine operators, assemblers, and drivers	Operadores de maquinaria industrial, ensambladores, choferes y conductores de transporte
General laborers	Trabajadores en actividades elementales y de apoyo

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Table S3. Most Frequent Words in Figure 5

English	Spanish
Engineer	Ingeniero
Commercial	Comercial
Supervisor	Jefe
Credit	Crédito
Administrative	Administrativo
Developer	Desarrollador
Operator	Operador
Promoter	Promotor
Physician	Medico
Collection	Cobranza
Security	Seguridad
Coordinator	Coordinador
Driver	Chofer
Supervisor	Supervisor
Cashier	Cajero
Helper	Ayudante
Technician	Técnico
Analyst	Analista
General	General
Manager	Gerente
Salesperson	Vendedor
Consultant	Asesor
Account Executive	Ejecutivo
Support	Auxiliar
Sales	Ventas
Production	Producción

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Table S4. Effect of occupations on Wage (OLS)

	Jan-Feb	March	April	May	June	July
Officers, directors, and managers						
Professionals and technicians	-0.12	-0.02	-0.20	-0.22	-0.15	-0.14
	[.0088]	[.014]	[.019]	[.019]	[.016]	[.016]
Administrative support staff	-0.52	-0.46	-0.72	-0.73	-0.67	-0.64
	[.0096]	[.016]	[.019]	[.021]	[.017]	[.017]
Merchants, Salesclerks and Salesperson	-0.35	-0.26	-0.70	-0.54	-0.52	-0.46
	[.0095]	[.016]	[.019]	[.02]	[.017]	[.017]
Workers in personal services and security	-0.60	-0.47	-0.76	-0.57	-0.60	-0.63
	[.012]	[.022]	[.023]	[.033]	[.025]	[.029]
Workers in agriculture, livestock, forestry, hunting, fishing	-0.24					
	[.18]	[.]	[.]	[.]	[.]	[.]
Handicraft workers	0.15	0.21	0.01	-0.31	-0.71	-0.24
	[.051]	[.076]	[.073]	[.064]	[.015]	[.11]
Industrial machine operators, assemblers, and drivers	-0.09	0.00	-0.26	-0.18	-0.15	-0.12
	[.027]	[.046]	[.048]	[.055]	[.073]	[.058]
General laborers	-0.49	-0.52	-0.81	-0.71	-0.11	-0.23
	[.01]	[.016]	[.019]	[.022]	[.045]	[.039]
Constant	9.51	9.47	9.61	9.75	-0.70	-0.65
	[.008]	[.013]	[.017]	[.018]	[.017]	[.018]
Number of observations	88267	32566	34074	22810	32812	32561
R^2 adjusted	0.08	0.08	0.16	0.11	0.13	0.11

Note: Authors' calculations. Dependent variable ln(wage), robust standard errors showed in brackets

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Employment changes by employer size using the Current Employment Statistics Survey microdata during the COVID-19 pandemic[1]

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We use the Current Employment Statistics Survey microdata to calculate employment changes since February 2020 by employer size. We find that for employers with 1-9 employees, the largest component of employment change since February is due to closings in all months (either temporary or permanent). For 10 or more employees by April, the largest component of employment change since February is employment changes within continuing employers, rather than those reporting zero employment or imputed closures from non-respondents in the survey. In percentage terms, the greatest overall employment losses shifted to larger and larger employers each month. By July, the largest cumulative employment losses were for employers with 100-499 employees, with 8% loss in employment since February, while employers with 1-9 employees had a loss of 4.3% in employment since February.

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Introduction

The Bureau of Labor Statistics' (BLS) Current Employment Statistics Program (CES) is one of the most long-running and relied-upon current measures of the labor market. The CES program collects data each month on employment, hours, and earnings and publishes preliminary estimates at a national level by industry on usually the first Friday of the following month, with revisions published in the two succeeding months. This survey dates back to 1915 for the manufacturing sector, (Ganbari and McCall, 2016) and many CES series are available in consistent format from the Bureau of Labor Statistics (BLS) back to 1939.[1] These data are among the principal federal economic indicators, and make headline news on their release each month.

The CES survey is collected from a large sample of establishments covered by Unemployment Insurance (UI) programs in the United States. Reports from the UI system are compiled in the BLS Quarterly Census of Employment and Wages (QCEW), which is the sampling frame for the CES and other BLS establishment surveys. The QCEW program also publishes estimates of Employment and Wages, and the QCEW data are linked together into the Business Employment Dynamics (BED) program, which publishes estimates of job gains and losses, including by employer size. QCEW data, collected from the full universe of employers covered by UI programs in the United States, are available in much greater detail than the CES data. The QCEW data are available five months after the end of the reference quarter, while employer size estimates from the BED are released about 7 months after that reference date. In ordinary times, employment change by employer size can easily be studied with BED data, and the time lag for these data to become available is only a minor inconvenience that is outweighed by the expanded detail not available with CES estimates. However, these are no ordinary times.

As BLS reported in the April Employment Situation[2] report, job losses associated with the effects of the Covid-19 pandemic on the U.S. economy in the spring of 2020 were the largest in the history of these data. Such enormous, rapid changes in labor markets have led economists to seek available data with as little time lag as possible. The prominent economists of the Opportunity Insights Team are publicly tracking the employment of low-wage workers using data from time card processor Homebase and financial management app Earnin and tracking the number of small businesses that have closed using data from small business credit card processor Womply (Chetty et al, 2020). Another group of prominent economists are tracking labor market outcomes with data from the time card processors Homebase and Kronos. (Bartik et al, 2020b). In this environment, we believe special tabulations based on the CES microdata are particularly valuable.

There is tremendous public interest now in how the economic disruptions of the pandemic are differentially affecting businesses by size, especially because pre-pandemic trends in Economic Census data show increasing market domination by large businesses. In pre-pandemic work, Autor, Dorn, Katz, Patterson, and Van Reenan (2020) showed increasing product market dominance by the largest and most productive firms in industries within the manufacturing, retail trade, wholesale trade, services, utilities and transportation, and finance sectors from 1982 – 2012, and a rising share of US employment in firms employing more than 5,000 employees from 1987-2016. Hsieh and Rossi-Hansberg (2019) showed these effects are strongest within the services, wholesale, and retail sectors, as national chains in these sectors expand into more and more local markets. Rinz (2020) showed that the 5 firms in each

[1] https://data.bls.gov/timeseries/CES0000000001
[2] https://www.bls.gov/news.release/archives/empsit_05082020.pdf

industry with the greatest employment have been expanding into more and more markets from 1976 – 2015.

Since the pandemic began, there have been few studies of employment dynamics by employer size. Those we have seen are based on non-representative surveys with sample sizes smaller than those generally used in producing official statistics. For example, Barrero, Bloom, and Davis (2020) show that many of the several hundred businesses that responded to the Survey of Business Uncertainty plan overall staffing increases, and list examples of large companies expanding their employment during the pandemic, even as many smaller businesses shrink or close.

Most studies of employers and employment patterns during the pandemic use data only for smaller businesses. Bartik, Bertrand, Cullen, Glaeser, Lucac, and Stanton (2020b) examined a survey of 5,800 small businesses, and found the likelihood of closure was highest for the smallest firms at the beginning of April. Fairlie (2020) uses CPS data to examine whether people whose main job is owning a business are at work. He shows that after a 22% fall in the number of these people who were working in April, a little less than half of these people returned to work in May. Bartik, Bertrand, Lin, Rothstein, and Unrath (2020a) use daily work records data from small-business scheduling software provider Homebase and show how hours worked in these data plummeted in mid-March, before starting a slow recovery in late April, with much of this pattern due to firm shutdown. Bartlett and Morse (2020) combine several sources of evidence to examine outcomes for businesses in Oakland, California with 50 workers or smaller, and find that businesses with 1-5 employees fared better than those with 6-49 employees or sole proprietorships. However, these papers cannot compare patterns of employment change for small and large employers.

To our knowledge, the only source of data (other than the CES) well suited for comparisons of employment change in recent months by employer size is the ADP data. These data are compiled from the payroll records of a human resources management company which serves as the payroll processor for about 20 percent of employees in the US. A group of economists (most affiliated with the Federal Reserve) has worked for several years to produce a weekly ADP employment series benchmarked to the QCEW (Cajner et al, 2018) although this weekly employment series is not publicly available except in research papers. In addition, the ADP series does not always exactly match the CES series, such as when ADP data suggested private payrolls had declined by 2.76 million in May,[3] while CES data showed an increase in employment of 2.73 million.[4] This spring, Cajner, Crane, Decker, Hamins-Puertolas, Kurz, Hurst, Grigsby, and Yildirmaz (2020) used these data to show that more small businesses than large businesses paid no employees in April, but the gap in overall employment patterns by employer size narrowed by the end of May as these small businesses reopened. By the end of May, the average employment change in small businesses is a smaller decline than for large businesses.

One focus of economic research this spring has been to use changes in employment and business survival by business size to study the impact of the Paycheck Protection Program (PPP), the unprecedented federal program enacted in March 2020. This program allocated $669 billion in forgivable loans, largely to businesses with 500 or fewer employees. Chetty, Friedman, Hendren, Stepner, and the Opportunity Insights Team (2020) use data from Earnin, a financial management cell phone app, matched with employer names and locations in the ReferenceUSA data, to measure weekly changes in employment rates by business size by industry from February to June. They find little

[3] https://adpemploymentreport.com/2020/May/NER/Report
[4] https://data.bls.gov/timeseries/CES0000000001&output_view=net_1mth

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difference in employment changes by employer size, although their size groups do not correspond neatly to the 500 employee size cutoff of the PPP program. Autor, Cho, Crane, Goldar, Lutz, Montes, Peterman, Ratner, Villar, and Yildirmaz (2020) use the ADP data to do more precise comparisons of employment, hours, and total wages paid by firm size by week from February to June. They find small differences in the changes in employment, total hours, and total wages paid for firms just above and below the 500 employee threshold, and these differences appear during the weeks that the PPP funds were distributed. However, these papers do not address differences in employment patterns by employment size more generally.

In contrast to the datasets described above, the CES offers a large, representative sample of employers. We present recent changes in CES employment by employer size.[5] The remainder of this paper is organized as follows: In the next section, we discuss several methodological issues in producing such estimates. The following section presents these estimates and a final section gives concluding remarks.

Methodology

Published monthly CES employment estimates have three components.[6] The first and largest component is the average rate of employment change experienced by responding establishments that report positive employment in the previous and the current month. The second component is an imputation for the employment change of non-respondents based on the rate of employment change for respondents reporting positive employment. The third component is a prediction from what is called the net birth-death model. This methodology addresses the fact that (a) establishments often do not report data for the month they go out of business and (b) there is about a 7 month lag between the time a new establishment opens for business and the time it appears in the sample frame. Typically, firm births and deaths nearly cancel each other out. Instead of attempting to separately estimate births and deaths, the net birth death model predicts net change in employment from establishment births and deaths on the basis of historical seasonal patterns. The two components are added together to produce a monthly estimate of overall employment change.

The sudden and enormous impact of the pandemic this spring required revisiting some of the assumptions underlying the birth-death model used in creating the official CES estimates. As noted above, prior to the pandemic, establishment reports of zero employment were not explicitly included in

[5] This is not the first effort to use the CES data to estimate changes in employment by employer size. In 2012, the CES program released experimental estimates by firm size for April 1990 through March 2011. (Details of this estimation program are available in Fett and Loya, 2015). However, user comments and internal analysis showed that these estimates required further work in benchmarking employment totals to the QCEW and accounting for new employer births. As efforts to release QCEW data more and more quickly succeeded, there was less reason for BLS to devote additional resources to the improvement of CES estimates by employer size, and the experimental estimates were removed from the BLS webpages in early 2020, as described at https://www.bls.gov/ces/notices/2017/size-class-discontinuation.htm. However, the scale of employment changes in recent months has given us new reason to produce more timely estimates of changes in employment by employer size. Furthermore, benchmarking is not an issue for our short run analysis and there have likely been few new employer births during the pandemic.

[6] For a more detailed and complete description of the CES estimation methodology in general and the net birth-death model in particular, see Mueller, 2006.

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the estimates, but were handled implicitly through the net birth-death model. Beginning in April, reports of zero employment above what would normally be expected were explicitly included in employment change estimates. Excess returns of employment from zero (which became more important in May) were also explicitly included in these employment change estimates. In addition, sample growth rates for the portion of the sample reporting positive employment were included in the net birth/death model to capture the fact that business openings and closing have a cyclical component.[7]

This paper takes a somewhat different approach to addressing the estimation issues raised by the pandemic. We focus on establishments that responded to the February survey and disregard births since then, on the presumption that openings have been negligible during this period of uncertainty and record high unemployment. [8] Similar to the published CES estimates, the major component in our estimates of employment change is the average sample growth rate of February establishments that continue to report positive employment in subsequent months. Unlike the published CES estimates, we explicitly include all reports of zero employment in our estimates of employment change. In addition, we explicitly impute the employment of non-respondents, using employment change for respondents in the same month and the proportion of non-respondents with zero employment based on previous years of QCEW data.[9] Since we disregard openings and estimate the fraction of non-respondents that have zero employment, we do not use the net birth/death model at all.

We measure employer size by total employment across all establishments using the same Employer Tax ID (EIN) when they file reports with state UI programs, assigning size groups from annual average employment for fourth quarter of 2019 in QCEW data, with no reclassification of size groups as employer sizes change over time. Readers should be cautioned that the EINs reported to the UI system are nonrandom pieces of firms; there are many instances in our data in which a large firm acquires an establishment but the payroll department of that establishment does not switch to the new firm's EIN in reporting employment to its state UI program.

As noted above, our methodology explicitly distinguishes between non-respondents and respondents with zero employment. Because a substantial number of establishments that do not respond in time for the first preliminary estimates do respond in time to be included in the second preliminary estimates, we use CES estimates based on the second preliminary estimates wherever possible. Thus, at the time this article was written in August, the most recent numbers were those available for June, with more preliminary numbers available for July. We intend to update this article as new data are collected.

A. Continuing employers

[7] A more detailed description of the adjustments in the CES estimation methodology can be found on the BLS website, at https://www.bls.gov/web/empsit/cesbd.htm.

[8] During the Great Recession, firm openings fell by 27% from their high in 2005 to 2010. The economic contraction we are currently undergoing is much more severe than the Great Recession, and it is reasonable to expect that openings have declined by a greater amount. Consistent with this, new business applications of likely employers, as tabulated by the Census Bureau's Business Formation Statistics Series, fell sharply from March through the first week of June. However, these increased beginning in June. We are not sure what to make of this, but note that to the extent this increase reflects potential births of new establishments, there is still often a lag between the application and the actual opening.

[9] As noted above, the CES also imputes the employment of non-respondents, using the average employment change for respondents in the same month. However, the CES also imputes this employment change to establishments that report 0 employment and subsequently corrects for this with the net birth-death model.

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Let emp_{iMSJ} denote the employment of establishment i in month *M*, size class *S*, and industry *J,* and emp_{iFebSJ} denote the employment of the same establishment in February. The change in employment between month M and February for all establishments that respond in month *M*, report positive employment in month M, are in size class *S*, and are in industry J is given by

(1)

$$\Delta EMP_{R,M,C,S,J} = \sum_{i\in R,C} (emp_{iMSJ} - emp_{iFebSJ})$$

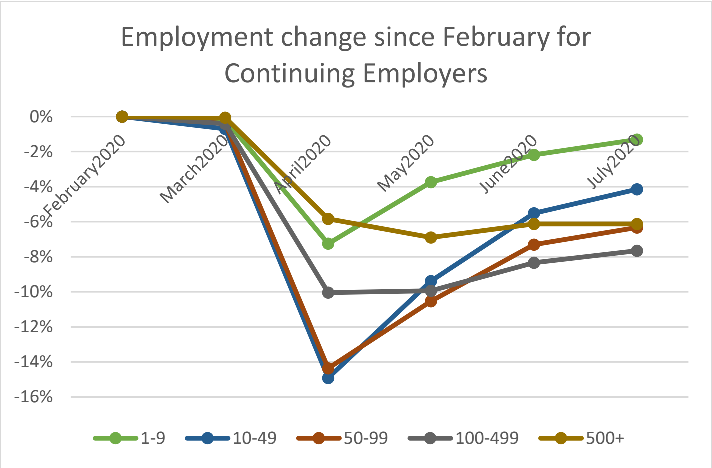
where *R* is the set of responding establishments and *C* is the set of continuing establishments with positive employment in month *M*. Then the percentage change in employment for these continuing establishments is

(2)

$$\%\Delta EMP_{R,M,C,S,J} = \frac{\sum_{i\in R,C}(emp_{iMSJ} - emp_{iFebSJ})}{\sum_{i\in R,C} emp_{iFebSJ}}$$

Figure 1 shows the percentage change in employment in each month for each employer size category, relative to February employment for these establishments. This estimate is obtained by estimating equation (1) using weighted employment in February[10] for each establishment in size class *S* across all industries. All size groups have had employment below February levels through July. In percentage terms, the greatest employment losses were shifting to larger and larger employers for each subsequent month. The employment trough was in April for all size groups except 500+, which had lower employment in May than in April.

Figure 1:



[10] Weighted employment takes the sample weight times the reported employment.

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B. Employers Reporting Zero Employment

The change in employment between month M and February for establishments that report zero employment in month *M*, in size class *S*, and industry J is given by

(3)

$$\Delta EMP_{R,C',S,J} = -\sum_{i\in R,C'} emp_{iFebSJ}$$

where *C'* is the set of establishments reporting positive employment in February and 0 employment in month *M*. The reduction in employment at closing establishments in month M relative to the average employment of respondents in February is given by

(4)

$$\%\Delta EMP_{R,M,C',S,J} = \frac{-\sum_{i\in R,C'} emp_{iFebSJ}}{\sum_{i\in R} emp_{iFebSJ}}.$$

Figure 2 shows the percentage of establishments that report zero employment in each month, relative to the number of establishments that existed in February, by employer size categories. In every month, the employer size category with the largest fraction of employers having no employment is the smallest size category: employers with 1-9 employees. However, there is again some shift in the distribution of employment loss from smaller to larger employers from April through June and July.

Patterns of establishment closure over time have been somewhat different for employers of different sizes. Among employers with 49 employees or less, the fraction of establishments closed was greatest in April and has been declining since. Among employers with 500+ employees, the fraction of establishments closed was highest in May. Based on preliminary numbers, the fraction of establishments with zero employment increased from June to July among employers with 50 to 499 employees.

Figure 2:

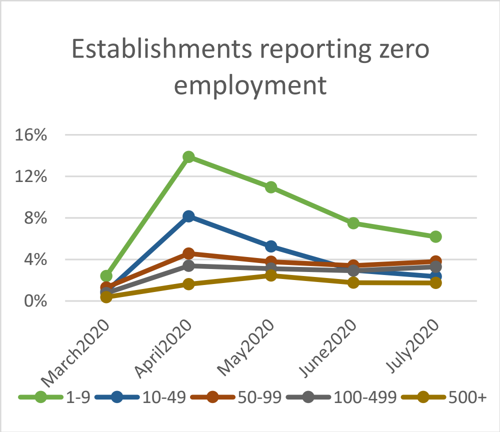

Figure 3:

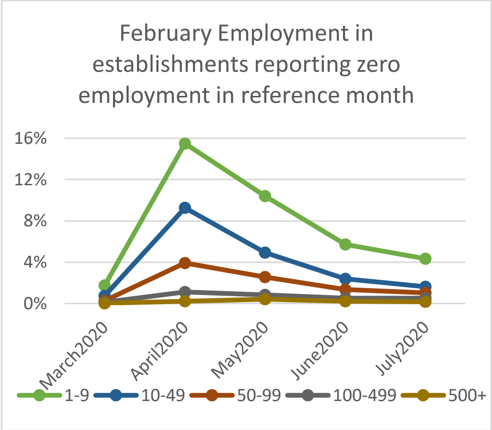

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Figure 3 shows the percentage of February employment lost in these establishments reporting zero employment each month. Patterns are very similar to Figure 2. However (except for the smallest two size categories in April), the fraction of employers reporting zero employment is always greater than the fraction of employment lost due to employers reporting zero employment. This suggests that within each category, smaller employers are more likely to have zero employment.

C. Employers that did not respond to the CES

Finally, we estimate the change in employment for establishments that responded to the survey in February but did not respond in month M. Similar to the birth-death model used in standard CES estimates, we assume non-respondents with positive employment in month *M* experienced the same changes in employment as similarly sized responding establishments in the same industry.[11] Additionally, using prior years' data from the QCEW to estimate the probability a non-responding establishment in the CES is closed, we include this probability of an establishment being closed in the imputed employment for each non-responding establishment in month M.

To estimate the proportion of non-respondents with zero employment, we use QCEW data from 2007 through 2018 to model the probabilities that responding and non-responding establishments subsequently close for good. For each month *M*, establishment size class *S* and industry *J* we calculate the proportion of CES non-respondents that last have positive employment in the QCEW in the same calendar year, $\alpha_{MSJ}^{R'}$. Similarly, we denote the proportion of CES month M respondents in size class *S* and industry *J* that last have positive employment in the QCEW in the same calendar year as α_{MSJ}^{R}. Let $c_{MSJ} = \frac{\alpha_{MSJ}^{R'}}{\alpha_{MSJ}^{R}}$ denote the ratio of these two proportions and let b_{MSJ} denote the fraction of responding establishments in size class *S* and industry *J* that report zero employment to the CES in month M in 2020, as depicted in Figure 2. We assume the fraction of non-respondents with zero employment is equal to the product of b_{MSJ} and c_{MSJ}.[12] For example, if non-respondents and respondents in size class *S*, and industry *J* closed with the same frequency in 2007-2018, we assume the fraction of non-respondents in month *M* that have zero employment is exactly the same as the proportion of respondents that report zero employment. If the non-respondents in size class *S*, and industry *J* in 2007-2018 close with a 20% higher probability than similar respondents, then our specification implies the fraction of non-respondents in month M that have zero employment is 20% higher than the proportion of respondents that report zero employment in month M.

Given the above assumptions, our estimate of the percentage change in employment between month M and February for non-responding establishments in size class *S* and industry J is

(5)

$$\%\Delta EMP_{R',M,SJ} = \%\Delta EMP_{R,M,C,S,J} - \left(b_{MSJ} * c_{MSJ}\right)$$

[11] This assumption is likely conservative in the present context since establishments with greater disruptions in employment may well be less likely to respond to the CES.

[12] Of course, this is an approximation since closing for good is not the same as having no employment in a given month, but c_{MSJ} should capture the fact that establishments with no employment are more likely to be non-respondents that establishments that are operating.

We estimate the percentage change in the employment of non-respondents as the percentage change in the employment of responding employments with positive employment minus the estimated probability that a respondent reports zero employment.

Figure 4 shows the percentage of sampled establishments that did not respond to the CES, conditional on responding in February, by employment size categories. For March-June, these are percentages as of the data when BLS compiles figures for the second preliminary estimate (6-7 weeks after the reference week), but for July the preliminary estimates are based on data collected only 2 weeks after the reference period. These percentages increase over time for the largest employer size category, but otherwise do not have much pattern over time.

Figure 4:

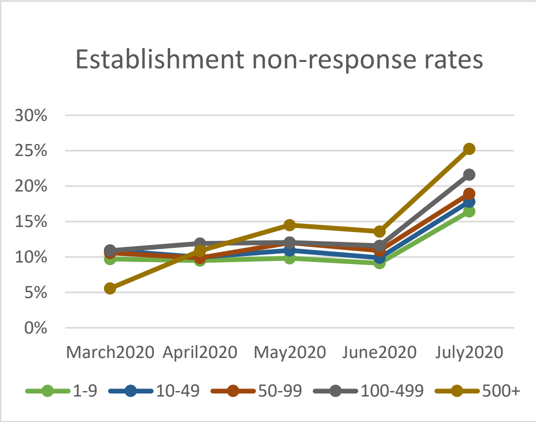

Figure 5:

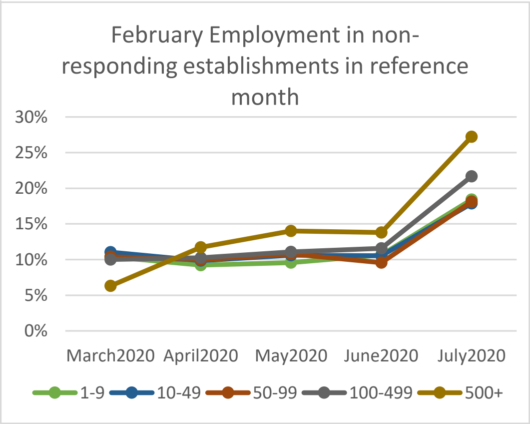

Figure 5 shows the percentage of February employment lost for employers that did not respond to the CES. This is very similar to Figure 4: there is a time pattern for March through June only for the largest employers. July's much higher non-response rate is strictly a function of using the 4-5 week earlier data collection cutoff compared to the other months. For this reason, we add a correction to the July estimates described in detail below.

<u>Overall Employment Results</u>

We now examine the contributions of these three separate components to the overall decline in employment since February 2020. Note that the estimated change in employment of non-respondents in size class *S* and industry *J* in month *M* is given by

(6)

$$\widehat{\Delta EMP}_{R',M,S,J} = N_{M,R',S,J} * \overline{emp}_{Feb,R',S,J} * (\%\Delta EMP_{R,M,C,S,J} - b_{MSJ} * c_{MSJ})$$

$N_{M,R',S,J}$ is the number of non-respondents in month M size class *S* and industry *J* and $\overline{emp}_{Feb,R',S,J}$ is the average establishment employment in February for the set of non-responders R' in month M, size

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class *S,* and industry *J*. Total February employment is determined by multiplying the number of eligible respondents in month M, size class S, and industry J by the average February employment, denoted by

(7) $$N_{M,S,J} * \overline{emp}_{Feb,S,J}$$

Dividing (1) by (7) yields the percentage reduction in overall employment of establishments in size class S and industry J due to the decline in employment at continuing establishments. Dividing (3) by (7) yields the percentage reduction in overall employment due to the decline in employment at closing establishments. Dividing (6) by (7) yields the percentage reduction in overall employment of establishments in size class S and industry J due to the decline in employment for non-respondents. For July, in order to correct for a higher non-response rate due to using preliminary data, we assign the non-response rate for the previous month to July's numbers. So

$$\frac{N_{July,R',S,J}}{N_{July,S,J}} * \frac{\overline{emp}_{Feb,R',S,J} * (\%\Delta EMP_{R,July,C,S,J} - b_{JulySJ} * c_{JulySJ})}{\overline{emp}_{Feb,S,J}}$$

Becomes

$$\frac{N_{June,R',S,J}}{N_{June,S,J}} * \frac{\overline{emp}_{Feb,R',S,J} * (\%\Delta EMP_{R,July,C,S,J} - b_{JulySJ} * c_{JulySJ})}{\overline{emp}_{Feb,S,J}}$$

where $\frac{N_{June,R',S,J}}{N_{June,S,J}}$ is the non-response rate for June for size class S and industry J.

Each of these three percentage changes are for a specific super sector industry. To obtain percentage changes for the economy as a whole, we sum across all industries.[13] Results are depicted in Figure 6. We see in Figure 6 that the massive employment changes of the last few months were driven by employment losses in continuing establishments in every employer size category except for the very smallest employers. For employers with 1-9 employees, job losses (and gains) were driven by employer closures and re-openings.

[13] More precisely, we estimate the employment change components separately for each size and industry group, and then weight them by the share of employment in each group.

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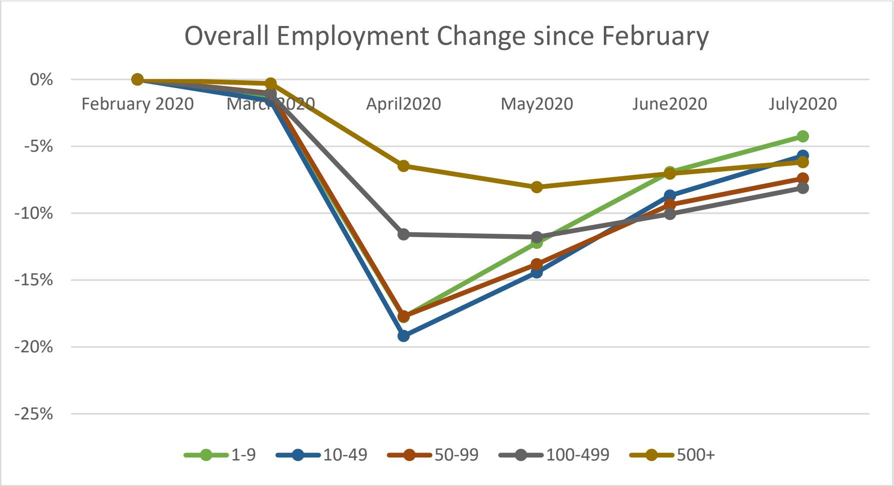
Figure 6: Employment change by size category and component of change

Overall employment changes are the sum of the changes in these components. Figure 7 shows overall employment changes since February by employer size. The largest declines in employment were in April for employers with fewer than 100 employees. . At the trough in April, employment loss was 17.8%, 19.2%, 17.7%, 11.6%, and 6.5% relative to February for size classes 1-9, 10-49, 50-99, 100-499, and 500+, respectively. Overall employment recovery since then has been much faster in smaller employers. Between April and June, employment levels largely recovered for employers with less than 100 employees, recovered much less for employers more than 100 employees. By May, overall employment losses for employers that had 10-49 employees were no worse than for employers that had 50-99 employees. In June (and in preliminary figures for July), the group of employers with the largest overall employment losses were those that had 100-499 employees. As of July, employment loss was 4.3%, 5.7%, 7.4%, 8.1%, and 6.2% relative to February for size classes 1-9, 10-49, 50-99, 100-499, and 500+, respectively.

Figure 7:



Conclusion

In this paper, we have documented the rationale of producing estimates of recent employment changes by employer size using a sample, we have explained our method of producing these estimates, and shown the results. Our methods for producing these special estimates rely on disregarding the net-birth-death modeling of the official CES publications and instead examining only the set of establishments that responded to the CES survey this past February. These procedures will only be appropriate as long as employer births remain negligible and this group of establishments does not rotate out of the CES sample.

We find that the massive employment changes of the last few months were driven by employment losses in continuing establishments in every employer size category except for the very smallest employers. For employers with 1-9 employees, job losses (and gains) were driven by employer closures and re-openings. The largest employment impacts of the pandemic were for employers of 1-99 employers in April, but as the pandemic-induced economic crisis continues, its employment impacts are shifting to larger and larger employers. By June and July, the largest impacts are for employers of 100-499 employees, and employment recovery for employers of 500+ employees appears slower than for smaller employers.

The patterns of employment losses since February that vary much less by employer size in July than they did in April, and of employment losses that by July are greater for employers of 50 or more employees than for employers of 1-49 employees, may surprise some readers. However, these patterns are similar to those shown for February through May in Figure 3 of Cajner et al, based on ADP data. Those researchers also document greater initial employment falls in late March and April for smaller employers, with a faster recovery in employment in May for smaller employers, leading to convergence in cumulative employment patterns for employers of different sizes by the end of May.

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Socially conscious investors: Mitigating stock market losses during the COVID-19 crash[1]

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Recent years have witnessed a surge in demand from investors who invest with a socially responsible mandate. We study stock returns associated with this practice in the COVID-19 crash and the months before it. Based on data on mutual funds, we find that stocks with greater socially conscious investor ownership experience superior returns, lower returns volatility, and better market valuations during the pandemic. No differences are to be found with respect to gross profitability, operating income, and sales growth as well as expectations about the long-term growth rate of earnings per share. This suggests that socially conscious investors can act as a moderating force by mitigating losses in the stock market in a time of crisis.

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1. Introduction

For a large group of investors in the United States, social considerations matter a great deal in their investment decisions. According to the U.S. Forum for Responsible and Sustainable Investment, socially responsible investments in 2018 add up to about a quarter of total assets under professional management (US Forum for Responsible and Sustainable Investment 2018).

With the socially responsible investing phenomenon being so big, what sort of impact has it had on stock returns? According to the model in the seminal paper by Heinkel, Kraus, and Zechner (2001), investors pay a cost in terms of stock returns in order to satisfy a social responsibility criterion as they drive up (down) share prices of the firms they prefer (eschew). Hong and Kacperczyk (2009) and many other notable empirical studies (e.g. Baker, Bergstresser, Serafeim, and Wurgler 2018) have since then confirmed this theoretical prediction in equities as well as other asset classes.

However, could there be more to this mechanism that links social preferences to stock returns? Specifically, under what regime might socially preferred stocks earn higher returns?

To investigate this question, we utilize the COVID-19 pandemic as a plausibly exogenous negative shock to the U.S. stock market and the economy as a whole. In a matter of weeks, as many investors rushed to sell off their stocks, the S&P 500 Index experienced a double-digit nosedive relative to its peak in late February 2020 with the plunge being fully apparent in the following month. To the extent that some stocks are held by enough investors for reasons owing to social motivations, then such stocks may experience superior stock market performance from a weaker decline in market valuations during this period of distress.

Using data on mutual fund holdings, we discover that stocks in the S&P 500 with greater ownership by socially conscious mutual funds did indeed perform better during the pandemic period. A one standard deviation increase in socially conscious mutual ownership corresponded to roughly a three percent increase in returns at the monthly level. An examination of book-to-market ratios reveal that firms with lower socially conscious mutual fund ownership exhibited a stronger decrease in market valuations. As one may expect with owners who are holding stocks in part to satisfy their social preferences, these stocks also exhibited lower stock return volatility in a time of massive selloffs.

In light of these facts from the market, could it be that the companies preferred by socially conscious mutual funds are better in terms of operating performance? After all, it is conceivable that the act of expressing social preferences in investment decisions may go hand in hand with choosing businesses that weathered the economic downturn better. To examine this possibility, we examine sales growth, gross profitability, and operating income. While businesses as a whole saw a marked decline along these key operating performance benchmarks, we find no evidence that the firms preferred by socially conscious mutual funds did any better.

To offer a more structured interpretation of these findings, we introduce a model related to Heinkel, Kraus, and Zechner (2001), Fama and French (2007), and Pastor, Stambaugh, and Taylor (2020) in which there are two groups of investors. The first group consists of socially neutral ones who only care about financial payoffs. The second group are socially conscious investors who exhibit social preferences in their portfolio choice by deriving utility from holding shares of socially preferred firms. As a result of this second group, the share price of the socially preferred firm is driven up as market valuations reflect both financial payoffs and social preferences. In our model, the preferences of these socially conscious investors are different from those found in these earlier models involving investor tastes in

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that such investors not only exhibit a preference for holding certain shares but, in pursuing a social responsibility investment mandate, also care less about the financial payoff of the socially preferred firms.[1] This latter feature is important as it helps to bridge a divide between existing theory and the empirical facts we learn from the pandemic in this paper. Specifically, the investor preferences found in the setup of previous models can generate predictions that are qualitatively consistent with the returns and volatility results we find but fail to account for the reduction in the decline of market valuations of the socially preferred firms in an economic downturn. By contrast, the preferences we introduce in our model for socially conscious investors resolve this issue while maintaining the same predictions for returns and volatility as before.[2]

This paper contributes to the empirical literature that studies the asset pricing implications of socially responsible investing by showing how socially conscious investors can affect returns differently depending on the economic environment. In doing so, we provide a rational economic explanation that links the theory of socially conscious investments sacrificing returns to a scenario under which such investments can enjoy superior returns. Previous efforts to document and explain the few instances of the latter have traditionally relied on arguments based on third-party company scoring with the premise that the scoring accurately measures corporate social responsibility (CSR), some notion of social capital, or employee satisfaction in companies (Lins, Servaes, and Tamayo 2017; Edmans 2011).[3]

[1]Some prior empirical evidence by Riedl and Smeets (2017) and Cao, Titman, Zhan, and Zhang (2019) suggests that socially conscious investors place lesser importance on the financial aspect of their investments.

[2]A more detailed discussion of the model is found near the end of the empirical analysis and in the Appendix.

[3]Even if these scores did accurately measure corporate social responsibility, it's not clear whether CSR is necessarily on average profit-enhancing as CSR activities may arise from

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In studying the transition from normal times to the COVID-19 period, we offer evidence in support of a different perspective to that of Albuquerque, Koskinen, Yang, and Zhang (2020), who study the phenomenon through the lens of the rating agency. In their paper, they document positive return effects associated with CSR ratings during the pandemic period but discover no corresponding effect on returns with respect to demand from socially responsible owners, which is inferred from ratings.[4] Higher-rated firms appear to have lower volatility and enjoy superior operating performance during the first quarter of 2020. Given concerns about the informational quality of opaque CSR ratings and the lack of clarity of what these ratings may be truly capturing[5], we take a 'ratings-free' approach in our analysis by directly calculating socially responsible ownership via mutual funds that identify themselves to investors as socially responsible.

agency problems (Masulis and Reza 2015).

[4]Ding, Levine, Lin, and Xie (2020) extend the analysis of stock returns to the international setting and reach similar findings of outperformance for better-rated firms. The authors interpret these results as evidence of socially responsible businesses faring better in the pandemic based on the assumption that the ratings are accurate measures of corporate social responsibility.

[5]These concerns were expressed by top financial market regulators in both the U.S. and the European Union over the lack of standards and accuracy in these ratings (Peirce 2019, Jones 2020). Based on the ratings from a leading provider, Yang (2019) offers the first evidence linking greenwashing to the inflation of environmental and social ratings via a natural experiment based on a regulatory crackdown on greenwashing by the Federal Trade Commission. Furthermore, Yang (2019) shows that controlling for greenwashing improves the quality of these ratings with respect to their ability to predict future penalties, lawsuits and negative CSR-related news. A subsequent paper by Berg, Koelbel, and Rigobon (2020) finds in addition substantial noise in the ratings across multiple providers.

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Subsequent research on the implications of socially responsible investing during the COVID-19 pandemic can be found in the works that explore themes pertaining to social responsibility and the behavior of funds. Glossner, Matos, Ramelli, and Wagner (2020) find that better corporate social responsibility characteristics did little to attract institutional investors, even ones like pension funds that tend to have long-term horizons, who instead cared more about firm leverage and cash holdings. Dottling and Kim (2020) study mutual fund flows and document substantial heterogeneity in these flows between retail investors, who appear to abandon their interest in funds with high social responsibility ratings, and institutional investors, whose interest in these funds remained steady. Independent work by Pastor and Vorsatz (2020) examines capital flows in and out of mutual funds. They find that funds with high social responsibility ratings were able to mitigate the huge outflows observed for the mutual funds in general. A closer examination by the authors reveals that this outflow mitigation effect is driven by exclusionary funds (i.e. funds with a mandate to eschew stocks of companies involved in animal cruelty, tobacco, etc.), which we study in our sample of socially conscious funds.

The rest of the paper is as follows. Section 2 introduces the data sources used in this study and explains how the variables are constructed. Their summary statistics are reported in the Appendix. Section 3 presents the empirical analysis and discusses the model, which is formally presented in the Appendix. Section 4 concludes.

2. Data and Descriptive Statistics

Our study focuses on firms that belong to the S&P 500 as of the end of 2018. Compared to other publicly traded firms, these firms are large and prominent businesses that collectively represent around 75 percent of the total market capitalization in the U.S. In line with many other studies, we drop firms that are in the financial services industry, which is identified by SIC codes that begin with the digit 6.

For information on mutual fund holdings, we gather data from the Center for Research in Security Prices (CRSP) Survivorship Bias Free U.S. Mutual Fund Database. Here, we look at funds that invest primarily in U.S. domestic equity. We exclude index funds, sector funds, funds with less than 5 million dollar in total net assets and funds with fewer than 10 stocks. Holdings data are also as of the end of 2018. For mutual funds that have multiple share classes, we combine these cases into one observation. Identification of socially conscious mutual funds comes not from ratings but from Morningstar, which collects information on what funds claim about themselves. Morningstar defines a socially conscious mutual fund in the following manner: any fund that invests according to non-economic guidelines; such funds may make investments based on such issues as environmental responsibility, human rights, or religious views. For a given stock, we calculate the fractional ownership of its shares by each mutual fund. Socially conscious mutual fund ownership at the end of 2018 is then calculated as the sum of fractional ownership by socially conscious mutual funds over the sum of fractional ownership by socially conscious mutual funds and conventional funds.[6]

Pricing data and quarterly accounting characteristics of the firm come respectively from CRSP and Compustat. Our main dependent variable of interest is the monthly stock return (in excess of the 1-month U.S. Treasury bill rate) as reported in CRSP with share codes 10 or 11. Following Lins, Servaes, and Tamayo (2017), we adopt a similar set of controls, which we winsorize at the 1 percent and 99 percent levels. The controls based on accounting

[6]Measuring our independent variable of interest at the end of 2018 is a conservative choice that mirrors Albuquerque, Koskinen, Yang, and Zhang (2020) and is analogous to Lins, Servaes, and Tamayo (2017), who use ratings at the end of 2006 to study the financial crisis that unfolded in 2008.

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information are: 1) profitability, which is operating income before depreciation over total assets; 2) long-term debt, which is divided by total assets; 3) short-term debt (i.e. debt in current liabilities), which is also divided by total assets; 4) cash (i.e. cash and marketable securities), which is scaled by total assets. The book-to-market ratio is calculated as the book value of equity over the market value of equity in the prior month. For instances where the book-to-market ratio is negative, we construct a negative book-to-market ratio dummy and set it equal to one for those cases. Accounting information is introduced with a three-month lag (e.g. operating income for the end of September 2019 is assumed to be known by January 2020). Momentum in the prior month is calculated as the buy-and-hold return over the prior 12 months excluding the prior month. Idiosyncratic risk is the standard deviation of the residual from the capital asset pricing model estimated over the prior 60 months using monthly data. The market value of equity is calculated as the price times number of shares outstanding (in millions) at the end of the month prior. Risk factor loadings based on the Fama-French three-factor model with momentum are calculated over the prior 60 months and serve as control variables for exposure to these loadings. Data on the risk-free rate and risk factors are obtained from the Kenneth French data library.

In addition to returns, we also consider monthly volatility, which is the standard deviation of daily returns in a given month. Table I reports the descriptive statistics of these variables. To study the operating performance of the firm, we examine gross profitability (i.e. revenue minus cost of goods sold over total assets), operating income (scaled by total assets), and quarterly sales growth.

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Table I

Main Descriptive Statistics

	Obs.	Mean	SD	Min.	25th Pct.	Median	75th Pct.	Max.
Excess Return	4,666	-0.013	0.108	-0.833	-0.062	0.003	0.048	0.508
Volatility	4,727	0.021	0.019	0.001	0.011	0.015	0.021	0.179
S.C. Ownership	4,727	0.152	0.124	0.003	0.066	0.116	0.207	0.649
Log Mkt. Equity	4,727	10.161	1.046	8.316	9.392	10.012	10.787	13.000
Long-term Debt	4,601	0.307	0.160	0.010	0.200	0.300	0.390	0.930
Short-term Debt	4,322	0.042	0.042	0.000	0.010	0.060	0.220	0.213
Profitability	4,587	0.039	0.020	0.010	0.020	0.030	0.050	0.110
Cash	4,715	0.113	0.134	0.000	0.030	0.060	0.150	0.620
Book-to-market	4,727	0.332	0.307	-0.088	0.121	0.251	0.464	1.543
BM Dummy	4,727	0.056	0.230	0	0	0	0	1
Momentum	4,727	0.079	0.238	-0.490	-0.083	0.089	0.239	0.691
Idiosyncratic Risk	4,666	0.062	0.022	0.030	0.045	0.056	0.075	0.136
Market Loading	4,727	0.960	0.406	0.075	0.697	0.985	1.206	2.157
Size Loading	4,727	0.061	0.439	-0.890	-0.250	0.044	0.339	1.382
Value Loading	4,727	-0.105	0.533	-1.554	-0.402	-0.097	0.212	1.254
Mom. Loading	4,727	-0.092	0.374	-1.230	-0.325	-0.054	0.170	0.758

3. Empirical Analysis

In this section, we begin our empirical analysis by investigating the effects of socially conscious mutual fund ownership on stock returns around the months leading up to and including the COVID-19 pandemic. In selecting the time window for analysis, we face several considerations. If the time window is too wide, we risk contaminating the sample with other influences that may affect the results. On the other hand, if the time window is too narrow,

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we diminish our ability to perform powerful tests. Furthermore, because the pandemic is a very recent and ongoing phenomenon as of this writing, we are currently limited to studying the first months of 2020.[7] We therefore focus our attention to monthly returns from March 2019 through March 2020 (i.e. the crisis in the U.S. in full force in March 2020 and the twelve months before it).

Formally, we start by analyzing the following specification:

$$Ret^{e}_{i,t} = \delta_1 S.C.\,Ownership_i \times Pandemic_t + \delta_2 Pandemic_t + \phi_i + \xi_{i,t}, \tag{1}$$

where $S.C.\,Ownership_i$ is the fractional socially conscious mutual fund ownership for firm i as defined earlier and $Pandemic_t$ is a dummy variable that equals to 1 if month t is March 2020 and 0 otherwise.

Table II, Column 1 presents the estimation results of Equation 1, where the dependent variable is the monthly excess return. As to be expected, δ_2, the coefficient for the pandemic dummy variable, is extremely negative with the stock market having suffered huge losses during the pandemic. The positive coefficient δ_1 indicates that, for firms with greater socially conscious mutual fund ownership, this negative impact is ameliorated. A one standard deviation increase in socially conscious mutual ownership translates to about a three percent reduction in returns loss at the monthly level.

[7]The standard research databases that we use generally make available data with a lag of several months.

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TABLE II

SOCIALLY CONSCIOUS OWNERSHIP AND STOCK RETURNS AROUND THE COVID-19 OUTBREAK IN THE U.S.

Panel A: Return	(1)	(2)	(3)
S.C. $\text{Ownership}_{\text{i}} \times \text{Pandemic}_{\text{t}}$	0.226***	0.226***	0.250***
	(0.068)	(0.068)	(0.071)
$\text{Pandemic}_{\text{t}}$	-0.220***		
	(0.014)		
Observations	4,666	4,666	4,115
Adj. R^2	0.215	0.416	0.460
Panel B: Volatility	**(1)**	**(2)**	**(3)**
S.C. $\text{Ownership}_{\text{i}} \times \text{Pandemic}_{\text{t}}$	-0.021***	-0.021***	-0.024***
	(0.009)	(0.009)	(0.008)
$\text{Pandemic}_{\text{t}}$	0.063***		
	(0.002)		
Observations	4,727	4,727	4,115
Adj. R^2	0.793	0.816	0.835
Controls?	No	No	Yes
Time FE?	No	Yes	Yes
Firm FE?	Yes	Yes	Yes

Notes. This table reports the estimation results of regressions where the dependent variables are the monthly excess returns and the monthly returns volatility over the period March 2019 - March 2020 for nonfinancial firms in the S&P 500. *S.C. Ownership*$_i$ measures the fractional mutual fund ownership of firm i by socially conscious mutual funds at the end of 2018. *Pandemic*$_t$ is equal to 1 if month t is March 2020 and 0 otherwise. Standard errors, clustered by firm, are reported in parentheses. Significance at the 10% level is given by *; at the 5% level **, at the 1% level ***.

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In the second column of Table II, we show that our core result is robust to the inclusion of time fixed effects to account for potential aggregate time trends in addition to the firm fixed effects already in place. In the third column of Table II, we introduce a host of lagged controls that describe the financial condition of the firm and its risk exposures. As defined in the previous section, these controls are long-term debt, short-term debt, profitability, cash, book-to-market ratio, a negative book-to-market dummy, idiosyncratic risk, log market equity, momentum, and risk factor loadings.[8] Again, our main finding remains unchanged. Standard errors are clustered at the firm level.

If these better returns are driven by investors who are dedicated to socially conscious investment mandates, then we should observe some reduction in stock return volatility as these investors would attenuate the overall panic and the rush to sell that ensued from the pandemic. In the first column of Panel B in Table II, we analyze Equation 1 but for monthly volatility as the dependent variable. Indeed, while the pandemic itself greatly increased stock return volatility, the presence of socially conscious mutual fund investors weakened it for socially preferred firms. A one standard deviation increase in socially conscious mutual fund ownership translates on average into nearly one-fifth of a standard deviation decrease in volatility. Columns 2 and 3 in Panel B of Table II show that reduction in volatility from higher socially conscious mutual fund ownership holds after including time fixed effects and same set of controls.

[8]In the Appendix, in relation to Albuquerque, Koskinen, Yang, and Zhang (2020), we find qualitatively similar effects on returns (and operating performance) from the CSR ratings measured in 2018 for these large S&P 500 firms in our sample. Consistent with the pattern of results found in Glossner, Matos, Ramelli, and Wagner (2020), the magnitude of coefficient estimates tends to be weaker for S&P 500 firms. Our results are robust to controlling for these ratings, which may identify a separate phenomenon relevant to smaller firms outside of the S&P 500.

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To the extent that we may be concerned with higher socially conscious mutual fund ownership being already on an upward path with predicting superior returns immediately before the pandemic, we consider the following dynamic version of Equation 1 with the full set of lagged controls and fixed effects over the same time window:

$$Ret^e_{i,t} = \sum_t \delta_t S.C.\,Ownership_i \times 1_t + X_{i,t-1} + \gamma_t + \phi_i + \xi_{i,t}, \tag{2}$$

where 1_t (omitting March 2019) is equal to 1 if the month return is in month t and zero otherwise.

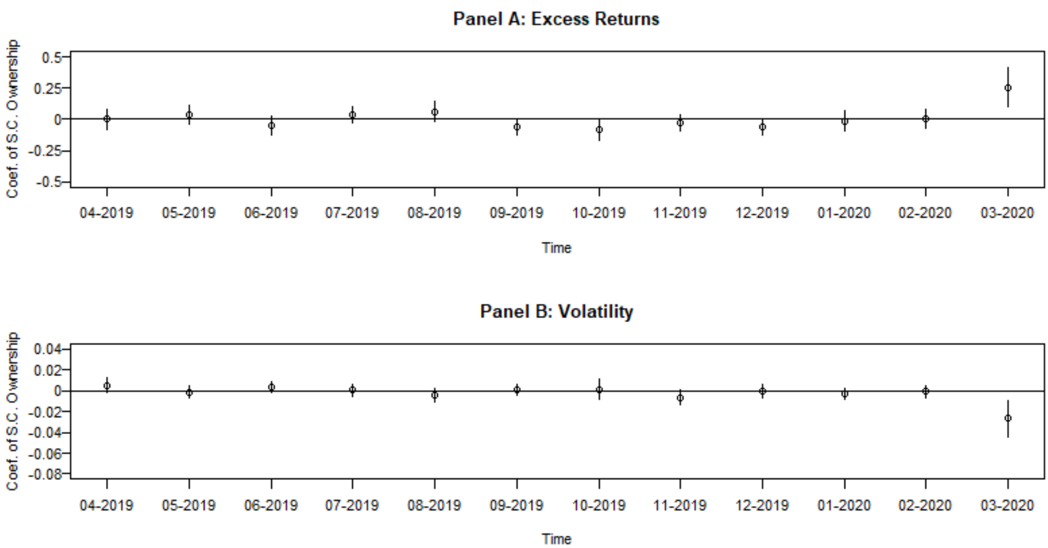

Figure 1

Dynamic Effects of Socially Conscious Mutual Fund Ownership

This figure plots the dynamic impact of socially conscious mutual fund ownership on monthly excess stock returns (Panel A) and monthly stock return volatility (Panel B) during the COVID-19 period and the months immediately leading up to it. The vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

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Panel A of Figure 1 plots the coefficients δ_t. Stocks with higher socially conscious mutual fund ownership were clearly not gaining with respect to returns prior to the pandemic coming into full force in March 2020, which then generated higher returns for these stocks. In Panel B of Figure 1, we apply the same specification in Equation 2 to our monthly volatility variable. We find that stocks with higher socially responsible mutual fund ownership experienced a sharp decrease in volatility during this pandemic period and not before.

To better understand the mechanism behind the difference in returns performance, we plot book-to-market ratios against socially conscious ownership for each month leading up to the pandemic in our sample in Figure 2.[9]

Panel A of Figure 2 shows firms with greater socially conscious mutual fund ownership enjoy higher market valuations. The other panels of the figure show that the negative relationship between socially conscious mutual fund ownership and book-to-market ratio is consistent throughout the subsequent months in our sample. In the last panel of Figure 2, we see a clear rise in the book-to-market ratios in the left half of the scatter plot as firms with low socially conscious ownership experience a decline in their market valuations. The right half of the same scatter plot reveals that the book-to-market ratios for firms with high socially conscious ownership are relatively stable.

[9]For brevity, we omit displaying scatterplots for months before August 2019 in our sample. We note that the scatterplots for those months look very similar to the one in August 2019. Figure 2 excludes negative book-to-market ratios in the plotting though we note that the overall patterns remain unaffected if we do include them.

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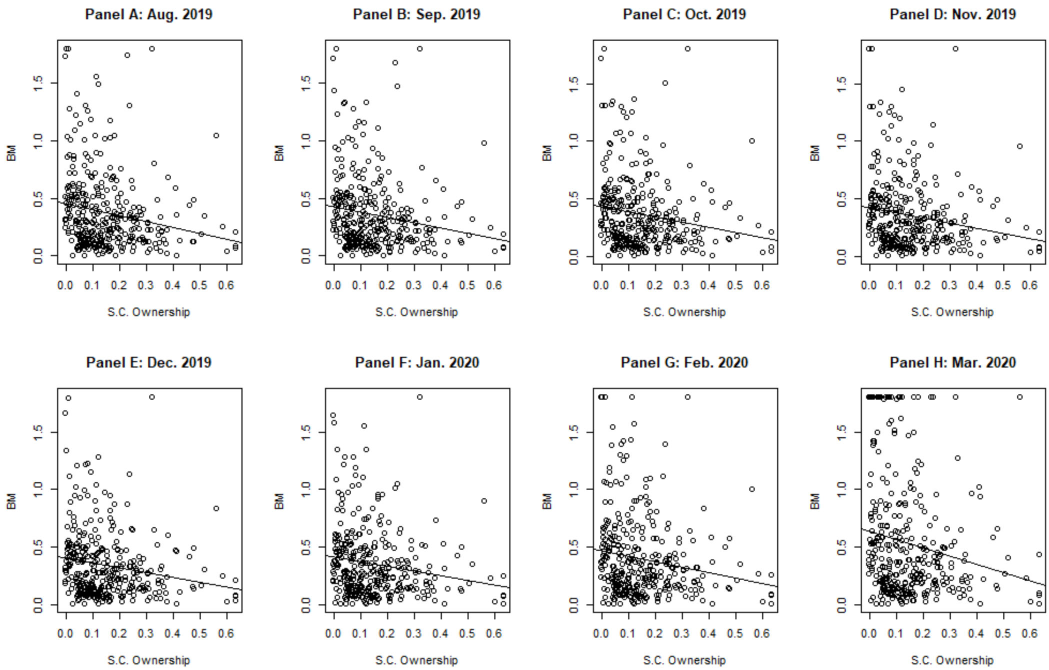

FIGURE 2

Book-to-market Ratio (BM) vs. Socially Conscious (S.C.) Mutual Fund Ownership

This figure plots the book-to-market ratios measured at the end of each month against socially conscious mutual fund ownership in December 2018 from August 2019 through March 2020. Each data point corresponds to an individual firm. Negative BM ratios are excluded. Data variables are winsorized at the 1% percent and 99% percentiles.

Table III evaluates our finding from the graphical illustration of Figure 2 in a specification similar to Equation 1 but where the dependent variable is instead the book-to-market ratio. In the first column of Table III, there is a clear increase in the book-to-market ratio as a whole in March 2020. However, for firms with greater ownership by socially conscious mutual funds, the hit on their market valuations is tempered. For a one standard deviation increase in socially conscious mutual fund ownership, there is a little less than one-fifth of a standard deviation decrease in the book-to-market ratio. The effects remain robust to the introduction of time fixed effects and lagged controls (i.e. long-term debt, short-term debt, profitability, cash, idiosyncratic risk, log market equity, momentum, and risk factor loadings).

Table III

Socially Conscious Ownership and Market Valuation Around the COVID-19 Outbreak in the U.S.

	(1)	(2)	(3)
	Book-to-market ratio$_{i,t}$		
S.C. Ownership$_i$ × Pandemic$_t$	-0.320***	-0.321***	-0.353***
	(0.101)	(0.101)	(0.083)
Pandemic$_t$	0.220***		
	(0.024)		
Controls?	No	No	Yes
Time FE?	No	Yes	Yes
Firm FE?	Yes	Yes	Yes
Observations	4,462	4,462	3,905
Adj. R^2	0.909	0.911	0.928

Notes. This table reports the estimation results of regressions where the dependent variable is the monthly book-to-market ratio over the period March 2019 - March 2020 for nonfinancial firms in the S&P 500. *S.C. Ownership$_i$* measures the fractional mutual fund ownership of firm i by socially conscious mutual funds at the end of 2018. *Pandemic$_t$* is equal to 1 if month t is March 2020 and 0 otherwise. Negative BM ratios are excluded. Standard errors, clustered by firm, are reported in parentheses. Significance at the 10% level is given by *; at the 5% level **, at the 1% level ***.

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To see if stocks preferred by socially conscious mutual funds belong to companies that fared better during the pandemic, we examine the operating performance as reported in March 2020 and the three quarters before it in a similar time window as the above analysis. Here, we consider sales growth, gross profitability, and operating income, which are winsorized at the 1 percent and 99 percent levels, with their summary statistics found in Table IV and we estimate the following specification:

$$Performance_{i,q} = \delta_1 S.C.\,Ownership_i \times 2020Q1_q + \delta_2 S.C.\,Ownership_i + \delta_3 2020Q1_q + \xi_{i,q}, \quad (3)$$

where $Performance_{i,q}$ is the operating performance for firm i in quarter q and $2020Q1_q$ is a dummy variable that is equal to 1 if q is the first quarter of 2020 and zero otherwise. To ensure the timing of the measurement window for each firm is consistent and covers March 2020, we exclude firms whose fiscal quarters do not end in March 2020.[10]

TABLE IV

DESCRIPTIVE STATISTICS FOR OPERATING PERFORMANCE ANALYSIS

	Obs.	Mean	Std. Dev.	Min.	25th Pct.	Median	75th Pct.	Max.
Sales Growth	1,212	0.009	0.131	-0.347	-0.050	0.008	0.063	0.492
Gross Profit.	1,208	0.069	0.044	-0.029	0.037	0.060	0.095	0.202
Op. Income	1,185	0.035	0.021	-0.041	0.022	0.032	0.045	0.096

Table V reports the estimation results of Equation 3 in Column 1 across all three key operating performance benchmarks in Panels A, B, and C. For brevity, we display the main coefficients of interest. During the first quarter of 2020, firms suffered a decline in their business. In particular, sales growth saw a precipitous drop of over 11 percent. Unlike pre-

[10]These non-March reporting firms account for 61 firms out of the 364 firms in our sample.

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viously, firms with greater ownership by socially conscious mutual funds appear to perform no better. Upon the inclusion of quarterly fixed effects and three-digit SIC industry fixed effects (and one-quarter lag controls, i.e. long-term debt, short-term debt, cash, and log total assets), no difference is to be seen right before the pandemic either.[11]

To explain these empirical results in a theoretical setting, we introduce an asset pricing model in which there are two groups of investors who provide funding to firms with projects that have identical financing and financial payoff characteristics. In the first group are investors who are conventional, or socially neutral, in that they only care about the financial payoff. In the second group are investors who are socially conscious in the sense that they obtain utility from holding shares belonging to a firm they deem to be of the good type (as opposed to of the bad type). As these socially conscious investors are using financial investments to satisfy a social responsibility agenda or mandate, they may furthermore care somewhat less about the financial payoff of the good type firm.

[11]For robustness, in a similar approach to Yu (2011), we consider investor expectations about the future prospects of individual firms by examining the monthly median analyst forecasts of long-term growth rates for earnings per share in the Appendix. These forecasts, also winsorized at the 1 percent and 99 percent levels, come from I/B/E/S and average around 0.097 with a standard deviation of 0.093 in our sample. The firm controls here are also long-term debt, short-term debt, cash, and size. In line with our operating performance analysis, these forecasts declined during the crash of March 2020 but exhibited no difference with respect to greater ownership by socially conscious mutual funds.

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Table V

Operating Performance Analysis

Panel A: Sales Growth	(1)	(2)	(3)
S.C. Ownership$_i$ × 2020Q1$_q$	0.008	0.008	0.004
	(0.082)	(0.082)	(0.084)
S.C. Ownership$_i$	0.007	-0.003	0.003
	(0.035)	(0.038)	(0.038)
2020Q1$_q$	-0.114***		
	(0.018)		
Panel B: Gross Profitability	**(1)**	**(2)**	**(3)**
S.C. Ownership$_i$ × 2020Q1$_q$	-0.001	-0.001	0.003
	(0.010)	(0.010)	(0.010)
S.C. Ownership$_i$	0.045***	-0.001	0.007
	(0.016)	(0.010)	(0.010)
2020Q1$_q$	-0.010***		
	(0.003)		
Panel C: Operating Income	**(1)**	**(2)**	**(3)**
S.C. Ownership$_i$ × 2020Q1$_q$	0.008	0.008	0.010
	(0.010)	(0.010)	(0.010)
S.C. Ownership$_i$	0.017**	0.008	0.010
	(0.008)	(0.010)	(0.009)
2020Q1$_q$	-0.010***		
	(0.003)		
Controls?	No	No	Yes
Time FE?	No	Yes	Yes
Industry FE?	No	Yes	Yes

Notes. This table reports the estimation results of regressions where the dependent variables are key operating performance benchmarks as reported in March 2020 and the three preceding quarters for nonfinancial firms in the S&P 500. Standard errors, clustered by three-digit SIC industry codes, are reported in parentheses. Significance at the 10% level is given by *; at the 5% level **, at the 1% level ***.

In the Appendix, we derive the equilibrium prices of the good and bad type firms. As asset prices reflect not only financial information but also social preferences, the price of the good type firm receives a boost (relative to that of the bad type) from the benefit in utility the good type firm shares provide to socially conscious investors. In a numerical simulation of these prices under conditions where firms experience a common decline in business amid increased uncertainty, we see that the good type firm enjoys better stock returns and lower returns volatility compared to the bad type in the case where the financial payoffs of the good and bad type firms matter equally to socially conscious investors (see Table A.1 in the Appendix). This, however, falls short as it predicts that the prices of the good and bad type firm shares drop by the same amount, which is not consistent with the empirical findings above. On the other hand, if socially conscious investors were to care less about the financial payoff of the good type firm, this qualitative gap between the model and the empirical evidence vanishes. A simulation of the model now produces the same pattern with superior stock returns and lower returns volatility for the good type firm as well as showing that the price of the good type firm shares dropped less relative to the price of the bad type firm shares.

Put together, the evidence points to a scenario where the very act of investing with accordance to social preferences can generate higher returns during a sudden crash. By devoting part of their investment objective to social considerations, socially conscious investors boost market valuations in the firms they prefer and help maintain these valuations even in a crisis as stock prices reflect not just financial information but also social preferences. These socially preferred stocks in turn experience lower volatility relative to the rest as the market tumbles. These results do not depend on socially preferred firms having stronger operating performance, which plays an important role in the explanations in other studies (e.g. Lins, Servaes, and Tamayo (2017), Albuquerque, Koskinen, Yang, and Zhang (2020)) that attribute superior returns to better businesses.

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4. Concluding Remarks

This paper investigates how demand from socially conscious investors can affect stock returns as the economy suddenly found itself in the middle of a sudden crisis in March 2020. Because stock prices incorporate information about investors social preferences (i.e. preference for socially preferred firm shares and a weaker emphasis on financial payoffs), socially preferred firms experienced a weaker reduction in market valuations and better returns as other firms saw their valuations and returns plummet in the midst of higher volatility in their stocks. Social preferences do not appear to guide socially conscious investors towards firms with better operating performance during this crisis. These results point to how social preference-driven distortions to market valuations that make socially conscious investments sacrifice returns during normal times can also generate a reversal of fortune during bad times.

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Appendix

Section A: Theoretical

To provide a theoretical foundation for the empirical analysis, we present a simple model of asset prices in the presence of investors with social preferences and simulate the impact of a general decline in business on these prices.

Suppose there are two types (i.e. g for good and b for bad) of firms. Each type has a project that requires financing amount F and generates a financial payoff normally distributed with mean μ_c and σ_c. Assume $\mu_c > F$. The financial payoff for each project is divided among N total number of shares. Financing is obtained by issuing N_g and N_b shares at prices P_g and P_b to I number of investors, each of whom displays constant absolute risk aversion (CARA) with with risk aversion A and belongs to one of two groups. In the first group are I_C number of conventional (i.e. socially neutral) investors who care only about financial payoffs. In the second group are I_R number of socially conscious investors who care about financial payoffs but also derive utility from holding shares of firms whose businesses they consider virtuous. Specifically, these investors assign $\delta \in (0, 1]$ as the degree of importance they place on the financial payoff of the good firm and enjoy γ from investing in the shares of the good firm.

Let $x_{C,g}$, $x_{C,b}$, $x_{R,g}$, and $x_{R,b}$ be the investor allocations into good and bad firms. From these allocations, the socially conscious investor obtains the utility

$$U_R = \frac{x_{R,g}}{N}(\delta\mu_c + \gamma) + \frac{x_{R,b}}{N}\mu_c - \frac{A}{2}\left((\frac{x_{R,g}}{N})^2\sigma_c^2 + (\frac{x_{R,b}}{N})^2\sigma_c^2 + 2(\frac{x_{R,g}}{N})(\frac{x_{R,b}}{N})\sigma_c\right) - x_{R,g}P_g - x_{R,b}P_b \tag{A.1}$$

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and the socially neutral investor obtains the utility

$$U_C = \frac{x_{C,g}}{N}\mu_c + \frac{x_{C,b}}{N}\mu_c - \frac{A}{2}\Bigg((\frac{x_{C,g}}{N})^2\sigma_c^2 + (\frac{x_{C,b}}{N})^2\sigma_c^2 + 2(\frac{x_{C,g}}{N})(\frac{x_{C,b}}{N})\sigma_c\Bigg) - x_{C,g}P_g - x_{C,b}P_b. \tag{A.2}$$

The market clearing conditions are then

$$I_C x_{C,g} + I_R x_{R,g} = N_g \tag{A.3}$$

$$I_C x_{C,b} + I_R x_{R,b} = N_b. \tag{A.4}$$

Taking first-order conditions of investor utilities with respect to their portfolio allocations and applying the market clearing conditions, we obtain the following for the prices for the good and bad firms are

$$P_g = \frac{I_R}{I}\frac{\delta\mu_c}{N} + \frac{I_N}{I}\frac{\mu_c}{N} + \frac{I_R}{I}\frac{\gamma}{N} - \frac{A}{N^2 I}(N_g\sigma_c^2 + N_b\sigma_c) \tag{A.5}$$

$$P_b = \frac{\mu_c}{N} - \frac{A}{N^2 I}(N_b\sigma_c^2 + N_g\sigma_c). \tag{A.6}$$

To see how these prices behave in a scenario where business deteriorates as a result of, for instance, a pandemic, we simulate the parameters μ_c and σ_c using the following processes: $\mu_{c,t} = -1 + \mu_{c,t-1} + \epsilon_t$ and $\sigma_{c,t}^2 = \sigma_{c,t-1}^2 + \xi_t^2$. ϵ_t and ξ_t are independent and normally distributed with means zero and variances m_1 and m_2.

Here, we are interested in the percentage returns of the firms (measured from $t = 1$

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through $t = 100$), their difference, the difference in the volatility of returns (i.e. returns calculated at each time step), the change in their share prices (i.e. final price minus initial price), and the relative amount of change in their share prices. The qualitative results are reported in Table A.1. '+/-/0' indicates a positive/negative/zero value.

Table A.1

Numerical Simulations of the Impact on Asset Prices

	$\mathrm{Ret_g}$	$\mathrm{Ret_b}$	$\mathrm{Ret_g} - \mathrm{Ret_b}$	$\mathrm{Vol_g} - \mathrm{Vol_b}$	$\Delta\mathrm{P_g}$	$\Delta\mathrm{P_b}$	$\Delta\mathrm{P_g} - \Delta\mathrm{P_b}$
$\delta = 1$	-	-	+	-	-	-	0
$\delta < 1$	-	-	+	-	-	-	-

Note: In these simulations, we use the following: $m_1 = 25$ and $m_2 = 1$ with initial values $\mu_{c,1} = 250$ and $\sigma^2_{c,1} = 5$ for $t = 2, ..., 100$. For the other parameter values, we set $F = 3$, $N = 100$, $I_C = 85$, $I_R = 15$, $A = 0.01$, and $\gamma = 200$. In the case of $\delta < 1$, we set $\delta = 0.7$. To check for consistency, we repeat the simulation 50 times.

As a simple example, if the good type firm's stock has a price of \$150 and the bad type firm's stock has a price of \$100, then a stock price drop of \$50 due to a common decline in business for both firms will result -33.3% decline for the good type and -50% decline for the bad type. If the financial payoff of the good type matters less to socially conscious investors, then the price drop of the good type firm will be less than \$50 (and hence the percentage decline for the good type firm is even smaller).

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Section B: Empirical

In this empirical section of the appendix, we first report our results for the analyst forecasts of the long-term growth rate of earnings per share. We then perform our analysis of stock returns with 2018 Corporate Social Responsibility (CSR) ratings (scaled by 100 so the range is 0 to 1) from Thomson Reuters (as studied in Albuquerque, Koskinen, Yang, and Zhang (2020)) and investigate the effects documented in the above analysis upon controlling for these ratings. Following Albuquerque, Koskinen, Yang, and Zhang (2020), we calculate CSR as the average of the environmental and the social scores. The ratings have a mean of 0.574 and a standard deviation of 0.219.

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Table B.1

S. C. Ownership and Long-term Growth Around the COVID-19 Outbreak in the U.S.

	(1)	(2)	(3)
	Long-term Growth$_{i,t}$		
S.C. Ownership$_i$ × Pandemic$_t$	0.031	0.029	0.026
	(0.030)	(0.030)	(0.030)
Pandemic$_t$	-0.020***		
	(0.006)		
Controls?	No	No	Yes
Time FE?	No	Yes	Yes
Firm FE?	Yes	Yes	Yes
Observations	4,249	4,249	3,843
Adj. R^2	0.793	0.807	0.809

Notes. This table reports the estimation results of regressions where the dependent variable is the monthly median analyst forecast of the long-term growth rate for earnings per share over the period March 2019 - March 2020 for nonfinancial firms in the S&P 500. *S.C. Ownership*$_i$ measures the fractional mutual fund ownership of firm i by socially conscious mutual funds at the end of 2018. *Pandemic*$_t$ is equal to 1 if month t is March 2020 and 0 otherwise. Standard errors, clustered by firm, are reported in parentheses. Significance at the 10% level is given by *; at the 5% level **, at the 1% level ***.

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Table B.2

CSR and Stock Returns Around the COVID-19 Outbreak in the U.S.

	(1)	(2)	(3)
	$\text{Return}^{e}_{i,t}$		
CSR $\text{Rating}_i \times \text{Pandemic}_t$	0.085**	0.085**	0.065
	(0.041)	(0.041)	(0.042)
Pandemic_t	-0.236***		
	(0.025)		
Controls?	No	No	Yes
Time FE?	No	Yes	Yes
Firm FE?	Yes	Yes	Yes
Observations	4,497	4,497	3,963
Adj. R^2	0.213	0.413	0.455

Notes. This table reports the estimation results of regressions where the dependent variable is the monthly excess return over the period March 2019 - March 2020 for nonfinancial firms in the S&P 500. *CSR* $Rating_i$ is the 2018 Corporate Social Responsibility rating of firm i. $Pandemic_t$ is equal to 1 if month t is March 2020 and 0 otherwise. Standard errors, clustered by firm, are reported in parentheses. Significance at the 10% level is given by *; at the 5% level **, at the 1% level ***.

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Table B.3

Socially Conscious Ownership and Stock Returns Around the COVID-19 Outbreak in the U.S.

Panel A: Return	(1)	(2)	(3)
S.C. $\text{Ownership}_\text{i} \times \text{Pandemic}_\text{t}$	0.219***	0.219***	0.242***
	(0.070)	(0.070)	(0.071)
CSR $\text{Rating}_\text{i} \times \text{Pandemic}_\text{t}$	0.076*	0.076*	0.054
	(0.041)	(0.041)	(0.042)
Pandemic_t	-0.264***		
	(0.026)		
Observations	4,497	4,497	3,963
Adj. R^2	0.218	0.417	0.461
Panel B: Volatility	**(1)**	**(2)**	**(3)**
S.C. $\text{Ownership}_\text{i} \times \text{Pandemic}_\text{t}$	-0.020***	-0.020***	-0.023***
	(0.009)	(0.009)	(0.008)
CSR $\text{Rating}_\text{i} \times \text{Pandemic}_\text{t}$	-0.005	-0.005	-0.003
	(0.006)	(0.006)	(0.006)
Pandemic_t	0.066***		
	(0.004)		
Observations	4,558	4,558	3,963
Adj. R^2	0.795	0.817	0.836
Controls?	No	No	Yes
Time FE?	No	Yes	Yes
Firm FE?	Yes	Yes	Yes

Notes. This table reports the estimation results of regressions where the dependent variables are the monthly excess returns and the monthly returns volatility over the period March 2019 - March 2020 for nonfinancial firms in the S&P 500. $S.C.\ Ownership_i$ measures the fractional mutual fund ownership of firm i by socially conscious mutual funds at the end of 2018. $CSR\ Rating_i$ is the 2018 Corporate Social Responsibility rating of firm i. $Pandemic_t$ is equal to 1 if month t is March 2020 and 0 otherwise. Standard errors, clustered by firm, are reported in parentheses. Significance at the 10% level is given by *; at the 5% level **, at the 1% level ***.

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Table B.4

Socially Conscious Ownership and Market Valuation Around the COVID-19 Outbreak in the U.S.

	(1)	(2)	(3)
	Book-to-market ratio$_{\mathrm{i,t}}$		
S.C. Ownership$_{\mathrm{i}}$ × Pandemic$_{\mathrm{t}}$	-0.318***	-0.319***	-0.359***
	(0.104)	(0.104)	(0.085)
CSR Rating$_{\mathrm{i}}$ × Pandemic$_{\mathrm{t}}$	-0.064	-0.064	-0.027
	(0.075)	(0.075)	(0.075)
Pandemic$_{\mathrm{t}}$	0.259***		
	(0.048)		
Controls?	No	No	Yes
Time FE?	No	Yes	Yes
Firm FE?	Yes	Yes	Yes
Observations	4,306	4,306	3,762
Adj. R^2	0.908	0.911	0.928

Notes. This table reports the estimation results of regressions where the dependent variable is the monthly book-to-market ratio over the period March 2019 - March 2020 for nonfinancial firms in the S&P 500. *S.C. Ownership*$_i$ measures the fractional mutual fund ownership of firm i by socially conscious mutual funds at the end of 2018. *CSR Rating*$_i$ is the 2018 Corporate Social Responsibility rating of firm i. *Pandemic*$_t$ is equal to 1 if month t is March 2020 and 0 otherwise. Negative BM ratios are excluded. Standard errors, clustered by firm, are reported in parentheses. Significance at the 10% level is given by *; at the 5% level **, at the 1% level ***.

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Table B.5

Operating Performance Analysis

Panel A: Sales Growth	(1)	(2)	(3)
S.C. $\text{Ownership}_\text{i} \times 2020\text{Q1}_\text{q}$	0.004	0.004	-0.001
	(0.086)	(0.086)	(0.087)
CSR $\text{Rating}_\text{i} \times 2020\text{Q1}_\text{q}$	0.032	0.032	0.037
	(0.041)	(0.041)	(0.041)
2020Q1_q	-0.131***		
	(0.027)		
Panel B: Gross Profitability	**(1)**	**(2)**	**(3)**
S.C. $\text{Ownership}_\text{i} \times 2020\text{Q1}_\text{q}$	-0.001	-0.001	0.004
	(0.010)	(0.010)	(0.011)
CSR $\text{Rating}_\text{i} \times 2020\text{Q1}_\text{q}$	0.008*	0.008*	0.007
	(0.004)	(0.004)	(0.005)
2020Q1_q	-0.014***		
	(0.004)		
Panel C: Operating Income	**(1)**	**(2)**	**(3)**
S.C. $\text{Ownership}_\text{i} \times 2020\text{Q1}_\text{q}$	0.007	0.009	0.009
	(0.010)	(0.010)	(0.011)
CSR $\text{Rating}_\text{i} \times 2020\text{Q1}_\text{q}$	0.008*	0.008*	0.008
	(0.004)	(0.004)	(0.005)
2020Q1_q	-0.014***		
	(0.004)		
Controls?	No	No	Yes
Time FE?	No	Yes	Yes
Industry FE?	No	Yes	Yes

Notes. This table reports the estimation results of regressions where the dependent variables are key operating performance benchmarks as reported in March 2020 and the three preceding quarters for nonfinancial firms in the S&P 500. For brevity, *S.C. Ownership*$_i$ and *CSR Rating*$_i$ are not displayed. Standard errors, clustered by three-digit SIC codes, are reported in parentheses. Significance at the 10% level is given by *; at the 5% level **, at the 1% level ***.

TABLE B.6

S. C. OWNERSHIP AND LONG-TERM GROWTH
AROUND THE COVID-19 OUTBREAK IN THE U.S.

	(1)	(2)	(3)
	Long-term $\mathrm{Growth}_{\mathrm{i,t}}$		
S.C. $\mathrm{Ownership}_{\mathrm{i}}$ × $\mathrm{Pandemic}_{\mathrm{t}}$	0.032	0.031	0.027
	(0.031)	(0.031)	(0.031)
CSR $\mathrm{Rating}_{\mathrm{i}}$ × $\mathrm{Pandemic}_{\mathrm{t}}$	0.025	0.026	0.024
	(0.018)	(0.018)	(0.018)
$\mathrm{Pandemic}_{\mathrm{t}}$	-0.036***		
	(0.010)		
Controls?	No	No	Yes
Time FE?	No	Yes	Yes
Firm FE?	Yes	Yes	Yes
Observations	4,092	4,092	3,697
Adj. R^2	0.793	0.807	0.809

Notes. This table reports the estimation results of regressions where the dependent variable is the monthly median analyst forecast of the long-term growth rate for earnings per share over the period March 2019 - March 2020 for nonfinancial firms in the S&P 500. $S.C.\ Ownership_i$ measures the fractional mutual fund ownership of firm i by socially conscious mutual funds at the end of 2018. $Pandemic_t$ is equal to 1 if month t is March 2020 and 0 otherwise. Standard errors, clustered by firm, are reported in parentheses. Significance at the 10% level is given by *; at the 5% level **, at the 1% level ***.

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